ECOLOGICAL NICHE PICKING: ECOLOGICAL INVALIDITY AS AN AXIOM OF EXPERIMENTAL COGNITIVE PSYCHOLOGY

MICHAEL COLE, LOIS HOOD, RAY McDERMOTT
The Rockefeller University

LCHC
AND
ICHD

Laboratory of Comparative Human Cognition

AND

Institute for Comparative Human Development

The Rockefeller University

Ecological Niche Picking:

Ecological Invalidity as an Axiom

of Experimental Cognitive Psychology

Michael Cole

Lois Hood

Raymond McDermott

The Rockefeller University

Preparation of this manuscript and the research on which it is based were supported by a grant from the Carnegie Corporation. Michael Cole's present address is Communications Program, University of California, La Jolla, Calif.

Abstract

The current state of theory in cognitive psychology is too weak a base to provide for principled means of making inferences from test and laboratory-based observations to the wide variety of intellectual behavior observed in non-laboratory settings (everyday life). A review of cognitive research programs reveals several plausible speculations about thinking in everyday life based on laboratory research and theory. Descriptions of several everyday-life scenes drawn from our research with a group of children show these speculations to be plausible only if our descriptions and interpretations remain within the constraints of the model systems from which they were derived. But such models systematically suppress or exclude basic principles that our analysis suggests are fundamental to the organization of behavior, particularly the dynamically organized influence of individuals on their environment. We conclude that current method and theory of cognitive psychology are invalid for the non-laboratory settings to which many researchers wish to generalize. The need for developing alternative methods for describing scenes or task environments which people encounter in everyday life is emphasized.

As part of a tradition that reaches back to the beginnings of psychology as a distinct science, psychologists have been concerned with the relation between the behaviors they observe and study under tightly controlled laboratory conditions and the wider range of behaviors that make up the repertoire of human beings (Dilthey, 1883; Luria, 1932; Wundt, 1916).

In recent years this discussion as it relates to intellectual life has taken four conspicuous forms. In connection with the continuing debates over IQ and ethnicity, there have been claims that laboratory-style tests of intellectual performance systematically misrepresent the intellectual abilities of certain population groups (Brace, Gamble and Bond, 1971; Cicourel, Jennings, Jennings, Leiter, MacKay, Mehan and Roth, 1974; Labov, 1970). There has been analogous criticism of laboratory studies as the data base for policy making about the lives of children. Laboratory procedures as models of nonlaboratory environments for behaving have been repeatedly criticized by those who advocate an ecological psychology (Barker, 1968; Bronfenbrenner, 1976, 1977; Neisser, 1976a) and in discussions about the relations between field and laboratory research (Parke, 1976; Willems, 1973, 1977; Weisz, 1978). Next, there have been increasing attempts to extend laboratory-based theories to behavior that is difficult to elicit in laboratory settings, but which is of obvious relevance to human thought and action (Caroll and Payne, 1976). Finally, there has been a growth of what are called cognitive inquiries in other disciplines--anthropology and sociology in particular--which have specifically rejected experimental techniques as inappropriate to the study of how people behave in their everyday lives.

In this paper we will argue that the current state of theory in cognitive psychology is indeed too weak a base to provide for principled means of making inferences from test and laboratory-based observations to the wide variety of intellectual behavior observed in non-laboratory settings (everyday life). In making this argument, we will not focus on the strengths or weaknesses of cognitive theories for which they were designed, namely, laboratory experiments. Sufficient discussion of the state of this art can be found in Estes (1975 - 1978). Nor will we take exception to experimentation as such. Experimentally derived models of psychological activity will continue to inform us about the possible parameters of the organization of behavior in laboratory settings. But we <u>will</u> argue that if laboratory models preclude the operation of principles essential to the organization of behavior in non-laboratory environments, theories and data derived from the laboratory cannot be used as a basis for predictions about the behavior of individuals once they leave the laboratory.

We are making such arguments because our own self-conscious attempts to contrast laboratory and non-laboratory settings where individuals engage in remembering, thinking, and attending activities suggest that important principles operating outside the laboratory are missing from current experimental procedures, and consequently, from current cognitive theories. In so far as our observations are correct, they provide the basis for our suggestion that ecological invalidity is an axiom (albeit an implicit axiom-in-practice) of current cognitive psychology.

Our discussion will proceed as follows: First, we describe how the evolution of our own research forced us to confront these issues. Next, we will review discussions of "ecological validity" which historically have been the dominant focus of attempts to formulate a productive interplay between laboratory and non-laboratory research settings. We then summarize some speculations about thinking in everyday life based on laboratory studies and some experimental inquiries which bear on these speculations. Our review suggests that so long as the laboratory-based descriptive apparatus frames our observations of everyday life cognition, psychological speculations about differences between intellectual activity in and out of the laboratory remain plausible. We next describe a number of non-laboratory examples of cognitive activity which, using these same procedures of analysis, appear consistent with the laboratory-based speculations. We will then criticize our own descriptions and the speculations they seemed to support, arguing that by restricting their terms to those provided by the laboratory cognitive tasks from which they were drawn, we are forced to ignore important principles of behavior, with the consequence that we are unable to apply knowledge gained in the laboratory to other settings. Our own analyses of non-laboratory settings for thinking have forced us to recognize that such environments are dynamically organized in ways that are represented neither in extant laboratory methods nor in contemporary cognitive theory.

We conclude with a discussion of the alternative responses that such a recognition leaves open to psychologists interested in the detailed analysis of individual behavior. To preshadow that conclusion as an aid to following our argument, we can summarize it here as follows:

1. Analysis of any behavior should begin with a descriptive analysis of at least one real world scene, the properties of which the psychologist may intend to model in an experiment. In our view, descriptive analysis is by no means a casual matter, but a serious and difficult problem. Further, experiments represent a subclass of real world scenes and are subject to the same descriptive requirement as any other setting.

2. The goal of both descriptive and experimental analysis should be a closed system account of behavior in which relevant stimuli, relevant responses, and their interactions are exhaustively specified.

3. If one decides that the behavior under observation can be modelled in an experimental setting, and if one is ultimately interested in generalizing back to the context from which it was derived, then the structure of the experiment must permit the operation of the basic principles that organize behavior in the real world setting(s) being modelled. Experiments which are not models in this sense cannot be used to make assertions back to other settings.

4. The successful experiment, following this approach, offers a closed system model of real world settings and an hypothesis about its essential organizing principles. Verification can only come from well described everyday life scenes in which the experimentally validated behavioral processes can be shown to be at work.

5. We must rely on observational techniques to inform us of missing key ingredients when current experimental technology does not allow a reasonable experimental modelof an everyday cognitive activity.

6. We must develop techniques for building descriptions of those scenes which approximate as closely as possible the level of detail for a psychological description of the important principles at work: important information of relevance to cognitive psychology is recoverable in so far as systematic description can produce replicable, closed system descriptions of an individual's behavior in non-experimental contexts.

Background Dilemmas

Cross-cultural studies

One point of entry into the discussion of laboratory and everyday life contexts for thinking was our attempt to specify the nature and origin of cultural differences in cognitive performance, especially differences associated with different amounts of formal education among rural, largely agricultural populations (c.f. Cole, Gay, Glick and Sharp, 1971; Sharp, Cole and Lave, 1979). For a variety of cognitive tasks (short term location recall, free recall, free association, syllogistic reasoning), performance varied as a function of years of formal schooling rather than the age of subjects. Even with allowances for selection artifacts and careful efforts to equate stimulus familiarity, motivation and comprehension of instructions, differences between schooled and unschooled populations were of sufficient magnitude to suggest that schooled subjects employed more powerful, flexible, and efficient ways of remembering and thinking than their unschooled counterparts.

On the basis of similar observations, many investigators have suggested that such performance differences may be more than a school-specific achievement. In a landmark article, Greenfield and Bruner (1968) suggested that schooling may be a prerequisite to the development of certain logical operations and classification skills. Brown (1976) asserted that "much of what we regard as the 'normal' course of development is, if not actually the outcome of formal schooling, at least greatly influenced by the process" (p.13). (See Scribner and Cole, 1973, for a summary and recent evidence compatible with this line of interpretation).

However, from the beginning of our research, we have been dubious about the strength of such inferences. Our skepticism has two sources, both of which arise from considering the relation between experimental tasks and non-experimental contexts for learning and thinking. First, our observations of unschooled people conducting their normal affairs were sometimes difficult to understand unless we assumed that they possess intellectual abilities that they had failed to manifest in our experiments. Our observations were supported, although not as rigorously as we would have liked, by ethnographic accounts of cognitive achievements in other cultures, e.g. in agriculture (Conklin, 1975) and navigation (Lewis, 1972), and various natural history accounts of the complexity of communicative events in which children engage in our own culture (Hood, 1977; McDermott, 1976). Second, we found isolated cases in which people's level of performance was markedly improved when we carried out experimental manipulations modeled on ethnographic observation (Cole et al., 1971). In both cases, the conclusion to which we were driven was that standard experimental tasks are somehow unrepresentative of the way people routinely encounter intellectual demands.

But when we thought about ways to explore systematically the "cognitive ecology" of the people we were studying in order to discover the general set of everyday circumstances associated with improved, experimentally controlled performance, we encountered seemingly insuperable barriers. For example, knowledge of mundane problem solving activities of a particular group of people presupposes a detailed description of the language and culture of these people at a level which few ethnographers (let alone cross-cultural psychologists) have achieved. Finding little in either the ethnographic or the cross-cultural psychology literature to encourage us (for reviews, see Laboratory of Comparative Human Cognition, 1978; 1979) we decided to study the representativeness of experimental, cognitive tasks in the everyday life of a culture we knew well--our own.

Tests, School and Club

In the fall of 1976, we undertook a study with 17 children 8-10 years of age who attended a small, private school in mid-Manhattan, New York City.

Our approach was as direct as it was simple minded. We video and audio tape-recorded activities of the children in a variety of school settings and in an after school club modeled loosely on those one would encounter at a community center. We also recorded hour-long testing sessions during which each child individually was presented a variety of laboratory-derived cognitive tasks. We were fully aware that we were sampling a limited set of situations, but we hoped that our observations would allow us to talk about how particular cognitive tasks and performances change as a function of settings.

The series of cognitive tests we selected were meant to be representative of tests used to predict and evaluate scholastic aptitude or cognitive development. Additionally, we sought, insofar as possible, test instruments that made visible what the child was doing. We also tried to sample widely from the spectrum of task demands that we imagined might be encountered in school and various non-school environments.

Our test battery included modified versions of the word-similarities subtest of the Wechsler Intelligence Scale for Children (WISC), a mediated memory test first developed by Leontiev (1929) and Luria (1928), a figure-matching task of the sort used to assess impulsivity, a syllogistic reasoning task, and a classification task employing common household objects. These tests were administered by a professional tester who did not know the purpose of the study. We suffered from no illusion that this set of tasks exhausted the possible list of intellectual demands that children encounter daily. But we were confident that they were relevant to at least the classroom.

We began observing in the children's classroom to see if: (a) we could specify the ways in which the children responded to intellectual tasks there, and (b) we could observe the occurrence of any task that could be administered to the children in a later test session. Ultimately, we wanted to determine if children responded to a given task similarly or differently in the classroom and test situations, but we wanted first simply to establish that we could identify cognitive tasks and the children's responses to them in the classroom. We videotaped samples of many kinds of classroom activities: directed lessons (such as an exercise in division or classification of the animal kingdom), individual study time (during which the teacher passed from student to student, checking on and assisting in a variety of assignments), group discussion of social interactional problems that arise in the classroom, and individual "free time" during which children could elect to engage in any one of a number of activities including drawing, playing board games, reading, keeping a diary, etc.

Initially we were encouraged because we seemed to be able to identify the occurrence of various cognitive tasks in the course of our classroom observations. Activities resembling classification, free recall, paired associate learning and a number of other well-studied experimental tasks could be found as a natural part of the children's activities, particularly during formally organized lessons.

To be certain, the tasks as encountered in the classroom were not isomorphic with the laboratory tasks. Nor were they constantly occurring; a good deal of time it appeared that "nothing was happening." But our initial results suggested that something like laboratory tasks did occur in actual school settings, so we had a starting point for making intersituational comparisons.

Midway into the fall, we also began to observe these same children in after school clubs. Half the children (chosen at random except as constrained by the after school activities planned by their parents) attended a club that emphasized nature activities while the remainder constituted a cooking club. These club sessions, which lasted one and one half to two hours, were conducted in a specially prepared playroom at The Rockefeller University where audio and visual recording equipment allowed us to obtain a relatively complete record of what the children said and did in the course of their activities. These activities included preparing various dishes (cakes, breads, entire meals), training animals, growing plants, experimenting with electricity, and a variety of similar "constructing" tasks. The children's behavior was not rigidly controlled, but we did attempt to structure the activities by varying the extent to which successful completion depended on information available from written instructions, the club leader, and other children.

The most striking feature of these club sessions was the extreme rarity of identifiable cognitive tasks. If the classroom could be characterized as an environment where cognitive tasks were observable with intervals of "doing nothing" interspersed, the club sessions could be characterized as an environment of chaotic activity with identifiable tasks interspersed at rare intervals. It was certainly <u>not</u> the case that the children were setting quietly, lost in thought. They were active, argumentative, and constantly busy. But classification, inference, and other tasks we had hoped to discover weren't easily detectible, even after several repeated viewings of our video-taped record. We found ourselves in the somewhat absurd situation where activities that clearly required the cognitive processes we were interested in studying <u>must</u> have been operating (the recipes got read, the cakes baked, the animals trained), but we could not identify how these goals were accomplished in a way that was directly related to those intellectual tasks that are the backbone of process-oriented, cognitive psychology.

We had originally set out to answer a number of questions: How often are the cognitive tasks that have been studied in the laboratory actually encountered in various classroom and club settings? Could we show similarity or differences in the behavior of individual children for tasks encountered in the different settings? Granting that the exact form of a given task would differ according to the context in which it occurred, could we specify how the context influenced the particular form of the task and the child's response to it? Our initial assumption that we could identify cognitive tasks outside the laboratory and classroom and answer these questions was clearly wrongheaded. But the solution to the problem of identifying and analyzing cognitive behavior across settings was not at all obvious.

Concepts of Ecological Validity

Perplexed by our inability to "see" the cognitive tasks represented in a non-school and non-test environment, we examined the background for our assumption that such specification was possible. Particularly, we examined a long-standing and recently revived concern for the ecological validity of experimental settings.

Consider some recent calls for ecologically valid cognitive research. Neisser (1976a) tells us that ecological validity is an important goal of cognitive research because it reminds psychologists that the artificiality of laboratory tasks may render the results irrelevant to the phenomena (implicitly, phenomena found outside the laboratory) that we really want to explain. He points to the "spatial, temporal, and intermodal continuities of real objects and events" as important aspects of normal environments which are generally ignored in laboratory research (Neisser, 1976a, p. 34). Earlier, J.J. Gibson (1966, 1977) had emphasized the same point, claiming that the crucial questions in the study of perception are to be resolved not so much by an attention to the perceiver as by the description of how the environment in particular everyday life arrangements "affords" a person perceptual information. Barker, whose name is closely associated with the concept of ecological validity, has made the point even more forcefully. "Experimental procedures have revealed something about the laws of behavior, but they have not disclosed, nor can they disclose, how the variables of these laws are distributed across the types and conditions of man..." (1968, pp. 1-2).

Bronfenbrenner (1977) has been especially influential in his insistence on the crucial role of ecological validity in modern psychological research, particularly in research on children that is purported to have public policy relevance. In these discussions, he insists that, in order to be ecologically valid, research must fulfill three conditions. First, it must maintain the integrity of the real-life situations it is designed to investigate. Second, it must be faithful to the larger social and cultural contexts from which the subjects come. Third, the analysis must be consistent with the participants' definition of the situation, by which he means that the experimental manipulations and outcomes must be shown to be "perceived by the participants in a manner consistent with the conceptual definitions explicit and implicit in the research design" (1977, p. 35).

Bronfenbrenner's injunctions should sound familiar. They are, we believe, close to a rephrasing of what we tried to implement in our study of children in tests, school, and clubs. Since we found these ideas so difficult to implement, we decided to back up still further. We sought the source of the concept of ecological validity in cognitive psychology, which led us to a discussion between Kurt Lewin and Egon Brunswik at a 1943 symposium on psychology and scientific method.

Brunswik put forth the notion of "ecological psychology" as a discipline in which psychological observations would be made by sampling widely the environments within which particular tasks are embedded. The purpose of such sampling is to determine the effect of different environments on the responses of the organism. As a complement to Brunswik, Lewin's contribution to this symposium was his formulation of "psychological ecology." This was a way of "discovering what part of the physical or social world will determine, during a given period, the 'boundary zone' of the life space" (1943, p. 309) of an individual. By 'life space' Lewin meant "the person and the psychological environment as it exists for him" (p. 306). In order to understand more clearly what Brunswik and Lewin meant and how their ideas are related, we turn now to an illustration of Brunswik's procedures.

Most generally, Brunswik was concerned with preventing psychology from being restricted to "narrow-spanning problems of artifically isolated proximal or peripheral technicalities... which are not representative of the larger patterns of life" (1943, p. 262). In order to avoid this problem, he suggested that situations, or tasks, rather than people, should be considered the basic units of analysis. In addition, these situations or tasks must be "carefully drawn from the universe of the requirements a person happens to face in his commerce with the physical and social environment" (p. 263).

As an example of such an approach, Brunswik made repeated observations on size constancy by an individual who was "interrupted frequently during her normal daily activities and asked to estimate the size of the object she just happened to be looking at" (p. 264). This person's size estimates correlated highly with actual measurements of the objects and not with their retinal image size. This result, Brunswik tells us, "possesses a certain generality with regard to normal life conditions" (p. 265).

To make Brunswik's idea concrete, consider the operations that he offers for evaluating the ecological validity of size constancy in an everyday environment. First, he poses a problem for the subject (asks a question) such as "How big is that chair?" which elicits a circumscribed response based upon limited aspects of the physical environment. Second, he has available a physical model of the stimulus elements that are critical to his analysis (a model of measurement which allows him to scale size of object, distance from subject, and, hence, physical size of the image on the retina). Third, he has a strong hypothesis which specifies relations between the physical stimulus and the subject's response--that either physical stimulus size (the "distal" stimulus) or stimulus size projected on the retina ("proximal" stimulus) will govern the subject's size-estimation response. Fourth, he obtains a very clear-cut result: correlation between reported size and physical size is essentially perfect, whereas the correlation with retinal size is poor. Of course, other settings could be investigated, and it might be possible to discover conditions in which the same result would not be obtained. However, the logic of the enterprise is clear from the example; only the scope of the generalization is in question.

