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Recollections of Kindergarten*

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There has been a good deal of speculation about early memories (Freud, 1953; Schachtel, 1947; Neisser, 1962; White & Pillemer, 1979), but few empirical studies have addressed their adequacy and accuracy, or the rate at which they are forgotten. What proportion of one's experiences in kindergarten, for example, can still be remembered five or ten years later? What kinds of experiences are remembered best? Does forgetting take place rapidly, or only gradually over the course of years? How trustworthy are early recollections? What is the range of individual differences in early recall? Such questions are fundamental for an ecologically relevant psychology of memory, but they cannot be answered because the necessary observations have never been made.

An opportunity to conduct a pilot study of early memories presented itself to us in 1978. Mrs. B., a kindergarten teacher at the Northeast School in Ithaca, New York, kindly allowed us to examine her daily planbooks for several previous years. These planbooks are quite detailed. For each period of each school day throughout the year, they describe the activity in which the children were engaged: story hour, games, singing, field trip, etc. In many cases specific descriptions of the activities are given: which story, which game, what songs. Our study was simple: we asked children of various ages what they remembered from kindergarten, and compared their responses with the planbooks. Since Ithaca is a relatively stable community (the Northeast School itself is in a predominantly white middle-class neighborhood), it was not difficult to locate some of Mrs. B's pupils even after a lapse of years.

The present paper reports results obtained with three age groups:

(a) The *eleven-year group.* These subjects had attended kindergarten in 1966-67. We located 12 of them in the spring of 1978, 7 boys and 5 girls. All were attending Ithaca High School, most in the eleventh grade. Subjects were contacted by telephone, and interviews were conducted either in their homes or at the Cornell Psychological Laboratories.

(b) The *eight-year group.* These children attended Mrs. B.'s kindergarten in 1970-71 and were tested in the fall of 1978. Because of time limitations, we interviewed only the 6 such children (3 boys and 3 girls) who could be found in the eighth grade of a particular junior high school. (There are two junior high schools in Ithaca.) Interviews were conducted during school hours, and at the school.

(c) The *five-year group.* These children had attended kindergarten at Northeast in 1973-74, and were still there (in the fifth grade) when they were tested in fall 1978. Six such children (2 boys and 4 girls) were interviewed, all during school hours and at the school.

All the interviews were conducted by Eric Gold. The subject's responses were tape-recorded. Each interview began with the open-end question "What do you remember about your kindergarten?" and continued through a fixed series of increasingly specific queries about classmates, teaching personnel, the classroom, activities, field trips, stories, games, art projects, songs, etc. A shorter interview was used with the younger children. The eleven-year interviews took about 45 minutes; the eight- and five-year interviews about 15 minutes.

Results

The most striking finding was the wide range of individual differences in recall. One subject could give no answer at all to the opening question "What do you remember..."? Although this eighth grader did produce some recall later in the interview, her performance contrasts sharply with that of the best subject, A. C., a male in the eleven-year group. A. C. is especially interesting because he was the only subject who had not attended the Ithaca public schools continuously since kindergarten: grades one through

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six had been spent in a local parochial school. His six-minute response to the opening question is worth quoting in full:

It was at Northeast School and I was in the second shift, from 12:00 to, I guess, 2:30. I walked in the morning and I was usually late because I'd be on the walkway and hear the 12:00 whistle. I was late and started running. I usually went in the side door, not the front door, because the class was just down the hall from there. We had all sorts of fun stuff to do. We always had a little kitchen and all the things to play with. We had these huge blocks that we used to make big houses out of and I was in the basement of these different level houses that we could build and I was in the bottom floor and the whole thing caved in on me. A couple of blocks hit me in the head but they moved the wreckage and I was alright and that is one of the things I remember because I talked about it when I came home with my ma. I don't remember a naptime, I don't think we had that but I'm pretty sure we used to have milk and graham crackers every day. Fingerpainting was my favorite because you'd goosh around in the paint and stuff. In one of the craft courses, I made a blue clay fox that I made as a paperweight for my father. He's still got it on his desk. The knobs on the stove, the play stove were loose and they used to fall off. I don't think it opened. I think the doors and stuff were just painted on. There were always a couple rusty teacups and things to play with and during recess we used to go outside in a little walled off area. One day we had these wagons and things on wheels and we had this one board that had big play wheels with rubber with the red inside. I guess we called it the bus because you could either lay down in it or have a lot of people in it. This one kid got on it before I did and I was mad so I, this isn't funny, I said I would push him. I went a little too fast and he fell over and got hurt but I got to use the thing. That's not really why I did it but I can remember pushing him by mistake. I remember the kids in the class next to us. I remember their teacher, I don't remember her name but Liz Burns was in the class next to us and their class used to always sing this ABC song. We never sang that. We had show and tell a lot and one girl, one time, talked about her back. She had an operation on it or something or some deformity, I don't really remember but everybody used to say that she had a brass back. She had a pin in it or something. I can remember when she fell off the teeter totter and everybody was worried. The cloakroom had hooks that were low for us and the door, I can remember closing myself in every once in a while during recess, you know, just playing around. We would play hide and seek and play in there and the doors were on runners and it's gray and it folds. Let's see, there's a big van up there now, a learning van or something I guess but it wasn't there when we were there. My sisters used to get off the bus at Northeast and come down and pick me up after school and walk home with me. They made sure I didn't get mugged or something. Mrs. B. was the teacher's name. I can remember ma talking about a progress report she got, something about being quiet. You know in kindergarten, you can't say much.

As this example suggests, people remember many different kinds of things from kindergarten. This variety creates a number of problems in the analysis of the data. Some of the recollections we obtained were general ("During recess we used to go outside...") and some were specific ("...a progress report ... about being quiet"). Some concerned people ("L.B.") while others dealt with activities, objects, and specific events. Some could apply to almost any kindergarten ("We had all sorts of fun stuff to do") and some were probably unique to this one ("The knobs on the ... play stove were loose"). Some were of the sort that could be verified by the planbooks ("...milk and graham crackers every day") while others obviously could not ("A couple of blocks hit me in the head..."). Moreover, the interview questions themselves could not be used as units of analysis, because it often happened that answers to later questions had been given earlier and were not repeated; in addition, the younger subjects did not always confine their answers to the question that was asked. Another difficulty arose because most of the subjects (except A. C.) continued to attend Northeast School after kindergarten; indeed, the fifth graders were still there at the time of our interview. Therefore it is not safe to assume that playmates, or features of the schoolroom, were being remembered from kindergarten itself. Many years of additional experience were available to support descriptions of the kindergarten classroom (which was much like other rooms in the same building) and the toys available there; many years of acquaintance or friendship must have made it easy to remember other children who had been in Mrs. B.'s class.

Two analyses of the data were made. Both were based on all the information provided by the subject in the course of the interview, including the initial response and later answers to more specific questions. (Certain recognition questions in the latter part of the interview were excluded because positive responses may not have indicated actual remembering.) Some subjects seemed to require more cueing than others. Specific cues often elicited more recall than general ones. Many subjects who had answered "No" to "Do you remember any field trips?" nevertheless remembered many details when they were then asked "Do you remember taking a field trip to an apple orchard?" ("We watched the apples go round on a conveyor belt. They showed us how cider is made.")

