Some Criteria for an Adequate Description of Mental Activities

Michael Cole
Lois Hood
Raymond McDermott

The Rockefeller University

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Introduction

The purpose of our discussion is to propose a set of criteria for the description of mental activities. By mental, we are referring to a particular form of behavior which appears to happen somewhere inside the head and which is glossed in everyday talk as thinking, remembering, reasoning, etc. By activities, we are referring to behavior which exhibits form, which can be said to be directed by way of its form to the circumstances at hand, and which appears to have some consequences given these circumstances. Because mental behavior is generally invisible, much of the study of such behavior has been absorbed with the task of making it visible by the systematic application of restricted eliciting contexts. We are going to point out some inadequacies in most approaches to making mental behavior visible, and we are going to suggest some alternatives which will allow us to study mental activities both in experiments and in other ongoing social contexts. Much like speech activities and gestural activities, mental activities can be understood within the contexts of their occurrence by a description of how they are directed to and have consequences for the ongoing organization of those contexts. And, much as in the study of speech (Bloom 1970; Hymes 1974; Sacks 1974; Schegloff 1972; Silverstein 1976; Volosinov 1973) and gesture (Birdwhistell

1970; Bateson 1972; Kendon ~~in press~~ 1977; McDermott, Gospodinoff and Aron 1978; Scheflen 1973), the crucial task before the student of mental activities is the location of the contexts which frame and give meaning to the activities, both to the analyst and the participant. ~~Accordingly,~~ we ~~will~~ offer criteria which ~~will~~ help us build toward an adequate description of the contexts for various kinds of mental activities.

We began our research with a seemingly empirical question: Do the tasks presented to children in psychological tests and experiments have a counterpart in everyday settings and, if so, do the children perform on these tasks outside the laboratory in similar ways to their performances in the more controlled settings? There were many reasons for starting with this question, but our overriding concern was a sense of crisis about the relevance of psychological tasks, and about whether or not some persons' performances on such tasks could tell us anything generalizable about the people, tasks, or processes in question. Without clear answers to these concerns, we could imagine no adequate theory of individual or cultural differences in thinking nor an adequate theory of pedagogy. ~~for instructing persons in certain kinds of thinking rather than others.~~

Our attempt to find clearly defined tasks of the type psychologists use in tests and experiments active in everyday life contexts soon came to a halt; generally, we couldn't see anything like them, and, when we did, we had no idea of how to specify them to the

satisfaction of any other observer (and least of all to other psychologists). Basically, there appeared to be too many variables in comparison to make the claim that any task is like any other task in a different situation. For example, we could locate examples of kids dividing larger numbers by smaller numbers both in class and in the cooking clubs we have arranged for them; but everyone quickly notices that in the cooking club, the kids can reach decisions together and that in the classroom they must each do the job alone. Two possibilities are candidate accounts for an explanation of our finding. One possibility (1) is that such clearly defined tasks simply do not occur in everyday life; it just isn't the case that children do free recall of long lists, or display paired associate learning in everyday life, the exception being when they are asked to do so by teachers or ~~the~~ psychologists. Two responses can be given to this first possibility. One (a) is that psychological tasks are simply uninteresting and have nothing to do with the world of the people tested. Such responses are legion among anthropologists and others deeply commited to situational relativity. The other response (b) is that everyday life is uninteresting to the student of mental activities, that it doesn't press the normal subject to the limits of capability and accordingly doesn't reveal the nature of the subject's skills. A second possibility (2) is that the tasks psychologists test for in their experiments are quite prominent in everyday life, but essentially invisible, given the requirements experimental psychologists use to warrant their descriptions. Rather than

arguing over the relative merits of the performances of people on tests over the performances of people in everyday life, this possibility reduces the issue to one of methodology. In order to carefully discern the relation between a particular task environment and a person's performance, psychologists have so overly restricted and under-specified the tasks they ask others to perform that it is not possible to locate such tasks in ongoing behavior, or to locate the experimental findings in terms of how the people operate in the world they inhabit.

We believe that our inability to specify in everyday life the same "cognitive processes"[1] that are carefully inferred from experimental data stems from the strict, ~~but generally inadequate~~, requirements psychologists use to specify their findings. In section I of the paper, we ~~will~~ examine these requirements and offer some work of Herbert Simon and his associates as examples of ~~a job well done in the area of problem solving.~~ In section II of the paper, we ~~will offer some criticism of these requirements by pointing to their~~ limitations, ~~by pointing to how they so constrain~~ the tasks that ~~certain dimensions of the task description are generally ignored and that~~ the possibility of moving to a generalizable ~~conclusion about matters~~ beyond the experimental conditions is ~~limited~~. (We will repeat an old argument from Carnap that psychology has locked itself in a blind alley by requiring that cognitive processes must be operationalized by the requirements currently in use,

for, given these requirements, a specific cognitive process is that specific cognitive process only when the conditions of that specific operationalizing are enforced; under other conditions, the formulated cognitive process must be something other than it was thought to be).