In our opinion, Brunswik's success was not accidentally related to the fact that the examples he actually worked out came from the area of visual perception, which represented (and represents) one of the most sophisticated areas of psychological theory. This gave him several advantages. First, because he could draw on the theory of physical measurement, he could confidently use a ruler to measure the dimensions of the objects whose sizes were being estimated, the distance from the subject to the object, and the size of the retinal image. In short, he could describe exactly the relevant aspects of the task environment and disregard such irrelevant aspects as the heat in the room, the color of the objects, etc.

Next, it is essential that Brunswik was confident of the behavior that the subject would engage in when asked "How big is that ___________?"[2] He had strong reason to believe that the question would focus the subject's attention on exactly those aspects of the environment that he thought relevant and that he could measure.

In addition, Brunswik could rely on competing hypotheses, derived from the laboratory, about how the theoretically relevant aspects of the environment mapped onto two aspects of the subject's response; he could specify the meaning of correlations with retinal and object size. Finally, he obtained essentially perfect prediction for one of the alternative hypotheses.

Consider what kind of difficulties Brunswik would have faced had he been forced to proceed without any one of these resources for interpretation. If he had obtained equivocal results with respect to constancy based on proximal or distal cues, he would have been in a quandary. He might have wanted to conclude that real-life perception depends upon a mix of distal and proximal cues; he might have pleaded that his subject was in some way atypical. He might have begun to worry about the efficacy of his question as a means of inducing the subject in a real-world environment to engage in a task that he had successfully posed in the laboratory.

While Lewin agreed in part with this formulation of Brunswik's, and saw their goals as compatible, it is clear that certain of his principles put forth at the 1943 symposium would lead him to question Brunswik's conclusions. On that occasion, Lewin argued his well-known position that behavior at time _t_ is a function of the situation at time _t_ only, and hence we must find ways to determine the properties of the situation "at a given time." By situation, Lewin was referring to the "life space" of the individual.

Pursuing the logic of Lewin's argument, we might suggest that Brunswik's questions may not have been appropriate to the life space of the person he asked. We could argue that, under such circumstances, there is a possible mismatch between the geographical and psychological environments, such that Brunswik's observations may not have been measuring the aspects of the environment that were a part of the subject's psychological environment just prior to the time he asked his question. Instead of observing the occurrence of someone making a size estimation in a real-life environment, Brunswik had made a size-estimation experiment happen in a nonlaboratory environment. He had, in Lewin's terminology, changed the subject's life space to fit the requirements of his predefined set of observation conditions. In light of later discussions of ecological validity in psychology and our own research, this distinction between sampling the occurrence of psychological tasks in different environments and sampling environments within which to engineer psychological tasks is crucial. It is a point which we have been slow to assimilate and one we think our colleagues have understood poorly.

Brunswik's and Lewin's early discussion, focused as it was on issues in cognitive psychology, retains special relevance for current efforts to expand the generality of cognitive psychology. Precisely because the issues were formulated so clearly and so early, we are moved to ask what impediments have stood in the way of developing the experimental-theoretical program for a generalized cognitive psychology laid out by these pioneers.

Issues of theoretical fashion aside, we believe that the major difficulty arises because in practice, if not in theory, the requirement to insure representative sampling of cognitive tasks and the requirement to define the "life space at a given moment" are in conflict with each other. Once we move beyond the laboratory in search of representativeness, our ability to identify tasks is weakened. Either failure to define the parameters of the analyst's task or failure to insure that the task-as-discovered is the subject's task can vitiate the enterprise. In general, psychologists have not come up with procedures that allow them to overcome the resulting ambiguities of task and behavior specification.

In modern versions of the ecological validity discussion (e.g., Bronfenbrenner, 1977; Neisser, 1976a), the assumption is that one can first identify some task of interest within a laboratory setting and then discover instances outside of the laboratory (in "real life") where these tasks occur, and thereby discover the extent to which the structure of tasks and behaviors in the laboratory is representative of the task and behaviors in other environments.

Note that crucial differences between these interpretations of ecological validity and the procedures proposed by Brunswik. Neisser, Bronfenbrenner, and others do not propose that we carry around our laboratory task and make it happen in a lot of settings. They propose that we discover the ways tasks occur (or don't occur) in nonlaboratory settings. Moreover, in Bronfenbrenner's version of this enterprise, we must also discover the equivalent of Lewin's "life space," e.g., how the task and all it involves appear to the subject. These new requirements for establishing ecological validity place an enormous analytical burden on the psychologist who would fulfill them. That burden is perhaps more than psychology can, or most psychologists would care, to take on.

Speculations About the Differences Between Laboratory and Everyday Life Contexts for Thinking

So far, we have suggested on the basis of our casual observations that laboratory and everyday contexts for thinking are different enough to disable generalization about characteristics of individuals in which data gathered from one context are used as a basis for inferences concerning these same individuals in another. We have also suggested that extant discussions of ecological validity provide no clear basis for solving these problems. Perhaps implicit recognition of these problems underlies the paucity of data on situational variability in thinking. However, although there has been extremely little direct research by cognitive psychologists into the characteristics of non-laboratory tasks and behaviors, it is possible to find speculations on the nature of such environments and people's responses to them from a variety of sources.

A good place to begin a documentation of psychological speculations about thinking in everyday life is the work of Bartlett (1958) who distinguished three kinds of thinking which can be roughly related to three classes of contexts for problem solving. First, he discussed the properties of <u>closed</u> systems, which have a fixed goal, a fixed structure and known elements. These are exemplified by anagram problems, extrapolation of numerical sequences (Kotovsky and Simon, 1976), problems such as the missionaries and cannibals (Reed, 1977) or the Tower of Hanoi (Simon, 1976b).

Bartlett contrasts <u>closed</u> systems where the task elements and allowable responses known ahead of time with <u>open</u> systems of which there are two main types. A person involved in <u>experimental</u> thinking is in the position of an explorer rather than a spectator. He has a notion of the goal, but the set of appropriate responses is unspecified, as are the constraints on his attempts to reach the goal. He must use whatever tools are available for adding to the structure that is not yet finished. "The materials that he must use have properties of their own, many of which he cannot know until he uses them, and some of which in all likelihood are actually generated in the course of their use" (p. 137). A biologist seeking a cure for cancer in a virology laboratory would be an example of a person working in an open, experimental system. Bartlett points out that in a closed system, <u>the answer is THE answer</u>; an answer is but the specification of a new step in the larger domain of interest.

Finally, Bartlett discusses a second class of open systems which he says characterizes <u>everyday</u> thinking, "by which I mean those activities by which most people . . . try to fill up gaps in information available to them in which, for some reason, they are especially interested" (p. 164).

Bartlett suggests that the kinds of comments one hears in casual conversation in public places, the speeches of politicians, and any topic about which a narrative account could be given, provide contexts which illustrate everyday thinking. His own empirical work on the topic was minimal, consisting of written accounts of problematic situations, which people had to read and discuss in an attempt to specify the factors that would lead to various outcomes. On the basis of interviews with his subjects, Bartlett concluded that:

1) In everyday thinking, conclusions are reached with far less consideration of logical alternative choices than in closed system thinking. "The psychological determination of this is that in popular thinking the end of the preferred argument sequence itself takes charge of the selection of particular items of evidence" (p. 175).

2) "It seems that the decisions are not so much reached through the evidence as that the evidence is picked out in accordance with some decision already made" (p. 176).

3) Sometimes the evidence offered is not the evidence given.

4) The evidence offered in support of conclusions is short of the evidence available. "It is usual for the decision as to the issue to be announced first, and then for a single head of evidence to be advanced as the alleged basis for the decision" (p. 177).

On the topic of evidence, Bartlett tells us that in everyday life, generalization and selected evidence are similarly strongly socially determined. The first can nearly always be found to be current in some group of which the thinker is a member, and the second, provided it is not just personal recall, is precisely the same evidence that many other members of the group also select.

According to Bartlett, everyday thinking is simplified further by what he calls "resort to ego." By this he means that when a complex problem about the real world is posed, people will decide its answer on the basis of a single instance dredged up from personal memory (cf. Abelson, 1978; Tversky, 1972).

As a result of these factors, operating in various combinations, everyday thinking is carried out with fewer mental steps than is characteristic of thinking in closed systems or experimental thinking. "Once the selected, or weighted evidence, and the accepted generalization are brought together . . . the required continuation of completion of the situation that has provided the occasion to think is simply 'there.' There are no traces of elaborate processes still interposed between the data for completion and the completion itself. The completion is accepted and asserted with conviction . . ." (pp. 180-181).

In light of our later discussion, it is important to note that the overwhelming majority of Bartlett's work on thinking and recent process-oriented, cognitive research deals with _closed_ systems. The fact that they provide an _a priori_ specification of task and allowable responses is a prerequisite to the design of experimental tests of process models (see Bruner, 1951; Estes, 1976; Simon and Newell, 1971, for very clear specification of this requirement). We also think it significant that Bartlett's examples of closed system thinking are from laboratory experiments while his examples of open thinking come from the domain we are referring to loosely as everyday life.

Quite recently, Robbie Case (1978a, b) has elaborated on the implications of the idea that in many everyday life settings, information is provided in a form that reduces the demands on individual thinkers. Case contrasts two features of Piagetian tasks as encountered in the laboratory with demands faced by children encountering analogous tasks in everyday life. One is that laboratory tasks are novel, ruling out previously prepared responses. The second is that the laboratory tasks are designed to be misleading, forcing the child to display the most complex thinking strategies he or she has available. Case speculates that such features are not prominent in the tasks children encounter in life beyond the experimental laboratory:

> While this sort of 'misleading' task is best suited for showing the dependence of general development and learning throughout the entire age range, it must be realized that many, if not the majority, of tasks which a child encounters in his everyday life are both familiar and facilitating. That is to say, the child is exposed to the task repeatedly, and the task has no feature which

> suggests the application of an incorrect strategy. For these sorts of tasks, it is not critical that the child bring a complex knowledge gathering strategy to the situation to begin with (1978a, p.).

As examples, Case (1978b) suggests that as a rule in everyday life when young children are asked to choose among two glasses to obtain the one with more water, they are presented two glasses of equal diameter, instead of two varying in diameter and height; similarly, the everyday analogue of balance beam problems, the teeter-totter, is typically subjected to variations in the weight of the participants, not weight plus distance from the fulcrum. As a result of these presumed characteristics of everyday Piagetian-type tasks, Case concludes that ". . .a great many familiar and facilitating tasks can be mastered in pieces, with each new element gradually being incorporated into the overall whole" (Case, 1978a, p.).

An important common ingredient to Bartlett's and Case's speculations is contained in the notion that everyday thinking can be carried out "in pieces" (to use Case's term) or "with fewer steps" (Bartlett). In either event, intellectual demands at any given moment are, in effect, reduced.

A rather specific, and controversial, contrast between thinking in everyday life and laboratory-like contexts (here represented by the classroom) is to be found in the writing of Arthur Jensen. Jensen has elaborated a "hierarchical theory" of mental functioning. Level I of the hierarchy refers to direct learning that requires little in the way of mental transformation of information; Level II involves transformation of information present from Level I process (Jensen, 1970). Although this theory is best know in connection with controversies over the heritability of IQ, it was formulated specifically in terms of thinking said to characterize children in and out of the classroom:

> The observations that initially gave rise to the studies that led us to the dual-process hypothesis proposed here (reference is to Jensen's distinction between Level I and Level II processes) were brought to the writer's attention by school psychologists and teachers in classes for the educable mentally retarded . . . It was the teacher's impression, confirmed by the writer's own observations made in the classroom, on the playgrounds, and in laboratoy testing, that the low SES children in the EMR groups appeared in many ways to be much less retarded, and in fact usually appeared quite normal, as compared to middle class children of the same IQ. . . The low SES children, whether white, Negro, or Mexican-American, appeared more mature and capable in social interactions and in activities on the playground than middle SES children, despite very similar scores on a variety of intelligence tests . . . and very similar performance in school subjects . . . (p. 242).

Jensen goes on to say that on Level I tasks, low SES minority group students perform in the "same average range as the majority of average IQ middle SES children" (p. 244). Adding to these observations the reports that the diagnosis of retardation is specific to the school setting, Jensen leaves us (implicitly) with a characterization of non-school environments: they are characterized by Level I thinking.

In schematic form, the argument seems to run as follows: Low IQ, lower class children are equal to or better than higher IQ or higher SES children outside of experimental learning tasks. They are also equal to or higher than such children on Level I laboratory tasks. They are deficient on Level II laboratory tasks. Therefore, learning outside of school must be Level I. It is important to note that Jensen's speculation about the structure of intellectual task demands outside the classroom represents an extreme variation on Bartlett's and Case's characterization of non-laboratory contexts for thinking. In such contexts, information is presented in a way (unspecified) that requires little mental transformation on the part of the thinker.

A suggestion of important differences between the requirements of laboratory and non-laboratory settings arises in Norman's (1975) discussion of memory skills required in the two settings. Norman introduces the problem by asserting that when he learns complex material in a setting normal for him (such as a symposium) "almost none of the learning requires the attentive rehearsal processes studied in the psychological literature on short-and long-term memory" (p. 530). In the context of a discussion about problem solving, Norman makes a very important suggestion about "laboratory--life differences" in the way the environments are organized vis-a-vis their demands for remembering:

> If conventional theories of learning and memory have taught us one thing , it is that factual knowledge is difficult to learn; it becomes a lesson in paired associate learning. Functional knowledge is different. Once the function is understood, knowledge appears with relative ease. It is derived, not memorized. The functions are mnemonic devices and so it is the function we should be teaching (p. 542).

Why is functional knowledge easy to remember? Norman suggests that it is because "a function has more constraints on its possible relations to other concepts than does a list of concepts" (p. 542).

A compatible characterization of non-laboratory remembering environments is offered by Cole and Scribner (1977) following a summary of cross-cultural research on memory. Borrowing a phrase from Bartlett (1932), Cole and Scribner suggest that "the situations in which the remembering. . . takes place are given structure and meaning by virtue of the 'dominant social tendencies' that organize all mundane life" (p. 267). In those relatively rare situations where individuals have to commit some large body of esoteric material to memory, societies have invented psychologically sound mnemonic devices for easing the individual's task. Writing is one obvious mechanism, but oral mnemonic devices are prevalent as well (Riesenberg, 1972).

An extension of Cole and Scribner's point concerning mnemonic devices can be found in a study by Kreutzer, Leonard and Flavell (1975). Based upon interviews with grade school children about memory and mnemonics, they emphasized children's readiness to use other people as storage and retrieval devices.

> The use of this resource requires still less in the way of school-taught representational skills, trades on a well-learned set of social help-seeking routines, and--shades of Luria--would be most unlikely to turn up as an observed "mnemonic strategy" in a conventional laboratory study of memory development. Other people are in fact remarkably useful "amplifiers" of our storage and retrieval capacities. They can help you prepare for future retrieval by guiding your learning strategy or otherwise assisting study, by sorting part or all of the information themselves, or by helping you commit it to a reliable store external to both of your heads. Similarly, they can aid retrieval by helping you select and execute internal or external search strategies, by actively joining in the search themselves, by letting you know if they should chance to recall or encounter the lost item later, or by recruiting others to similar active or passive

> helping roles. The younger children in this study seemed to be aware of at least a few of these possible forms of mnemonic assistance by others (pp. 51-52).

Thus far the general direction of the speculations has been more or less univocal; in a variety of ways, non-laboratory tasks are likely to be structured in ways that reduce their information processing demands. But not all psychologists who have written on the subject agree with this characterization. Consider, for example, Neisser's (1976b) recent discussion of the differences between academic and general intelligence. Neisser points to sources of complexity in tasks as they occur in everyday life:

> Intelligent behavior in real settings often involves actions that satisfy a variety of motives at once--practical and interpersonal ones, for example--because opportunities to satisfy them appear simultaneously. It is often accompanied by emotions and feelings, as is appropriate in situations that involve other people. Moreover, it provides continual opportunities for cognitive growth of many kinds, because most situations turn out to have facets of which we were formerly unaware (pp. 136-137).

By contrast, in school we are expected to:

> . . . solve problems that other people have set. Notice also that problems on school tests are supposed to be 'fair"--that is, all the information needed to solve

> them is typically given from the beginning. The pupil does not find out anything as he goes along that might have been otherwise (p. 137).[3]

L.S. Vygotsky (1962, 1978) has been one of the few psychological theorists to concern himself directly with the role of the social environment in shaping the structure of higher psychological function. Although cast in a developmental mold, Vygotsky's ideas apply to adult as well as child thinking. Simplified somewhat for the purposes of the present discussion, Vygotsky's (1978) proposal is that the child's social environment served as a system of supports for the child's cognitive activity. He argued, in effect, that the child's abilities were to be discovered not just within the child, but in a system which included the child and the structured environment provided by adults and peers. He pointed out that this "child plus environment" system possesses properties that are generally absent from psychological experiments. One of the most important poinst emphasized by Vygotsky and his followers (e.g. Leontiev, 1929; Luria, 1928; 1932) is that in most everyday life settings, the motivation of the child's activity is rarely to engage in a particular cognitive process; rather, the goals of behavior assemble specific processes of the kind studied in the laboratory (memory, classification, inference) in accord with the socially defined nature of the activity. In everyday life, motives and actions are inextricably fused, and activity is constrained by socially given norms.

Vygotsky's approach precludes any simple distinction between laboratory and non-laboratory contexts for thinking; while for purposes of analytic simplicity, laboratory contexts may be constructed to conform with characteristics such as those contained in previously described speculations, they may be more complex than descriptions of "closed" system lead one to believe. In like manner, where social constraints are appropriate, laboratory-like tasks can be found in non-laboratory settings, of which the school is but a single example. Each setting and the activities in it must be analyzed for motives that organize it, the (possibly multiple) goals that are being sought, and features of the environment relevant to each possible task/goal (i.e. stimuli) in order to discover the structure of activity that <u>is</u> thinking.

We could extend our list of cognitive psychologists who have demonstrated a concern for the possibility that laboratory and everyday life environments for thinking and remembering differ in significant ways from standard laboratory tasks. However, a more exhaustive summary of this line of speculation would not add substantially to our knowledge about how such environments really interact with subject characteristics to produce performance, because, as far as we know, no one from the recent tradition of cognitive psychology has taken the necessary step of investigating presumably relevant real world environments directly.