The first analysis was a tabulation of all the memories produced by each subject, except that recollections of physical objects (toys, play equipment, features of the classroom) and of individuals (classmates, teachers) were excluded because they might be based on information from later years. The subject's responses were divided into distinct "memories," comparable to the "idea units" often used in studies of story recall. Each memory was classified as either "specific" or "general." Specific memories described events that took place once or only a few times during the year; general memories seemed to be based on events that occurred daily or often, and on overall knowledge of the kindergarten. In this first tabulation, no attempt was made to verify the memories by using the planbooks. Moreover, no distinction was made between items unique to the subject's own kindergarten experience at Northeast School and those that were common to many kindergartens and might easily have been guessed. Everything was counted.

The tabulations of specific and general memories were made by Eric Gold, using the written transcript

of each interview. The results of this tabulation, for both types of memory and each age group, are presented in Table 1. (The correlation between the numbers of specific and general memories, across all 24 subjects, was .53. We do not attach much importance to this figure, which may just reflect variations in talkativeness.)

Table 1
Mean Number of "Specific" and "General" Memories (Unverified) Per Subject in Each Age Group*

	Specific Memories	General Memories
5 —year Group (n = 6)	5.17 (1-9)	15.33 (10-19)
8 —year Group (n = 6)	6.17 (3-11)	12.67 (5-21)
11—year Group (n = 12)	4.08 (2-10)	12.00 (8-18)

*Ranges given in parentheses.

Table 1 shows surprisingly little evidence of forgetting across the six-year span from fifth to eleventh grade. Although the eleven-year group does exhibit the least mean recall in both categories, the differences are slight and all the groups overlap almost completely. Separate Kruskal-Wallis one-way analyses of variance by ranks were computed for specific and general memories; no significant effect of age group appeared in either case.

The second analysis was restricted to information that could be definitely verified from the planbooks. In practice, this limited us to certain recurrent activities that Mrs. B. planned more or less explicitly in advance: *stories, games,* and *art projects.* The planbooks record how many periods were devoted to each of these activities. In many cases the specific story, game, or art project is identified: *The Littlest Reindeer,* "Tall and Small," making leaf prints. In other instances the planbooks are less specific; an entry like "story hour" may not specify the story. These non-specific entries are important, because they allow us to make a rough estimate of the *total* numbers of stories, games, and projects in a given kindergarten year. Dividing the number of specifically identified stories, etc., by these totals yields *Verifiability Ratios* (VRs) for the various categories. Although the VRs are only rough estimates (a game may have been played more than once; two brief stories may have been read in a single period), they nevertheless indicate what proportion of our subject's memories we can expect to validate. If the planbook for a given year lists only about one-third of the stories that were actually read, for example, we should expect to verify about a third of the story-memories produced by the subjects. The other two-thirds are probably not confabulations or inventions, but just stories that Mrs. B. did not note in her planbook. Table 2 presents the base data and the VRs for the three chosen categories in each planbook. A fourth category, *songs,* was not included because the large number that can be sung in a single period makes it impractical to compute a VR. A fifth category, *Field Trips,* was not analyzed because these trips were often restricted to the morning group or the afternoon group alone; hence it was hard to be sure whether a particular child had actually participated in a trip or not.

Table 2
Stories, Games and Art Projects in the Planbooks

	Stories			Games			Art Projects		
	SIA	TNP	VR	SIA	TNP	VR	SIA	TNP	VR
1973-74 Planbook (5-year Group)	53	150	.35	32	76	.42	59	61	.97
1970-71 Planbook (8-year Group)	21	91	.32	31	48	.65	45	52	.87
1966-67 Planbook (11-year Group)	35	93	.38	38	72	.54	39	51	.76

SIA: specifically identified activities of each category; TNP: total number of periods devoted to the category; VR: verifiability ratio, SIA divided by TNP.

The recall data for the three verifiable categories are given in Table 3. The left half of the table makes it possible to evaluate the accuracy of the recalls. The most easily interpretable statistic is the *accuracy ratio* AR: the proportion of the responses dealing with stories, games, or art projects that could be definitely referred to a particular activity mentioned in the planbooks. AR is quite high for art projects in all three age groups; i.e., most of the projects described by the subjects had been explicitly recorded by Mrs. B. The observed ARs are much lower for games and stories. This drop should not be interpreted at face value, however, because VRs are lower for these categories as well; the planbooks identify only about half the games and a third of the stories in the first place. When this is taken into consideration it appears that fifth-graders' recall of games and stories is just as accurate as that of art projects. The same cannot be said of the eight and eleven year groups, however. They seem to be "recalling" so many undocumented games that some must be inventions or confabulations. (They may be games played in some other setting or some other year, now wrongly attributed to kindergarten.)

The right half of the table indicates how many activities of each type were actually mentioned by the subject. The numbers are very low. The average fifth grader remembered about one story, one game, and one art project from kindergarten. These are probably correct recalls (we have seen that fifth graders are accurate in all categories), but they amount to less than 2% of the games and projects, less than 1% of

Table 3
Accuracy and Amount of Recall From Three Verifiable Categories: Stories, Games, and Art Projects

Table 3A: Stories

	Recall Accuracy					Recall Quantity		
	MVR/G	TNR/G	AR	VR	IC	MR/S	TNP	RP
5-year Group (n = 6)	2	6	.33	.35	0.95	1.00	150	.0067
8-year Group (n = 6)	0	5	0	.32	0	0.83	91	.0091
11-year Group (n = 12)	0	7	0	.38	0	0.58	93	.0062

Table 3B: Games

	Recall Accuracy					Recall Quantity		
	MVR/G	TNR/G	AR	VR	IC	MR/S	TNP	RP
5-year Group (n = 6)	3	6	.50	.42	1.19	1.17	76	0.154
8-year Group (n = 6)	1	5	.20	.65	0.31	0.83	48	.0173
11-year Group (n = 12)	1	9	.11	.54	0.20	0.75	72	.0104

Table 3C: Art Projects

	Recall Accuracy					Recall Quantity		
	MVR/G	TNR/G	AR	VR	IC	MR/S	TNP	RP
5-year Group (n = 6)	5	6	.83	.97	0.86	1.00	61	.0164
8-year Group (n = 6)	5	6	.83	.87	0.95	1.00	52	.0192
11-year Group (n = 12)	10	12	.83	.76	1.09	1.08	51	.0212

NVR/G: number of verified recalls in entire group of subjects; TNR/G: total number of recalls whether verified or not; AR: accuracy ratio, NVR/G divided by TNR/G; VR: verifiability ratio from Table 3; IC: index of correctness, AR divided by VR; MR/S: mean recall per subject; TNP: total number of periods in kindergarten for this category; RP: recall proportion, MR/S divided by TNP.

the stories. Art projects are apparently remembered about equally well by all three groups; the average eleventh grader also still remembers one project. Where games and stories are concerned, however, there may be some effect of age. The eight and eleven year groups recall less in these categories than the fifth graders. Since we also know that even what they do "recall" cannot be found in the planbooks as often as might be expected, we suspect that some forgetting of these categories may still occur after fifth grade. The evidence is by no means compelling, however. The numbers are too small for a satisfactory test of statistical significance. Moreover, we must bear in mind that the fifth graders were still in the same school building as Mrs. B's current kindergarten. It is easy to imagine that they occasionally have an opportunity to notice kindergarteners playing a game they used to play themselves, or to overhear a story being read aloud. The opportunity for continued exposure, which led us to avoid any analysis of memories for people and physical things, may have a slight influence on games and stories as well.