In section III, we ~~will~~ claim that a ~~natural history~~ analysis of ~~an ongoing~~ problem-solving behavior in a social context shares many of the same problems of an experimental analysis as tasks ordinarily analyzed by cognitive psychologists dealing with ~~Accordingly,~~ we ~~will~~ suggest three criteria for ~~moving~~ beyond these problems ~~for the analysis of mental activities~~ in either experimental or natural ~~history~~ settings. In addition, we suggest that a "natural history approach to ~~mental~~ intellectual activities which reaches across settings may help us to transcend the limitations of current analytic ~~hold out the possibility of reaching our criteria before~~, ~~although probably in conjunction with~~ practices as applied to, experimental analyses alone. We will make this claim because the analysis of ongoing behavior in social settings forces on the analyst two crucial descriptive tasks: (1) because no one ever knows for sure what is going to happen next in everyday life, a natural history analysis of such behavior may force on the analyst a level of task specificity not available to the experimentalist, namely, the possibility of specifying the task in terms of how the persons reflexively constitute part of the very task or problem they work on[2]; in addition, and in ways we can barely imagine (2), a natural history analysis of ongoing behavior may offer us the possibility of speaking to the issue of generalizability, for the tasks discerned in such an analysis

offers the analyst sources of information that are not present in experiments where the stimulus situation is assumed to exist in a specifiable, static form prior to and during the experiment. In particular, because participants in any social setting reflexively constitute the task for each other, other participants can provide information about the stimuli and their dynamics which constitute the problem for the "subject." Descriptive analyses of social interactions as cognitive tasks also offers one means of increasing the breadth of tasks which can qualify as intellelctual activities,

does generalizability to what issue

are necessarily discerned in terms of a hierarchy of behavioral contexts, and, as these contexts may be used in ordering many other same, or specifiably different scenes, such an approach raises the possibility that psychological and more sociologically oriented forms of context analysis, such as called for by many ethnographers, can be brought together in making more useful statements about how and when it is that people engage in particular kinds of mental activities. In section IV, we ~~will~~ offer a fledgling example of such an analysis. It will be more of an illustration of the problems facing such analyses than an adequate analysis, but it ~~will help us~~ raises some issues and hopefully ~~to generate~~ a little excitement.

Requirements for specifying cognitive processes in experimental psychology

In our initial attempt to specify cognitive processes in non-test environments by using the terminology and constructs of cognitive psychology, we come to understand and appreciate the requirements that cognitive psychologists try to apply in their research. While it is true that much of the work done in cognitive psychology does not live up to these standards - tasks are underspecified, results are over-generalized, and claims about abilities, knowledge, competence are generally unwarranted - our purpose here is not to criticize cognitive psychology, but to describe how it operates and to suggest how we can expand on its foundations. We want to set out the standard requirements for ~~an adequate~~ description of cognitive processes and illustrate their application with examples of what we think is good cognitive psychology. We then ~~will~~ show, in the next section, why cognitive psychology, even at its best, is unable to address the issue of ~~cognitive~~ intellectual activities outside the laboratory. In other words, we ~~will~~ discuss the limitations of cognitive psychology as it pertains to a specification of behavior in naturally occurring events.

Most cognitive psychologists proceed as if three requirements must be met for an adequate description of cognitive processes. These are (1) that the dimensions of the tasks presented and the behaviors of the subjects must be well defined, (2) that the behavior of the subjects must vary consistently over a range of parametric variations, and (3) that the relations be-

tween the tasks and the range of elicited behaviors must be specifiable in terms of some theoretically derived model of how people operate and in ways that would predict the actual consistency in behavior and experimental tasks. Cognitive processes are then inferred; indeed, they are the inferences, from the relation between the specified tasks and the observed behaviors, in conjunction with (invariants) by the theory that specifies task-

Examples of the adequate use of these requirements are available in some recent work on problem solving by Simon and his coworkers (Hayes and Simon, 1977; Simon 1975, 1976; Reed, 1977; Siegler 1976). In all these studies, the experimenters attend carefully to the first requirements and start their inquiry with a detailed analysis of what the subject might have to do to accomplish a legal solution to the posed problems. The tasks are intriguing. For example, in the Tower of Hanoi problem, subjects must transport a set of differently sized disks, from bottom big to top small, from one of three available shafts to another of the available shafts, with the stipulation that only one disk can be moved at a time and that no disk can be covered by any disk larger than itself. In the river crossing problem, three missionaries and three cannibals must cross to the other side of a river in a boat that only carries two people; they have the additional worry that the number of cannibals can never be greater than the number of missionaries on either shore lest they be eaten (we will leave unspecified which group will do the eating--if you picked the cannibals you are a competent speaker of English and

an ethnocentric dog). The task analysis proceeds by stipulating how many legal moves are logically possible at any time along the way to task completion.

