

# Cross-Cultural Psychology's Challenges to Our Ideas of Children and Development

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The earliest cross-cultural psychological expedition was organized around the turn of the century by a group of social and medical scientists from Cambridge University. The efforts of the Torres Straits expedition are referenced in most of the growing number of books devoted to cross-cultural psychology, yet the influence of Thorndike's cat in a puzzle box, Freud's psychoanalytic cases, Binet's work on school aptitude tests, Broca's aphasia patient, and other celebrated work in psychology seems to render the results of the Torres Straits expedition little more than a historical curiosity.

The subsequent history of cross-cultural research has not in fact been particularly noteworthy for its contributions to research on child development. Nevertheless, the lure of cross-cultural studies continues. If we journey to the Torres Straits or West Africa or southern Yucatan, we will find striking contrasts in the conditions children experience while growing up. We will, in the textbook phrase, "increase the range of our independent variables." In this way, it is hoped that hypotheses about the universality of general psychological principles can be tested and the consequences of particular environmental conditions for development discovered.

To be sure, this direction of research has not been totally unproductive. To a limited degree, we have been able to use different cultures as settings for research into important issues of child development: the impact of malnutrition on physical and cognitive growth and the contribution of formal schooling to mental development, for example.

But for the most part, cross-cultural research has failed in its ambition to increase the generality of psychological laws. It has proved an untrustworthy tool for unraveling the twisted knot of variables affecting growth. Its clearest lesson is that variables of interest to developmental psy-

chologists are rarely, if ever, independent. Such variables instead present themselves as interconnected elements of social, physical, and cultural reality (Whiting, 1976). It is for this reason that the study of environmental influences on perception by Segall, Campbell, and Herskovitz (1966) was undermined. Their unusually careful study confounded cultural factors and physical characteristics of the populations studied (Bornstein, 1975). In a similar fashion, research attempting to separate maturational and educational influences has been weakened by problems of subject selection and a host of factors that covary among populations even in the most auspicious circumstances (Sharp, Cole, & Lave, 1979).

The complexity of the interactions among social, cultural, and cognitive variables has by no means halted cross-cultural research. Rather, there has been an enormous increase in the use of multivariate techniques intended to control by machine that which cannot easily be manipulated directly. Indeed, judging from the rapid growth in the number of journal articles, monographs, and textbooks in the past decade, the hopes of those who journeyed by steamboat to the Torres Straits are still alive and well.

Another reason for psychology's indifference to cross-cultural work can be traced to dependent variables, doubts about the validity of the tests and measures that constitute the hard data of the cross-cultural effort. So, for example, if Aus-

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tralian aborigines fail a Piagetian conservation test, or Central Asian peasants draw conclusions from logical syllogisms on the basis of their concrete experiences, overriding the logic of the problem, or Mexican peasants fail to discover the multiplicity of ways that a set of eight colored geometric figures can be arranged in exhaustive classes, there is the lurking suspicion that the instructions may have been poorly understood, the stimuli meaningless, the subjects bored or frightened. In short, rather than indicating crucial differences in psychological makeup, such research may do little more than rediscover problems in the conduct of developmental research. These problems of method can easily be found without going to the trouble and expense of traveling abroad.

Despite its weaknesses, cross-cultural psychology is mounting a challenge to traditional developmental research. As paradoxical as it may seem, we believe that the broad and growing recognition of cross-cultural psychology's weaknesses may be the central source of this challenge.

Although the two enterprises differ in some of their theoretical goals, they are each engaged in comparing the performances of different groups: in one case, people of different cultures; in the other, children of different ages. The two enterprises share methodological difficulties. First of all, in either cross-cultural or developmental research, subjects from one group may not understand the materials, motives, or procedures demanded by the experimental setting in the same way as subjects from the comparison group. Second, the tasks which are chosen as samples of performance or ability and which are appropriate to one group may fail to tap abilities that are actually present in the other group.