This does not mean however, that no evidence exists relevant to the speculations we have been reviewing. In the following sections, we will take up evidence based on experiments that model the speculations. Then we will provide descriptions of non-experimental data from our own research which we will use eventually to highlight the inadequacies of these experimental models and which challenge the notion that current practice in cognitive psychology will permit generalizations between laboratory and everyday contexts for thinking.

Laboratory Analogues of the Contrast between Laboratory and Everyday Life Contexts for Thinking

One way to gain relevant evidence is to undertake laboratory studies in which tight experimental controls can be enforced under circumstances which model as closely as possible the conditions speculated to characterize thinking in everyday life. In this section we will review experiments which we think model these speculations although few of the experiments were conducted with that aim in mind. Of necessity, individual examples will speak to particular speculations, but the corpus of examples we have gathered suggests important points of convergence between data and speculation and indicates the limitations of research on this issue which does not confront nonlaboratory settings as a part of the enterprise.

Varying the content of the task. Experimental attempts to model the relation between laboratory and everyday life settings have taken several forms. Before discussing experimental analogues of specific speculations, it is worthwhile to comment on a more general tactic for relating laboratory and non-laboratory settings, if only because its use is so widespread. This is the technique of introducing "reality" by changing the content of the task in order to provide what are presumed to be the familiar elements said to affect performance by Case and others.

For example, Morton and Byrne (1975) report a study entitled "Organization in the kitchen" in which people are asked to remember and classify real-world objects that are found in kitchens. Morton and Byrne suggest important aspects of the kitchen environment as they might affect remembering, for example, the fact that most objects are multiply classified depending on context and the function into which they are being fitted. However, the actual environment for the research is a laboratory and not a kitchen; the data are recollections about the real-environments-as-imagined. The subject has not only to deal with the presumed organization of the real-world kitchen, but to do so from memory, leaving us with little knowledge of how "organization in the kitchen" as an activity which goes on in kitchens (moving through it, getting something cooked, put away, etc.) really gets done. (For a similar exercise, see Byrne, 1977 and Graesser, 1978).

A different way of inserting "real-life" content into experiments is to represent it using common objects and their allied properties (e.g., plants in varying states of health said to have been fed different kinds of food as in

Kuhn and Brannock, 1977; Kuhn and Ho, 1977). Reasoning with such stimuli is contrasted with more traditional problem solving stimuli (e.g. Piaget's pendulum problem). While it has been shown on occasion (Irwin and McLaughlin, 1970; Price-Williams, Gordon and Ramirez, 1969) that the simple introduction of "natural" content into an experimental task is sufficient to increase the sophistication of the way subjects deal with it, it is in no way clear what natural means in such cases. Certainly there can be no claim that such manipulations create natural problem content in the sense that the subjects have the same information about the content as they would if they had structured the problem solving for themsevles, if other people had been around, if the problem were part of an habitual activity, etc. Perhaps for this reason, there are important inconsistencies in the literature concerned with the effects of "content familiarity" on thinking (See for example, Greenfield, 1974; Johnson-Laird and Wason, 1977; Sharp, Cole and Lave, 1978).

A crucial shortcoming of this work as a representation of the speculations about differences between laboratory and everyday life contexts for thinking is that while both the speculations and these "content variation" manipulations assume familiarity ("everydayness") of problem content, the emphasis in the speculations is on the way that content is organized in terms of function, amount, and timing. These "dynamic features" supposed to differentiate the two contexts are not represented in experiments that have varied familiarity of problem content. The speculations require a different kind of contrast for their verification.

Fewer mental steps. Research on everyday and logical reasoning by Johnson-Laird and Wason (e.g., Johnson-Laird & Wason, 1977; Wason & Johnson-Laird, 1968; 1972) comes close to modelling Bartlett's speculations concerning the fewer mental steps involves in everyday thinking. For example, Johnson-Laird and Wason have been concerned with how people handle negation, in particular denial. In several experiments, subjects found it easier to negate an exceptional item in terms of the property that makes it exceptional rather than to negate an unexceptional item in terms of a property of the exceptional item. For example, given a display of 8 circles, one of which is red (circle 1) and 7 of which are blue it is easier to complete the sentence "Circle 1 is not blue" than it is to complete the sentence "Circle 5 is not red." Wason & Johnson-Laird (1972) suggest that in everyday language denial functions to correct the misconception that it denies. "Circle 5 is not red" is more difficult because no misconception has been established. This phrase requires that subjects perform an extra mental step, "as if the affirmative preconception has to be recovered before the meaning of the negative can be grasped. . . On the other hand, in everyday life this extra step goes unnoticed because the preconception has already been processed as part of the context of the utterance" (Wason & Johnson-Laird, 1972, p. 39).

Similar evidence for "mental shortcutting" can be found in studies of event probabilities. A key result in this literature is contained in an experiment by Jenkins and Ward (1965) who presented college students with information about a hypothetical experiment in "cloud seeding." Trials were presented one at a time and only 17% of the students made estimates of rain probability that were consistent with the idea that they were estimating the actual event correlations. Most students responded as if they had failed to remember those occasions on which seeding was not followed by rain, or when it rained and no seeding occurred--in effect, focusing on confirming cases only.

Although the structure of the task is quite different from the tasks suggested by Bartlett in his examples of everyday problem solving, it seems plausible that the behavior of Jenkins and Ward's students and Bartlett's subjects who "resort to ego" arises from similar causes.

On the effects of reducing memory load. An example of an experiment that appears to model a central speculation about laboratory-everyday life differences in thinking is provided by Eimas (1970) who produced striking evidence that the nature of concept formation activities is strongly influenced by the ease with which already-experienced instances of a to-be-learned concept are remembered. His studies represented a modification of hypothesis-testing procedures for assessing concept learning popularized by Levine (1966). In Eimas's study, children were presented pictures of geometric stimuli that differed according to obvious physical dimensions such as their size, color, form, and the number of pictures per stimulus card. For any given problem, a particular stimulus attribute on one of these dimensions (say, all the red cards) was deemed correct. The children's hypotheses were assessed by carefully constraining the order in which stimuli were exposed and noting the pattern of errors on diagnostic "probe" trials on which no feedback was given. In the typical procedure on which this line of work was based, cards were displayed one at a time. Following this procedure, Eimas, like Gholsen, Levine, and Phillips (1972) found that young children differed sharply from adults in the hypotheses that they tried out while solving the problem: the youngsters were more likely to make stereotyped choices of a single attribute or simple complexes of stimulus sttributes, whereas adults focused on dimension-attribute combinations that were consistent with the past information they had collected. However, when Eimas introduced the simple modification of allowing the young children to view continuously the past instances they had been shown while responding to the critical probe trials, they produced adult-like,focused hypothesis testing.

An analogous result was obtained in an experiment by Ciborowski and Cole (1972) with Liberian natives who varied in age and the amount of formal education they had received. Ciborowski and Cole were pursuing the possibility of cultural differences in the relative rates of learning conjunctive (red triangle) versus disjunctive (red or triangle) concepts. Their method was an adaption of procedures used by Bruner, Goodnow, and Austin (1956) in which subjects were presented with cards one at a time and asked to sort them into one of two piles, one for "positive" instances and one for all the remaining (negative) instances. A modification in the procedure was instituted that paralleled the modification that Eimas introduced into the experiment discussed above. The stimuli were presented in two rows so that the subject could see all past instances at the time he made a choice concerning a new stimulus. This "running memory" not only greatly reduced the number of trials required to solve the problem, but what is important for our present point is that when the running memory procedure was used, conjunctive concepts were learned roughly twice as rapidly as disjunctive concepts. Again, it appears that reducing the subject's memory load at crucial points in problem solving or concept learning had done more than improve performance, it had induced a qualitative change in the nature of the problem solving behavior.

In a number of papers, Case (1974, 1978a;b) has demonstrated that presenting Piagetian problems in formats that reduce the load on working memory (e.g. the number of items of task-relevant information that have to be considered simultaneously) and that give clear feedback about the causes of success and failure (in effect, a further reduction on the individual's memory load) sharply decreases the age at which children can handle these problems (conservation, seriation, etc.). The implication of these demonstrations, as applied to the present discussion, is that children will be able to respond correctly to similar problems more readily in everyday settings than in laboratory settings.

A quite different example of the influence of memory load on reasoning is to be found in the literature on the learning of event probabilities and contingencies, examples of which were mentioned above (e.g. Jenkins and Ward, 1965). Estes (1976) has replicated the observation that negative instances are ignored under circumstances that allowed him to demonstrate that the simplified memory representation of event frequencies underlies the widespread phenomenon of probability matching (the matching of event frequencies to their relative probability of occurrence). Veridical problem solving has been observed in such studies only when the information is summarized in a two-by-two contingency table, a device that has the effect of drastically reducing the memory load on the learner.

These results bear an obvious analogy to the Eimas and Ciborowski and Cole studies in showing that a decrease in memory load increases the sophistication of problem solving. They could also be seen as evidence that everyday environments are not always simplified with respect to the memory load they impose on people. Cloud seeding may not concern us very often, but the work of Estes and others has employed a number of plausibly mundane contents in their laboratory tasks.

They do so as a means of providing subjects with a framework for interpreting the task (estimating who will win an election on the basis of primary outcomes, the location of heavy traffic on the way home, the location of tuna during the fall run, etc.). That these everyday life "cover stories" may operate as effective surrogates of reality is suggested by D'Andrade (1974) who has demonstrated similar simplifying procedures in recall of social interactions and by Shweder (1977) who has speculated that simplifying mechanisms may play a key role in personality assessment and reasoning. Both simplification and complexity of non-laboratory task environments with respect to memory demands seem important on the basis of the research adduced here.

Studies of attention. Another large literature in cognitive psychology that provides a basis for expectations about the structure of intellectual activity in natural settings concerns attention. In an early formulation, Cherry (1957) described the difficulties of listening to two sources of verbal input as the Cocktail Party problem, in reference to the selective attention demands of any human interaction. Most inquiry into this topic has remained within the laboratory because it is nearly impossible to locate naturally occurring events that require people's undivided attention to simultaneous stimuli that occur repeatedly in a measurable fashion. Nonetheless, several recent reviews of this topic (Kahneman, 1973; Rabbitt and Dornic, 1975) reinforce our folk knowledge that it is generally difficult to attend to more than one thing at a time.

A number of ingenious experiments, from Broadbent (1958) to the present, have demonstrated both the severe constraints on our capacity to process information from different sources simultaneously and the special conditions under which simultaneity, or near simultaneity can be achieved with minimum interference between tasks. Interference between concurrent activities is minimized if they share either perceptual or response mechanisms or when the effort imposed by the competing activities is low. It is unreasonable to expect such conditions to be a part of most everyday environments, but it should be clear that the effort required to carry out many components of everyday tasks is considered to be relatively light in the speculative opinion of cognitive psychologists. Most of us can walk and chew gum at the same time. With a little more effort, more complex tasks can be added. From the experimental data and speculations on everyday analogues we might conclude that people will commonly do what they cannot do under most experimental circumstances--engage in more than one task at a time. This will be true not only because task components are said to be relatively effortless, but also because many everyday actions extend over the 200-250 msec. intervals that investigators favor for showing the difficulty of attending concurrently to multiple intputs. In effects, the inputs stay around longer, so time sharing is feasible.

If Norman and Case are correct in their suppositions about the structured, functional and non-misleading nature of everyday environments, we can expect further benefits vis-a-vis the allocation of attention because the individual can anticipate the future course of the action at hand. To quote Kahneman,

> The possibility of anticipation is essential to the adequate performance (of concurrent tasks) . . . anticipation facilitates performance in several ways; it permits response integration, and thereby effectively reduces the number of discrete choices and decisions that must be made. It also permits a smooth adjustment of effort to the difficulty of each choice in each response (1973, p. 192).

What makes anticipation possible, and often correct, has yet to be described for everyday environments. Everyday contexts may not only allow for two things to be done at once by virtue of their being well enough structured for participants to time-share the two tasks, that is, to alternate operating on the two problems over time. In addition, everyday life may require people to do two things simultaneously and may provide the practice for people to develop such skills. This possibility has been made clear in an intriguing series of studies criticizing limited-capacity theories of attention (Hirst, Neisser and Spelke, 1978; Neisser, 1976a; Spelke, Hirst, and Neisser, 1976). These experiments have demonstrated that, given enough practice and training, some people are able to read, copy dictated words, and categorize these same words seemingly at the same time. The authors speculate that such skills must be developed by a great many people in the course of carrying out everyday work assignments,giving further support to those who speculate that everyday environments routinely permit people to carry on multiple activities simultaneously. Like Vygotsky before them, Hirst, Neisser and Spelke suggest that "laboratory" and "everyday life" are not appropriate contrast terms, a point to which we will return.

On reorganizing the goals of activity. Vygotsky's insistence that the social environment acts as an organizing device for the child's activity and his emphasis on the way in which the goal of activity organizes component processes is illustrated by two Soviet experiments carried out in the early 1940s. In both, standard experimental conditions were contrasted with conditions involving several participants. The experimental settings were so arranged that the cognitive process of interest (remembering--Istomina, 1975; self-control of movement--Maniulenko, 1975) were not the goal of the child's activity. The basic procedures and outcomes are well illustrated by Istomina's work, which focused on the storage and retrieval of discrete items in a free recall task.

Isotomina observed children from 3 to 7 years of age in two different situations. One was framed as a traditional experiment; the other was modeled on everyday activities in the preschool and the market. For half of the children the task was to "learn a lesson" that required them to listen attentively to a list of words and to recall them about a minute later. For the remainder of the children, the recall task was embedded in a game that involved them in play-acting as school staff personnel (teacher, cook, director) or store employees (salesclerk, cashier, guard). One of the activities to be carried out in the game was to go to the store to get supplies for the school. This assignment constituted the "presentation of to-be-recalled items" which were the same as those employed in the free recall task. Retrieval occurred when the child arrived at the store and asked the salesclerk for the items needed.

Embedding the free recall task in this game selectively enhanced performance in theoretically important ways. For the youngest children, restructuring the activities had no effect; they did not grasp the inner logic of the remembering activities in either situation. Slightly older children benefitted from the "game" version of the task, demonstrating by their overt behavior that the game provided them with an understandable motive which induced appropriate remembering activities. The oldest children also failed to benefit from the game situation, but for reasons opposite to those which applied to the youngest children--the older children were able to use the goals provided by the mere request to try to recall in order to organize their activities as well as they did in the game.

In order to explain the difference between the laboratory and game versions of her task at the different age levels, Istomina was led to examine the "particulars of the child's active involvement" rather than the external conditions as defined by the analyst. Istomina observed that intermediate age children in the game situation repeated the items to themselves as they were presented, or repeated the entire set right after they had all been read. The children would often run back from the store commenting that they knew they had forgotten something, or would stand at the cash register apparently racking their memory for a missing item. Relatively little such behavior could be discerned in the experimental condition. The differences in performance for the group in the intermediate age range were attributed directly to the differences in activity elicited by the two situations.

Underlying the difference between the activity elicited in these situations is the differing relation between the child's activity and the motivation for it. According to Istomina, in order for the child to become explicitly conscious of mnemonic goals, such as remembering and recalling, two things are necessary: (1) he or she must encounter a situation that requires an active effort to remember and recall, and (2) the act must have some relation to what gives rise to the child's activity, that is, to the motivation for the act.

> It is this relationship that gives meaning to the goal, and consequently to the act corresponding to the goal. Thus, the differentiation and awareness of a goal on the part of the child depend not only on the existence of objective conditions and on the requirements imposed by those conditions but also on the motivation provoking the child's activity in the first place (p. 153).

For the youngest children, in the laboratory version, the specific nature of the motivation does not directly link up the goal of remembering with the operation of remembering.

> These two factors are linked for the child only in an external fashion, i.e. remembering the words is something the experimenter interjected into his social communication with the child, i.e., into what he was doing with the child. But for the child, his interchange with the experimenter does not entail the need to recall or memorize anything (p. 154).

In contrast, the relation between the goal and the act in the game situation becomes an intrinsic relation; remembering and recalling have real meaning for the child. Only when the child has learned to internalize the goals-as-set-by-adults does the experimental condition come to evoke the organized remembering activity of the game.

Another of the few studies that have compared performance of the same children on test-like and more natural situations is Shaltz and Gelman (1973; see also Gelman and Shatz, 1977). Skeptical of the Piagetian position that the inability of young children to deal with standardized referential communication tasks occurs because they are generally unable to assume the perspective of others (Piaget, 1926), and critical of more recent experimental work showing the failure of children to adjust their speech to their listeners, Shatz and Gelman explored the communicative skills of 4-year-olds in natural settings as well as in more traditional experiments. The two testing situations they employed were modeled on the typical referential communication task (e.g. Glucksberg, Krauss, and Weisberg, 1966) and the

Piagetian three-mountain problem (Piaget and Inhelder, 1956), both of which were scaled down to be simpler and shorter, utilizing the child's mother as a real listener. In the natural communication task, the child explained how a toy worked to the adult experimenter and a two-year-old. Spontaneous speech samples of children talking with their mother, with two-year-olds, and with peers were also collected for comparison.

There were several striking differences between how the four-year-olds talked to adults or peers and to two-year-olds. With two-year-olds, they produced shorter utterances, used fewer complex sentence constructions, and used more words that were attention-getters. Shatz and Gelman conclude from these results that four-year-olds can indeed take the perspective of others into account and adjust their speech to the listener, although these same children failed to do so on the experimental problems: only 37% of the children "passed" each test. While this study provides a direct comparison of intellectual behavior under laboratory conditions and in everyday life, it also points up the central difficulty of the enterprise. As Shatz and Gelman make clear, their experimental and natural tasks are very different in important respects.

> In contrast to the previous studies on communication skills, we have shown that young children can adjust to their listeners. Recall that we chose to assess this aspect of communication skills in a domain over which our Ss had considerable control. That is, we took advantage of the child's wide range of syntactically

> varied responses. Because the earlier studies [and their pretests as well] were conducted in areas where the young child's range of abilities is questionable, such studies may be more appropriately considered assessments of cognitive capacities than of communication skills (Shatz and Gelman, 1973, p. 30).

In other words, the two tasks (i.e. the traditional experiment and the toy description) designed to permit the children to display their communication skills are very different. In addition, the methods of assessing the children's performance differed; for the test situations, Shatz and Gelman used the traditional methods of scoring egocentrism, but for the natural situation, they used various measures of linguistic complexity. Both these factors--the tasks and behaviors analyzed--reduce the conclusion that can be drawn from this study. This limitation will be of central concern when we turn to other observations of behavior in natural settings.