The second requirement in the specification of a cognitive process is that the behavior of the experimental subjects varies consistently along a range of parametric variations. Simon and his associates attend carefully to this requirement by offering their subjects formally isomorphic tasks, which have identical logical steps in the solution process but which differ in their wording, or analogous tasks, which differ by the addition or subtraction of a logical step in the solution process. For example, isomorphic tasks used by Hayes and Simon involved three different sized monsters holding three different sized globes. The task was to alter the situation so that the size relationships that held between monsters matched the size relationships of the globes (with certain stipulations not relevant to the point at hand). One of the variations in the wording of the problem concerned the kind of changes that could be made: in one variation, the globes were to be transferred, in another, they were to be shrunk or enlarged. The number of steps needed to solve the two problems was the same, even though the wording of the problems was different. [4] An example of formally analogous tasks are variations of the river crossing problem used by Reed. In one case, Reed increased the number of missionaries and cannibals to five each with a boat that carries three at a time. Another variation involved changing the stipulation of characters from missionaries and cannibals to jealous husbands and wives,

and added the requirement that no man or woman may cross the river together in the boat unless they are married.

More than most experimenters, Simon and his associates are careful to stipulate the theoretical devices they use to specify the relation between their tasks and the range of behaviors they elicit. In particular, Simon employs a computer model of problem solving to interpret the behavior of human subjects on the problem task; by specifying what a computer would have to know in order to solve the question at hand, Simon can specify what his human subjects know how to do to the extent that they can follow in the same paths as the different computer programs. This form of specification is quite powerful in comparison to the more generally used theoretical frameworks, the developmental framework, for example, which is little more than a gloss of the fact that behavior differs across ages and which seldom stipulates how those changes come about.

In the next section, we will present results from several studies of cognitive processes which reveal both the failure of cognitive psychologists to meet these requirements under experimental conditions, and more important, the inadequacies of these requirements for specifying behavior in non-experimental conditions.

III The limitations of cognitive psychology

The careful use of the requirements we have been discussing appears to dictate some research policies which make us uncomfortable. Our main problem is that in the name of using well analyzed and systematically variable tasks, psychologists have so constrained and controlled the behavior of their subjects that it is hard to know what to do with the results of any given line of experimentation. The claim that mental activities are invisible has been taken too literally, and the struggle has been to limit the amount of disturbance which can take place between the presentation of a task environment (stimulus) and the consequent behavior (response). One prominent and successful approach to this problem is to limit the amount of time (to within a single saccade, for example) a subject has to deal with a stimulus array on the assumption that the response to such an array is uninterfered with and in some sense pure. Another approach is to train subjects to attend in certain ways to a stimulus array.

But such pure performances can say little about how people perform under less constrained conditions, or even how the constraints put on subjects under such pure conditions are a systematic part of the performance. While most psychology is cheaning out the possible interferences to make inferences about cognitive processes, many of us are anxious to make statements about people in the world, complete with a myriad of interferences. For until we know how to describe mental activities as they occur

in ongoing everyday life, until we know how to build all the interferences into our accounts of people's behavior, how shall we ever be content with any statement about the skills a particular child has developed for relating to the world, or about the skills members of different cultures have developed. Time and again, the statements are made. And time and again, their power to generalize across situations in one child's life, or across the range of situations which help to define a culture's focus of activity, has been called into question and shown to be inadequate (Cole, et al, 1971; Lab Comp Human Cog, 1976; Scribner, ~~in press~~ 1976). So clearing out all the interference has brought us some limited blessings.

At the root of our problem is that psychology has never moved adequately beyond the Stimulus-Response (S-R) frame. Because behavior has always been understood as a response to a stimulus, there is a gap to be accounted for in the organism's organization of a response to a stimulus, which often is filled with the organism's projected history of the particular schedule of reinforcements at hand or with an inferred set of cognitive processing skills developed in the life of the organism. No matter how cognitive the theory becomes, the method of contemporary psychology, using the requirements we have specified, gives us only statements of the type, "X is a response to Y". No matter how well specified the task environment is, and no matter what theory is used to make something of the results, the descriptive requirements in

use necessarily generate a static account of the task environment. What is left out is an account of how a stimulus-response field is co-ordinated or framed in the way that it is. Dewey (1896) stated the problem well many decades ago. A stimulus-response model "assumes sensory stimulus and motor response as distinct physical experiences, while in reality they are always inside a co-ordination and have their significant purely from the part played in maintaining or reconstituting the co-ordination." And an occasional echo can be heard on the contemporary scene: "The deficiency of this scheme lies in its exclusion of the content-bearing process that establishes the subject's real connections with the world of objects" (Leont'ev, 1975).

The requirements currently in use for the description of cognitive processes are bathed in a static notion of the relation between task environment and behavior, and this fact raises insoluable problems for any attempt to use the requirements to generate any statements about individual or cultural differences or a theory of pedagogy. We will consider each of the requirements and their limitations one at a time.

First, how shall we know that a task is well analyzed? Clearly, if all people operate on a given task with the same result, for example, if all people appear to exhibit the same limits in short-term memory experiments, then for all practical purposes we can consider the task well described. To be thoroughly accepted as an adequate task analysis, a task must

predict completely the behavior of the respondants. When this is not possible, some further analysis of the task must be done in order to distinguish how any two subjects can respond differently to them. Thus, any task analysis is in trouble when the same task (i.e., what is analyzed as the same task) is responded to in different ways by different persons or when isomorphic tasks, i.e., tasks which are, by the task analysis in use, formally identical to each other, are responded to in different ways by the same person.

