Our plans called for us to bring the content of everyday activity into experimental settings and to discover the basic requirements of mental tasks in the everyday environment. But the research just described all centered on institutionalized settings like schools and forms of interaction that look more or less like testing. We were not being successful at getting into the intuitively interesting settings that occupied our imaginations in the planning phases. So we decided to create an environment that had a lot of the properties of non-school settings (often, and equally awkwardly, called everyday settings). Using a child language facility created jointly by G. A. Miller and Cole, Ray McDermott, Lois Hood, Ken Traupmann and I created afterschool clubs around the themes of cooking and nature club.

The clubs were set up in a cooperative arrangement with the Manhattan Country School, a remarkable private school located on the border between Harlem and the fashionable East Side, across the park from a heavily Hispanic area of the city. A teacher at MCS allowed us to come into her classroom and video tape. We were also allowed to give each child psychological tests from time to time so long as we coordinated with the teacher. The parents agreed that after school, one day each week, half the class would come to the afterschool club which would focus on cooking and nature activities, but would include outings, games of various kinds, and would keep the kids healthily occupied. We were allowed to tape-record these proceedings with as much fidelity as our facilities and limited expertise would permit.

The logic of this enterprise went like this: Schools are places where societies gather children together to instruct them in the basic elements of activities that the children will encounter as adults. A good deal of selection also goes on in school with respect to the ease with which students readily "take" instruction. Tests are indexes of mental ability on the one hand, and samples of school-like activities on others (this being the issue we struggled with in the Yucatecan research). As index/samples, they are used to select those children who, for whatever reason, are facile with its set of requirements. Schools, on the other hand, sample the larger society. Not everything is taught (despite some critic's comments), but a great variety of skills centering around the use or numeracy and literacy to acquire adult skills *are* taught. Therefore, just as it should be possible to see test-like behaviors appearing in school contexts, it should be possible to see school like behaviors (and their test-like components) popping out in everyday settings outside of school. If we set up an environment that was rich in the production of school-related skills, but "everyday" in its social organization, perhaps we could figure out how to talk about the relations between tested performance and competence in the world. This was our way of posing the problem of ecological validity, coming from cross-cultural studies of intellectual testing and the effects of schooling.

This enterprise produced a number of provoking results. While it proved relatively easy to identify test-like activities in school, it did not at all follow that we could see test-like activities in the clubs. In our planning, we had been able to gain support for the club idea from worried adults and teachers by talking about all the school like activities that go on their. The children *read* instructions, *write* written records of what they do, *measure* quantities, and *solve* problems. We could say this genuinely. We had some familiarity with the notion of a camp, and we fully expected such things to happen. In addition of course, we expected as social scientists to be able to say something principled about them when they did.

We satisfied the teachers' and the parents' expectations very well. There were the usual number of hassle about bus schedules and hurt feelings, but the children liked the clubs. Despite the heavy academic and social schedule of a New York 9-10 year old, they came pretty regularly and they got to know the staff quite well. They also got to know things about each other. So it is safe to say we satisfied the kids. But we blew the brains out of our initial expectations about cognitive tasks in everyday settings. The short form of our conclusion, so short that it makes the point appear trivial, is that *cognitive tasks were socially difficult to arrange* without recapitulating the control structure and invoking the norms of the school. On the first day of observation 8 active children and two adults baked four cakes in one hour, starting from written instructions a cold oven, and the usual raw materials. But when the observers emerged at the end of the bustle, they were unable to report a single good example

of any given category of cognitive task. In the bustle of getting the cakes done, the cognitive work load was partialed out in so many piecemeal ways that it was impossible to recapture the conditions necessary to claim the existence of a cognitive task.

As the project continued, we began to analyze the nature of the social divisions of labor and the kinds of social structuring that were making cognitive task identification so difficult. We created variations that removed adult contributions to the structuring in various ways. We created natural excuses for remembering and problem solving.