As was the case for the speculations that extrapolated laboratory observations into imagined everyday life contexts for thinking, we could provide a more extensive set of laboratory studies that model the speculative contrasts. The major point has been made, however: controlled laboratory experiments seem to support the contentions that (a) "fewer steps" are required to reach acceptable conclusions to everyday reasoning problems; (b) reduced memory load will lead to qualitative changes (improvements) in problem solving; (c) decreased problem difficulty and structured environments will permit people to divide their attention so that they can work on more than one problem at a time; and(d) restructuring the understood purpose of an activity can transform it to another activity.

Using this evidence as a starting point, we can turn to direct observation of non-experimental contexts for thinking with the expectation that they will embody the characteristics we have enumerated. As we turn to direct observation, however, we should keep in mind that the experimental analogues have failed to represent speculations suggesting that everyday life settings are characterized by increased complexity, arising from multiple tasks and multiple goals operating simultaneously. This, and other lacunae in our evidence will concern us once we have a direct look at some non-laboratory examples of cognitive activity. When analyzed as closed systems with the language derived from experimental contexts, these examples will illustrate the speculations concerning the reduced cognitive demands of everyday life tasks. In the subsequent sections we will raise doubts about the adequacy of this conclusion as it is drawn from either the experimental literature we have just reviewed or the descriptions of non-laboratory tasks we are about to offer.

Descriptions of Some Non-Laboratory Cognitive Tasks

As described briefly in the introduction to this paper (pp. 7-11) we regularly videotaped children in after-school clubs. Drawing on the corpus of taped materials, we will present a number of instances which appear to represent examples of the laboratory/non-laboratory contrasts that we have been reviewing.[4]

Problem solving: Reaching conclusions with fewer required mental steps. One of Bartlett's major speculations about thinking in everyday life is that it often requires no fixed series of mental steps. Problem solving is accomplished when a piece of evidence brought to bear on an accepted generalization "fits" and the problem is "solved." It isn't even necessary that the facts offered as evidence in fact be evidence in a logical sense, just that they be treated as such. Further, sometimes one piece of "evidence" can be offered, sometimes another, or, sometimes the same information can be taken as evidence for two different conclusions, even opposing ones. Example 1 appears to illustrate some of Bartlett's generalizations. These data are from the beginning of a cooking club. On this occasion the children are going to make cranberry bread. The children and Ken, the club leader, are discussing who will work with whom. Ken's original plan was that the 8 children would make 4 breads. However, one child is absent and this creates a slight problem. Six of the children--Nadine and Dolores, Reggie and Archie, and Helene and Lucy--form three pairs, and Rikki if left out. There is some discussion about who will accept Rikki for a threesome.

Example 1

1. Ken: Look, we can only make, we can only make three breads today, alright? Rikki seems to be out of the shuffle. Who's gonna work with Rikki? (Helene whispers to Lucy.)

2. Helene: I had to go with Rikki last time.

3. Reggie: Us, (tapping Rikki on arm) Rikki, you can work with (pointing to Archie and himself) me and Archie.

4. Archie: Naw.

5. Dolores: Rikki you can work with us.

6. Nadine: Archie that's mine! (taking an umbrella away)

7. Lucy: Archie it's only fair.

8. Helene: Archie c'mon.

9. Archie: Well, if you're so desperate why don't you take her? (in a harsh whining voice)

10. Lucy: The reason why we don't take her is because Rikki and Helene get in a lot of arguments therefore I don't want to hear her--Rikki, and Helene--arguing . So shut up.

11. Dolores: You wanna take her? (to Nadine) (Nadine smiles to Dolores)

12. Rikki: It's only sometimes we don't argue, we're just lucky when that happens.

13. Helene: Today we haven't argued once.

14. Rikki: Um-hum.

15. Helene: C'mon Rikki. (stands up, gesturing to Rikki). You can work with us.

16. Lucy: So let's not try it.

17. Reggie: Yea ! Archie we're alone!

18. Helene: Come on (to Lucy who is still seated) 'cause me and Rikki got along last week.

19. Ken: Okay so that's the way it's gonna be?

20. Nadine: Yeah.

21. Rikki: Now this is gonna be dangerous (nervous giggle).

In the course of this brief (47 seconds) skein of discussion, the club leader Ken (line 1) specifies the problem--that the children must form three groups, one of which must include Rikki as a third member--the children discuss who Rikki might work with and come to a conclusion (certified by Ken at line 19).

The features of everyday thinking claimed by Bartlett are evident in the transcript. Three possible solutions to the problem of who Rikki will work with are discussed by the group: a rejection by one group (line 2), an offer by the second group (line 3, followed by a refusal from a second member of the same group in line 4), and an offer by the third group (line 5). At line 10 Lucy explains why the refusal of line 2 should stick. Rikki seems to support Lucy's reasoning in line 12, but by line 15 Helene (who offered the original refusal at line 2) has concluded that Rikki should work together with her and Lucy, while Lucy (at line 16) reaches the opposite conclusion. Lucy's contrary conclusion comes almost immediately following the termination of Helene's statement "Come on Rikki." As Bartlett would lead us to expect, single "heads of evidence" are selected out as alleged bases for different conclusions. Reggie (line 17) and Nadine (line 20) accept the conclusion on behalf

(Helene examines cup; pours contents of it into sifter;
Dolores sifts)

This activity is recognizable as an instance of children needing to transfer material from one container to another of a different size and shape. The precise requirements of the task, however, conflict with the children's understanding of the metric in use, making the environment a misleading one. "Cup" is understood to mean "the cup" (a physical object) and "a (measured) cup" (a metric quantity). This is only one of many examples where the environment for problem solving, contrary to speculations about it, contains salient <u>and</u> misleading cues.

However, it is also possible to discover instances that supply credibility to the idea that environments such as these will be relatively free of misleading elements. Dolores and Nadine are making cranberry bread. The recipe and instructions call successively for ingredients measuring 1 and 1/2 cups, 1 cup, 3 cups, 3/4 cups, 1/2 cup, 1/4 cup, and 1 cup. The total to be combined is thus 8 cups. The instructions call for them to use a "small" bowl, a "large" bowl and a bowl of unspecified size, in that order. Consistent with the speculation of a facilitating environment for pouring ingredients, all of the bowls available to the children were sufficiently large to contain all of the ingredients. A mistake with respect to "large" or "small" would have no detectable consequences. It is relevant to add that in deciding on the amount to put into the (2 cup) measuring cup, these children demonstrated considerable of the sub-groups of which they are members. Ken sanctions the conclusion at line 19 and Rikki (nervously) accepts it at line 21. The group then breaks up to being its baking task.

Facilitating Environments for Problem Solving

According to Case, tasks encountered in everyday life differ from tasks encountered in the laboratory in being facilitating and familiar as opposed to misleading and novel. Case's speculation derives from Piagetian-type tasks in the laboratory. While we were able to locate several instances of potentially misleading and/or novel situations in our clubs, they were of a general nature, and everyday life analogues to specific Piagetian tasks were rare. Cooking club did provide two instances of this kind of problem-solving however, both having to do with the kinds of equipment used. Quite by accident, for example, we equipped the cooking club kitchen with a two-cup measuring cup which confused the children no end. Example 2 illustrates one such confusion and resolution.

Example 2

(Helene is pouring flour into the cup from two pound box).

Dolores: One cup is all the way up to there (pointing to top of cup Helene is pouring flour into, i.e. two measured cups)

Helene: (to Dolores) One cup, Dolores, cup, cup, cup (in clipped, precise manner)

Dolores: That's one cup (pointing to cup)

Peter: (to Helene) Yeah, you just do it--you go up to where it says one cup.

uncertainty, repeating their arguments over what constituted "1 cup" and fractions thereof, as exemplified in Example 2.

While Case restricted his discussion to particular kinds of tasks, we would like to extend his characterization because we found that clubs often provided novel situations to the children. For example, during cooking club, the children often come up against something they've never before encountered. Example 3 shows another kind of misleading situation in the cooking club. In this session, we inadvertently listed the ingredients for making banana bread in one column of an instruction sheet and the steps in another column on the same page. This format caused varying degrees of confusion for the children. Illustrated here is one of the most severe confusions an individual child has (but also one of the most explicit formulations of the correct solution by another child).

<u>Example 3</u>

Archie: (coming over to table, looking) Where's the yogurt? Oh. (walks around table to yogurt)

Nadine: (to Archie) You're up to yogurt already?

Archie: Yeah.

Nadine: (to Archie) Where're the bananas?

Archie: We, um, they didn't give us bananas yet (holds measuring spoons over yogurt)

Nadine: Well, go get 'em!!!

Ken: Bananas are there on the shelf.

Archie: But this is our second thing (holds out recipe)

Lucy: (to Archie) That is a teaspoon; that is a tablespoon. (pointing to successive spoons in Archie's hand).

Archie: This is a teaspoon, and we said--

Lucy: It says tablespoon. Two tablespoons. (pointing to recipe)

Archie: We're right here, Lucy. Lucy, we're right here. (pointing on recipe where it says baking powder on ingredient side)

Lucy: That's (pointing to recipe)

Nadine: That--that's the ingredients not the instructions (pointing to recipe)

Lucy: That's baking powder.

Archie: Whatta' you mean, baking powder?

Nadine: (to Archie) You go in this order. (pointing to recipe)

Helene: (drops recipe disgustedly) Oh God (walking toward Nadine)

Nadine: Look (takes recipe from Archie) This is the instructions (pointing to instruction side of page) That's just what you need to do all this (pointing to the ingredients side; then back to instructions).

Example 4 presents another misleading situation, where the cranberry bread recipe called for nutmeg, an item most of the children were not familiar with. Nadine and Dolores are partners.

Example 4

Nadine: (picks up the recipe and reads) 3/4 a teaspoon of nut-nutmm-nutmeg.

Dolores: Here's the nutmeg (holds plastic bag with chopped walnuts).

Nadine: Here's the recipe.

Nadine: (gets recipe) Is that nutmeg? Let's just skip that.

Dolores: It's nuts.

Nadine: One cup of sugar. O.K. What do we need? (looks at recipe) 3/4's, 3/4's a teaspoon nutmeg. We have to, how do you sift this? (measures 3 1/4 teaspoons of the nuts and puts them in sifter)

(Dolores and Nadine "sift" the nuts)

Lucy: (coming over to table) Where's the flour?

Nadine: I don't know. This is bad. I don't think this is working out right. How are you supposed to sift nutmeg? (puts hand in sifter)

Rikki: (comes over) You are, Nadine. You're supposed to sift it.

Nadine: Where's the nutmeg?

Rikki: Over on the table.

(Nadine, Rikki, and Dolores go to other table)

Nadine: Where?

(Rikki hands Nadine the nutmeg.)

Remembering

The major speculations about remembering in everyday life are that environments for storage and retrieval of information are highly organized (thereby providing individuals with a readymade structure--a circumstance that has been widely demonstrated to facilitate recall performance) and that everyday life environments are replete with recall cues, both human and physical, to which the individual has recourse. A major implication of reducing memory demands is to permit the individual to devote less of his limited mental resources (Norman and Bobrow, 1976) to the activity of remembering and more to other activities demanded by the tasks at hand.

The fact that many everyday environments (e.g. kitchens) are physically structured in a manner that lessens memory load is so ubiquitous that it is difficult to see. For example, in cooking club, the room was arranged so that utensils were on one set of shelves and ingredients on another. If a child needed to remember where the spatula or measuring spoons were, for example, he or she would have to remember where utensils in general were in order to find the spoons and the spatula. However, when one's expectations concerning the organization of the environment are not met, its facilitating nature becomes apparent.

This can be illustrated by the following excerpt from a cooking club, where the children are making cranberry bread. The recipe calls for melted shortening, and the children look to the usual place for ingredients in order to find the shortening.

<u>Example 5</u>

Nadine:	One half a cup melted shortening. Where're we gonna get melted shortening?
Dolores:	Where's the shortening?
Lucy:	(comes over) Where's the melted shortening?
Nadine:	(at other table) Is this shortening? (Dolores stops sifting, watches)
Rikki:	(going over to shelves) This is shortening (pointing to Crisco can on shelf). But is that where you melt it? (gestures toward stove)
Helene:	(turns from stove to Lucy) Here's the shortening Lucy (points to measuring cup of melted margarine on stove)
Nadine:	(turning from shelves) Where's shortening?
Helene:	Sittin' on the god damn stove (Rikki, Lucy, and Helene go to stove)

Example 6 is an instance of how the memory demands on individuals can be lessened both during storage and in retrieval. This scene is from a club session where the children had cut up some fruits and vegetables, extracted seeds from them and are about to plant the seeds.

<u>Example 6</u>

Mark:	There's not enough trays.
Mike:	Anybody planted anything yet?
Jackie:	No.
Mark:	Hmmm.
Mike:	O.K. now, let's wait. What do we got to do?

Jackie: There's not enough trays.

Mike: But there will be enough trays if we plant it right.

Carl: I-I-I-I need another popsicle stick for um, Robert.

Robert: You're doing tomatoes.

Carl: Oh! Oh! Oh!

Mike: What do we . . .

Mike: Rikki.

Rikki: Yes.

Mike: What dif-, how many different kinds of things do we have to plant? What do we got going?

Rikki: Gourds.

Mike: The gourd (pause).

Andy: Tomato.

Jackie: Green pepper

[Jackie: Green pepper.

Mike: Green pepper.]

Andy: Tomato.

?: Yeah.

Mike: Tomato.

Jackie: Tomato.

Rikki: (inaudible)

Mark: Apple.

Mike: Apple.

Mark: Orange.

Mike: Orange. And what about _____ (Andy begins to put seed into tray) Wait! Wait! Wait! Andy--nobody can farm like that! What're you gonna take it and just slop it down all over and then how'll we know what's sprouting? O.K.? What does a good farmer do?

Mark: He makes a hole.

Mike: He makes little holes and he starts a line.

There are two ways in which the memory load in this memory task is lessened relative to one encountered in the laboratory. First, the items to be remembered are present in the environment: The children have just cut up the fruits and vegetables; their remains are still on the table a few feet away. Second, no one child has to remember all items. Together, the children recalled 5 of the 6 items, but no child individually recalled more than two.

Another characteristic of memory tasks in everyday life appears in Kreutzer, Leonard, and Flavell's (1975) speculations that individuals routinely use other people as storage and retrieval devices. This can be seen clearly in the following example from a club session. The children are arguing over how many rooms they have in their apartments. Dolores initiates the task by questioning the number of rooms in Jackie's apartment (first by asserting she has 13, then by questioning her own assertion).

Example 7

1. Dolores: (pointing to Jackie) Well she has thirteen (rooms). No you don't.
2. Jackie: Uhhuh, I just made, I, we just made a bathroom.
3. Mike: They cut one (room).
4. Dolores: (to Jackie) Count them all (Dolores holds up pinky).
5. Jackie: Three bathrooms (holds up three fingers)
6. Dolores: That's not a room.
7. Reggie: They're rooms. Bath rooms.
8. Jackie: No, I'm counting bathrooms. For my (holds our arms) whole house we got thirteen rooms. There are two (inaudible).
9. Dolores: (holds up 3 fingers) Three, two (adds 2 more fingers).
10. Jackie: Five (holds up 5 fingers also; Dolores adds one more). O.K. (Jackie holds up six too).
11. Dolores: No five (holds up five).
12. Jackie: O.K. (holds up 5 too). My, um, mother's and father's room. Six (both hold up six). Then we have (pause) um.
13. Dolores: Your room.
14. Jackie: My room and my brother's room (both hold up eight) and then we have um.
15. Dolores: The livingroom (9 fingers).
16. Jackie: The livingroom (9 fingers).
17. Dolores: The kitchen (10 fingers).

18. Jackie: The kitchen (10 fingers). And the place where we eat. The place where we eat. 11. And then we have, then when you walk (moves hands indicating direction) (inaudible) and you walk here. You know (pause) well we made a new room.

19. Dolores: Oh, well you counted that already.

20. Jackie: No, I didn't count the new room.

21. Dolores: Yes, you did.

22. Reggie: Yes, you did count the livingroom.

23. Dolores: No, the new room. You counted the new room.

(Discussion continues)

Here remembering is called for, and the items are counted, but the information can come from anywhere just so long as it is relevant to the task before the group. We see some of the specific points made by Kreutzer et al exemplified in Dolores' behavior: She stores part of the information for Jackie (with her fingers), and she actively joins in the search, supplying the necessary information (line 13, line 17) and her contributions act as mnemonic cues for Jackie (lines 15).

Attention: Doing two things at a time. The burden of speculations about attention in everyday life settings is that people will routinely be found engaging in two or more tasks at a time. This conclusion followed from experimental data indicating that if the "work" (Kahneman, 1973) or "control processing" (Shiffrin and Schneider, 1977) associated with one of two concurrent tasks is minimal, then simultaneous, or near-simultaneous, processing of the two tasks is possible. The same conclusion is suggested by data showing that when tasks are highly practiced (Spelke, Hirst, and Neisser, 1976) or when individuals have highly developed expectations about the sequences of activities that are unfolding, they are able to attend to multiple tasks. If we add the possibility that in everyday life the strict millisecond time durations that are used in laboratory tasks of attention are rarely encountered, then it becomes evern more plausible to suppose that people in everyday life will seem to be attending to more than one task at a time.

Our experience with the afterschool clubs provided considerable support for this kind of speculation. For example, in the course of baking cakes the children were required to pay attention to the steps necessary to cake baking (e.g. find the proper ingredients, determine the correct amounts, incorporate them into the batter in the appropriate order, etc.). At the same time, with seven children and two adults in the room all engaged in the same activity, each individual had to attend to what other people in the immediate environment were doing. Thus, obtaining the flour required knowledge of who was using the flour and when it would be available. The presence of seven children rather than eight (on the occasion we will describe presently) presented a problem of arranging someone for the seventh child to work with and each remaining pair of children spend a good deal of its time in efforts to avoid this child. Time constraints on the duration of the club, combined with the time needed to allow the cakes to bake, required everyone to keep track of their progress, a constraint that was heightened when several of the children overtly completed to see who could get their cake ready for baking the earliest. These, and other features of the "task environment" represented by the cooking club, were present with varying demand characteristics throughout the hour and a half that the children were at club.