Discussion

This preliminary investigation of recollection from kindergarten suggests only a few definite conclusions. Not many kindergarten activities are long remembered. Most have been forgotten by fifth grade. Some forgetting may continue to occur over the half-dozen years after that, but it did not reach statistically significant levels in this study. The occasional story, game, or project (about one in a hundred) that is still remembered in the fifth grade evidently has a good chance of making it to the eleventh. (The fact that art projects seem a little more resistant to forgetting than stories or games may be worth further consideration. These projects tend to involve the child more personally than most other activities, result in a concrete and memorable product, and may lend themselves especially well to visual imagery.) Overall, however, we find surprisingly little verifiable kindergarten memory in these data.

We are intrigued by the fact that our best subject, A. C., was the only one who left Northeast School after kindergarten was over. This may have had the effect of isolating and insulating his recollections of kindergarten from the rest of his memories. For all our other subjects, memories of kindergarten must have tended to blur together with other events that took place in the same building and involved the same people. However, one subject hardly establishes a hypothesis.

It is also noteworthy that our subjects exhibited so little tendency to fabrication. The art projects they remembered from kindergarten were real ones; the stories and games they recalled (at least the fifth graders) had really been read and played. Of course there are many recollections that we cannot verify: did those blocks really hit A. C. on the head? Perhaps some kinds of memory remain accurate over the years and some do not. We hope that further research will clarify this issue.

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Psychological Differentiation and Operational Development: A Cross-Cultural Link*

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Over the past few years the status of developmental theories in cross-cultural research has given rise to some controversy. Cole and Scribner (1977) have argued that the use of developmental theories, such as those of Piaget and Witkin, implies that people can be characterized in terms of a single "level of development," along a value-oriented sequence, and that cross-cultural differences may thus be interpreted as deficits; these authors fear that a "child-like status" is ascribed to adults in non-Western cultures. In fact, some anthropologists seem to be tempted to characterize the functioning of a social group as a whole in terms of developmental stages drawn from psychological theories (e.g., Hallpike, 1976). Dasen, Berry, and Witkin (1979; cf. also Witkin, 1977; Berry, 1980; Berry, Dasen, and Witkin, in press) are of the opinion that developmental theories are useful starting points, in so much as they suggest hypotheses which can be tested through cross-cultural comparisons. They reject the automatic ascription of any "deficiency" to populations or individuals on the basis of differences in task performance, these being interpreted in the context of ecological functionalism (Berry, 1976) as adaptations to eco-cultural demands and values; they also reject any facile but unwarranted transfer between the ontogenetic, phylogenetic, and cultural evolutionary dimensions.

An interesting contrast and integration of "contextualism" (represented by Bruner, Cole, and others) and "constructivism" (represented by Piagetian orthodoxy) has been presented by Harris and Heelas (1979). Table 1 shows an outline of their scheme. According to this analysis, "contextualists" are reluctant to admit that new intellectual principles are constructed in the course of development, but think that the same, invariant skills are transferred to an increasingly wider variety of relatively impermeable contexts; similarly, "cultural differences in cognition reside more in the situations to which particular cognitive processes are applied than in the existence of a process in one cultural group and its absence in another" (Cole et al., 1971, p. 233). Thus, according to an extreme form of this alternative, "all the basic intellectual operations are present at birth, any failure to demonstrate such principles being due to the difficulty of finding a suitable context for their elicitation" (Harris & Heelas, 1979, p. 214). "Constructivists," on the other hand, argue that only a few basic sensori-motor schemes are inborn, with later intellectual structures being constructed, according to a sequential order of stages, through the interaction of individuals with their environment.[1] Each stage is characterized by a limited set of principles, which apply to a wide variety of domains. Thus "Piaget assumes the permeability of different intellectual contexts" (Harris & Heelas, 1979, p. 214), in other words, a *"structure d'ensemble,"* or, domain consistency.

Table 1
Alternative Approaches to Cognitive Processes*

	Contextualism Cole & Bruner Cole & Scribner	**Constructivism** Piaget	**Local Constructivism** Harris & Heelas
Basic Operations	invariant	constructed	constructed
Contexts	impermeable	permeable	impermeable
Models	functional cognitive systems	structure d'ensemble	valleys of construction

*Following Harris and Heelas (1979)

Because a review of the cross-cultural evidence (e.g., Ashton, 1975; Dasen & Heron, 1980) does not support the hypothesis of domain consistency, Harris and Heelas suggest a third approach, called "local constructivism," which combines the developmental construction of basic operations with a relative impermeability of contexts. As a metaphor, they use the phrase "valleys of construction": progress in any one of the intellectual valleys has a constructive stage-like character but there is little communication between valleys.

"Hence the fact that the child makes good or poor progress in one particular valley is unlikely to permit accurate predictions concerning his progress in any of the others.... The existence of cross-cultural varia-

* This paper has been presented at the XXIInd International Congress of Psychology, Leipzig, July 1980, in symposium 17, Cross-Cultural Studies — Theories and Methods.

[1] Harris and Heelas (1979), in their otherwise most telling analysis, forget to mention that this interaction of individuals with their environment is assumed by Piaget to occur at such a general level that the developmental sequence may be expected to be universal (e.g., Piaget, 1974). Thus both the contextualist approach and orthodox Piagetian theory may be seen to defend the assumption of psychic unity. However the cross-cultural data do not support this assumption fully. I have suggested (Dasen, 1977b, in press; cf. also Dasen & Heron, 1980) that the universality of the sequence may be verified at the "competence" if not the "performance" level, but of course this claim, pushed to its limits, is unfalsifiable. There are thus parallels between the contextualist and the constructivist positions which Harris and Heelas (1979, p. 216 n) have apparently misunderstood.

tion provides a natural laboratory for discovering the extent to which concepts treated as a coherent constellation in our own culture may be cleaved apart by the specializations of a particular culture" (Harris & Heelas, 1979, pp. 219-220). One example of this differential valuation of conceptual domains is my study of concrete operational development in nomadic, hunting and gathering populations, *versus* sedentary, agriculturalist ones (Dasen, 1975, 1977a).

"Local constructivism" fits the empirical data much better than either contextualism or constructivism alone. However, both Harris and Heelas (1979) and Cole and Scribner (1977) are setting very high demands on the cross-cultural extension of Piaget's theory in asking for complete domain consistency or radical permeability of contexts. Piaget himself, beyond the notion of *horizontal décalages,* does not expect an individual to function consistently at the same stage in every aspect of daily life (e.g., Piaget, 1937, 1972). Intracultural studies with a psychometric data-base, such as the recent work by Longeot (1978), show the limits to the homogeneity to be expected within a stage and provide more realistic alternative models.