We wrote the first draft of our findings in 1977-78, and sent them to the *Psychological Review,* then under Estes' editorship. We got two reviews in reply. The first said that our treatment was terrific and so important that it should be published immediately; the second said that we had simply rehashed what everybody knew. In the absence of a positive solution to the problems we laid out, the article was little more than a complaint that science is difficult. The article was not published, and in retrospect I think it is a good thing. We had a great deal to learn before we ourselves could figure out something useful to do with the knowledge gained and we did not want to make ourselves even more difficult to understand by illusions for prescriptions. This work has had an active underground existence since its publication and certain parts of it have appeared in print.

Sample Publications

Cole, M., & Means, B. (1981). *Comparative studies of how people think.* Cambridge: Harvard University Press.

Cole, M., & Traupmann, K. (1980). Comparative cognitive research: Learning from a learning disabled child. 1979 *Minnesota Symposium on Child Development* (Vol. 12).

McDermott, R. P., Cole, M., & Hood, L. (1978). "Let's try to make it a good day"--Not so simple ways. *Discourse Process,* 3, 155-168.

Joint Theoretical Efforts

During this period the group engaged in a number of collaborative writing efforts that grew out of the weekly LCHC seminar. It seemed appropriate, since work grew up in joint discussion (and often involved a lot of input from people nominally outside the group) to list LCHC as the author, and append people's names in alphabetical order (scrambled just enough to keep people like A.B. Anderson from getting in trouble with the alphabetically less astute).

These were generally methodologically-oriented efforts which allowed us to proceed in a disciplined way despite our interdisciplinary constitution. In making things clear to ourselves, we found that it was helpful to require ourselves to be clear to outsiders. These publications have evoked some interesting discussions about the institutional role of authorship; one University head complained that we were being irresponsible. We blunted these comments by pointing out the joint teachingllearning nature of the seminars, and urged that they be used by committees as evidence of teaching ability.

Other important lines of theoretical work were done by various sub-groups within the lab or in specially created groups. Cole, working with SSRC support, got together several mini conferences on comparative methodology, which eventually produced a book for use by undergraduates. Scribner collaborated closely with Cole on questions posed by Soviet Psychological theory for general theories of mind. Hall and Dore collaborated on the development of a speech act theory adequate to Hall's data. McDermott collaborated with Dore on a critique of that same approach using McDermott's data. Throughout these activities, fellows and more junior research staff were deeply involved. It was a rich period of research in which a great deal of progress was made, but it was progress toward new levels of complexity.

Sample Publications

Cole, M., Hood, L., & McDermott, R. P. (1978). *Ecological niche picking: Ecological invalidity as an axiom of experimental cognitive psychology.* Laboratory of Comparative Human Cognition, University of California, San Diego & The Rockefeller University. Excerpts reprinted in 1982 in U. Neisser (Ed,), *Memory observed.* San Francisco: W. H. Freeman & Co.

Laboratory of Comparative Human Cognition. (1976). Memory span for nouns, verbs and function words in low SES children: A replication and critique of Schutz and Keislar. *Journal of Verbal Learning and Verbal Behavior,* 15, 431-435.

Laboratory of Comparative Human Cognition. (1978). Cognition as a residual category in

anthropology. *Annual Review of Anthropology,* 7, 51-69.
Laboratory of Comparative Human Cognition. (1979). What's cultural about cross-cultural cognitive psychology? *Annual Review of Psychology, 30, 145-172.*
Laboratory of Comparative Human Cognition. (1979). Cross-cultural psychology's challenges to our ideas of children and development. *American Psychologist,* 34(10), 827-833.
Laboratory of Comparative Human Cognition. (1982). Culture and cognitive development. In W. Kessen (Ed,), *Mussen handbook of child development* (Vol. 1). New York: Wiley.
Laboratory of Comparative Human Cognition. (1983). Culture and cognitive development. In W. Kessen (Ed,), *Mussen handbook of child development* (Vol. 1). New York: Wiley.