The various features of an environment which presumably made possible the execution of these multiple tasks are available for analysis in our taped recordings of these sessions. Not only do the different tasks seem to vary in the demand characteristics central to the speculations, but the children often appear to arrange an attentional division of labor according to which one child of a pair focuses on decisions about required next steps for making the cake, (including checking the recipe, finding ingredients, measuring) while the other child worries about the more automatic tasks such as sifting the flour or greasing the pan. The child who takes on the simpler tasks is thus freed to worry about social relations with the other pairs of children and the adults. The children also sequence the tasks they attend to so that when the cake needs full attention from both partners, it gets it. And when social interaction demands full attention it also wins out. In other words, a number of tasks can be worked on almost simultaneously over time without interference.

A useful example is available from an occasion when Dolores and Helene make a cake together. Helene has focused intently on the making of the cake. By explicit agreement, Dolores is responsible for the automatic mixing aspect of the cooking task. She also concentrates on matters involving social interaction and coordination, especially the task of insuring that she and Helene do not have to work with the seventh child (who has been out of the room, but who will be assigned to one of the pairs upon his return). Once that issue is cleared up, Dolores and Helene both center on measuring (Helene) and sifting (Dolores) the flour. This is not an easy task; they must measure out 1 and 3/4 cups of already sifted flour from one bowl to another (with a two cup measuring cup). To do this successfully, they have to hold onto the flour for a long period of time in case they need some more--while the other groups need it. With this much background, it is possible to appreciate the following scene in which the adult (Nita) working with Peter and Archie at one end of the table comes to the other end of the table to take Dolores and Helene's flour. Before Nita even reaches the table, Dolores, who apparently has been listening to Nita's interaction with Archie and Peter, while at the same time attending to the sifting and measuring tasks, breaks from her work with Helene to tell Nita that they have not finished with the flour.

Example 8

Time (seconds)	Pair 1 (Dolores and Helene)	Pair 2 (Archie and Peter)
	Helene is studying the measuring cup; Dolores	Archie and Peter are both looking at recipe.

Time (seconds)	Pair 1 (cont'd)	Pair 2 (cont'd)
	is wiping flour that spilled from sifter off the table.	
12:45	Helene pours flour from bowl to measuring cup while Dolores picks up sifter.	PAUL: (reading loudly from recipe) ONE AND 3/4 CUPS OF FLOUR.
12:47	Helene is still pouring; Dolores is wiping flour from table.	NITA: NOW, THE FLOUR THEY (gesturing in the air and glancing at shelf) HAVE IS TAKEN.
12:50		NITA: UHH (stepping towards Dolores and Helene's end of table)
12:52	Helene continues pouring and measuring; Dolores begins to move her head up to Nita, who is entering about four feet away.	
12:54	DOLORES: (to Nita, now about 18 inches away) WE'RE NOT FINISHED. Helene continues pouring and measuring.	

Time (seconds)	Pair 1 (cont'd)	Pair 2 (cont'd)
12:55	NITA: OK,OK (turns away)	
12:56	Dolores turns back to table and continues clearning up the flour; Helene continues pouring and measuring; Nita begins to walk back to Archie and Peter.	
12:59		NITA: I'LL HELP YOU WITH THE FLOUR, GUYS. (grabs box of flour from distant shelf) HERE'S THE FLOUR.

Throughout this episode, Helene remains fixed to her task of measuring the flour. The final decision about how much to put into the bowl is made by both of them. From this brief example, it is possible to see how everyday life tasks are constructed to allow the apparent attention to two tasks at once, first, because they are made up of subtasks which people are used to handling automatically, and second, by allowing that things be attended to sequentially, rather than in brief 200 msec periods of time most often used in laboratory studies of attention.

This concludes our attempt at concretizing the speculations about thinking in everyday life by applying them to our club scenes. Some features of the speculations have not been presented in our illustrations. We will return shortly to Neisser's reference to the complexity of everyday life scenes. More importantly, we will consider most carefully Vygotsky's (1978) speculations as an alternative to the descriptions presented here.

Descriptions as Data

These descriptions (which are but a sample of those available in our corpus) are phrased in the terminology that we would use to describe behavior in the tightly controlled experimental tasks which they resemble. Referring back to the terminology of Bartlett, we have described everyday examples of cognitive activity as closed systems in which we specified the stimuli in terms of which the actor(s) was (were) behaving and the behaviors (responses) that were occurring. This assumption--that we could specify stimuli and responses, along with the relations between them--held even when we were giving an example in support of Bartlett's speculation that everyday thinking would be more characterized by "openness" than standard laboratory tasks! This seeming paradox remains because if we take seriously the notion that the problem solving environment is as open as Bartlett speculates, we find it all but impossible to describe; we would not know what aspects of the environment and behavior were relevant to the description.

Any experimental psychologist--indeed, any reader--should take two points from our descriptions set against the backdrop of our prior discussion:

1. Psychologists' speculations about thinking in everyday life, where experimental tasks provide the basis for analogies, find some support in observations of children in our clubs: in everyday life settings we can discover examples of cognitive activity that is recognizable as such.

2. There is something underspecified from these example; not only is the specification of stimuli and responses circular; it is suspect. While a quick reading of any example makes recognizable the basis for one or another speculation, each example seems far too complicated to fit the constraints that the analogue suggests. For each example one feels that we could come up with very different stories for what is happening. The evidence severely underdetermines any of the theoretical speculations.

In our view, uneasiness with our examples is well motivated. It is, in fact, this exact problem which induced us to write this paper. The central difficulty is captured in that part of our earlier discussion that emphasized the crucial role of closed system analysis to the enterprise that is cognitive psychology. Bruner put the central requirement very succinctly when he said:

> It is the essence in any given experiment that we define in advance what we as experimenters mean by relevant information and do not depend upon the subjects' response to do it for us; otherwise we would be in a complete circle (1951, p. 131).

Given the evidence from our examples, we think that psychologists must take seriously the possibility that defining relevant stimuli in advance is a goal, not an acceptable practice, at least for environments such as those included in our examples from club sessions. In fact, there is nothing more familiar nor more frustrating to the beginning psychologist than the fact that experiments breed more experiments than definitive conclusions. Repeated experimentation around a circumscribed problem is necessary in part because replication and variation operate as the experimentalist's hedge against a false *a priori* definition of the closed system environment under analysis.

In brief, one is advised to think of our examples of everyday cognitive tasks as bearing a relation to closed experimental tasks that is analogous to the relation of a sieve to a bowl. If the bowl is an environment which completely constrains its contents, a sieve is more open space than netting; there is enough metal netting to provide the sieve with the recognizable shape of a bowl. But, like a sieve, and unlike a bowl, our specification of task and behavior in everyday life cannot hold water.

There are a number of reasons why current analyses of cognitive performance in everyday life seem to crumble under close inspection. First, as Neisser asserted in the passage we quoted earlier (p.30), the scenes in which these tasks are embedded are characterized by multiple motives and often strong emotions so that instead of talking about *the* task, or even *these* tasks, we face a situation in which there are several tasks operative at once, some embedded within others, some of which seem to run in parallel, all of which need to be discovered. Second, individuals seem to be able to take advantage of the multiplicity of tasks by selectively responding to one or another at any given moment with the result that response demands for what the psychologist deems to be <u>the</u> task are often substantially reduced. We have come to talk of tasks as being "negotiable" by which we mean not only that the requirements for an adequate response to a particular demand characteristic of the environment are flexible, but that individuals use this flexibility to change what the tasks-of-the-moment are.

A third feature of many everyday life settings for thinking which permits (indeed, promotes, people to change the "stimulus conditions" for responding is that other people with variable relations to the multiple tasks at hand are a part of the stimulus environment. In changing the nature of the effective stimulus at different parts of a given cognitive task the subject is operating on an environment that simultaneously operates on the subject. For example, in deciding who to bake a cake with, figuring out how many trays are required to plant seeds in, etc. the subjects operate on other people, who, in turn, are operating on them in a continuous process of interaction. Consider also the complication that various actors in any setting are engaged in carrying out their own tasks (which may or may not be the same as those of the subject, but are likely to have to be coordinated with those of the subject in some way) and that cognitive activities are engaged in not as an end in themselves, but as a means to at once, some embedded within others, some of which seem to run in parallel, all of which need to be discovered. Second, individuals seem to be able to take advantage of the multiplicity of tasks by selectively responding to one or another at any given moment with the result that response demands for what the psychologist deems to be the task are often substantially reduced. We have come to talk of tasks as being "negotiable" by which we mean not only that the requirements for an adequate response to a particular demand characteristic of the environment are flexible, but that individuals use this flexibility to change what the tasks-of-the-moment are.

A third feature of many everyday life settings for thinking which permits (indeed, promotes, people to change the "stimulus conditions" for responding is that other people with variable relations to the multiple tasks at hand are a part of the stimulus environment. In changing the nature of the effective stimulus at different parts of a given cognitive task the subject is operating on an environment that simultaneously operates on the subject. For example, in deciding who to bake a cake with, figuring out how many trays are required to plant seeds in, etc. the subjects operate on other people, who, in turn, are operating on them in a continuous process of interaction. Consider also the complication that various actors in any setting are engaged in carrying out their own tasks (which may or may not be the same as those of the subject, but are likely to have to be coordinated with those of the subject in some way) and that cognitive activities are engaged in not as an end in themselves, but as a means to some transformation on the environment (getting the cakes baked, seeds sprouted, friendships established) as Vygotsky suggests. The everyday world consists of dynamically organized environments which find no representation in laboratory models of intellectual activity.

It is these ubiquitous features of non-laboratory tasks which create the conditions permitting some psychologists to suggest that everyday contexts for thinking are relatively simple vis-a-vis the demands of psychological tests and experiments (because individuals divide the labor, the environments are designed to facilitate problem solving, etc.) while others can point to its great difficulty (because so many tasks can, and often must, be dealt with simultaneously).

Difficult or easy, if we are right in claiming that everyday life settings for thinking have some of the characteristics just listed, there are two severe consequences for the experimental, cognitive psychologists concerned with the ecological validity of their observations: (1) everyday life contexts for thinking differ in important ways from the contexts assumed to obtain in laboratory tasks, thereby undermining efforts to say that an individual behaves more or less effectively in one environment than in another and 2) the analytic apparatus we bring to these environments from experimental psychology <u>does not apply</u>.

These are strong statements. In order to make clear their meaning while providing some elaboration of their abstract claims, we will review the examples provided in the previous section, pointing to what we believe are the holes in the sieve.

Describing the number of mental steps. Example 1 (p.51) purports to illustrate the nonsystematic nature of reasoning in everyday life, which is characterized as entailing fewer mental steps than reasoning in laboratory situations. Recall that our description of this scene, in which the problem is what to do with Rikki, illustrated some of the specific features Bartlett mentioned: contradictory conclusions are reached from the same "evidence"; facts are taken as evidence without their being evidence in any logical sense; a conclusion is accepted and the problem "solved" on the basis of a "fit" between an accepted generalization and a piece of evidence. There are a number of reasons such a characterization of this scene is problematic, even if we restrict our description of the task to the one predefined as "what to do with Rikki."

Features of this scene not remarked on when we presented it as support for Bartlett's speculations give us reason to think that a description which makes it appear to be an instance of problem solving with fewer mental steps is both inadequate in its particulars and wrong in principle.

Our basic problem with any hypothesis about the number of mental steps in any task (in experiments or everyday life scenes) is that it cannot be substantiated without an adequate description of the stimulus environment and the many constraints on the behavior of the people under analysis. The basic environment within which people are taking their mental steps must be well defined in terms of how that environment shifts with each step along the way. Our previous description is inadequate because it defined the landscape in advance of each of the steps that defined the problem environment for each participant, but these were knowable only as the problem unfolded. We judged each child's actions as they either contributed to or in some way subverted the tasks, allowing a thoroughly "rational" discussion of where Rikki should go--rational at least as it is defined by the outside in search of a well formulated road map of the children's activities.[5]

By leaving out a description of the problem environments which the children face from moment to moment, we are skipping the evidence that we need in order to produce ecologically valid accounts of behavioral processes, evidence that could help us to decide just what task the children are doing and just how it might be different from or the same as other tasks that we might want to analyze either in the laboratory or in everyday life. The evidence exists in the behavior of the group members which provides a shifting stimulus environment for each participant in the course of a problem. We have analyzed the talk of the children as if there were only a single goal which we can use to analyze their mental steps.

Their thinking about what to do with Rikki is probably more complex than that analysis suggests. We have good reason to believe that there are many goals each of them might be working on. For example, it seems as if a primary goal of each child is to work with a buddy, and Rikki represents a possible problem for each of the pairs. It is also a goal (expressed repeatedly prior to this scene) of each of the pairs to get to the job of making the cake, and this seems to be a strong enough goal that if it means taking Rikki on as a third partner then that will be done. Each of the children also seeks to look as virtuous as possible while trying to avoid working with Rikki; for the most part they argue normatively in justifying their moves or calling on others to justify their moves. All these goals can be cited as possible without even mentioning the many goals that each child might be operating on in terms of specific other children given their specific histories of likes and dislikes. The children somehow reach a consensus and Rikki goes off to work with Helene and Lucy. How this is accomplished is the process we must describe in order to offer an account of the mental steps of any of the children if we wish to compare their behavior with each other or with other persons in other scenes.

This scene richly illustrates our contention that not only can the problem environments facing people be multiple, but negotiable and organized in the course of interaction as well. The problems which the children are asked to solve are, in an important sense, created by their own behavior with each other; every move that everyone of them makes in some way alters the problem environments and contributes to the solution of these problems. Given this ongoing and mutually created nature of their problems and solutions, an analysis of problem solving that rests on a predefined account of the problem is clearly inadequate.[6]

Unspecified in our description of Example 1 is the range of information available to each participant in terms of which they can try to reach a solution. For example, when Dolores offers a solution (lines 5 and 11) it fails to "stick." Why? Nadine does not overtly contravert Dolores, but her behaviors (grabbing an umbrella, smiling without speaking while looking away from Rikki until Dolores also smiles without repeating her offer) seem to function as rejections. Rikki's statement on line 12 can be interpreted in two ways: either as supporting evidence that she should not work with Helene (the first half of her remark) or evidence that she could work with Helene (the second half of her remark). Because we do not know the meaning of these stimuli as a part of the stimulus environment for Helene's problem solving, we cannot reject the possibility that she is engaged in a rather detailed reasoning process which includes her status vis-a-vis the other children with whom Rikki might work, which includes the information that the last time they worked together they didn't fight, that they had not argued on this occasion, that trouble was offered from the other pairs if she worked with them, from which she drew the conclusion that Rikki could work with them. When Helene stands up and begins to act on this solution (no other solution has been acted on or indeed agreed upon), everyone accedes to her conclusion, which allows the group to get on with the baking.

Describing facilitating environments. The speculation that everyday life presents a facilitating problem-solving environment clearly demonstrates the problem of underspecification which vitiates all our examples. When extrapolating from laboratory tasks, it is too easy to make the error of conceiving of the environment as a series of stimuli which are judged to be facilitating or misleading, independent of the activity that occurs in that environment. We saw (p. 54) that in making cranberry bread, the children were faced with what could have been a real-life conservation problem--three different sized bowls which were to be filled with varying amount of ingredients. However, it is illusory to think that this example either illustrates or supports speculations about facilitating environments for problem solving, because close analysis reveals that in terms of the analogy, <u>there was no problem</u>; even the smallest bowl was large enough to hold all the ingredients. However, the different sized bowls did become a problem for Dolores and Nadine (albeit not a problem in conservation), who realized that differing amounts had to go in different bowls, but were not aware that all the bowls were large enough. For them, the situation was made problematic, even though from the analyst's point of view, it was facilitating relative to a Piagetian conservation task.

The inadequacy of the description of this example is analogous to the inadequacy of the description of Example 1 using Bartlett's terminology. To say that the environment is facilitating is meaningful only in so far as it is specified in terms of a particular person and a particular task. Whether the environment is facilitating or not cannot be specified in terms of aspects of the environment alone. It also requires a specification of what has to get done in that environment (a point well understood by Case, whose imagined examples of everyday environments contain these requirements). In this instance, if the goal of the activity is to pour flour into a bowl without any of it spilling over, then the fact that whatever bowl you use will work may mean that the environment is facilitating--for that task. But, it is unlikely that the children are involved in making sure no flour spills as an activity in its own right. Other likely tasks that Dolores and Nadine are involved in include: following the recipe correctly (which does refer to small, medium, and large bowls); measuring ingredients in such a way that they don't look incompetent and get laughed at; keeping track (and ahead of, if possible) the other pairs of children. These tasks often overlap each other.

We include here portions of the transcript of Dolores and Nadine making cranberry bread. Although what is presented here is not continuous nor does it include what is going on around Nadine and Dolores, it should serve the purpose of showing 1) that the bowls are truly an issue for them, and 2) that this problem is embedded in multiple activities with multiple steps and participants, some of which can be seen in the identity investments the children make in which bowl they should use. After they finally finish the task, Nadine sums up their efforts by saying "Dolores, you're so stupid sometimes". More than one task is brought to a close with this line.

Example 9

(Nadine brings over a bowl to the table; Rikki comes over and takes it).

Dolores: She just took our bowl (Nadine turns to Rikki; Nadine and Dolores go after Rikki) Rikki.

Nadine: You just took our bowl.

Dolores: Give us back our bowl (Dolores takes it; they return)

Ken: You don't have to take bowls, there are plenty of bowls.

Nadine: I like this bowl.

Dolores: This one.

Nadine: Yeah.

Dolores: O.K. but it's dirty.

Nadine: I don't like that bowl though (pause) O.K. ingredients (reading aloud), combine these ingredients in a small bowl and set aside. Oh we have to (inaudible). One and a half cups of chopped raw cranberries. Sift these ingredients (pause) into a large bowl and set aside (turns to other table). Luckily we did <u>that</u> one in <u>that</u> one. (reading) Three teaspoons baking powder (puts recipe down, picks up measuring spoons) Baking powder. Where's the baking powder? Baking soda.

Dolores: Now what?

Nadine: Half a teaspoon (baking powder). We hafta, shit, we hafta sift these (touches sifter).

Dolores: Shit(picks up bowl) You can sift.

Nadine: Oh no. We have to get another bowl to put this in (looking around for bowl).

Dolores: We have to sift more you said (picks up bowl)

Nadine: I know, well keep it in there (pointint to another bowl) and get that (Nadine laughs) (Dolores empties ingredients into original bowl, laughs, too, gets sifter) Dolores, you're so stupid sometimes.