If development in one "valley of construction" is a poor predictor of development in another "valley," it does not make sense to speak of anything as global as the "cognitive status" of an individual and, a fortiori, of a population. Harris and Heelas conclude: "More generally, it is misleading to speak of anything as stage-like as a 'primitive mind' " (p. 221). I could not agree more with his conclusion.

Whereas Harris and Heelas go on to discuss the difficult problem of the relationships between cognitive development and collective representations, I would like to turn to a more psychological issue which has been almost completely disregarded within Piagetian psychology, be it cross-cultural or not: individual differences. The study of individual differences in operational development is not at all of interest to Piaget himself, who always insists that he is an epistemologist and not a psychologist. As long ago as 40 years, however, Inhelder (1943) used the tasks derived from Piaget's theory to study the reasoning of mentally-retarded children; later, Schmid-Kitsikis (1969; Schmid-Kitsikis, de Ribaupierre, & Rieben, 1976), Rieben (1978), and others gradually introduced the use of Piagetian tasks into clinical practice. Vinh Bang in Geneva, and others elsewhere (Laurendeau & Pinard 1968; Tuddenham, 1969; Longeot, 1974, 1978) have contributed to this trend by turning Piagetian tasks into diagnostic tools. At the same time, attempts to standardize the tasks have shown that individual variation in performance is much greater than would have been expected from the theory.

The question of the origins of individual differences in operational development has hardly been addressed at all. Reuchlin (1972), in a review paper on socio-economic factors in cognitive development, has provided a theoretical analysis, based on Piaget's assimilation/accommodation model, of the aspects of the environment which are likely to foster cognitive development. Lautrey (1980) extends this analysis and uses it as a basis for an empirical study. He suggests that an environment will be most favorable to cognitive development if it presents both of the following general characteristics:

(a) It produces perturbations

(b) It contains a certain regularity.

Both characteristics have to be present if re-equilibrations, i.e., new constructions, are to occur. Depending on which of these characteristics is predominant, Lautrey (1980) distinguishes three types of environments:

(1) Random environment: the first characteristic is predominant. Events occur randomly and cannot be predicted; they produce a disequilibrium, but this cannot be compensated because of the absence of any regularity.

(2) Rigid environment: characteristic 2 is predominant. The relationship between two events is always simple, and is never moderated by an event outside of this relationship. This type of environment presents the regularities necessary for cognitive constructions, but these are limited by the absence of perturbations.

(3) Flexible environment: both characteristics 1 and 2 are present. In this environment there are explicit or implicit rules which allow subjects to predict the consequences of their actions, but the relationship between two events is relative to other predictable events, which have to be taken into account.

Within the context of the daily life of French children with whom Lautrey's study was carried out, several common situations were analyzed according to this framework (e.g., rules about TV watching, table manners, week-end activities, permission to use scissors or matches, etc.) and these could easily be adapted to different cultural contexts. Lautrey finds that the three types of structurations are distributed differently according to social class; irrespective of social class there is a statistically significant relationship between type of structuration and several measures of intellectual development.[2]

My own interest in individual differences stems from attempts to interpret the significance of "time lags" and "asymptotes" in the development curves of various concrete-operational concepts obtained in various cultural groups. Linking these to eco-cultural demands (Dasen, 1975, 1977a), and showing, through

[2] These theoretical analyses will have to be refined in order to predict differential outcomes in various cognitive domains.

the use of training studies (Dasen, Lavallee, & Retschitzki, 1979; Dasen, Ngini, & Lavallee, 1979; Lavallee & Dasen, 1980), the extent to which the lags can be reduced or bridged and how a distinction between competence and performance can be made (Dasen, 1977b, in press), are steps toward this goal. It still remains to be explained why, in any one seemingly homogeneous cultural group, some children reach a given sub-stage for a given concrete-operational concept earlier than others, or why some reach the last sub-stage whereas others apparently don't. Cultural differences, at the level they have been tackled so far, cannot explain these individual differences.

In the search for a theory and methods to study the origins of individual differences in operational development, one obviously turns to Witkin's theory of psychological differentiation, which has been used extensively across cultures (Witkin & Berry, 1975; Okonji, in press). It has the advantage of being quite explicit on the origins, in early socialization and child-rearing practices, of differences in cognitive style (Goodenough & Witkin, 1977).

The relationships at the theoretical and empirical levels between the theories of Piaget and Witkin have been analyzed and reviewed by Huteau (1980). In both systems, development occurs from a less to a more differentiated state and the capacity to overcome the figurative components of a situation increases with age. Field-independent (FI) subjects, as compared to field-dependent (FD) subjects are more efficient on operational tasks when structuration-restructuration capacities are needed (conservation tasks, most spatial tasks and tasks in which several factors have to be dissociated) but not when these capacities seem less useful, such as in tasks concerning the logic of classes and relations, and some combinatory tasks.

The various theoretical avenues which have been briefly described provide the background to the rationale and design of a study carried out among Kikuyu children in Kenya (East Africa). Data collection has been completed, and the analysis is presently in progress.[3] An outline of the study is provided in Table 2.

[3] Field-work was carried out while the author was a Senior Research Fellow with the Bureau of Educational Research at the University of Nairobi; data analysis is carried out with a grant from the Fonds National Suisse de la Recherche Scientifique (grant no. 1.048.0.79).

Table 2
Outline of Study

BACKGROUND VARIABLES

Socio-economic status (1973, 1978)
Parents' age
Parents' education
Birth order
Child's education
etc.

BEHAVIOR OBSERVATIONS

(B. Whiting) 1973
Mother/child interactions
(siblings/child)
Child-rearing practices
Socialization
Cognitive ambience
Structure
etc.

SPOT OBSERVATIONS 1978

Work load, task complexity
Play, games
Distance from home
Social interactions
etc.

COGNITIVE MEASURES
OPERATIONAL DEVELOPMENT (PIAGET)

Space	*Classification*	*Conservation*
Orders	Class inclusion*	Liquids
Rotation	Reclassification (emic/etic)	
Horizontality*	Matrices	*with training

PSYCHOLOGICAL DIFFERENTIATION (WITKIN)
(Cognitive style; field-dependence/independence)

Children's Embedded Figures Test (CEFT)
African Embedded Figures Test (AEFT)
Portable Rod and Field Test (PRFT)
Kohs** (Pattern Matching from Queensland-Test)

PSYCHOMETRIC (QUEENSLAND TEST)

Knox Cube Imitation
Beads (memory)
Passalong
Form Assembly
Pattern Matching**

SCHOOL PERFORMANCE

Marks on district exams
Teachers' assessments

The sample consists of 64 schooled children aged 5 to 16 years, living in a semi-rural location about 30km from Nairobi, with approximately even numbers at each age-level and for each sex. These children and their families have been taking part in various research projects of the Child Development Research Unit (now Bureau of Educational Research) since 1969, and large amounts of background data are therefore available; furthermore, it is possible to take into account the change in these variables over the years, allowing for an assessment of social change.

