

Cross-Cultural Research in the Sociohistorical Tradition¹

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Abstract. The growth of the 'sociohistorical' approach to human development in the USSR and the USA is discussed in terms of their cross-cultural theorizing and research. The key idea in common between these schools is that culture plays a crucial, mediating, role in the process of human development. Differences between approaches are described and an optimistic summary of future development is proposed.

Evidence about psychological functioning in different cultures has occupied a central role in the thinking of scholars working in the sociohistorical tradition since its inception as a distinctive school of psychological theory and research. However, the manner in which cultural comparisons have been used, and the nature of the data upon which such comparisons have been based, have changed markedly since the school's inception in the mid-1920s.

In this paper I will sketch a history of the uses of cross-cultural evidence by sociohistor-

ical scholars. I will claim that there have been two major stages of development in this research program, each of which arose within a distinct national tradition. At the present time these two traditions are merging, and a new, synthetic theory is arising which is helping to fulfill the aspirations of the sociohistorical school's founders.

Early Formulations of the Sociohistorical School

The unique place of cross-cultural research to sociohistorical psychologists derives from the central thesis upon which the school was founded: The structure and development of human psychological processes are determined by humanity's historically

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developing, culturally mediated, practical activity [Leontiev, 1981; Luria, 1976; Vygotsky, 1978].

Each of the terms in this formulation is tightly interconnected with, and, in some sense, implies the others, making it difficult at times to discern how each contributes to the overall theoretical claim of the sociohistorical school. As a means of analyzing their separate contributions, I will adopt a favorite strategy of the school itself. I will examine the very earliest formulations of these ideas and the paths of their development as they relate especially to cross-cultural data.

In the period between 1928 and 1930 the founders of the sociohistorical school published a three-part series of articles under the general rubric of the 'cultural development of the child' in the *Journal of Genetic Psychology*. These articles are remarkable in part because they appeared almost simultaneously with the initial publications in Russian describing the first wave of their theorizing and research as a self-consciously organized scientific group [Leontiev, 1928; Luria, 1929; Vygotsky and Luria, 1930].

The first article, by Luria [1928], opens with the well-known assertion that 'Man differs from animals in that he can make and use tools'. These tools 'not only radically change his conditions of existence, they even react on him in that they effect a change in him and his psychic condition' [Luria, 1928, p. 493]. The consequence of tool making/using for the basic structure of behavior when human beings are compared with other creatures is that

instead of applying directly its natural function to the solution of a particular task, the child *puts between that function and the task a certain auxiliary means ... by the medium of which the child manages to perform the task* [Luria, 1928, p. 495, italics added].

As each of the authors makes clear by way of concrete examples, these 'auxiliary means' (or psychological tools, as they are sometimes referred to) include not only physical objects but the words of human language.

In the second article, Vygotsky expands on the way that the new structural relationship between the individual and the environment characterizes what he calls a 'cultural method' of thinking (from whence the title of the series of articles).

All processes forming part of that method form a complicated functional and structural unity. This unity is effected, first, by the task which must be solved by the given method, and secondly, by the means by which the method can be followed ... It is precisely the structure which combines all separate processes, which are component parts of the cultural habit of behavior, which transforms this habit into a psychological function, and which fulfills its task with respect to behavior as a whole [Vygotsky, 1929, pp. 420, 421].

Taken together, these statements clearly stake out the central role of culture and the concomitant emergence of a qualitatively new structure of psychological processes as defining characteristics of *Homo sapiens* according to the ideas of the sociohistorical school. The historical aspect of the theory follows directly from the specification of cultural mediation as the unique characteristic of human psychological processes, since culture and mediated behavior must have emerged as a single process of hominization; human beings live in an environment like no other creature because it is an environment created by the accumulated tool/culture-mediated interactions of prior generations, reaching back to the dawn of the species. It is the mutual constitution of goals and means in practical activity that gives rise to the structure of specifically human psychological processes.

These connections are brought out clearly in Leontiev's [1930] article on voluntary attention, the last of the series to be published. Leontiev includes three historical scales in his discussion: phylogeny (species history), cultural history, and ontogeny (individual history). With respect to phylogenetic history he concludes that with a few disputed exceptions, 'we do not meet in the animal world any special forms of action having as their sole and special end the mastery of the behavior of other individuals by attracting their attention' [Leontiev, 1930, p. 59]. He illustrates the history of voluntary attention using cross-cultural data:

Already the tribal hunts which were the earliest instances of collectivism in man entailed the necessity of controlling the attention of the hunting group; this was an indispensable condition for organized hunting. The function of the leader here was to submit the behavior of the collective to a common end, which meant that first of all the aim had to be indicated, that is, attention had to be drawn to it [Leontiev, 1930, p. 58].