In the field of child development, problems of this sort are being investigated with increasing frequency. In laboratory communication tasks, for example, young children apparently are unable to take into account the information needs of the listener. The results in this case could easily be interpreted as showing an absence of communication skills. But children of the same age, in more naturalistic settings, adapt their messages to the needs of those with whom they are interacting (Shatz, 1978; Shatz & Gelman, 1973). Analogously, in her studies of the drawing skills of Honduran children, Bernbaum (1974) has shown that unschooled children do not display an age-graded sequence when faced with paper-and-pencil tasks but do show such a sequence when

allowed to trace the same figures with a stick in the sand. As in the case of communication skills, the alternative form of the task provided means for displaying skills that were not manifested in the children's performance on the original task. A host of such examples may be found in Gelman's (1978) review of cognitive developmental research.

The similarity of paradoxes encountered in developmental and cross-cultural research invites us to analyze in more detail the problems as they arise in standard cross-cultural research. Such an analysis can identify the sources of problems in both fields and point to productive solutions. We need not bring out a new line of research to illustrate our point. The research carried out in the Torres Straits can serve admirably.

One delegation member, W. H. R. Rivers, conducted one of the expedition's basic experiments, designed to contrast visual acuity among the natives with that among the European samples he had studied for many years. Using a standard laboratory device for measuring visual acuity (conceived as the ability to perceive a small gap between two lines), Rivers the psychologist found only a small difference favoring his South Pacific subjects. But Rivers the ethnographer spent time with his subjects when they were not being tested. On one occasion he was astonished when his companion, gazing across a valley into the forest, spotted an interesting animal. Try as he might, Rivers could not see the object to which the man pointed. If the two settings were considered measures of the same ability, this result would represent a serious paradox. The explanation Rivers (1901) offered for the native's "indifferent" visual acuity in one setting contrasted with seemingly excellent acuity in another setting seems as plausible today as it was 75 years ago:

The usual accounts of natives' astounding acuity are attributed to the "power of observation," depending on the habit of attending to and discriminating any minute indications which are given by the organ of sense. Another fact which travellers have usually omitted to take into account is that the observations which have been held to show extraordinary sense acuity have been made in surroundings with which the savage is extremely familiar. A feat, which to the outsider may appear to depend on a marvellous degree of acuteness of vision, may depend merely on a correct inference founded on special knowledge. (p. 12)

Rivers made an appeal to plausible factors not in evidence—"special knowledge"—to explain the native's performance in the second setting.

In effect, Rivers was making asymmetrical assumptions about the validity and cultural significance of his two observational settings and, in so doing, donned the filtering spectacles of cross-cultural psychology through which important paradoxes rarely pass. He had assumed the experiment to be culturally neutral.

If cross-cultural psychology has mounted a challenge to developmental psychology in recent years it is because it has forced recognition of the fact that *no context of observation, despite the care taken in its construction, is culturally neutral. Settings for behavior are socially organized, and they are embedded in larger systems of social organization which influence them.* Membership in the society that organizes the observational setting provides participants with "special knowledge."

Whether the setting is a laboratory or a forest, participants in any setting of observation use cultural knowledge to make sense of the task and to organize their behavior in it. We believe that Rivers may well have been right in attributing the Torres Straits native's superior performance in the forest task to "special knowledge," but an explanation of that performance requires a specification of what that special knowledge was and how it was applied. Further, Rivers ignored the possibility that Cambridge subjects may have brought special knowledge to the laboratory acuity task. Such knowledge must be included to complete his argument.

The assumption that behavior in such laboratory settings is culturally transparent was criticized by Boas (1911) many years ago:

The existence of a mind absolutely independent of conditions of life is unthinkable. Experimental psychology, in its earlier stages, was sterile because it operated with the theory of the existence of an absolute mind, not subject to the environmental setting in which it lives. (p. 133)

In subsequent years, psychologists have failed to take the implications of this statement seriously. To do so would entail recognizing that laboratory tasks cannot stand alone as a privileged means for discovering psychological principles. We recommend taking seriously the fact that laboratory tasks—like walks in the forest, or drawing in sand—are examples of socially embedded settings for behaving. The interpretation of behavior requires an understanding of both the setting itself and its relation to the larger social context. This assertion does not imply that laboratory tasks are useless instruments for the study of either cross-cultural or developmental phenomena. Instead, it

implies that they are problematic instruments, the properties of which must be studied as part of the research enterprise.