The Rockfeller Phase: Restrictions

The Rockefeller environment was excellent in so many respects that it was very difficult to think about moving the laboratory to another location. However, there were structural barriers to its development which promoted the thought of moving, if a suitable location could be found. The major difficulties were:

1) The hierarchical laboratory structure which allowed a tenured position to only one member of a unit (laboratory). This structure gave great power to the head of a laboratory and promoted efficiency for certain kinds of research. But in LCHC, where every effort was made to have researchers invent new ways of doing things (ways that differed from one ethnic group to another) such efficiency got in the way. Our multicultural approach granted equal validity to the many different kinds of expertise needed to synthesize different disciplines and forms of cultural knowledge; that form of equality ran counter to the structure of the institution and by 1978 was hampering our work. The lack of access to tenured positions for LCHC members undermined our efforts to create a system of equal control.

2) Rockefeller is an overwhelmingly male, Anglo Saxon community. It was not perceived as friendly by the minority group community, rendering the position of an Anglo head of a laboratory very problematic in terms of a long-term training/research effort where minorities were supposed to have an important, and eventually, guiding, role. The charge of colonialism could not be effectively countered given these conditions.

These difficulties were compounded in 1978 with a change in the presidency of Rockefeller. Joshua Ledeberg made it very clear that the behavioral approach used by LCHC did not accord with his idea of a basic research that would answer to medical problems, at least not in the way that he thought appropriate for the institution. Although we had begun to do research relevant to the problem of learning disabilities, that focus was too narrow to sustain the search for a culturally grounded psychology.

It is significant that when LCHC left Rockefeller University, Ledeberg refused to replace it with another behaviorally oriented laboratory, thereby bringing the behavioral science group below critical mass. The following year, both William Estes and George Miller left the University, and neuroscience replaced behavioral science as an organizing concept. This biologizing trend was, of course, precisely the trend that had been signaled so clearly at the beginning of our program by the social response to Arthur Jensen. The fact that culture is a *biological* characteristic defining homo sapiens had no foothold in science. Our mission was clearly unfilled.

Phase 4: UCSD, 1978-1984

The Promise

When a group gives up a privileged position in a privileged institution like Rockefeller University to take up residence in a public institution with a major commitment to undergraduate training (Rockefeller University is exclusively a research institution with a very small, elite graduate program), some sort of explanation is necessary. In this case, the explanation is that Rockefeller's shortcomings for the special mission of LCHC seemed matched by UCSD's virtues. The most important elements appeared to be the following:

Third College. During the late 1960's the UCSD campus of the University of California responded to the social upheavals of the times by creating Third College, an academic structure with a unique formulation that had survived the retrenchment of the 1970's with sufficient strength to appear in the 1983 catalogue as follows:

… Third College is guided by the belief that education should not be divorced from the social

> imperatives of our time...it has a distinctive academic focus on understanding the diverse elements which effect societal change and development and the alleviation of contemporary social problems … From its inception, Third College has been dedicated to the establishment of a multiracial, multicultural academic community.

While a good deal of the energy had drained from this effort, the superstructure was still there, and Third College still enrolled an exceedingly large ratio of minority group students. The UCSD faculty included two tenured Black psychologists, several outstanding social scientists with whom LCHC had been in contact over the years, and an organized research unit called the Center for Human Information Processing that appeared to be an excellent home for LCHC, providing intellectual support analogous to that provided by the behavioral science group at Rockefeller.

The Communication program and resources. All of these virtues would not be sufficient if there was not an academic unit that was legitimately located within Third College with open positions into which LCHC faculty could be fit. Here again there appeared to be a unique opportunity in the form of an interdisciplinary program in communications, the charter of which made it an integral part of Third College. Again citations from the catalogue are informative:

> Communications at UCSD is an interdisciplinary effort, drawing upon the strengths of the social sciences such as anthropology, linguistics, political science, psychology and sociology. In their courses, communications students will master theories, concepts, and methods for dealing with the study of interaction at the political, societal, group, and individual levels … The program is housed on the Third College campus, and plays a central role in the efforts of Third College.