Starting from such crude beginnings, indicative behaviors were said to undergo a process of development in which they become differentiated and specialized, evolving into conventional signs, that is, once again, into psychological tools. This second level of history is then linked explicitly by Leontiev to the third level and hence to the overall theme of the set of articles, the cultural development of the child:

The history of one man's mastery over the regulation of behavior of another repeats in many points the history of his mastery over tools. It presupposes a change in the structure of behavior, which turns behavior directed to an end into behavior directed circuitously [Leontiev, 1930, p. 59].

It probably does not need emphasizing that the entire process being described here implies the social nature of psychological processes, as well as their historical origins and culturally mediated structure. As Vygotsky [1929, p. 423] remarks, 'All means of social behavior are in their essence social'. They are social too, in the dynamics of their origin and change, as expressed in the well-known 'general law of cultural development':

Any function in children's cultural development appears twice, or on two planes. First it appears on the social plane and then on the psychological plane. First it appears between people as an interpsychological category and then within the individual child as an intrapsychological category [Vygotsky, 1978, p. 57].

It is of some interest to note that at no point in these articles do the authors mention the sources of these ideas in Marx and Engels. In *The Development of Higher Psychological Functions* written a very few years later, but not published for almost 30 years, Vygotsky opens by citing Engels: 'The eternal laws of nature to an ever greater extent are changing into the laws of history.' In a recent monograph Lektorsky [1980, pp. 136-137] summarizes the implications of Marxist epistemology for cognitive psychology in terms that appear to support its claim to having established a Marxist psychology:

Marxist philosophy posits that practical activity itself must be understood in its specifically human characteristics, namely as joint or collective activity in which each individual enters into certain relations with other persons; as mediated activity in which man places between himself and an external, naturally emerging object other man-made objects functioning as instruments of activity; and finally, as historically developing activity carrying in itself its own history.

Early Uses of Cross-Cultural Data

The foregoing discussion indicates that the special importance of data from other cultures, especially so-called primitive cultures, is that they simultaneously provide evidence about the special structure and the developmental dynamics characteristic of human psychological functions. Just as early stages in the development of culturally mediated psychological processes in the child reveal their internal structure because the functions are not 'fossilized', so too the early stages of cultural history reveal such structures.

The early writings of the sociohistorical school are full of examples from preindustrial cultures, such as Leontiev's example of the role of the leader in hunting. Two central points emerge from such examples: (1) All psychological processes are organized in accordance with the mediated structure of human activity. (2) The systems of mediation undergo development through history. Hence, in Leontiev's example, the primitive hunters display uniquely human mediated activity in contrast with lower animals, yet this activity is maintained as an interpsychological function; it has not been internalized to become the independent function of individuals.

The sociohistorical theorists were by no means unique in using such data to bolster their theoretical claims. Wundt, the Gestalt psychologists, the French sociological school, and Piaget all made references to ethnological data in support of their claims about human psychological functioning. As discussed extensively elsewhere, data of this kind are extremely problematic for purposes of psychological analysis [Boas, 1911; Cole, 1976; Cole and Scribner, 1974; Jahoda 1982; Scribner, 1985].

In the hands of many psychologists of the late 19th and early 20th centuries, ethnological reports were used to support recapitulationist and parallelist theories of history and development, e.g., the idea that modern children go through mental stages that repeat or parallel early stages in cultural/mental history. In some forms, these theories claimed that ontogeny recapitulates phylogeny, in others that phylogeny recapitulates history.

Scribner [1985] decisively illustrates the error of attributing such views to the sociohistorical school. I will not repeat her evidence and argumentation here. Rather, I will restrict myself to citing her general conclusion and a few comments on the misunderstandings that nonetheless arise from the use of secondary data and cross-cultural analogies. Scribner draws a crucial distinction between the theoretical enterprise of the sociohistorical school and Piaget's genetic epistemology as follows (speaking here of Vygotsky):

He does not represent higher systems as general modes of thought or as general structures of intelligence in the Piagetian sense. Vygotsky addressed the question of general processes of formation of particular functional systems, a project quite at variance from one aimed at delineating a particular sequence of general functional systems [Scribner, 1985, p. 132].