In Example 2 (p. 54)(as well as in most of the other scenes in which our 2-cup measuring cup was potentially misleading) there were other people--partners, other pairs, adults--who supplied information to disambiguate the situation and contribute to moving things forward. In this case, Peter, who understands how to read the measurements on the cup, explains to Dolores how to measure. A major point is that he does this; i.e. that the activities the children are engaged in are not isolated and discrete; figuring out how much one cup is, where it is on the 2-cup measuring cup is part of some larger activity. The children need to know how much one cup is in order to measure flour; they need to measure flour in order to bake a cake, and so on. The goal of this particular activity is not the cognitive activity in isolation; as a consequence we see clearly the way in which participants provide part of the information (thinking) needed to accomplish a task for each other.

Describing remembering. Our description of Example 5 (p..59) illustrated how everyday environments are physically structured in a way that reduces memory load. What is unspecified in that description are the following features. First, in this case, the structuring is actually misleading. That is, Dolores: No watch, watch, wait, just--

Nadine: (hits fist on legs frustratedly) This is great. What are we supposed to do now? (Dolores hits table). It doesn't have to be sifted (picks up mixing spoon).

Dolores: Just (moving another bowl over) just (picking up the sifter, empties ingredients into sifter)

Nadine: It says a large bowl. Sift, sift. (Dolores sifts, reads) Three, three fourths a teaspoon salt (Dolores begins to transfer sifted ingredients back into larger bowl) No! Three-fourths teaspoons. Yeah, right.

Lucy: (reaching over to get salt) Excuse me.

Nadine: (grabbing salt) Sorry, our salt. (Lucy walks away. Nadine laughs) Three-fourths a teaspoon.

Lucy: Forget it Nadine. I don't need your salt. I have my own.

Nadine: (measuring) Here one fourth, two, three fourths (Dolores sifts out what is left in sifter) Will you put this back, we have to sift more. (Dolores wipes table) Dolores will you ha- (Dolores throws ingredients from table into bowl)

Dolores: O.K. (lifts bowl)

Nadine: Just bring that over here (Dolores empties into smaller bowl) What are you (stamping feet) Don't put it in!

knowing that the shortening is always on the shelves doesn't lead to finding the melted shortening; it actually leads to a detour (the Crisco). The memory that is in the environment interferes (temporarily) with the solution. Second, the utilization of the organized environment gets done not by Nadine, the one who originated the question ("Where're we gonna get melted shortening?") but by another child, Rikki, who points to the Crisco on the shelves. And third, the next step along the route to solution--finding the melted shortening--is done by a third child, Helene. These aspects of this scene nicely illustrate how problem solving in everyday life can be a social activity, in which interaction provides a multiplicity of well timed stimuli, a feature of our club environments not capturable by the language of the speculations.

The main shortcoming of our description of Example 6, (pp. 59-61) where the children were ready to plant seeds, is that to characterize it as a memory task analogous to a laboratory recall task is misleading. If Mike, the originator of the questioning, had meant it to be such, he would not have settled for 5 out of 6 items being recalled, nor would he permit Rikki, the child he questioned, to supply only one of the five items while allowing others to respond. What is left out of our previous description is the multiplicity of activities that are going on, the interactional nature of these activities and the shifting and negotiable nature of the problem-solving environment. That remembering fruits and vegetables is <u>not</u> the goal of the activity is made clear by both the children ("There's not enough trays") and Mike ("There <u>will</u> be enough trays if we plant it right").

In fact, it appears that the actual number of items to be planted (remembered) is unimportant at least at this precise moment; that outcome is clearly negotiable. What all the participants agree is necessary is an arrangement permitting seeds to be arranged in trays. This is true in terms of the number of items mentioned (the 'answer') and also the planting task itself. When one of the children becomes impatient and puts a seed into the dirt, the activity abruptly changes. The theme and task in general remain the same (how to be a good 'farmer') but the specific subtask shifts from how many rows are needed to how the seeds have to be put in the dirt.

Our description of Example 7 (pp. 62-63) in terms of the last speculation concerning remembering activities in everyday life--that other people are utilized as recall and retrieval devices--appears to be a reasonable characterization of this scene in terms of how one child, Jackie, recalls the rooms in her apartment. What is missing from the speculation but evident in our description is the joint nature of the activity, which is far more complex that simply a cued recall task happening in everyday life. Dolores' role is not merely one of a mnemonic device for Jackie; rather, Dolores' role, and consequently what she and Jackie are doing together, fluctuates. For example, Dolores initiated the challenge ("Count them all"), yet she takes an active part in keeping track for Jackie of how many rooms they've counted, adding items that she herself recalled, and correcting Jackie. The activity in turn changes, and it is not for long a clear-cut case of a recall task done by one child.

Describing attention and effort. Our validation of the speculations that people in everyday life can attend to two or more things (Example 8, pp. 66-68), also appears to be irremediably problematic; it is not at all clear how the range and limits of people's attention in everyday life can be specified. As a starting place, we would need a strong definition of the tasks with which a person was involved, and, as we have seen repeatedly, this is difficult to come by even in the laboratory.

We have made the case that Dolores, while working on clearing flour from the table, is able to attend to a statement by Nita, the adult, to the two boys at the other end of the table. The statement has possible consequences for Dolores, and she displays her attention by addressing Nita before Nita can even ask her for the flour. But we cannot say how many things Dolores is attending to or how much effort any of them might demand. Even if we knew approximately how many tasks the group was able to maintain for the members to attend to, it would take a detailed account of their scheduling to specify the approximate difficulty of any given task. In our description of Dolores attending to two problem environments at one time, we claimed that her activity was made possible in part by her taking on the simpler jobs faced by the two girls in making the cake. But we must remember that Dolores, after defending flour against Nita, is back to the cake task in time to participate in the final measurement of the flour--presumably a difficult task for these newcomers and one that is best worked on with two heads. And so it is throughout the cake making; Dolores returns from her various forays into the social borders around her just in time to engage in decisions about baking (with the exception of a few instances where her delayed return brought shouts from her partner). Dolores appears to be attending to multiple tasks in a complexly scheduled way that allows her to get to each of them at crucial moments. In addition, others in her environment are attending to Dolores, doing part of her "attentional work" for her. If this is the case, then the possibility of specifying either the number of tasks the child is attending to at any given time or how difficult she is finding them is open indeed.

Now consider the most difficult problem of all. We have been discussing Dolores' attentional efforts as if the world simply floats by her, rewarding her for attending more to some things than to others, and as if our only task was to count up how many of those tasks she tried to attend to at a single time and with how much effort. However, one consistent feature of Dolores' environment throughout her time in club is that she is an active part of it. She not only has to attend to and anticipate tasks in her environment, she must also help to construct the environments in terms of which her anticipations are useful. Dolores may be far busier than our description allows for. Even if her attentional work takes less effort than we have been assuming, we are missing an account of the work Dolores does to construct the environments in which she displays her attentional know-how. Furthermore, no leads to such an account are provided by the speculations concerning attention derived from experimental tasks.

Conclusions

The preceding sections describe our attempt to understand the sources of our difficulties when we tried to determine if the intellectual performances of a group of youngsters administered a variety of cognitive tasks could be compared with their performances in a sample of situations which we have glossed as everyday life. Without repeating the evidence that we have considered, we will close by characterizing our conclusions regarding current practices.

It appears to us that our initial formulation put the experimental cart before the everyday horse. In effect, we were searching for models of experiments in non-laboratory environments instead of seeking to model important everyday life activities in the "model" settings that are the raison d'etre of experimentation. When our search ran into difficulties and we began to seek the cause(s) in a closer examination of a variety of everyday life settings, we discovered principled differences between everyday cognitive activity and the laboratory activities described by cognitive psychologists.

In creating model systems to study hypothetical cognitive processes, experimental investigators remove features of everyday life settings from their experiments. So long as the importance of the features abstracted to construct the model is acknowledged and the model has not inadvertantly removed a feature which renders its laws fundamentally restricted and unrepresentative of the source environment, we witness only the path of development of any science in which experimentation is a tool. But if the model system we construct should fail to include a central feature of the setting from which it is abstracted (for example, if it should have happened that in creating a vacuum tube, physicists created an environment in which gravity was also inoperative), the theory evolved in that model system will be applicable only to settings which share its restrictions. It is here that our quarrel with current practices rests. Our data urge on us the conclusion that many currently existing cognitive experiments represent the analogue of a gravity-less vacuum tube; they systematically suppress or exclude the interactional influence of individuals on their environments (including other people) that transforms the nature of the task environment in the course of responding to it.[7]

A detailed explanation of the reasons for removing this kind of subject-environment interaction from consideration in experimentalists' model settings would take us far beyond the framework of the current discussion. In brief, it appears that since its inception as a science, psychology has used task environments which assume a framework of analysis in which stimulus causes precede response effects, coupled with a strong claim that pre-assigned, initial stimuli are <u>the</u> stimuli to which some later indicator of individual behavior is assigned the status of <u>the</u> response. When we consider the added analytic requirement that cognitive theory must rest on specification of a closed system, current practices seem overwhelmingly necessary. So necessary does the normative experimental framework appear, in fact, that cognitive psychologists currently considering the problem of generalizing their techniques to complex settings are confident that the extrapolation entails no problems that are, in principle, new to non-laboratory settings.

For example, Herbert Simon presents a lucid argument of the proposition that "cognitive social psychology is cognitive psychology" (Simon, 1976a), by which he means that the basic processes discovered in the former enterprise will hold (albeit in changed configurations) in the latter. His characterization of the enterprise can serve as a clear indicator of current thinking on this issue.

> Several of the experiments from the chapters that I have already mentioned indicate that there is no such separation of processes. These experiments also provide a general experimental paradigm that can be used to test the commonality of cognitive processes over a wide range of task domains. The paradigm is simple. We find two tasks that have the same formal structure (e.g., they are both tasks of multi-dimensional judgment), one of which is drawn from a social situation and the other is not. If common processes are implicated in both tasks, then we should be able to produce in each task environment phenomena that give evidence of workings of the same basic cognitive mechanisms that appear in the other (p. 258).

While we have no quarrel with a plausible speculation, we have every reason to doubt that it is verifiable because <u>we cannot</u>

find two tasks with "the same formal structure." When we deal with real life examples of such tasks as multidimensional judgments we find that equivalent formal structures escape us because the participants do not adhere to the presupposed framework on which Simon draws.

The implications of this situation seem clear to us: In so far as we are unable to specify formal equivalence of tasks across settings, we cannot generalize about the behavior of individuals from one setting to another. The force of this point is emphasized by Schwartz and Taylor's (1978) discussion of test validation:

> Does the test elicit the same behavior as would the same tasks embedded in a real, noncontrived situation?. . . Reality is a difficult notion. Both physics and the psychology of perception tell us that human interactions with the real world are usually--some would say exclusively--mediated by models that inform the vision of the beholder. Further, even to speak of the same task across contexts requires a model of the structure of the task. In the absence of such a model, one does not know where the equivalence lies (p. 54).

It follows that where experimental procedures systematically preclude determining features of non-laboratory tasks, they limit the possibility of obtaining the required formal equivalence.[8] Therefore, as currently conceived, laboratory experiments are ecologically invalid; They cannot serve as a vehicle for making statements about the cognitive processing of individuals across settings.

We consider this conclusion to be an accurate description of the current state of our science. Having reached such a conclusion, what courses of action are open to us?

Some Alternative Ways to Evaluate and Extend Cognitive Analyses

I. Experimentation and Observation

a. Use observations to restrain ecologically invalid generalizations

One strategy for incorporating everyday life contexts for thinking into psychological theory characterizes the work of Cole, Scribner, and their colleagues who have insisted on the importance of analyzing naturally occurring tasks as a means of "situating" experiments (to use Scribner's (1976) apt phrase). Performances in everyday life often appear to require the application of some cognitive activity that is of interest to the psychological investigator. In Gay and Cole (1977), the ability to use interlocking units of measurement was demonstrated, along with its restriction to the domain of measuring the volume of rice. In Cole, Gay, Glick and Sharp (1971) analysis of court cases and traditional riddle stories turned up instances of hypothetical reasoning that were difficult to elicit in experimental tasks not modelled on the everyday life originals. This finding induced the authors to restructure problems in order to map more closely onto their hypothesized structure of the everyday life examples, with the outcome that hypothetical reasoning was elicited in what was now a laboratory-style task. In a similar manner, observations of story telling led to the construction of modified versions of free recall tasks that elicited reflections of organized recall that were missing from prior laboratory versions of free recall tasks. Finally, analysis of the practice of reading an indigenous script resulted in demonstration of abilities in the analysis of language by tribal Vai people, abilities which appeared nonexistent using tasks borrowed directly from the experimental literature (Scribner and Cole, 1978). WIth the exception of this last study of literacy, all these efforts can still be faulted for initially putting the cart before the horse. That is, while everyday life scenes have informed experimental procedures, the starting point was still the experiment. The closed system was opened to allow for modifications in laboratory tasks only to be quickly closed again.

All of these efforts are subject to criticism because the analysis of the everyday life settings is relatively unsophisticated. However, they have the virtue of reporting on ubiquitous and often highly valued activities, and of restricting statements about what people can and cannot do made on the basis of laboratory studies alone. Thus, for example, Gay and Cole's observations lay to rest conclusions such as "tribal people can't measure," "tribal people have no concept of measurement" (both of which were popular at the time this work was done), and the observations of Cole et al (1971) forced them to turn to new avenues of investigation in order to determine why people who could manifestly reason hypothetically did not do so in their experimental tasks. In like manner, Shatz and Gelman's (1973) analysis of children modifying their speech disabled conclusions that preschoolers are generally unable to take their listeners' needs into account (although their analysis leaves unspecified why preschoolers can do so in the "explain a toy setting" while they fail to give adequate referents in the standard referential communications task).

The success of these initial attempts at checking the fit between experimental and everyday behaviors suggests one principle for increasing the ecological validity of experimental, cognitive psychology: any experimental model should be tested against a detailed description of at least one such setting. Engaging in such efforts should begin to give us a better understanding of how, with an appropriate sense of distrust and irony, to test some hypotheses about that setting as if we could construct a full account of the particulars of its organization.

b. Use observation to formulate ecologically valid experiments

If, as we have asserted, limitations on the generalizability of laboratory-based observations to everyday life settings come about because important principles were removed from analysis in the course of constructing the experimental model, an obvious avenue that experimental, cognitive psychologists should consider is the expansion of their repetoire of experimental settings to include important characteristics of the settings they are supposed to model. To a limited extent, such modelling goes on all the time, although it is more likely to be constructed on an anecdote (as Cherry's cocktail party anecdote seemingly captures attention phenomena) than on a serious analysis of the phenomenon *in situ*.

studies are found, and if these are of sufficient interest to warrant the work of experimental analysis, they may well serve as a starting point to which we may wish to generalize (Istomina, following Vygotsky, viewed school work as such a setting).

To take a very different example, Bjork (1978) argues that "updating"one's memory is an important everyday activity, and he begins a review of his research on this topic with several persuasive examples (remembering where we left our car in the parking lot _today_, remembering our _current_ phone number). There are a variety of ways to model the activities that go into up-dating, and Bjork's experimental paradigms suggest several. But analyses of updating that do not make close connection with the intuitive examples which serve to motivate the research leave too much to be specified, as the cooking club transcripts we analyzed in some detail make clear. In Bjork's case it would be of interest to take one of the examples he offers and to study how well his models of updating fit behavioral reality. The example of an air-traffic controller's job seems to offer an excellent place to begin, for as Bjork argues, the air-traffic controller is "responsible for a set of information that denotes the status of some number of planes. At some later point that set will be replaced by a new set of information and it is highly desirable that the controller not be confused as to the set membership of any given item of information" (Bjork, 1978, p.). It seems plausible that in actual practice, air traffic controllers

Note that in asking that the experiment be a model of the settings to which we will want to generalize, we are not foreclosing on the possibilities of introducing severe constraints on what people are allowed to do, presenting people with artificial stimuli. Rather, we are saying, in effect, that we must keep track of the introduced variations and we must evaluate their impact not simply in terms of the behavioral products generated but in terms of how the different products are organized. This is by no means an impossible task, as research reviewed above indicates. For example, the research on children's memory by Istomina (1975) was built on a model of the activities that children are ordinarily asked to engage in when they have to remember something for a short while and then use that information (e.g., when they are asked to go to the store). In the course of this work, Istomina also provided an essential contrast with the free recall experiment which turned out to be a poor model of the remembering situation that the children faced in the kindergarten-to-market task.

The direction implicit in this description is important. We might say the the kindergarten-to-market-task is a poor model of free recall (which it is). But since Istomina began by trying to model the structure of environments that the children ordinarily face, and because she identified certain features of that environment with a broader class of everyday situations for remembering <u>which were the starting point of her analysis</u>, we are fully justified in claiming that it is the free recall situation that is the poor model. However, if there are real-life settings where the kinds of constraints placed on people in free recall engage in some or all of the activities required by Bjork's experimental models, but as his research program makes clear, it is not a simple matter within his experiments to constrain people to behave exactly as the model specifies; entire experimental series are needed to map out the possibilities in even a relatively simple form of the model system. Thus, it is equally plausible that if Bjork were to observe carefully the work of actual air-traffic controllers he would be induced to modify his model system to include behaviors that he had not imagined, beginning as he did from anecdotes and intuition to build a model system that _then_ comes to be justified in terms of its representation in everyday life.

Viewed from this perspective, a great deal of cognitive psychological experimentation can be interpreted as the construction of analytic models based on anecdotal accounts of settings where people face a constrained task: it may be a sixth grader facing a math test, a voter trying to decide between the lesser of two evils, or an astronaut struggling to keep track of a complex guidance system displayed on meters. An important job facing those who seek to maximize what experiments tell us about people in a variety of such environments is to discover those settings where the structure of typical experiments and valued non-experimental activities have a good deal in common, for these will be the settings where experimental analyses as currently embodied will have the most to say.

Thus, for example, we should expect that during those parts of the school day when sixth graders are constrained to study or problem solve under conditions that look like a memory or problem solving experiment (say, learning Spanish vacabulary or inducing the effects of inclined planes), their activity may come to approximate the settings studied by cognitive psychologists. The correlations between tested cognitive performance and school performance, while statistically significant, are of a magnitude which tells us that a great deal of the variability in children's sixth grade performance is not captured by our current experimental settings. Nor, as Estes (1974) has pointed out, is the problem likely to be solved by further refining our tests. A more fruitful direction to look for increased predictive (generalizing) power is to a better description of the scenes that we want to generalize to, as a presursor, a supplement, a validator, and (in cases where experimentation is not possible) a replacement for further experimental analysis.