This formulation fit very nicely into the theoretical ideas that were growing within LCHC wherein the two part, stimulus-response formula of the 1950's and 1960's was replaced by a three part semiotic formulation, which allowed *culture* into the system as the "medium" of interaction.

Not only does this formulation fit the goals of LCHC, but the special circumstances of the Communication Program in 1978 made it possible to envision hiring faculty who would implement the stated goals in manner consistent with LCHC's mission. Owing to longstanding internal conflicts, more than 300 students were majoring in Communication, but only 3 full-time faculty were assigned to the program. This faculty/student ratio was recognized as a serious distortion, so that the move of LCHC to UCSD could be accompanied by the prospect of hiring sympathetic faculty in the years ahead.

This potential has been realized beyond reasonable expectation in the intervening 5 years. We have available a description of the newly formed Communication Department, whose structure maps in a genuine way back on to the overall mediational formulation underlying the current theory guiding LCHC.

Psychology Department. As a member of the Psychology Department, Cole could admit graduate students. A Black faculty member in Psychology, A. B. Anderson, agreed enthusiastically to join in the LCHC effort. In addition, several outstanding psychologists in the department offered their support to the training program.

The social science faculty and TEP. A major resource for the Laboratory at UCSD was the presence of several prominent social scientists, in addition to those in the psychology department, whose areas of expertise fit closely with our concerns. Between 1975 and 1978 there was extensive contact over research issues between Cicourel and Mehan in Sociology and D' Andrade in anthropology over issues of cognition and social interaction. When Mehan conducted a study of the organization of classroom lessons with Courtney Cazden, a series of meetings were held in New York and San Diego to review methods and data.

As director of the Teacher Education Program, Mehan was particularly instrumental in the group's move. He found teachers interested in working with cognitive researchers in their classrooms. The multi-cultural emphasis of TEP meant that there could be a more direct tie between research and teaching in the University and appearance of the fruits of that effort in the classroom.

Initial Configuration of UCSD activities

Administratively, LCHC was made a part of the Center For Human Information Processing (CHIP) with its offices and lab space located in the building housing the Communication Program and TEP.

Using temporary faculty money, Luis Moll was hired in Communication to teach about language and society as well as problems of bilingual communication. He applied for, and obtained, a grant from NIE to study the organization of bilingual reading instruction.

Alonzo Anderson, in psychology, was looking for a project that would combine his background as an experimental, social psychologist with a strong interest in ethnic differences in socialization practices. After some preliminary feasibility studies, he hit upon the study of the way that working class families socialize their children into literacy practices. He obtained a grant from NIE to study literacy practices in lower class Black, Chicano and Anglo homes. His plans, and LCHC's suffered a blow in 1979 when his contract was not renewed by the psychology department, but a grant from the Spencer Foundation enabled him to continue the work. He was later able to obtain a second NIE grant to extend his observations into the early school grades.

Jim Levin, a cognitive scientist who obtained his degree at UCSD joined the Laboratory and the Communication Program as a specialist in microprocessors as a medium of instruction and communication.

With help from Bud Mehan, Mike Cole gathered a small research group to conduct the next step in the work on inter-relationships between psychological tests, classroom organization of instruction, and everyday cognition. Mehan convinced Peg Griffin, a former teacher and highly experienced sociolinguist to join the project. Denis Newman, who had worked with Dore at Rockefeller, also participated.
The basic working arrangement whereby minority group scholars had their own base of operation within their own community settings meant that we had a legitimate context for pre-doctoral and post-doctoral A Twelve Year Program of Research and Training in Cultural Psychology

training. UCSD provided something else, albeit temporary, faculty positions, along with space and administrative support. The sympathetic group of social science colleagues provided a source of expertise on difficult problems of method and theory. Leavened by the participation of fellows using training money carried over from the unfinished grants in New York, this group reconstituted LCHC in its Western setting.

During its years at Rockefeller University our group had put together a rather thorough critique of existing approaches to the intertwined phenomena of mind, culture and school performance. From a great many sources we were getting the message that it was time to turn from critique to positive action. It was this task which has occupied the center of our attention. I will review the work with respect to four major components: theory building, research, training, and institution building.