In either case, it is reasoned that the task must be further analyzed to establish how these differences are a possibility. For example, in the face of differential performance on the parts of the same subjects dealing with the monster-globe problem, Hayes and Simon (1977) had to seek out a more specified account of how the tasks could be different. Their first strategy leans to the kind of solution we might seek to this problem. They examined the work sheets of the subjects to see how the different subjects constructed differential environments for themselves to help guide their way through to problem solution. Hayes and Simon did not find a source of difference in the work sheets, and wound up, quite reasonably, putting the differences in the heads of the subjects, specifically, in the differing representations the subjects would have had to have in order to solve the problem when it involved transferring globes ~~and~~ is different from when it involved expanding or shrinking globes.

As soon as Hayes and Simon went back into the head to account for their subjects' differential performances on the two tasks, they gave up the possibility of an adequate task analysis; they gave up on an account of how the differences between their subjects across problems had to do with the larger frame of their activity. The question is whether we can accept their account of the differences in the same person across time to do these problems, or whether we have to seek some other environmental account of the behavior in the two tasks. We are not suggesting that Hayes and Simon did not offer us a reasonable account of how the one problem appears to be harder than the other. The question we are raising is about the nature of the criteria we shall use to accept or reject their account. Is their task analysis adequate to allow that the difference in subjects facing the two problems has to do with the differential skills of each subject for these problems? Or can we look to some further analysis of task environment-behavior relations, one which Hayes and Simon do not have available to them, to account for the differential performances.

It was once reasonable to assume that a few points difference on an IQ test made a difference in people's ability to do school work; it was once reasonable to assume that uneducated Africans had superior rote memories or lesser skills to do syllogistic reasoning then their educated counterparts. But such accounts are not reasonable any more. What kind of account of a task

environment might give us some warrant for stopping at one place rather than another; what kind of task environment analysis will allow us to be specific enough about how persons proceed to work on a task so that we could say what it was that was controlling their behavior? We are suggesting that a task analysis which does not take into account the larger framework for a specific relation between a task environment and a person's behavior to become visible cannot give us an answer to our questions about the differences between persons or tasks. Any analysis which offers the task as a static stimulus to an everaccepting organism cannot get to an adequate account of the mental activities a person generates and what they may be in relation to.

In order to delineate these problems more fully, we would like to consider an intriguing paper by Siegler (1976). It is concerned with what children have to know in order to appropriately predict events in the sue of a balance scale. Children are examined across age in the hopes of discerning what they learn that allows them to perform appropriately. A simple quantitative analysis of the children's performance showed a marked change in their behavior between the ages of 5 and 9, but could not speak to what it was that they learned or how, what later developments they might go through, what else they might be able to learn about the task at different ages, etc.

In order to unpack the possible "richness of the pattern",

Siegler divided the task into four apparent levels of reasoning that a person would have to apply in order to perform appropriately in all cases. Each level was specified by an apparent rule which, if it appropriately glosses the child's state of knowledge to be applied to the balance scale problem,would govern a particular kind of right or wrong response to different problems. The adequacy of the task analysis is argued for by the children falling into each of these levels in their actual performance of the tasks. The claim here is simply that if the task environment can be broken down into four levels of complexity, each with its own rules for reaching solutions, the children's behavior can best be understood as developing across these four levels as if they accumulated these rules for appropriate performance one after the other in the sequence of increasing complexity.

In order to make a wider, more cognitive claim about how the children actually seemed to follow these rules, Siegler offers an even closer analysis of the tasks before the children. Without instruction, older children do better than younger children in general. They appear to learn more efficiently from their exposure to the task than the younger children. Given this, Siegler wondered if these are differences "in the way that children of different ages go about learning that are independent of their knowledge of specific tasks." Traditionally, such differences are glossed in terms of readiness, but Siegler astutely points

out that this attribution "only labels the phenomenon, it does not explain it."

To disentangle the problem of readiness, Siegler examines the behavior of the younger children and finds that they consistently attend to the inappropriate aspects of the tasks put before them. "Five year olds are less able to acquire new information than 8 year olds because their encoding of stimuli is less adequate." When asked to remember different arrangements of the balance scale once it was removed from sight, 5 year olds were much less able to recall the organization of the task presented to them. So of course, they learned less than the 8's who, although they were operating at the same level, remember quite clearly. Interestingly, once they were trained to attend to these features, the 5 year olds began to perform more appropriately on all the balance scale tasks.

Siegler's paper carries a number of important lessons for us. What is most crucial is that when pressed for an analysis of the task which can distinguish between the performance of five year olds and eight year olds, who show a differential rate for moving to the next level of skill, Siegler had to set up a <u>different</u> task to see whether there were any differences in what the children of the two age groups could remember seeing after the balance scale was hidden from view. The results from this experiment lend some warrant to his encoding hypothesis, and he offers some cogent arguments for seeing his task environments and the children's

behavior tied together by certain developmental principles. But it will never be possible, using this procedure, to show what particular skills the children use to generate their differential performances. This becomes crucial when you consider that it was possible to train quickly the 5 year olds to encode the kind of information they needed in order to move to the next level in efficient balance scale prediction behavior. Apparently, the five year olds had all the requisite skills for recording both weight and memory in their dealings with the balance scale, but they did not have sufficient experience with the scale to encode the information and to use it in their predictions. The crucial question is: What is it about the relations between the balance scale task environment and certain children that engages one group to organize their skills in a particular way for a particular display. Another way of tackling the same question is to ask how the children from the two groups involve themselves in the task so that they differentially use certain skills at certain times.