Among the psychological data available in the files were behavior observations collected in 1973 under the direction of Whiting (Harvard University), at a time when the subjects of the present study were between 2 and 11 years. Kikuyu research assistants had been trained to record and code with a high degree of reliability all social interactions occurring during periods 15 minutes in length. Among the sample children, 46 had sufficiently detailed observations, their number varying between 1 and 11, with a mean of 5.6 (1 hour, 24 minutes), and a total of more than 4000 recorded interactions.

A number of categories or dimensions in the area of socialization and child-rearing practices were selected on the basis of Witkin's theory; others were inspired by previous cross-cultural studies by Munroe and Munroe (1971, personal communication), Nerlove et al. (1974), and Irwin et al. (1977), particularly those related to work load, task complexity, play and games, and distance from home. Still other categories were derived from intuitions regarding what Heron (1974) has called "cognitive ambience," environmental structuration, and other variables possibly linked to different aspects of cognitive development. Most of these dimensions were rated on 5-point scales after reviewing all the behavior observations available.[4]

A preliminary analysis of the distributions of these rankings shows that the child-rearing practices are not as homogeneous as an outside observer may have expected at first. For example, 11 mothers (of 45, the observation of one subject being insufficient to judge this category) prove to be authoritarian and ask to be obeyed immediately, whereas 16 are lenient and encourage their child to take some decisions him- or herself, 18 being ranked between these two extremes. A reading of the literature may have led one to expect all of these mothers to be rather authoritarian. Some child-rearing practices however, do appear to be stereotyped in this sample: for example, all mothers encourage their child to take responsibilities in the community, none use harsh punishment (at least during the time sampled), none encourage curiosity. These dimensions, on which there is insufficient variation for them to be used in a correlational study, are still interesting as a description of socialization practices and child-rearing styles. Some dimensions are predictive of field-independence rather than (as would be expected from the literature) field-dependence. It is obvious that some of the relationships between these variables and psychological differentiation cannot be carried over directly from the American context and have to be viewed more critically in the context of specific cultural values and practices.

While this quasi-longitudinal part of the study presents obvious advantages, I am also interested in linking the daily behavior of the children to different aspects of cognitive functioning. Thus a total of 1708 spot observations were carried out with 55 of the sample children.[5] The dimensions to be analyzed are similar to those of the behavior observations, although in a more limited range, with emphasis on work load, task complexity, play and games. Kikuyu children have to share in the upkeep of the family by performing numerous tasks such as child-minding, carrying water or firewood, cleaning, cooking, farming, herding, etc. The work load assigned to each child is variable, and depends partly on sex and birth order. Some children, for example the oldest girl in a family, are given work almost continuously, often without an explicit goal or an obvious sequencing of the chores. Performing these duties certainly entails responsibility, but the child has little independence and no time to structure activities him- or herself. At the opposite extreme, some children, for example younger boys, are hardly given any tasks and adults make no attempt to help them structure free-time activities. Like Lautrey's (1980) rigid and random environments, these extremes are likely to foster operational development less than in the intermediate situation in which the child is asked to take some responsibilities but is allowed time to play.

Listed on the right-hand side of Table 2 are the cognitive measures being used in this study. No attempt will be made to attribute a general stage or level to subjects; each task is treated separately, under three conceptual headings: space, classification, and conservation. The study is designed to relate performance on these tasks to all the variables on the left-hand side of the table, as well as to measures of psychological differentiation. These include the African Embedded Figures Test (AEFT) designed by van de Koppel for a study of Bantu and Pygmy populations in the Central African Republic. Instead of Kohs Blocks, which are sometimes taken as a measure of FDI, the Pattern Matching (PM) sub-test of the non-verbal Queensland-Test (QT) is used.

Using measures derived from major developmental

[4] This part of the study was carried out with the help of N. Darnton.

[5] This part of the study and the cognitive measures were carried out with the help of L. Ngini and M. Mberia.

theories does not ipso facto imply an uncritical acceptance of the universality of these theories. However the measures provide a set of hypotheses which ought to be tested in various cultural contexts. It may well be that this sampling of cognitive skills is not the most appropriate in every eco-cultural context, and these measures will have to be progressively complemented or replaced by better-adapted tools when these become available. Developmental theories are useful in cross-cultural research if they are placed in the perspective of the dialectics between their universality and their cultural relativity.

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What's Special about Experiments as Contexts for Thinking*

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I have been asked to write about experiments as special contexts for thinking. Experiments might be viewed as exceptional circumstances for problem solving and as unusual social occasions. A great deal has been said by psychologists about relations between laboratory experimentation and everyday activities. Many of the relevant caveats were presented by Wundt (1916). They have been restated, amplified, and added to by Brunswik (1955), Bartlett (1958), Barker (1968), Neisser (1976), Bronfenbrenner (1979), Cole, McDermott, and Hood (1978), to mention only a few appropriate references. To these discussions, I will add an example and a point of view.

The example I have chosen is from my own research among tribal tailors in Liberia. I gathered data on the tailor's uses of arithmetic in their daily routines in the tailor shop and in experimental situations and found that the problem-solving activities of the tailors look quite different in the two settings.

This example serves to illustrate my point of view. Most psychologists' critiques begin with experiments as the normative basis for describing thinking. They then end up treating everyday life as: (a) less demanding than the laboratory experiment (Bartlett, 1958; Case, 1978; Norman, 1975; etc.); or (b) unorganized and only given order by the organizing activity of the mind (this is Barker's (1968) characterization of "most psychologists' views"); or (c) simply, "the residual term which takes on specific meaning as it contrasts with the laboratory." (Cole, McDermott, and Hood, 1978, comment critically about the existing state of the art.) As an anthropologist I started out with an everyday scene as the primary source of information about how people use their heads, and have treated experiments as exotic and narrowly circumscribed events in the lives of the people studied. This point of view leads to questions about how experiments compare with other new situations that might arise in the tailors' mundane work lives.

To compare experiments with mundane social scenes requires a model of those features of everyday situations in tailor shops which might affect the methods tailors used to solve everyday arithmetic problems. I describe below a model of mundane situations and apply this model in a comparison of experiments and everyday situations in the tailor shops.

Background

The research on which this comparison is based stretched over a period of five years. I began by observing in tribal tailor shops, learning the production processes and other routines of tailoring, and studying how apprentice tailors learn their craft. This was followed by a series of experiments on transfer of training which compared the impact of apprenticeship and schooling on performance of more and less familiar tasks. There were two phases to this work. The first set of tasks incorporated problems taken directly from tailoring or school arithmetic. The circumstances surrounding the solving of these particular problems in experimental settings were similar to those found in the mundane setting: that is, the problems were ones the tailors routinely expected each other to solve without help from others. Such problems were viewed by the tailors as challenging previously acquired knowledge or skill. I then invented other, less familiar problems to contrast in specific ways with the problems known to be routine in the shop or school setting. The data for each tailor were analyzed for changes in performance across increasingly unfamiliar problems.