Theory Building: Constructing an Alternative Framework

From the frequency with which our work was being cited in the relevant literature and our success in obtaining research grants, it was clear that our criticisms were having an impact.

For example, Gelman pointed out that the principles of cross-cultural research contained in our work seemed to apply to age comparisons in her 1978 *Annual Review of Psychology* article. Donaldson, in an outstanding book, *Children's Minds* cited our studies of inference as evidence for her formulation or the principles of cognitive development. But we had not offered the crucial connecting links, and for the most part our colleagues were waiting for us to come up with the "theory of situations" that would allow specification of the patterns of performance conditioned by different cultures.

The 1980's have witnessed a marked change in both the theoretical foundations and networks of cognitive development research in America and Europe which have moved our formulation closer to the academic mainstream. From many sources have come formulations of theories of adult cognition and cognitive development that adopt a context-specific approach to understanding cognition in place of broad stage and ability formulations. Representative is the work of Robert Siegler, who claims in the context of problem solving research that

> "Developmental differences (on cognitive tasks) seem more to involve improvement in the range of conditions under which appropriate representations are formed than in the inference process itself."

Very similar formulations are to be found across a wide spectrum of otherwise-diverse developmental research, including the work of Kurt Fischer, Robert Sternberg, Howard Gardner, and Katherine Nelson to name a few.

In the literature on adult cognition, the widespread move to schema-based theories has yielded precisely the same result. Thus, we get such generalizations as the following from David Rumelhart

> Most of the reasoning we do apparently *does not* involve the application of general purpose reasoning skills. Rather, it seems that most of our reasoning ability is tied to particular schemata related to particular bodies of knowledge.

Three specific implications of this view will be of concern to us here.
First, contemporary approaches imply that behavior in familiar situations will differ from behavior in unfamiliar or less familiar situations because "Familiar situations are those for which schemata have already been formed and in which top-down processes playa large role" (J. Mandler). Second there will be marked variability of performance of cognitive tasks *within* individuals across cognitive domains/real world settings. The third implication to be drawn from the current work in cognitive psychology is that transfer is hard to explain. Theories specify a context-dependent unit of analysis, but as Jean Mandler savs. "We know relatively little as yet about either the circumstances or the possible developmental changes

that encourage their generalization."

The consequence of this shift in theory has been to provide us with precisely the links that we need to be in the mainstream of work on cognition and cognitive development. We have emphasized the situational variability and limited transfer of cognitive skills; this notion has moved from the periphery to the center of contemporary thinking.

At the same time, we felt that we had some useful additions to make to this effort. We focused on a problem that our cross-cultural entry point emphasized, but which was submerged in domestic research: if learning is context-specific and transfer is limited, how does *change* occur? We laid out our approach to this central issue of instruction and development in two major *Handbook* chapters, one oriented toward the concept of "intelligence," the other toward the concept of "development." In formulating an answer to the mechanisms of cognitive change, we also provided an alternative framework for the study of cognition, a framework which insists that culture and cognition are different aspects of a single system of interactions. We had arrived at the idea of a cultural theory of cognition, in which one had to treat each context of observation as a sample of culturally organized activity. Both interactions *within* the context and interactions between contexts had to be studied in order to account for performance and performance change. Moreover, the intertwining of cultural and "natural (biological)" factors constraining performance had to be studied as well.

The central notion of our concept of intelligent human behavior can be summarized as follows:

1) Cognitive acquisitions are (learning is) initially context specific.

2) Generality of cognitive acquisitions is tightly connected with the social organization of relations between contexts (including the way in which context relations are coded in language). Put less formally, change is the result of socially organized interaction.

3) The process of acquisition within contexts is interactively achieved; these interactions are often mediated by one or more communicative "tools" (language, print, films, etc.)

4) The resources brought to the interaction plus constraints (biological, social, institutional, economic) on the interaction must be assessed as part of a general theory of cognition.