We cannot offer an answer to the questions we put to Siegler's study. The point is that neither can Siegler. Neither can anyone who is going to offer task environments, as if they were the same, as if they were static, to different persons and then make judgments about the persons on the basis of their responses. We need some rigorous way of locating the frame, what it is that the person is up to at the specific time that the skills are in evidence

or not, in order to understand how there is a specific tie between a task environment and the person's behavior which occasions the display of certain mental activities rather than others.

For the kinds of tasks Siegler is presenting to his subjects, we are not at all sure that we could unpack the mutually constituted nature of the task environment and the behavior in question. One of the ironies of contemporary psychology is that in studying situations with as little interference as possible, experimenters have cut themselves off from the possibility of using anything other than a static stimulus-response paradigm (their particular methods having been adapted in response to such a paradigm). When we try to shift from an S-R paradigm, we are asked to see mental activities in settings which are set up and constrained on the assumption that such activities are invisible. As it is not clear what such an assumption gains us, we are going to suggest allowing naturally occurring interferences and see whether we can use ~~it~~ them systematically in the analysis of tasks. Siegler's tasks do not allow enough disturbance for us to attempt a context analysis. So it is not easy to say how the balance scale and the children engage each other under the conditions he has specified. Other tasks in more complex settings may prove to be cleaner eliciting tools.

In raising questions about the adequacy of task analyses in contemporary psychology, we have hopefully pushed you to the point of worrying about the requirements which underlie attempts

of parametric variations of the original task. The possibility of a description actually meeting this requirement has been questioned many times in the past, and we will draw on some of these critiques.

The delineation of cognition processes on the basis of consistent task environment-behavior relations across a range of variations has been treated in various ways in psychology. For the most part, unfortunately, it has been ignored; this trend has been most apparent in psychometric approaches to individual differences and in cross-cultural psychology, two fields which have given us few allowable generalizations as a result.

A seond approach to variation induced by different task environments has been to train it out, that is, to get the subject to treat two task environments as if they were the same for some purpose. This is a clear case of missing the point unless it is also the case that the specifics of the present "purpose" are also well defined and shown to be an ongoing organizational device used by the subject (and the experimenter as a part of the task environment).[3]

Vygotsky has criticized the assumption that we can learn from experiments in which the subjects have been pretrained to the same level before the experiment formally begins. According to

Vygotsky, such procedures in part account for psychologists studying behaviors that have become "fossilized". He criticizes "the standard practice of discarding the data from initial sessions, when the response is being established. Uniformity was sought, so that it was never possible to grasp the process in flight; instead researchers routinely discard the critical time when the reaction appears, and its functional links are established and adjusted." (pp. 14-15). This can lead to drawing unwarranted conclusions about similar processes because of some similarities in responses. (See Appendix I: Vygotsky.)

A more constructive approach to variable task environments has been to recognize their importance and to exploit them as resources for the delineation of cognitive processes. In fact, much of the impetus for this paper has been a decade of surprises in the work by Cole and his associates on the situational nature of cognitive activities. Slight changes in task environments have led to substantial differences in the cognitive activities people display. Some of these differences have been locatable, and a finer analysis of the two situations promises to reveal some of the relevant connections. We are thinking here, for example, of Scribner's (in press) account of the different interpretations of syllogisms by uneducated Africans on the basis of their using different speech genres to mark the use of the different kinds of mental work that gets done in the literal versus the propositional handling of the syllogisms. Other differences, such as why

important as this research is for raising questions, the reasons for the situational differences are difficult to locate, as they must be specified in terms of the contexts in which they most immediately occur. We must be careful to remember that a situation, i.e., a different task environment, is not necessarily a variable factor in people's performance. And when it is, it must be shown how it comes to take on such power over the organization of behavior. It must be shown how the variables in the task, as analyzed by the psychologist anyway, come to be constitutive of the task so that it is necessarily attended to as a different task from what it was previously analyzed to be.

The fourth way that task environment differences have been handled in psychology has been mainly limited to a few studies published in Russian a few decades ago and recently translated into English (Istomina, 1975; Manuilenko, 1975). The paper by Istomina is particularly interesting for an understanding of how people's cognitive activities must be understood in terms of the subjects active involvement with, rather than simple response to, a particular task environment. Istomina observed children from 3 to 7 years of age in two different situations for the "free recall" of a list of items. For half of the children the task was to "learn a lesson" which 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 playacting as "kindergarten 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 kindergarten. This assignment constituted the "presentation of to-be-recalled items" in the free recall task. Recall occurred when the child arrived at the store and asked the salesclerk for the items needed.