Even at the outset we can see cogent arguments for according laboratory tests of cognition a preferred status for certain theoretical purposes. First and foremost, such tests are designed to support inferences about individual-environment interactions of the sort that we label psychological processes. Whenever our concern is to compare models of how individuals might process information in particular contexts, well-designed cognitive tasks are likely to be the tool of choice. We cannot assume, however, that they are uniformly and systematically useful for reasoning about the behavior of individuals in other contexts.

The high level of privilege afforded experimental methods in psychology is increasingly questioned in cross-cultural psychology. Laboratory-based experiments, and standard ability tests modeled after them, have often been used for reasons extraneous to the explanation of psychological processes in constrained domains (e.g., to select children for inclusion in special academic programs). This practice is likely to continue because performance in such settings correlates with school performance, which in turn is one of the most powerful proximal predictors of occupational attainment the research world has yet discovered. This fact, however, only defines problems for analysis and in no way constitutes a theoretical warrant for using experiments as a uniformly reliable method of inquiry into the development of psychological skills in various cultures.

Explicit recognition that educational evaluation and sorting for occupations are major reasons for valuing high performance on psychological tests of ability helps explain the intentional or implicit reliance in so much cross-cultural research on the school graduate's behavior as the model of successful performance. Such recognition also sensitizes us to the ever present danger of substituting the product of behavior for descriptions of process (Werner, 1937). Further, though a good deal of research indicates that schooling may produce highly valued outcomes for a small percentage of the world's population, schooling is not the only medium of expression for intellectual achievement. Are a majority of the world's people to be treated as a residual category simply because we have no accepted techniques to study the way they use their minds in the contexts that make a difference to them?

This subversive question leads to another that we hear from time to time: How can it be that uneducated people (in Third World countries) or poorly educated people (in technologically advanced countries) seem incompetent at school but generally competent at home and at work? We can speculate at length about the answer to this question (indeed, many have). But a scientific approach requires that we have at our disposal tools for specifying such people's competencies and activities outside of school. Goodenough (1936) noted the central limitation of our most common tools:

Now the fact can hardly be too strongly emphasized that neither intelligence tests nor the so-called tests of personality and character are measuring devices, properly speaking. They are sampling devices. (p. 163)

Because our standard tests are deliberate samples of the tasks and behaviors necessary for success in schools, they are likely to be inappropriate as samples of the skills actually to be found in cultures where schools are not indigenous or, so far as we know, of the skills we use outside of school settings.

Our cross-cultural research leads us inexorably away from the contexts we know how to analyze into domains we have only begun to understand; to deepen our understanding we have to take non-school contexts for behavior seriously and begin to view school (and the psychological tasks that act as its proxy for analytic purposes) as part of a larger set of contexts for behaving. The more seriously we engage this undertaking, the more sharply we are made aware of the trouble we are in. It turns out that once we move beyond the highly constrained confines of our laboratory tasks and standardized tests, not only do we lose the technology for making statements about psychological process, we also lose the framework within which we are accustomed to *describe* intellectual behavior. This danger is brought into sharp relief by recent research on the cognitive consequences of education.

A large number of investigators (Cole, Gay, Glick, & Sharp, 1971; Greenfield, 1966; Sharp et al., 1979; Stevenson, Parker, Wilkinson, Bonnevaux, & Gonzalez, 1978) have recently demonstrated that schooling enhances cognitive performance on a wide range of developmentally sensitive tasks; indeed, to some people it seems that cognitive-developmental research in the United States has been measuring *years of schooling*, using *age*

as its proxy variable. From the perspective of those interested in accounting for age/school-related increases in cognitive performance, this work fulfills the traditional role of cross-cultural research; it unpackages the independent variables. But the implications of this work for those who have not been to school are completely unclear. Are we to conclude that *memory* or *reasoning* doesn't develop because performance in the absence of schooling does not improve on our tests? We have resisted this general conclusion because memory and reasoning are defined by the tasks that we employ to specify them. Consider what must be done to confirm or refute such conclusions. We should look for settings, sufficiently structured to be analyzable by known techniques, that are equally experienced by both schooled and non-schooled populations. But where would we find people engaging in syllogistic reasoning in which the empirical content of the problem is irrelevant? Or what about taxonomic classification, the purpose of which is to discover all of the possible bases for categorizing a set of pictures?