This analysis raised issues which could not be settled with the data from the first set of experiments. As Ginsburg (1977) has pointed out, it is important to compare data on problem-solving processes to draw conclusions about transfer. So on the second set of tasks protocols were collected. Fortunately, tailors learn one set of arithmetic procedures in the tailor shop and a different set in school. This makes it possible to often identify which method tailors were using on a given problem regardless of the setting in which they are solving the problems. The second round of experimentation also differed from the first

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in exploring more systematically the formal domain of arithmetic and possible dimensions of transfer of training, including numerical difficulty, mundane/exotic problem content, and ways of presenting problems which required different degrees of decoding work by the problem solver.

The first round of experiments used the tailors' everyday activities as a basis for constructing experimental tasks but did not explore the boundaries of everyday competence. The second round included systematically generated problems, sampled a formally generated problem space, and had the virtues of consistency and representativeness of a formal knowledge domain, but did not grow out of the everyday experiences of the tailors. In the first case it was relatively easy to specify relations between experimental and everyday tasks, but hard to account for relations between my experimental tasks and the tasks of more standard cognitive experiments; in the second round this set of circumstances was reversed.

The results (details in Lave, n.d.) may be summarized as follows: In the experimental situations, those who had learned arithmetic in school as well as in the shop used school-learned problem-solving techniques to proceed through the experimental task. Those who had learned arithmetic in the shop used what could be characterized as a maximum-effort version of shop arithmetic and only a subset of shop arithmetic strategies. Many of the maximum effort strategies appeared to be invented on the spot.

It appears from these results that the experimental situations were ill-specified ones for the tailors. But they individually filled in the gap between their understanding of the situation and mine. Some did so by reference to their problem-solving experiences in school, some by reference to the shop. Those who used their shop-learned skills as a model felt called upon to produce a version of those procedures which was never seen in the shop. They also omitted many techniques which they would have used in the shop.

After analysis of the experimental work, I was very curious as to how well the experimental data on problem-solving processes would generalize to mundane situations. Consequently, in a third round of fieldwork, I observed everyday arithmetic activity in the tailor shops. The results of this work could be summarized as follows: Those who learn arithmetic in the shop use a rich and varied, and a stream-lined version of this arithmetic in their work lives in the shop. Those who learn arithmetic in both shop and school (and used school math in the experimental setting), use shop arithmetic in the shop on a day-to-day basis.

Problem

It would certainly be useful to tackle the question of why mundane shop problems and experiments "pulled" such different kinds of behavior from the tailors. What features of everyday life in the tailor shop make it a special context for thinking and account for the special kinds of arithmetic strategies employed there by all of the tailors? Are there differences between critical features of everyday situations and experimental ones which help to account for changes in strategies from one situation to the other?

The Model of Everyday Problem-Solving Situations

It may be helpful to simply state the main features of the model of everyday arithmetic problem-solving situations. "Situation" as it will be used here includes crucial features of both inner and outer environments of the problem solver, as each shapes the other. Experimental and everyday situations can be compared on these features, using the data on Liberian tailors.

The outer environment: Firstly, in the tailors' lives, certain kinds of arithmetic problems routinely reoccur. Secondly, problem solving often occurs in the context of social interaction or is at least vulnerable to social demands, most of which have higher priority than math. Thirdly, arithmetic problem solving is almost never an end in itself. It is instead an instrumental activity, undertaken in order to arrive at a wide variety of higher order goals. Finally, it takes place in an environment rich with information for the particular problems which are frequently encountered.

The inner environment: Arithmetic problem solving makes heavy demands on attentional resources; it is effortful. Most arithmetic problems can be solved quickly if all the required information is present, although this condition is not often met.

Comparing Mundane and Experimental Problem-Solving Circumstances

The outer environment: The first issue is that of routine reoccurrence. Given the repeated occurrence of arithmetic problems in daily life, it should not be surprising that tailors show little difficulty representing problems to themselves. What is problematical in everyday circumstances becomes the input for these problems. Even the information rich environment of the tailor shop is sometimes not rich enough to permit a tailor to solve a problem at the time he recognizes that it exists. Both the reoccurring nature of problems, and potential difficulties in obtaining new inputs, help to explain why procedures for solving arithmetic problems in the shop very often focus on relations between old and new instances of the same problem.

All of these features of everyday problem solving stand in contrast with the problem-solving tasks presented in an experimental context. One goal in choosing the problems for the experiments was to make at least some of them unfamiliar to all subjects. If the experimenter were successful, any strategy which involved comparing old and new versions of the same

problem would be unavailable to the subject. Furthermore, the experiment, as a situation, is a one-of-a-kind occasion. This is not a situation in which it could be said that problems routinely reoccur. Everyday strategies which take advantage of routine reoccurrence will not be effective in the experimental situation. Since there is little time for adaptation of methods during an experiment, experiments are always "learning transfer" situations. Learning transfer is a relatively rare occurrence in everyday life.

Second, the outer environment is peopled; social interaction has very high priority in the tailors' lives. Instrumental activities are lower in a goal hierarchy and require social management in order to compete for resources of attention. Very often in the shop the tailors handle this problem with a fluid, shifting division of labor. A tailor dealing with a customer passes the measuring or other figuring along to some other tailor who solves it and gives him the answer while the first tailor continues to attend to the customer. Checking problem solutions, which in addition to objective results provides reassurance that calculation was properly done, are often social, done in parallel by two or more. (For a similar finding see Kreutzer, Leonard, and Flavell, 1975). All of this contrasts with experimental circumstances in which problem solving is assumed to be an exclusive engagement between a person and the problem. Social strategies are not permitted.

Third, arithmetic problem-solving, like most of the cognitive procedures which are the target of experimental investigation, is a low-level means employed in everyday life in the service of a wide variety of higher-order goals. In an experimental setting where math problem-solving procedures are the topic of investigation, "solving math problems correctly" is the highest order goal made explicit in the situation. Defining tasks through the practice of "giving instructions" ignores the customarily embedded, instrumental nature of arithmetic activity. More important, it often leads to expectations on the part of the experimenter about what constitutes appropriate (i.e., elaborate, high effort) problem-solving procedures. The same expectations would not be appropriate for problem solving seen merely as an instrumental activity.

The means/end relationship between problem-solving goals and problem-solving procedures has a number of implications. First of all, in the everyday setting in which arithmetic is (only) instrumental, minimizing attention allocated to math makes sense. In experiments, in which solving the problem correctly is a major goal, it makes sense to maximize efforts at problem solving. This is certainly what I observed the tailors doing. Once again the contrast between the two sets of circumstances suggests that procedures appropriate in either one are not appropriate in the other.

Everyday strategies for solving problems include ones which violate many of the usual experimental constraints. In everyday circumstances, standard techniques include simplifying problems, delegating problem-solving work, and rejecting problems. More importantly, it is often useful to compare old and new inputs to a reoccurring problem, note the difference between them, and make a decision vis à vis the higher order goal rather than solve the arithmetic problem (e.g., the eggs are 30 cents higher this week. That's too much. We'll get them somewhere else). This contrasts with the assumption in an experiment that the task must remain fixed; that procedures which involve reframing the task are not permitted.