As might be anticipated, embedding the free recall task in a game enhanced performance for all but the youngest subjects. Istomina considered and rejected several situational explanations of her results, but to explain the difference between the laboratory and game versions of her task, she was led to examine the "particulars of the child's active involvement" rather than the external conditions as defined by the analyst.

It is worth noting in some detail the observations Istomina made and her interpretations of them because they are suggestive of the approach we are advocating. What she observed was that children in the game situation engaged in a set of remembering activities which were not displayed in the experiment. These included repeating the items to themselves as they were presented, or repeating the entire set right after they had all been repeated. The children would often run back from the store commenting that they knew they had forgotten something, or would stand at the

mental condition and attributed the differences in performance directly to the differences in activity elicited by the two situations. (Although her language is more more mentalistic than we would like, wer think her terms are translatable.) These differences in activity are in turn related both to the chlid's matery of the individual operations that the activity requires and his understanding of the relevance of those activities to the situation at hand.

Very young children have some understanding of what remembering means, but they do not understand what it means to remember simply for the sake of remembering. In laboratory tasks, the motivation to remember ("this friendly adult is asking me to remember something") bears no necessary relation to any remembering activities. As Istomina tells us, "For something to become the goal of an act, it must stand in some relation to what gives rise to the child's activity, i.e., to the motivation for the act." (p. 59). This relationship gives meaning to the goal and (most important) organized the child's activities. In the language of this paper, task and environment must be mutually constituted in ways which occasion such remembering activities. The make-believe motive and goal that is lacking in the experiment, making recall a goal which is intrinsically necessary to being competent in the overall activity (e.g., being a good kindergarten director).

Under these circumstances, young children begin "spontaneously" to manifest in their activity the very processes which we would have assumed absent or deficient if we had restricted our analysis to the experimental situation as a universal probe of remembering ability. More important from our present perspective, Istomina's procedure illustrates how task and behavior are intimately tied to create a particular kind of mental activity.

The last requirment for an adequate approach to cognitive processes is that the relation between behavior and task be specified by a theoretical model. It is in this context that the static relation between task and behavior given most experimental designs is so damaging, for it is never quite clear what any given set of experimental results might mean independent of the specifics of the particular task environment in which the results were established. Generally, the tasks are so underspecified in terms of the involvement of the subject, and the behavior of the subject so overrestricted by the constraints of the task environment, that it is not possible to locate the subjects' behavior in relation to the rest of their behavior outside the experimental conditions. For example, Reed (1977) makes the valuable point that a Simon-like task analysis cannot be performed on a task environment which can be operated on by moves not accounted for in the formal analysis of the task as presented by the psychologist. The subject is forced to operate within the analyst's set of rules, within the

analyst's task analysis at the same time that the adequacy of the task analysis depends on the subject's behavior. Until the two are shown to be mutually constitutive, that is, until the circular definition of the task environment and the subject's behavior is shown to be circular in exactly the way that the subjects themselves are defining the two in terms of each other, then the overly constrained nature of the tasks given to the subjects will keep us uninformed about what the subjects' behavior could possibly be about, except as a statistically significant response to the only partially-specified task environments the psychologist offers the subjects. What either the task environments or the behavior have to do with the wider world must remain systematically unclear.

This problem of an overly constrained, but yet unspecified, task environment-behavior relation not leading to generalizable results is similar to the problem faced by anthropologists who have attempted an approach to native categories via carefully controlled eliciting frames. They soon found that they were reduced to elaborate projects relating certain lexemic units to other lexemic units given only their contexts in specific sentences produced in a controlled stimulus-response question-asking routine. In this case, plying frames proved to be dangerous, and we are

relieved by the recent call to open up the frame analysis to include larger frames of references, to include an analysis of how scenes are organized for the appropriate application of certain questions and not others (Frake, 1974).

The same thinking can be applied to cognitive psychology. The frames which might make many psychological experiments sensible in terms of something other than themselves are generally missing from consideration. A description of the mutually constitutive relation between task and environment may push us to a consideration of the larger contexts in terms of which tasks come to be attended to and worked on in the way that they are. The theories which hold the task enviroments and person's performance together may thus be something other than hypotheses about how the world works; rather they may themselves be made up of descriptions of actual circumstances in which it can be said that certain kinds of tasks and task appropriate-behaviors might emerge.

settings (including experiments)

In this section, we would like to develop some criteria for the adequate description of mental activities, criteria which would overcome the pitfalls which mark the requirements traditionally used by cognitive psychologists. We have developed these criteria in response to our attempt to describe mental activities (specifically, problem solving) in a natural history setting. Not surprisingly, we found it no easier to meet the psychologists' requirements for an adequate description of cognitive processes in this setting than in the laboratory. If anything, it is even more difficult to define the tasks and to vary them systematically, although we have the sense that it would be easier to generalize across settings if in fact we could have described the tasks adequately. (Cole, Hood, and McDermott, 1977). The criteria we are going to propose should be of interest and hopefully applicable to both experimentalists and the more ethnographically prone, if only that they should make obvious the respective limits of either approach.