In an attempt to address this problem, new lines of research are being developed which promise a deeper understanding of both our traditional contexts of evaluation and the far broader range of tasks of which they are a subset.

One useful approach has taken as its starting point domains of knowledge which a priori analysis suggests are important across many settings and which have a logical structure that can frame the analysis. A naive beginning in this direction is represented in the work of Gay and Cole (1967), who explored the way in which mathematical knowledge is used by a Liberian farming people.

They found that although their subjects did not have a general grasp of mathematics as a comprehensive systematic body of knowledge, in areas where it affects their well-being, Liberian farmers had worked out systematic measurement and calculational systems that they used with great expertise. In recent work, Ginsburg (1978) and his students have greatly extended this line of investigation by using a more sophisticated taxonomy of mathematical knowledge and deeper analysis of mathematical usage in neighboring but contrasting cultural groups. Similarly, Lave (1977) and Posner (1978) have used the structure of differing number systems to study everyday mathematical reasoning. Reed and Lave (in press) used the formally analyzed and systematic domain of arithmetic to look at the impact of education on

cognitive skills among Vai and Gola tailors in Liberia. After observation of the arithmetic skills learned and practiced both in the traditional apprenticeship system and in school, tasks were developed and tested with tailors who had varying exposure to apprenticeship and school systems. By analyzing the errors on specially designed problems, a mathematician not present in Liberia but familiar with the different numerical systems used in traditional and school settings was able to identify accurately those tailors relying on the apprenticeship system and those relying on methods learned in school. In another application of a formal system, Lewis (1972) used star charts to study indigenous Polynesian navigation. The cross-cultural study of well-structured domains of knowledge is on solid ground because a theory of important aspects of the task is highly formalized in advance, and claims are not extended beyond the range of the theory.

A second approach, characteristic of cognitive anthropology, uses tasks found in naturally occurring, everyday situations. (For a review of this field from the perspective of the current authors, see the article by the Laboratory of Comparative Human Cognition, 1978.) A model of the structure of the task is constructed in terms that map the native informant's conceptions. These conceptions, if described with sufficient formal rigor, become useful domains for comparative analysis. Thus, for example, Quinn (1976) described the decision making involved in court cases, as well as the processes involved in deciding where to sell one's fish (Quinn, 1978), and Hutchins (1979) presented a formal model of the calculus used by Trobriand Islanders for settling land disputes. This work is important because it suggests approaches to domains of knowledge that do not seem at first to be amenable to description. These investigations take on added strength because the topics studied are central to the lives of the people involved, even though these people have never formalized their knowledge, have never attended school, and would probably do poorly on standardized tests.

A different approach to structuring observations has focused on "locating" experiments in the societies in which they are employed (Scribner, 1976). This work acknowledges the special strengths of laboratory tasks as *tools* for specifying psychological processes, and it recognizes the limitations of such tasks as samples of the *contexts* for displaying these processes. One such approach

begins with standard laboratory tasks. After analyzing the cognitive demands of these tasks, the researcher moves out to people's everyday lives, seeking contexts in which similar tasks might be required of people. Examples of this approach include the work by Cole et al. (1971) on memory and that by Scribner (1975) on syllogistic reasoning. But this approach, we feel, is limited because the structure of such tasks is so closely tied to schooling that it may prove difficult or impossible to find extracurricular contexts in which the conditions modeled in the experiment occur.

Another facet of our enterprise, represented in the work of Scribner and Cole (1978), reverses the laboratory task - everyday life direction on a broader scale than did the early effort by Gay and Cole (1967). Scribner and Cole's study of literacy among the Vai of Liberia centered on the requirements of reading and writing *as practiced among the Vai*; only after the indigenous task had been analyzed were experiments devised to illuminate the nature and breadth of the skills that indigenous practice entailed. This approach, though retaining laboratory methods, has much in common with the work of the cognitive anthropologists.