Higher-order goals in everyday problem solving also vary the precision constraints on the problem solutions. Because of the instrumental nature of arithmetic and other demands on attention, it makes sense to pay attention to precision constraints. In general people solve problems no more precisely than necessary to meet the higher-order goal for which they are calculating. Attending to precision constraints is a skill of everyday arithmetic that does not much come into play in experimental situations, since solving math problems is the goal. Perhaps the tailors have a default position: Under ill-specified precision constraints and minimal other demands for attention, be as precise as possible. This would help to account for the maximum-effort arithmetic procedures used on the experimental math tasks.

One further implication of the instrumental uses of arithmetic in everyday life has been touched on at several earlier points. Usually the higher-order goals are well enough defined in everyday situations to provide adequate information about precision constraints, error cost and so on. In experiments the goal may seem well specified: "I want you to solve some arithmetic problems." But this takes into account only the instrumental level of the problem-solving activity and not the crucial function of higher-order goals in determining appropriate problem-solving procedures. Viewed in comparison with a higher-order everyday goal, e.g., "getting groceries," goals which would provide comparable precision constraints in experiments are not clear. This confusion may be a serious problem with many experimentally defined tasks.

Inner environment: No matter what the circumstances, mental calculation is effortful and requires heavy attentional resources. It is also a rapid process (most often less than a minute) if all needed information is at hand and if there are not competing demands for attention. At the same time calculation is slow enough to disrupt conversation. All of this applies in experimental settings as well as in everyday settings.

In everyday settings, however, it may take days to solve a given arithmetic problem. Problem solving is subject to interruption and also to absence of information. The contrast between customary speed when problem solving is in progress, and the enormously

greater time periods which are often encountered creates difficulties in "problem management," (e.g., holding onto whatever inputs are available, and the problem representation, seeking additional inputs, pushing to assemble them all at once, or storing some and waiting, etc.). These problems are not generally addressed in assessing math skills in experimental settings. In experiments inputs are given and it is generally possible to solve difficult problem-representation circumstances and relatively easier input acquisition circumstances than everyday life provides.

I have not previously mentioned the impact on problem-solving strategies of experience over time in some environments. Change in strategy over time arises as a function of interaction between outer and inner environments. It seems likely that methods used in solving problems (e.g., memorization or interpolation or re-calculation, etc.) are chosen partly in response to experience with the frequency of reoccurrence of different problems in the environment along with the simplest possible extrapolation to the future ("what has happened in the past is what I expect in the future"). (Kaheman, 1973, discusses some implications of this point.)

But tasks and problem-solving methods in experiments have unspecified relations to the extensional domain[1] of everyday life. Experimental tasks are typically selected from domains which bear no specified relationship with everyday tasks and problems. Certainly they are not carefully constructed samples of problems with different (known) frequencies in the domain of actually occurring problems.

Discussion

It could be argued that an important measure of peoples' problem-solving skills is what happens when they are asked to solve new problems in new circumstances. In this frame of reference experiments make sense as a tool for investigation, since experiments present new problems in a new situation. But if this argument is taken seriously it changes the appropriate comparison to make to everyday situations. The appropriate comparison might be other *new* problems which arise in mundane settings, rather than *routine* problems in mundane situations.

One example of a new problem in a mundane setting occurred in a tailor shop. A man came into the shop one day and requested that a tailor make a set of burial clothes. None of the tailors in this shop had made burial clothes before. But all present felt the customer had come to the right place to get a solution to his problem. Bargaining, sewing, the setting, different kinds of clothes, are all familiar. Only the specific item to be made was new, and it could be compared to other closely related types of garments. In short, people's experiences with new situations in everyday lives tend to be a good deal more like previous experiences in everyday situations than are experiments. It is possible to suggest several ways in which the circumstances of problem solving in new situations are quite different when experiments and other new situations are compared.

Experiments gain much of their power as tools for investigating cognition from the fact that they are simpler situations than the typical everyday experiences of most subjects. On the one hand, the non-negotiable definition of tasks, the complete presentation of specific tasks is simpler than the fuzzy, often incomplete, unfolding nature of tasks in everyday situations (Cole, McDermott, & Hood, 1978). On the other hand, experiments lack specification of higher-order goals which routinely guide the choice of problem-solving method in everyday situations, including new ones. For instance, the burial clothes were extremely simple and also voluminous. No one measured the "customer" and precision constraints on fit were extremely broad, under the circumstances. Yet the goal was there, "make loose-fitting garment x," at the same level as usual, routinely translatable in its impact on sub-portions of the task.

Experiments constitute ill-specified new situations in other, more complex ways. For instance, neither the experimenter nor the subject is likely to know how the situation is related to previous situations in which the subject has been routinely involved. Neither is the experimenter likely to investigate differences between previous problem-solving experiences and activities in the experimental setting. And there is unlikely to be a clear understanding of differences between the distribution of problems-to-solve routinely encountered by subjects, and the experimental tasks as samples from that or some other domain of problems. In the example of the burial clothes, the situation was a slight variant on routinely occurring ones. Previous problems and previous experience solving problems were clearly specified. This was not the case in my experiments.

Experimental situations also differ from other new situations, in the timing of performance demands. In everyday life one would rarely be called on to perform immediately in a new, or ill-specified situation, until one understood "what's going on." Thus, no one in the shop thought of asking an inexperienced apprentice to make the burial clothes, even though several were available, and skilled enough. Only highly experienced masters talked it over and decided on one of their number.

A third way in which experiments differ from most other new problem-solving situations is in the degree of consistency of certain major features of the situation over a series of routine reoccurrences. Experiments often arbitrarily change features of the situation

[1] The "extensional domain" of arithmetic problems is the set of actually occurring problems in a given situation.

in ways that mundane new situations rarely if ever impose. This is especially true for (a) social circumstances of performance, and (b) means/goals status of the problem-solving procedures under study. Some tasks have a strong social component, others do not. But in everyday life the social features of a daily activity are very likely to remain constant across numerous reoccurrences. Arbitrary change in the social and physical matrix of an activity is not common. It does happen from time to time — occasionally we cook in someone else's kitchen or go grocery shopping with a friend — with predictable performance difficulties. Experiments, unfortunately, very often create this arbitrary change in the social conditions of activity. It is also rare in everyday life that a task which was an end in itself in one setting becomes instrumental in relation to some other end in another mundane context. In everyday situations where this does happen there are very likely to be strong signals to the actor, including clear specification of higher-order goals where appropriate. *Most of the cognitive skills typically addressed in experiments move from instrumental to goal status as they move from everyday situation into experimental ones.* The math activities described earlier are a good example. But memory experiments, perception, logic problems, and most other foci of heavy experimentation suffer from the same arbitrary change. This may help to explain why it is difficult to "see" cognitive skills in everyday settings, a problem emphasized in Cole, McDermott, and Hood.