We are choosing to talk of mental activities rather than cognitive processes. Unlike processes, which have to be modeled, activities are locatable in their consequences. We follow Dewey (1927) in asserting that mental facts are to be considered activities: "If we see that knowing is not the act of an outside

spectator but of a participant inside the natural and social scene, then the true object of knowledge resides in the consequences of directed action." By directed, we do not mean automatic or determined, but rather that the activity evolves from moment to moment in conjunction with "objective circumstances". This retrospective/prospective, feedback/feedforward nature of the organization of behavior is traditionally not grasped in the study of mental activities. "under most laboratory conditions or in pedagogical experiments, we almost always present the subject with a 'prepared' goal, and the goal formation process thus usually escapes the investigator". (Leontiev, 1975)

We are less interested in task environments described in terms of how people respond to them, than in the procedures used or the contexts which allow for the construction of certain task environments in terms of which people display their mental activities. The significant features of a task environment cannot be decided on beforehand; they must evolve, and be shown to evolve, in the course of the subjects' involvement with the task environment. A description of a mental activity along these lines is at present only a theoretical fantasy. We will need some criteria for knowing whether or not we have accomplished such a feat.

I. Given the difficulties of the first requirement for the description of cognitive processes now in use by psychologists, we would like to suggest a first criterion which will allow us a more dynamic sense of the relation between a task environment

and a person's behavior. Accordingly, we will insist that any description of a mental activity must be specified in terms of a well-analyzed relation between the task environment and the behavior as they mutually constitute each other. That is, it will have to be documented how the task environment engages persons, holds their attention, leads them from step to step along a solution line and, finally, informs them of when a solution has been reached. At the same time, we will need a statement of how the persons, by their own involvement, organize the task environment so that their next action is partially in response to the environment they have just established. This reflexive definition of the task is hard to locate, but essential for any description of activities be they verbal, gestural or mental. In this paper, we are only dealing with the difficulties of establishing criteria for the description of mental activities, and we will not elaborate the specific procedures used to adequately describe the verbal and social activites which give us access to mental activities (McDermott, Gospodinoff, and Aron 1976).

II. With our second criterion, we would like to move beyond the difficulties of the traditional requirement that psychological processes be inferred on the basis of task environment-behavior relations as they change in similar ways across a range of parametric variations. Once we have located one mental activity as it occurs in a particular mutually constituted organism-

environment co-ordination or context, we would like to see if we could locate an activity similarly functional within a similar organism-environment context. The purpose of this exercise is to specify the differences between different contexts and the mental activities that each of them occasions. This variation across task environments will not be our only warrant for defining mental activities. Each activity will necessarily have a life within its own context and each will have to be well described. By moving to other contexts, we will merely strengthen our case and add specificity to how the activity might be used in response to similar demands.

III. Our third criterion will hopefully buy us the generalizability which is now so badly lacking in the analysis of psychological phenomena. Basically, our criterion is that any candidate for the status of a cognitive activity must be specifiable, and therefore generalizable, in terms of the multiple contexts which frame the particular organization of task environments and behavior under analysis. We have already pointed out how psychological investigations generally leave out any account of how a task environment comes to be engaged; simply, it is defined beforehand and the organism's performance on various subgoals is recorded in a yes or no fashion. This leaves out not only an account of how the organism attends to some dimensions of the task rather than others, but also an account of how the organism even attends to the task at all, or even how the organism came to be in the situation at all. These omissions are crucial, for

of the immediate contexts in which they occur, and until the most immediate contexts are specified as systematic parts of more inclusive contexts.

An Example of Problem Solving

The situation we are about to describe took place at a weekly cooking club held at The Rockefeller University. Two adults and seven children are present. The children, 4 girls and 3 boys, are between eight and ten years old and all are in the same class in school. This is the third session of the cooking club for the adults as well as for the children. Before the children arrived, the adults had decided that the children would be divided into four pairs, and that each pair would make a different kind of cake. One child is absent though, and this requires some reshuffling of the pairs, which begins as soon as the club begins.

We will argue that there are (at least) two tasks that the participants involve themselves in during the course of the one and a half hours of club. One is doing the actual cooking; the other is getting organized to do the cooking. We want to claim that getting organized in this instance involved a complete restructuring of the social order, due to the adult-imposed pairings. These tasks are not distinct; there is much overlap of tasks for individual members as well as between members. That is, at certain times, given children are both getting organized and cooking, and at other time, some children are only getting organized or only cooking.

We want to claim that both getting organized and cooking involve various mental activities. Our evidence for this comes

adult imposed social order and in the ways they go about searching for and responding to information about how to get the cakes made.

What follows is a description of only the "getting organized" part of the cooking club. We will claim that getting organized to bake the cakes involves the members in problem solving activity. We hope to specify the particular sub-problems that the members of the group both orient to and create. What follows represents our first, rather crude attempt at such specification.

At the beginning of the session when the children come in to the club room, take off their coats, and gather round the tables, they are paired as follows:

Time 1: A & B C & D E & F G

1. A, B, C, and D are girls; E, F, and G are boys.
2. Two adults, Mike and Nelda, are also present.
3. For a schematic of their spatial organization, see Fig. I.