II. Observationa and Adequate Description

On various occasions it will be impossible to isolate and manipulate in experiments what appear to be the key ingredients suggested by a careful description of a setting in which we are interested. In such cases, description will have to stand on its own. The questions we face then are how to do as adequate description as possible and how to increase our power to test hypotheses about the principles organizing behavior in the absence of experimental procedures. The answers to these questions are by no means clear. On the issue of observation, we have a better idea of how <u>not</u> to proceed than how <u>to</u> proceed; we will discuss each aspect of the problem in turn. On the issue of how to generalize from observational data, we will have even less to say, although we are more impressed now than we were before undertaking this paper that theoretically motivated generalizations about people across settings depend critically on an analytic description of each of the settings involved using a common set of descriptive categories.

a. Using experimentally derived coding schemes to describe everyday life

Our analysis leads us to reject as misleading attempts to break out of the limitations of experimentation by merely applying to everyday life scenes a descriptive scheme derived from experimental models of cognitive activity. The reason for our negative evaluation of such efforts should be clear: If an experimental setting (say, for the study of problem solving) is missing important principles which organize everyday behavior, a descriptive scheme based on the laboratory model will distort systematically the representation of the everyday life scene because its categories are flawed in principle.

In our view, exactly this problem afflicts some portion of that research which purports to provide an ethological analysis of intellectual behavior. For example, Charlesworth and colleagues (Charlesworth, 1976; 1978; Charlesworth and Spiker, 1975) have developed a taxonomy of responses to various problems that children encounter in the organization of their cognitive and social activities at school and at home. Charlesworth's definition of a problem is taken from the experimental psychological literature on this topic and the categories of stimuli and responses arise from this source.

Charlesworth forthrightly declares his intention to focus on actual behavior rather than on conventional definitions in an attempt to identify the exact environmental conditions that are involved in different problem solving episodes. However, technique seems to overcome spirit. Only a small portion of the children's behavior (about 6% of the time in a normal preschool) is spent solving problems according to Charlesworth's criteria, and only those problems that appear in isolation and in a linear sequence simple enough for an observer to put post hoc into a stimulus-response frame can make their way onto the checklist.

As a result of his procedures, which presuppose the legitimacy of his analysis, Charlesworth can offer only weakly specified tasks. Sometimes task descriptions are bolstered showing a relation between performance on hypothesized tasks and other dimensions of the subjects' social biographies (e.g. their status as "retarded" or "younger.") Attempts to specify process have to be given up, a point acknowledged by Charlesworth, but too easily overlooked in a strong push for any technique to describe cognitive behavior outside of the laboratory.

This same difficulty is evident in the work of Quinn (1976) who has described decision-making mechanisms of litigation procedures used in a West African coastal town. Although it is never made clear just what litigators do, or how much work they put into their decisions, Quinn describes their decision-making as a simplified version of the one they would need in order to consider all the possible inputs into each case. Her description matches expectations from the work of Simon (1956) and Tversky (1977) who attempt to model such complex decision-making schemes. The grounds for her claim that the cognitive model fits the litigation behavior rests on a weak task description of actual litigation; the most one would want to claim is that the cognitive theory provided a useful framework within which Quinn began the job of describing naturally occurring problem solving. A great deal more analysis would be needed before this description could be used to compare individuals within or between settings.

Another research problem which relies on categories carried over from experimental settings is the work of Nerlove, Roberts, Klein, Yarbrough and Habicht (1975) who set out to locate natural indicators of cognitive development in two Guatemalan villages. Spot observations of the children in various settings in the village were the basis for analyses of how much of their activity was "self-managed" and "voluntarily instituted". Descriptions of behavior in terms of these categories were then correlated with the children's performance on some standardized cognitive tests; the positive relations between the two scorings were taken to demonstrate the degree to which child-initiated and self-managed sequences of activities were useful, natural indicators of cognitive growth.

Nerlove et al justify their approach on the basis of correlations between frequencies of entries in their observational categories and test data. They do not provide a close definition of the tasks or behaviors under analysis in the two settings. This should not be surprising because "self-managed" and "voluntarily-initiated" are concepts with a long and stormy history in experimental psychology, where it has been all but impossible to get agreement on criteria for their application. As a result of Nerlove et al's procedures, two problems arise: 1) there is no theory available to explain why the various tasks intercorrelate and 2) the demonstration is difficult to replicate (as shown in Rogoff's (1978) careful attempts to do so). By underspecifying the task in the situations being correlated, Nerlove et al are forced to rely on adequate correlations with test scores to test the power of their observations. This assumption is too great a leap for us, for we can imagine no way to confirm or deny it--whatever the correlations. Hilgard's (1955) remarks on this kind of use of correlation more than two decades ago seem especially relevant. Speaking of Brunswik's use of correlations in his probabilistic-functionalist approach, Hilgard concluded that "...correlation is the instrument of the devil. One has to be extremely careful in making any kind of analysis into scientific laws on the basis of correlational analysis unless one already knows the causal determiners..." (p.).

b. Use everyday life events to provide descriptive categories of thinking activities

Another approach to the study of thinking in everyday life has been inspired more by ethnography than ethology. A basic tenet of this work is to begin one's analysis from the natives own definitions of the tasks they set for themselves. However, in practice, we see that the virtue of not predefining a task for closed system analysis gives way to the vice of leaving the tasks people face every day grossly underspecified.

This point can be made with reference to the extremely interesting work of Gladwin (1970) who provided a description of the navigational activities of Micronesian sailors. On the basis of discussions with several navigators, observations of navigational lessons on land and some sea voyaging, Gladwin was led to conclude that:

> Navigation requires the solution of no unprecedented problems. The navigator must be judicious and preceptive, but he is never called upon to have new ideas, to relate things together in new ways (p.).

In view of our prior discussion, it should be clear that Gladwin is making strong claims for navigation as a closed system in which he can specify both the relevant stimuli and the permissible range of responses.

It is a bit unnerving, then, when Lewis (1972; 1977) offers descriptions of long and successful sea voyages in which the navigators solve problems that Gladwin tells us they would never face (and would be unable to solve if they did face them because new, "heuristic" problem solving behavior is required). We must get even more uncomfortable when Reisenberg (1972) provides us with 11 different, complex navigational systems used by the same people with whom Gladwin worked and informs us that he is certain that he has not exhausted the full set of such systems. Moreover, Reisenberg offers compelling evidence that systems of local mythology which Gladwin presumed to be totally non-functional with respect to navigation are in fact elaborate mnemonic devices (of the sort described by Vygotsky, 1978) to insure recall of of important navigational information.

While many other examples could be culled from the anthropological literature, the main point is that it is exceedingly difficult to achieve a closed system description of cognitive activity through observation.[9]

Recognizing these difficulties, some ethnographers have begun to develop systematic descriptive techniques which have as their goal closed system descriptions of behavior, sharing many properties of the systems which psychologists seek via experiment.

One such effort, termed context analysis by its practitioners (Kendon, 1977; Scheflen, 1966; 1973), is to use behaviors that precede, accompany, and follow the behavior of interest to establish the "context" for the behavior being analyzed. In these analyses, it is taken as axiomatic that no behavior can be counted or interpreted meaningfully in isolation; all behavior must be defined and described in terms of its relation to other behaviors that characterize a person's interaction with the environment. In practice, this procedure requires the analyst to discover the relevant stimuli (including one's own and others' behavior), the range of allowable behaviors and the relations between them. In an important sense, "context" operates in such descriptions to support hypotheses about a <u>closed system analysis of the behavior-environment interaction</u>.

To date, such studies have centered on social interactions between dyads or small groups of people in relatively constrained settings (for a review, see McDermott and Roth, 1978). In the course of interacting, participants must be reactive to each other in a manner which is finely tuned and "displayed"; that is, each participant's behavior must be sufficiently interpretable by the other participant(s) to coordinate their behavior together. Context analysts use the information individuals provide to each other in constructing their interaction to test hypotheses about what each individual is doing at any given moment. Ideally, every behavior in an interaction is accounted for--every move and every utterance--as elements in a hierachy of contexts which organize ongoing, concerted behavior.

Thus, for example, if a child in a classroom waves an arm while a reading lesson is taking place, we cannot decide if the arm wave is a call for a turn to read, a ploy to make the child look as if he or she is calling for a turn to read, or a swat at a fly (or all of these and more) unless we can determine the sequence of moves of which it is a part and the environments of which that sequence is a part. To describe the moving arm, it is important to know if the child is in a reading group; within that context there will be sequences of more local-level contexts which will determine if calling for a turn to read will be responded to as appropriate or not. The definition of each more localized context in terms of the ongoing behavior of all the participants in the scene is extremely difficult and time consuming, because it must account for the fact that each individual's behavior is creating the environments for his/her own and other's behavior as well as responding to the contexts (stimulus environments) provided by others. It requires that the analyst justify any description of the stimulus environments through the participants' manifestation of those environments in their behavior (McDermott, Gospodinoff and Aron, 1978). In any isolated instance, the descriptions proposed are clearly circular; but as the amount of behavior-environment interaction encompassed in the description increases and multiple descriptions using the same basic units accumulate, the plausibility of rival hypotheses rapidly decreases to the point where one can begin to feel that an adequate description has been achieved.

There are advantages and disadvantages to incorporating this kind of analysis into psychological research concerned with generalizing about behavior across settings. A prominent disadvantage is that such analyses are exceedingly time consuming; hundreds of hours have and must be spent on analyzing single scenes, and single scenes are certainly no basis for strong generalizations. At a minimum, one would seek multiple descriptions of scenes of the same type (for example, the behavior of children and a teacher in a small group reading lesson). Faced with such a chore, it may be tempting to eschew theoretical analysis and opt for pragmatically useful devices like IQ tests which may account for only 20-25% of the variance in a narrowly designated range of children's behavior, but which at least accomplish that level of predictability with a minimum of effort.

Our experiences in mixing experiments and observation have urged upon us the fruitfulness of taking seriously the possibility that careful description promises enough theoretical and practical benefits to warrant a good deal more attention before we give up in favor of enginerring solutions to the problem of behavior generalization. Several investigators have asserted that future progress in developing practical instruments for predicting about individuals requires careful theoretical analysis of the processes underlying performance, because the tasks which are presently used for this purpose have evolved to the limits that pragmatic, atheoretical techniques will allow (e.g. Estes, 1974; see also Resnick, 1976). While these discussions have emphasized the need for more careful task analysis and theory construction for the predictor tasks, the theme in this paper is that the criterion task environments need just as careful attention; we see no real alternative to the kinds of context-sensitive description which we have characterized as "adequate," that is, a description which approximates the closed system analysis which is the goal of all scientific theorizing.

Although we have emphasized the difficulties and inadequacies of current methods of describing real world scenes and the parallel difficulties of linking such descriptions to experimentally derived accounts, there are also some immediate benefits to be seen in such an effort. We are encouraged that there are some examples of descriptions which are more-or-less adequate (in the sense of providing a plausible, closed system analysis) available for settings which cognitive psychologists look to as relevant arenas for the application of their experimental analyses. These include school room academic lessons (Griffin and Humphrey ,Note 1 ; Mehan, 1979; McDermott, 1976), therapy sessions (Labov and Fanshel, 1978; Sacks, 1974; Schelfen, 1966; 1973) and telephone conversations (Jefferson, 1973; Schegloff, 1972; 1977). In our own work (see Hood, note 2) we have been impressed by the power of careful analysis of compelx scenes such as that represented in our corpus of cooking club materials to help us specify the difference between what, on the basis of test performances, are called a specific learning disability and low intelligence; analysis of the behavior in situ, makes clear the ways and conditions under which a specifically disabled child can cope beautifully with tasks that more conventional analysis would force us to conclude he could not accomplish.

These instances point to the possibility that careful description may increase the possibility of ecologically valid accounts of behavior. Recall that in our earlier discussion of Brunswik and Lewin (pp.14-20) we emphasized the conflict between achieving both a representative sampling of the cognitive tasks that people face in everyday life and a description of the environment from the individual's point of view (e.g. the "life space"). The kind of description we are advocating offers part of the overall solution to this problem: The descriptions of the cognitive tasks are taken from real world scenesand the categories for those descriptions are accomplished via careful attention to the sensitivities of the participants in those scenes. The contribution which closed-system description can make to cognitive psychology has to be considered uncertain until we obtain more and better descriptions of how different scenes are organized and related to each other. At least this kind of descriptive work is not axiomatically irrelevant to solving the ecological validity problem in the long run; it is in non-experimental scenes that we do our thinking most of the time and it is to such scenes that we must generalize our experimental findings.

Adequate description, then may save us from generalizing on the basis of ecologically invalid data (which puts psychology in the curious position of reasoning with a minimum of mental steps, without a clear line of argument from premise to conclusion--exactly the practice that Bartlett characterized as the thinking of people in everyday life).

We also think it significant that our observations, including those sketched in the sample protocols which figured in our earlier discussion, can force a re-evaluation of theories derived in the laboratory and extrapolated on the basis of casual observation to non-laboratory scenes. Thus, for example, our analysis of our cooking clubs--scenes such as those which Jensen (1969) claimed as the source of his hypothesis that out-of-school environments can be successfully negotiated by the application of Level 1 skills--leads us to reject Jensen's claim. No scene that we have analyzed can be understood as constructed by individuals rote-responding their way through an interaction, engaging in no mental transformations of stimulus input. Quite the contrary, transformations on input are sufficiently complex that the kind of arduous context analysis we have described briefly is required to make clear what the stimuli are. On the other hand, our characterization of non-experimental contexts for thinking as negotiable and aimed at non-cognitive goals, so that individuals routinely provide crucial information to each other in a well timed manner, suggests some important ways in which such environments are managed competently by people who may well find it difficult to transform information mentally in experiments and school tests (e.g. within an opportunity to interact actively with the environment).

In this regard, we find that observation of non-experimental non-school scenes leads us to find limitations in Vygotsky (1978), although his general approach and prescient observations have been a rich source of insight in our work. What we find especially attractive about Vygotsky's theory is the way in which he incorporates features of social/environmental forces directly into his specification of cognitive processes, both as their source and part of their content. But what Vygotsky did not prepare us for is that children and adults would spend so much of their time arranging their environments so that they did not need to engage in cognitive activity without environmental support. While internalization of activities originating in the environmnet may be a proper characterization of what people become more able to do as they grow from infancy to adulthood, and what they do when constrained sufficiently, non-internalized thinking, in which cognition resides in the environment as much as the individual, is a pervasive phenomenon.

In our current way of thinking, scene (or setting, or situation, or context, including its participants) has become the unit of analysis, rather than the person. This shift is more than a shift in language; it precludes talking about the skills which people carry around "in their heads." We are not denying the existence of such skills or the theoretical value of modelling them as best our state of knowledge can offer. Nor are we claiming a determinative power of scenes as if they existed without their participants, their constitutents, or, even more strongly, their creators. We are simply confronting the fact that cognitive skills have to be specified in terms of the activities (environments) in which they occur. While experiments have always been grounded on this insight (if only one experiment at a time), we are going one step further to claim that we need systematic procedures for relating the different environments. Experimental practices have supplied us with only incomplete accounts of experimental environments, and no procedures for linking these environments with the others people encounter in their everyday lives. It is this situation which leads us to insist on the importance of defining some of the scenes or task environments people encounter in everyday life in order to initiate a psychology of thinking, practiced by people together and alone as they are forced to march through the contingencies of daily life.

Footnotes

1. A small group of friends rushed to our aid and read an earlier draft against a deadline, and for their support and criticism we thank them: Lois Bloom, William Kessen, Deborah Malamud, Hugh Mehan, George A. Miller, Roy Pea, Esther Thorsen, Eric Wanner, Sheldon White, and Michael Wolff. Karen Pakula supplied invaluable aid in locating and transcribing many of the scenes used here. Support for this research was supplied by a grant from the Carnegie Corporation to Michael Cole.

2. Both Hilgard (1955) and Postman (1955) highlight the need for a well defined task and corresponding theory of task-related behaviors as a precursor to Brunswik's type of representation study. Hilgard's suggestion that Brunswik could have obtained essentially the same result with a blindfolded subject who could rely upon remembered properties of the object's name provides a non-amusing alternative to Brünswik's perceptual interpretation of his results.

3. The reader will notice that although Neisser does not refer to Bartlett, there is a strong analogy between his contrast of academic and general intelligence, and Bartlett's closed-open distinction.

4. Our description of the examples throughout the paper is selective and incomplete. The transcripts are greatly simplified from the actual speech behavior; we have not included pauses, overlaps, error corrections, tone of voice,

etc. Nor have we included much information about the activities which make up the background for any transcript. For some forms of analysis, these limitations would be lethal. For present purposes, we suspect we have presented even more data than the reader will need in order to judge the plausibility of our claims.

5. Garfinkel (1967) provides a discussion of the use of the term rationality and its applicability to the analysis of the constraints on people in everyday life; for an extension of Garfinkel's formulations to an analysis of conversations of the type under consideration here, see Sacks, Schegloff and Jefferson (1974).

6. The point is not so much that everyday scenes can be elaborated almost ad infinitum (Garfinkel, 1967), but that we must carefully constrain our interpretations of what is going on in the scene and allow only conclusions that take these constraints into account.

7. While a detailed discussion of the topic is beyond the scope of this paper, we should mention that a number of writers have pointed out that even the experimental psychologist's description of the normative order of the laboratory cognitive task is subject to the same sources of openness that we have claimed as a regular feature of everyday cognition. Friedman (1967) for example, cautions that "in the constant talk about extrapolating from the experiment to the 'real' or the 'social' world, we must not forget that the experiment is itself a part of that real and that social world"

(p. 169; see also Orne, 1970 and Rosenthal, 1976). There is a tendency to treat such remarks to mean that if one could eliminate social-interactional demands from experiments, then purer forms of task analysis would solve the difficulty. It should be clear that the difficulty solved in this way would only exacerbate the problem of inference from experimental to non-experimental context. Persons interacting with mechanical environments do not leave us immune to the problem of task indeterminacy (see, for example, Garner, Hake and Ericcson, 1956).

8. The extend to which this problem is a serious impediment to theory construction even within currently accepted experimental practices can be seen in the extreme difficulty of making detailed inferences about the behavior of individuals across theoretically similar experimental tasks, a problem emphasized by Underwood (1975). Hayes and Simon (1977) explore this problem in their discussion of the differential solutions of problem isomorphs that differ only in the imagery a person must use to go through the required steps to solution.