If the propositions above are acceptable (that both social circumstances and means/goal status are often changed when transported into an experimental context), then a point made earlier becomes even more important. In everyday situations where there are newcomers or novices, there are almost certain to be provisions for induction, temporary peripheral participation, or at least dramatic signals to flag shifts in social or means/goals circumstances. There is likely to be social support for identifying the out-of-the-ordinary features of the situation and adapting to them. Experimental situations seem atypical situations in being impoverished in the social circumstances which lead people to make rapid and successful adaptations in new mundane situations.

Conclusion

If conventional experiments do not masquerade well as "new mundane situations," is there any hope for generalizing from experimental to everyday situations? Actually, the question is an experiment-centric one. It may profitably be revised to, "Is there any hope that we may learn from contrasting performances in contrasting situations?" From my own experience working in Liberia, I would answer in the affirmative. I disagree, however, with the argument set forth in Cole, McDermott, and Hood, about the nature of appropriate generalization. It is argued there (p. 15 and elsewhere) that "the experiment should be treated as a simulation of the properties of the scenes to which we want to generalize." But if any critical features of experiments cum situations contrast with basic features of mundane situations, an ecologically valid simulation of everyday situations is not possible. If context and performance interact, there are almost certainly important features of the situation which won't agree between experiment and mundane circumstances.

It is possible, however, to make predictions about expected *differences* in performances across contexts, given a careful description and analysis of the differences in problem-solving circumstances in some specific mundane setting(s) and in an experimental one. By trying to understand an experiment as an actual experience in the lives of subjects, by focusing on how the circumstances it presents *differ* from those of routine situations, and by successfully predicting performance differences in the separate contexts, theories (rather than experimental results) can become general without automatically becoming invalid at the same time.

Secondly, the notion that rigorous proof of particular kinds of cognitive processing can *only* come from experimental manipulation seems too narrow. If you understand the social organization of a commercial dairy and the division of labor within it, you should be able, like Scribner, to predict who will be good at one kind of arithmetic but not another, and who will solve customer order problems in terms of pints and quarts, and who in terms of cases and half cases. De la Rocha (personal communication, 1980) predicts from a three-stage model of Weight Watchers curriculum, who will carry out new calculations about food servings in one way rather than another; Murtaugh (1980)[2] predicts on the basis of the functional role of a particular food in a person's food management system whether the person will calculate before buying that item in the grocery store. Confining theory testing or theory development to experiments is an excessive limitation on sources of knowledge, and grows out of the model which specifies that the goal of experimentation is to produce a literal reproduction of the target behavior under study. But indirect evidence abounds, including data on the social structure, data on what people do *not* do under certain circumstances, data on what kinds of mental effort people avoid through the use of external inventions or social skills. These can shed light on problem-solving processes with reasonable rigor. Producing rigorous indirect evidence, rather than literally

[2] Proposed research: A Hierarchical Decision Model of American Grocery Shopping.

reproducing target behavior, is a useful goal for at least some new exploration of cognitive processes.

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Jackson, W., & Espino, L. *Cultural Antecedents of Cognitive Style Variables in Mexican American Children.* Paper presented at Early Childhood Education Forum: A Bilingual Perspective. University of Texas at Austin, August 2, 1980.

Using a rather unique methodological approach, which they label "predictive ethnography," the authors collected data on seven broad cultural variables in an attempt to predict the probable scores of ten Mexican American third graders on two measures of cognitive style. The two measures were the Children's Embedded Figures Test (Field dependence/independence) and the Matching Familiar Figures Test (Conceptual Tempo). Other studies have attempted to document the relationship between Chicano socialization practices and subsequent cognitive styles but none have used ethnographic methods as a basis for predicting performance on specific measures of cognitive style.

As part of a three year study on cognitive styles among Chicanos, the authors collected information on practices and behaviors associated with such ethnographic variables as language usage, early childhood experiences, child rearing patterns, values and beliefs, kinship patterns and socioeconomic status. The authors did not, however, attempt to tie specific cognitive style categories to specific cultural experiences before the ethnographic data were collected. Rather, their predictions and substantiation of those predictions concerning a student's probable style were based on "ethnographic judgements, examining cultural experience and behavior patterns as they were expressed, without preconceived categories (pp. 13-14)." The authors admit to the probability of having their judgement influenced by previous knowledge concerning possible linkages between cognitive styles and culture, but the possibility of such biases exists in any research and is not a major concern in this case. Even were this bias a more serious concern, that would not deter from the fact that this is an excellent first attempt at research that seriously seeks to tie specific, observed sociocultural practices and behaviors to an individual's actual score on measures of field independence and conceptual tempo.

In presenting their findings the authors provide a brief sketch of each child and then list the cultural antecedents (derived from the ethnographic data) thought to support their respective predictions (made blind). The accuracy of their predictions ranged from low to moderate with a 70% and 50% accuracy rate for each respective author on the field independence measure and 60%/50% (time), 60%/30% (errors) on the conceptual tempo scores. This was not their most striking result. Of the 23 cultural and behavioral variables used to predict field independence only three showed even a weak predictive power. Only one of 18 predicted to conceptual tempo. The authors also found a high degree of intra-cultural diversity among the ten subjects which was quite surprising since they were all from the same small community and attended the same school. They note that "of the more than 100 cultural experiences and behavior patterns evidenced in the life history data, only three were found to characterize all ten children (p. 34)." These findings lead the authors to warn that "researchers or educators who ascribe any characteristic to the Mexican American culture need to exercise caution, and realize that differences in socialization practices language usage patterns, kinship systems, values and beliefs, and interaction styles probably exist within any cultural group, even within the same community (p. 35)." They add, further, that extreme caution is warranted when psychological constructs such as cognitive styles are used for educational [and research] purposes.

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Snyder, Lynn S. *Pragmatics in Language Disabled Children: Their Prelinguistic and Early Verbal Performatives,* Unpublished Dissertation, University of Colorado, 1975. Available through University Microfilms International, #76-11,610.

This unpublished dissertation examines the use of language and gesture in 30 beginning speakers of normal and delayed ages. Matching her subject groups on MLU (m = 1.03) and socio-economic status, and screening for normal development on other than language abilities, Dr. Snyder found differences in the groups' likelihood of using language to achieve communication, that is, to function pragmatically. Children were placed in contexts where the experimenter knew they would be likely to use declarative and imperative communicators and where the elements in the context to which the child was attending could be measured.

Results showed that language-disabled subjects relied less on linguistic performatives to communicate:

in situations where declaratives were appropriate they tended not to respond at all or to use sensorimotor modes; for imperatives, they were most likely to use sensorimotor means of communication. Snyder too found differences in their responses to aspects of stimulus events. Language disabled children responded to less informative elements in a context more often than normal children did, suggesting divergence in the presuppositions driving the illocutionary aspects of their early utterances. Particularly, responses showed encoding of more concrete contextual relationships.

This work gives us evidence that information input for language disabled children is offset by the time speech begins and that their representational responses to the environment differ from normals'. We now need to know the effects those responses have on those who organize learning contexts because they, in turn, modify input further and become partners in constructing prosthetic environments.

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