Mike announces that the children are going to be working in pairs; Nelda tells them they'll be making 4 kinds of cakes and working in 4 groups. (See Appendix II: Transcript)

These statements by the adults represent a formulation of a potential problem for the children, namely, to organize themselves so that they can work with whom they want. B immediately orients to this, stating her preference for a partner ("I wanna work with A"), before any specific pair assignments have been made. The potential problem becomes a real one as soon as the first pair is announced by Nelda ("Group 1 is G & B").

When all the pairings have been announced by the adults, the children have been arranged (potentially, not actually) in the following pairs:

Time 2: G & B D & F A & C E & Mike

This arrangement differs from that at Time 1; none of the children has been paired with whom he or she wants (that Time 1 represents the sought-after pairings is implied by the children's stated preferences)(1. B: I wanna work with A, 2. F: Me and E, 3. E: Me and F, 4. A: B,I wanted to be with you).

All the children except G orient to this problem but in different ways. Four of the children, A, B, E, and F, explicitly state their preference for a particular partner (e.g., "I wanna be with A"; "Me (F) and E.") before and during the pair announcements; D objects to her pairing but does not state her preference; and C queries "who am I with?"

We will assume that the children's various responses to the announcements of the pairings are attempts to change those pairings and subsequent actions on their part seem to verify this. At this point the most direct approach works. F has been the most vocal and explicit, stating his preference for working with E 6 times, even once as a threat ("me and E are a group or I'm not doing anything"). Mike allows for the possibility of change for D, E, and F ("You guys unhappy - you two (E and F) wanna' do it together?"). Thus, the arrangement (Fig. II) has now changed to:

Time 3: G & B D & Mike A & C (E & F*)

with one of the sought-after groups, E & F becoming established at Time 3.

The problem has ceased to exist for E and F; they have gotten together and begin to work on getting the cake made. For the time being, E and F cease orienting to the general problem of organization into social groups. Neither of them says anything relevant to

*One circle represents the establishment of a pair; two circles indicate that the establishment occurred at this particular time.

the topic of "who's with who" once they are together, and they talk mostly about cooking their cake (5. F: OK, now wait, I can't read these instructions cause it's hard. 6. F: Grease the pan. 7. E: What pan? 8. E: You don't use butter.) (As can be seen, E and F later reorient themselves to the social organization, when their unit is threatened at Time 6).

In contrast, the other children continue to talk about "who's with whom." After this point, the direct approach used by E and F is prohibited. B begins a complaint, but Mike does not allow it (9. Mike: Oh now B don't start in on me like that OK? That's just too much to handle.) The type of talk that gets done now differs from that at the beginning, where the children were explicitly stating their preferences to the group. Now there are individual conversations between small groups of children, primarily to determine who G wants to be with. Recall that G was paired with B; she has stated that she wants to be with A, but G has remained silent. B and D separately query G for his preference, each beginning with the suggestion that he do it with Mike (e.g., B: "G, you wanna' to it with what's his name?"; D: "Wanna trade and be with him?"). When asked, G states that he wants to do it with E.

After it's been established that G wants to be with E, B whispers to A, they get up, approach Mike and ask if they can go to the bathroom. Immediately, they leave the scene. At this point, (Fig. III) the groupings are:

Time 4: A & B C D & Mike E & F G

By removing themselves, A and B have temporarily solved the problem of organizing themselves into a pair. Only C, D and G still have the problem of grouping to contend with. G leaves the room. C and D approach it directly; together they state the problem generally (C: "Can't we both be with who we wanna be with?") and then specifically in terms of who wants to be with whom (e.g., C: "B and A wanna' be together"; D: "the boys wanna be with E and she (C) wants to be with me. I wanna be with her(C)."). Mike's reaction to this is to suggest that C and D work together. They agree and the pairing (Fig. IV) are now:

Time 5: A & B C & D Mike E & F G

At this point, 3 of the sought-after pairs have been established, and all the children except G are with whom they want to be. The problem now shifts: the established pairs seek to exclude G from any of the groupings. D is the first one to orient to this problem. She has been told (10. G: I want to do it with E) and has stated (11. D: F wants to be with E and G wants to be with E) that G wants to be with E. But E is already working with F. If the 3 boys (E,F,G) are allowed to work together, then G will be with whom he wants and C and D won't have to be with G. D asks Nelda if 3 people can work together. Both Nelda and Mike say it's O.K. C and D appear to take this as a solution to their problem, and D informs E and F that G is allowed to work

[illegible] them (with Mike's O.K.) that G won't have to be with them (14. Nelda: Don't worry about G. We'll work it out somehow. Mike, is it alright if these two do their own cake? 15. Mike: Oh, yeah. 16. Nelda: It's O.K. you guys. You can do your own cake.). When G returns, he is greeted by three pairings from which he is excluded. G asks who he's with, and Mike at first tells him to work with E and F. They object strongly (17. F: Well, you're not doing it with us. 18. F: No you're not! (with us)), and Mike asks G, "Wanna do it with me?" G agrees, and the two of them begin to bake.

The social organization (Fig. V) is now exactly that originally staked out 15 minutes before:

Time 6: A & B C & D E & F G & Mike