9. The matter is particularly vexing in the anthropological literature because the analyst may be working with implicit categories that constrain description, so that the reader cannot evaluate the level of specification. This seems to be the case with Gladwin, who was strongly influenced by an over-reading of mentalistic categories contained in Miller, Galanter and Pribram (1960) and Newell, Shaw and Simon (1960).

Reference Notes

1. Griffin, P. & Humphrey, F. Talk and task at lesson time. In _Children's Functional Language and Education in the Early Years_. Final Report to Carnegie Corporation, Center for Applied Linguistics, 1978.
2. Hood, L. "Let's _try_ to make it a nice day"--Some not so simple ways. Paper presented to the Center for Psychosocial Studies, September 1978.

References

Abelson, R.P. The reasoner and the inferences don't talk much to each other. In Nash-Webber (Ed.), Natural language processing. Cambridge: Bolt, Beranek & Newman, 1978.

Barker, R.G. Ecological psychology. Stanford: Stanford University Press, 1968.

Bartlett, F.C. Remembering. London: Cambridge University Press, 1932.

Bartlett, F.C. Thinking: An experimental and social study. New York: Basic Books, 1958.

Bjork, R. The updating of human memory. In G.H. Bower (Ed.), The psychology of learning and motivation (Vol. 12). New York: Academic Press, 1978.

Brace, C.L., Gamble, G.R., and Bond, J.T. (Eds.) Race and Intelligence. Washington, DC: American Anthropological Association, 1971.

Broadbent, D. Perception and communication. London: Pergamon Press, 1958.

Bronfenbrenner, U. The experimental ecology of education. Teachers College Record, 1976, 78, 157-204.

Bronfenbrenner, U. Toward an experimental ecology of human development. American Psychologist, 1977, 32, 513-531.

Brown, A. Development, schooling, and the acquisition of knowledge about knowledge: Comments on Chapter 7 by Nelson. In R.C. Anderson, R.J. Spiro, & W.E. Montague (Eds.), Schooling and the acquisition of knowledge. Hillsdale,NY: Erlbaum, 1977.

Bruner, J. Personality dynamics and the process of perceiving. In R.R. Blake & G. Ramsey (Eds.) Perception, an approach to personality. New York: Ronald, 1951.

Bruner, J., Goodnow, J. & Austin, G.A. A study of thinking. New York: Wiley, 1956.

Brunswik, E. Organismic achievement and environmental probability. The Psychological Review, 1943, 50, 255-272.

Byrne, R. Planning meals: Problem-solving on a real data base. Cognition, 1977, 5, 287-332.

Carroll, J.S., & Payne, J.W. Cognition and social behavior. Hillsdale, NJ: Erlbaum, 1976.

Case, R. Mental strategies, mental capacity and instruction: A neo-Piagetian investigation. Journal of Experimental Child Psychology, 1974, 6, 544-573.

Case, R. Intellectual development from birth to adulthood: A neo-Piagetian interpretation. In R. Siegler (Ed.), Children's thinking: What develops? Hillsdale, NJ: Erlbaum, 1978a.

Case, R. Piaget and beyond: Toward a developmentally based theory and technology of instruction. In R. Glaser (Ed.), Advances in instructional psychology (Vol. 1). Hillsdale, NJ: Erlbau, 1978b.

Charlesworth, W.R. Human intelligence as adaptation: an ethological approach. In L.B. Resnick (Ed.), The nature of intelligence. Hillsdale, NJ: Erlbaum, 1976.

Charlesworth, W.R. Ethology: Its relevance for observational studies of human adaptation. In G.P. Sackett (Ed.), Observing behavior (Vol. 1). Baltimore: University Park Press, 1978.

Charlesworth, W.R. & Spiker, D. An ethological approach to observations in learning settings. In R.A. Weinberg and F.H. Wood (Eds.), Observation of pupils and teachers in mainstream and special education settings: Alternative strategies. Reston, VA: The Council for Exceptional Children, 1975.

Cherry, E.C. On human communication: A review, a survey, and a criticism. Cambridge: MIT, 1957.

Ciborowski, T. & Cole, M. A developmental and cross-cultural study of the influences of rule structure and problem composition on the learning of conceptual classifications. Journal of Experimental Child Psychology, 1973, 15, 193-215.

Cicourel, A.V., Jennings, K.H., Jennings, S.H.M., Leiter, K., MacKay, R., Mehan, H., and Roth, D.R. Language use and school performance. New York: Academic Press, 1976.

Cole, M., Gay, J., Glick,J., Sharp, D. The cultural context of learning and thinking. New York: Basic Books, 1971.

Cole, M., Sharp, D., Lave, C. The cognitive consequences of education: Some empirical evidence and theoretical misgivings. The Urban Review, 1976, 9, 218-233.

Cole, M., & Scribner, S. Cross-cultural studies of memory and cognition. In R.V. Kail, Jr. & J.W. Hagen (Eds.), Perspectives on the development of memory and cognition. Hillsdale, NJ: Erlbaum, 1977.

Conklin, H.C. Hanunóo agriculture: A report on an integral system of shifting cultivation in the Philippines (Reprinted edition). Northford, CT: Elliot's Books, 1975.

D'Andrade, R.G. Memory and the assessment of behavior. In T. Blalock (Ed.), Measurement in the social sciences. Chicago: Aldine-Atherton, 1974.

Dilthey, W. Einleitung in die geisteswissenshaften. Leipzig, 1883.

Eimas, P. Effects of memory aids on hypothese behavior and focusing in young children and adults. Journal of Experimental Child Psychology, 1970, 10, 319-336.

Estes, W.K. Learning theory and intelligence. American Psychologist, 1974, 29, 740-749.

Estes, W.K. The cognitive side of probability learning. Psychological Review, 1976, 83, 37-64.

Estes, W.K. (Ed.), Handbook of learning and cognitive processes (Vols.1-6). Hillsdale, NJ: Erlbaum, 1975-1978.

Friedman, N. The social nature of psychological research: The psychological experiment as a social interaction. New York: Basic Books, 1967.

Garfinkel, H. Studies in ethnomethodology. Englewood Cliffs, NJ: Prenctice-Hall, 1967.

Garner, W.R., Hake, H.W. & Ericcson, C.W. Operationism and the concept of perception. Psychological Review, 1956, 63, 149-159.

Gay, J., & Cole, M. The new mathematics and an old culture. New York: Holt, Rinehart & Winston, 1967.

Gelman, R. & Shatz, M. Appropriate speech adjustments: The operation of conversational constraints on talk to two-year-olds.In M. Lewis & L. Rosenblum (Eds.), Interaction, conversation and the development of language. New York: John Wiley, 1977.

Gholson, B., Levine, M. and Phillips, S. Hypotheses, strategies and stereotypes in discrimination learning. Journal of Experimental Child Psychology, 1972, 13, 423-446.

Gibson, J.J. The senses considered as perceptual systems. Boston: Houghton Mifflin, 1966.

Gibson, J.J. The theory of affordances. In R. Shaw and J. Bransford (Eds.), Perceiving, acting, and knowing: Toward an ecological psychology. Hillsdale, NJ: Erlbaum, 1977.

Gladwin, T. East is a big bird. Cambridge: Harvard University Press, 1970

Glucksberg, S., Krauss, R.M., Weisberg, R. Referential communication in nursery school children: Method and some preliminary findings. Journal of Experimental Child Psychology, 1966, 3, 333-342.

Graesser, A. How to catch a fish: The memory and representation of common procedures. Discourse Processes, 1978, 1, 1-13.

Greenfield, P.M. Comparing dimensional categorization in natural and artificial contexts: A developmental study among the Zinacantecos of Mexico. Journal of Social Psychology, 1974, 93, 157-171.

Greenfield, P.M.& Bruner, J.S. Culture and cognitive growth. In D.A. Goslin (Ed.), Handbook of socialization theory and research. New York: Rand McNally, 1969.

Hayes, J.R. & Simon, H.A. Psychological differences among problem isomorphs. In N.J. Castellan, D.B. Pisoni, & G.R. Potts (Eds.), Cognitive theory (Vol. 2). Hillsdale, NJ: Erlbaum, 1977.

Hilgard, E.R. Discussion of probabilistic functionalism, Psychological Review, 1955, 62, 226-228.

Hirst, W., Neisser, U. & Spelke, E. Divided attention, Human Nature, 1978, 1, 54-61.

Hood, L. A longitudinal study of the development of the expression of causal relations in complex sentences. Unpublished doctoral dissertation, Columbia University, 1977.

Irwin, M.H. & McLaughlin, D.H. Ability and preference in category sorting by Mano school children and adults. Journal of Social Psychology, 1970, 82, 15-24.

Istomina, Z.M. The development of voluntary memory in preschool-age children. Soviet Psychology, 1974-75, 13, 5-64. Also in M. Cole (Ed.) Soviet developmental psychology: An anthology. New York: M.E. Sharpe, Inc., 1977.

Jefferson, G. A case of precision timing in ordinary conversation: Overlapped tag-positioned address terms in closing sequences. Semiotica, 1973, 9, 47-96.

Jenkins, H.. & Ward, W.C. Judgment of contingency between responses and outcomes. Psychological Monographs, 1965, 79, 1-17.

Jensen, A.R. A theory of primary and secondary familial mental retardation. In A.R. Jensen, Genetics and education, London: Methuen and Co., 1972.

Johnson-Laird, P.N. & Wason, P.C. A theoretical analysis of insight into a reasoning task. In P. Johnson-Laird & P. Wason (Eds.), Thinking: Readings in cognitive science. Cambridge: Cambridge University Press, 1977.

Kahneman, D. Attention and effort. Englewood Cliffs, NJ: Prentice-Hall, 1973.

Kendon, A. Studies in the behavior of social interaction. Lisse: De Ridder Press, 1977.

Kotovsky, K. & Simon, H.A. Empirical tests of a theory of human acquisition of concepts for sequential patterns. Cognitive Psychology, 1973, 4, 399-424.

Kreutzer, M.A., Leonard, C., and Flavell, J.H. An interview study of children's knowledge about memory. Monographs of the Society for Research in Child Development, 1975, 40, (1, Serial No. 159).

Kuhn, D. & Brannock, J. Development of the isolation of variables scheme in an experimental and "natural experiment" context. Developmental Psychology, 1977, 13, 9-14.

Kuhn, D. & Ho, V. The development of schemes for recognizing additive and alternative effects in a "natural experiment" context. Developmental Psychology, 1977, 13, 515-516.

Laboratory of Comparative Human Cognition. Cognition as a residual category in anthropology. Annual Review of Anthropology, 1978, 7, 42-60.

Laboratory of Comparative Human Cognition. What's cultural about cross-cultural psychology?. Annual Review of Psychology, 1979, in press.

Labov, W. The logic of non-standard English. In F. Williams (Ed.), Language and Poverty. Chicago: Markham, 1970.

Labov, W. & Fanshel, D. Therapeutic Discourse. New York: Academic Press, 1978.

Leontiev, A.N. Studies on the cultural development of the child. Journal of Genetic Psychology, 1932, 40, 52-83.

Levine, M. Hypothesis behavior in humans during discrimination learning. Journal of Experimental Psychology, 1966, 71, 331-338.

Lewin, K. Defining the 'field at a given time'. Psychological Review, 1943, 50, 292-310.

Lewis, D. We the navigators. Honolulu: University of Hawaii Press, 1972.

Lewis, D. Mau Piailug's navigation of Hokule'a from Hawaii to Tahiti. Topics in cultural learning 5. Honolulu: East-West Center, 1977.

Luria, A.R. The problem of the cultural behavior of the child. Journal of Genetic Psychology, 1928, 35, 493-505.

Luria, A.R. The nature of human conflict. New York: Liveright, Inc., 1932.

Manuilenko, Z.V. The development of voluntary behavior in preschool age children. Soviet Psychology, 1975, 13, 65-117.

McDermott, R.P. Kids make sense: An ethnographic account of the interactional management of success and failure in one first grade classroom. Unpublished doctoral dissertation, Stanford University, 1976.

McDermott, R.P., Gospodinoff, K. & Aron, J. Criteria for an ethnographically adequate description of concerted activities and their contexts. Semiotica, 1978, 24, in press.

McDermott, R.P. & Roth, D. The social organization of behavior: Interactional approaches. Annual Review of Anthropology, 1978, 7, 321-345.

Mehan, H. Learning lessons. Cambridge: Harvard University Press, 1979.

Miller, G.A., Galanter, E. & Pribram, K.H. Plans and the structure of behavior. New York: Holt, Rinehart & Winston, 1960.

Morton, J. & Byrne, R. Organization in the kitchen. In P.M.A. Rabbitt & S. Dornic (Eds.), Attention and performance V. New York: Academic Press, 1975.

Neisser, U. Cognition and reality. San Francisco: W.H. Freeman, 1976a.

Neisser, U. General, academic, and artificial intelligence. In L.B. Resnick (Ed.), The nature of intelligence. Hillsdale,NJ: Erlbaum, 1976b.

Nerlove, S.B., Roberts, J.M., Klein, R.E., Yarbrough, C. & Habicht, J.P. Natural indicators of cognitive development: An observational study of rural Guatamalan children. Ethos, 1974, 2, 265-295.

Newell, A., Shaw, J.C., & Simon, H.A. Report on a general problem-solving program. In Proceedings of international conference on information processing. Paris: UNESCO, 1960.

Norman, D.A. Cognitive organization and learning. In P.M.A. Rabbitt & S. Dornic (Eds.), Attention and performance V. New York: Academic Press, 1975.

Norman, D. & Bobrow, D. On the analysis of performance operating characteristics. Psychological Review, 1976, 83, 508-510.

Orne, M. Hypnosis, motivation and the ecological validity of the psychological experiment. In W.J. Arnold & M.M. Page (Eds.), Nebraska symposium on motivation. Lincoln: University of Nebraska Press, 1970.

Parke, R.D. Issues in child development: On the myth of the field-lab distinction. Newsletter of the Society for Research in Child Development. Winter, 1976.

Piaget, J. Language and thought of the child. New York: Meridan Books, 1955. (Originally published in 1926).

Piaget, J. & Inhelder, B. The child's conception of space. London: Routledge and Kegan Paul, 1956.(Originally published in 1930).

Postman, L. The probability approach and nomethetic theory. Psychological Review, 1955, 62, 218-225.

Price-Williams, D.R., Gordon, W. & Ramirez, M. Skill and conservation: A study of pottery making children. Developmental Psychology, 1969, 1, 769.

Quinn, N. A natural system used in Mfantse litigation settlement. _American Ethnologist_, 1976, _3_, 331-351.

Rabbit, P.M.A. & Dornic, S. (Eds.) _Attention and performance V_. New York: Academic Press, 1975.

Reed, S.K. Facilitation of problem solving. In N.J. Castellan, D.B. Pisoni & G.R. Potts (Eds.), _Cognitive theory_ (Vol. 2), Hillsdale, NJ: Erlbaum, 1977.

Resnick, L.B. (Ed.). _The nature of intelligence_. Hillsdale, NJ: Erlbaum, 1976.

Riesenberg, S.H. The organization of navigational knowledge on Puluwat. _The Journal of the Polynesian Society_, 1972, _81_, 19-56.

Rogoff, P. Spot observation: An introduction and examination. _Quarterly Newsletter of the Institute for Comparative Human Development_, 1978, _2_, 21-26.

Rosenthal, R. _Experimenter effects in behavioral research_. (enlarged edition) New York: Irvington, 1976.

Sacks, H. An analysis of the course of a joke's telling in conversation. IN R. Bauman & J. Sherzer (Eds.), _Explorations in the ethnography of speaking_. New York: Cambridge University Press, 1974.

Sacks, H., Schegloff, E., Jefferson, G. A simplest systematics for the organization of turn-taking for conversation. _Language,_ 1971, _50_, 696-735.

Scheflen, A.E. Communicational structure. Bloomington, IN: Indiana University Press, 1973.

Scheflen, A.E. How behavior means. New York: Anchor, 1974.

Schegloff, E. Notes on a conversation practice: formulating place. In D. Sudnow (Ed.), Studies in social interaction. New York: Free Press, 1972.

Schegloff, E. Identification and recognition in interactional openings. In I. Pool (Ed.), The social impact of the telephone. Cambridge, MA: MIT Press, 1977.

Schwartz, J.L. & Taylor, E.F. Valid assessment of complex behavior: The Torque approach. The Quarterly Newsletter of the Institute of Comparative Human Development, 1978, 2, 54-58.

Scribner, S. Situating the experiment in cross-cultural research. In K. Riegel & J. Meacham (Eds.), The developing individual in a changing world (Vol. 1). Chicago: Aldine, 1976.

Scribner, S. & Cole, M. Cognitive consequences of formal and informal education. Science, 1973, 182, 553-559.

Scribner, S. & Cole, M. Literacy without schooling: Testing for intellectual effects. Harvard Educational Review, 1978, in press .

Sharp, D., Cole, M. & Lave, C. Cognitive consequences of education. Monographs of the Society for Research in Child Development, 1978, in press .

Shatz, M. & Gelman, R. The development of communication skills: Modifications in the speech of young children as a function of listener. Monographs of the Society for Research in Child Development, 1973, 38, (5, Serial No. 152).

Shiffren, R.M. & Schnieder, W. Controlled and automatic human information processing: II. Perceptual learning, automatic attending, and a general theory. Psychological Review, 1977, 84, 127-190.

Shweder, R.A. Likeness and likelihood in everyday thought: Magical thinking in judgments about personality. Current Anthropology, 1977, 18, 637-658.

Simon, H.A. Models of man: Social and rational. New York: John Wiley, 1956.

Simon, H.A. Discussion: Cognition and social behavior. In J.S. Carroll & J.W. Payne (Eds.), Cognition and social behavior. Hillsdale, NJ: Erlbaum, 1976a.

Simon, H.A. The functional equivalence of problem solving skills. Cognitive Psychology, 1976b, 7, 268-288.

Simon, H.A., Newell, A. Human problem solving: The state of the theory in 1970. American Psychologist, 1971, 26, 145-159.

Spelke, E., Hirst, W., Neisser, U. Skills of divided attention. Cognition, 1976, 4, 215-230.

Tversky, A. Elimination by aspects: A theory of choice. Psychological Review, 1972, 79, 281-299.

Underwood, B.J. Individual differences as a crucible in theory construction. American Psychologist, 1975, 30, 128-134.

Vygotsky, L.S. Language and thought. Cambridge, MA: MIT Press, 1962.

Vygotsky, L.S. Mind in society. Cambridge: Harvard University Press, 1978.