Because coordination between interacting systems is very complex, change involves conflict, and cannot always be interpreted as development.

The implications of these ideas are worked out in the context of several research projects within LCHC. Each project concentrated on: 1) a selected set of contexts; 2) the way in which the social order arranged for those contexts to arise and for their internal structure; 3) the interactions within context that assemble new cognitive acquisitions (particularly, the way in which the interpersonal organization of behavior becomes intra-

personal, or "psychological'); 4) the social and individual resources that are brought to bear on the problems at hand, and the way that these contribute to judgments of competence.

Empirical Studies and Their Implications

The cognitive consequences of literacy. Although the empirical work was completed by 1977 it took a great deal of time and effort to complete the data analysis and write up the work on the consequences of literacy begun in 1974-75. This work gave us our first working model of how social constraints organized around different domains of activity and political power combine with available technology to promote the development of different *kinds* of literacy, each with its own set of "cognitive consequences."

So long as we stuck to test results from the standard tasks used to assess the cognitive consequences of schooling, it appeared that becoming literate in a native language in order to conduct business was cognitively equivalent to total illiteracy. However, when tests of cognitive consequences were tailored to the structure and domain of the literate activity, people literate in their native language could be shown to outperform schooled populations. At the same time, the effectiveness of schooling in changing the information processing proclivities of students for a wide range of tasks that fit importantly into modern economic activity was confirmed.

These results stand as a strong challenge to those who assume that schooling operates to change basic thought processes *in general* by demonstrating the context-specificity of all literates' accomplishment. It also provided an example of how native peoples, using their own technology of communication, could organize to improve their lot through literacy. The book describing this work won a prize from the African studies association in 1982.

We learned an important lesson about one work from the favorable review that appeared in *The New York Times*. When the reviewer attempted to apply the lessons of the researcher, his comments evoked a lot of controversy among our colleagues. We had shown that the effects of literacy are intimately connected to the socio-political *constraints* (among others) on the uses of print. But when this general point was removed from its African context to New York, its lessons were by no means easily agreed upon. We needed to work directly on the problems we had raised here at home. Sylvia Scribner subsequently carried this work directly into the American workplace.

Cognitive science and education. The work on tests, schools, and everyday settings that we had begun at Rockefeller University had adopted the deliberately naive notion that it is possible to discover cognitive tasks in everyday life as a way of demonstrating the special properties of cognition in schools. This next attack on the problem applied the diametrically opposite strategy. We constructed cognitive tasks with clear structure and well worked out positions within developmental and instructional theories. We then embedded them in differently organized contexts to see how the context would invade and disassemble our tasks.

We applied this strategy in an ethnically heterogenous 3-4th grade classroom with the help of teachers who had graduated from the UCSD TEP program. And we chose to address a vexing practical problem which stemmed from the general uncertainties of standardized assessment that we had been wrestling with for the previous decade.

The practical problem centered on coding schemes to assess the teaching/learning process. There were many calls for such coding instruments for evaluation of large federal assistance programs, teacher effectiveness, the impact of different social arrangements within the classroom, and a host of other issues related to educational improvement. A good deal of work on input-output models of educational effectiveness had left the mechanisms of change as cloudy as the results of the regression analyses, and there was general agreement that" process measures" of education were needed.

Our particular interest was in on-line coding schemes that purported to be *cognitive* assessment schemes. Our prior work had led us to be deeply suspicious of on-line cognitive coding, especially when applied to non-normative children. We latched on to this problem as a natural interface between our theoretical concerns and NIE's concerns (soon to be abandoned) with educational equity.

Over a two year period we constructed curriculum units in a variety of different educational content areas which doubled as "cognitive task tracers" for our analytic benefit. Units were conducted in electricity, chemicals in the home, native American cultures, remembering, long division, and mapping. Each unit was divided into lessons, taught by the regular classroom teachers, to groups organized to provide a wide range of constraints on the teaching learning process. There were lessons where the teacher taught all 30 children