Finally, we have begun to explore methods that do not directly involve experimentation but that retain a cognitive psychological framework to explore everyday contexts for thinking (including schoolrooms). Working with a single group of children across a wide variety of contexts, Cole, Hood, and McDermott (in press) sought to identify for each individual child the distribution of cognitive tasks and responses to those tasks. They became acutely aware that all of the scenes were interactional accomplishments in which each participant helped to shape the stimulus environment for all other participants. They discovered that testlike activities could be identified outside of tests, but only to a limited degree; because the accomplishment of these activities requires a great deal of social organizational work, establishing the generality of psychological principles means identifying those contexts requiring similar social organizational work, to ensure that "the same task" occurs in various settings. Since tasks in everyday life are frequently performed by more than one person, ways of jointly accomplishing cognitive tasks and the consequences of different participant structures need to be examined.

These new programs, still in their infancy, seem to us an absolute requirement for development of

a theory of culture and behavior and a valuable approach to overcoming the shortcomings and paradoxes posed by current procedures for the study of children's lives. Comparisons of different groups using existing procedures are only *conditionally valid*—conditional upon school-type tasks as the criterion of worthwhile performance, whether the groups involved are children of different ages or adults of different social classes or educational levels.

In our opinion, current debates about laboratory versus field approaches to the study of behavior have outlived their usefulness. It is no longer a matter of choosing one kind of method or the other. Whatever other disagreements psychologists have, they all agree that the specification of psychological processes represents a specification of environment-organism interactions. In order to exploit the potential of cross-cultural research we must vastly increase our power to describe environments for behaving; *culture* or *schooling* as surrogates for environmental variation are too often vitiated by a reliance on a small set of "microenvironments" (e.g., those represented by laboratory devices). The goal of observations of children outside laboratory settings is to increase the range of contexts to which we can legitimately generalize. To reach this goal we must learn how to specify a wide range of frequently encountered activities in sufficient detail to permit us to construct and evaluate theories of what people are doing.

The existing corpus of well-described examples of varieties of culturally valued activities is relatively small, but it is not nonexistent. Colby (1973), building on the work of Propp (1958) and Chomsky (1965), described Ixil (Guatemalan) folk stories in terms of a "narrative grammar," which organizes the distinctive "chunks" of the folk tale. Initially a descriptive device, the narrative grammar can provide a framework within which to interpret performance differences between, say, novice and experienced storytellers.

Basso (1976) provided a semantic analysis of a culturally valued class of Western Apache metaphors called "wise words." The semantic structure identified by Basso can be used to predict which metaphors the Apache will consider to be wise words and which they will not. Like Colby's narrative grammar, Basso's semantic structure of wise words provides a framework within which masters of language are discriminated from simple users of language.

Sometimes the structure of a social practice can be used to frame a description of a cognitive task. Ebbesen and Konecni (1979) studied decision making by judges who had to set penalties in felony cases. American court procedures produce a finite set of criteria on which a judgment should be made. Using this explicit information, Ebbesen and Konecni were able to assign relative weights to each kind of information and to predict decision-making outcomes accurately. (Significantly, the description of the decision making in actual cases did not coincide with simulated cases using the same information by the same judges; Sudnow, 1965, suggested important processes at work in the courtroom that would tend to make simulations based on explicit criteria inadequate to explain actual behavior.) Another nonexperimental setting that has been described in terms explicit enough to permit useful comparisons of cognitive performance is the classroom. Ethnographically sound descriptions of classroom activities are important because except for those infrequent parts of the day when children are constrained in ways similar to typical psychological tasks, we don't know how to describe what they are doing. Mehan (1979) first suggested a means of describing classroom discourse rules and then used this description to trace the learning of classroom discourse by first-third-grade children over the course of the school year.

Cross-cultural psychology has forced upon us the recognition that interplay between laboratory and observational research is a necessity if we are to understand how experience affects our minds. It has helped us discover techniques for making more sense of our observations than does untutored common sense.

The contributions of cross-cultural psychology were originally expected to add dimensions to the object of study. As we view it, the contributions are to the process of studying. The universality of psychological findings is not only affected by new information from the diverse cultures with which cross-cultural psychologists interact; it is also affected by the challenges that meeting with diverse peoples brings to the psychologist's working tools and assumptions.

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