I believe this statement to be correct and whenever sociohistorical theorists have adhered to its principles, they have correspondingly been exempt from the general criticism of recapitulationism. However, in their earliest work, the founders of the sociohistorical school had not elaborated all of the implications of their basic ideas, including the idea of functional system. In some cases, as a consequence, they made assertions that are diffi-

cult to interpret except in recapitulationist terms, as the following example taken from Leontiev's [1930, p. 61] article illustrates:

Half-civilized tribes feel a certain repulsion to ordered labor ... whereas in primitive man the power of uninterrupted, persistent attention was very poorly developed, with us it has attained a very considerable degree ...

Thus, the transition of the savage from capricious and fitful dissipation of energy to the specific, systematic, and organized labor of man, signifies, as we see, the transition to a higher form of activity and attention.

Statements such as these, examples of which can be found in many places in the early writings of the sociohistorical school, are certainly compatible with their goal of demonstrating that processes of human development themselves develop. But they simultaneously raise the specter of simplified recapitulationism that make it sound very much as if, indeed, primitive adults think and behave in a manner very similar to modern children, a problem which soon bedeviled the major empirical investigation of changes in thinking conducted by Soviet founders of the school.

The Expeditions to Central Asia

Aware of the shortcomings of secondary data, in the early 1930s Vygotsky and Luria organized two scientific field trips to Central Asia, subsequently conducted by Luria, in order to obtain first-hand empirical evidence concerning the historical development of cognitive functions in place of the second-hand reports upon which they had relied up to that time. The field sites were rural locations in Uzbekistan and Kirghizia which

were undergoing rapid socioeconomic and cultural change as a consequence of the collectivization movement begun in the late 1920s.

The scientific and sociopolitical context in which this work was carried out differed markedly from the conditions within which the basic ideas of the sociohistorical school had been created. Internationally, fascism was becoming a major force to be reckoned with. The fascist interpretation of cross-cultural differences in racial/genetic terms lent a special urgency to demonstrations that population differences were historical and cultural in origin. Internally, the country had just undergone the trauma of massive collectivization of agriculture; there was also a sharp struggle within Soviet psychology over the proper definition of a Marxist psychology, a struggle in the course of which the cross-cultural research eventually played a significant, if not constructive, role.

In a remarkable example of internationalism, Alexander Luria published an invitation to scholars of other countries to help in the planning and execution of the research in the American journal *Science*. No Americans made the long trip to Moscow and Central Asia, but Kurt Koffka, who then lived in the United States, did go on the second expedition (only to fall ill and have to return home), and Melville Herskovitz subsequently corresponded with Luria about the problem of racial theories in psychology.

Our knowledge of the thinking that went into this research and the results eventually obtained is distorted by the complex events which followed immediately upon its completion. Only a brief report of the results of the second expedition was published in English [Luria, 1934]. So far as I can tell, no results were published in Soviet journals.

Instead, when some of the basic results became known in Moscow, Luria, Vygotsky, and their colleagues were severely criticized for insulting the intelligence of the builders of socialism living in the Central Asian republics [Razmyslov, 1934]. That is, precisely the interpretation of the cross-cultural findings in developmental terms that led to concern over their use of secondary data sources generated a potent political controversy over their own empirical research.

These criticisms, combined with attacks against other aspects of the sociohistorical school's activities, such as their involvement in psychological testing, put an end to it as a public force in Soviet psychology for approximately 25 years. Not until the late 1960s did Luria [1971] publish a Soviet article based on results from the research, and not until 1974 was a full-scale monograph prepared [Luria, 1976].

Writing in 1934, Luria characterized the goal of the research as the study of

the system of thinking of primitive societies, the development of the psychological functions in their thinking, and the pointing out of those changes which this thinking undergoes in social and cultural transformation connected with socialist growth [Luria, 1934, pp. 255–256].

Luria's [1976] characterization of the goal of the research demonstrates both continuity with the original formulation and a greater subtlety with respect to characterization of the developmental comparisons intended. He writes that he seeks to demonstrate that

many mental processes are social and historical in origin ... important manifestations of human consciousness have been directly shaped by the basic practices of human activity and the actual forms of culture [Luria, 1976, p. 3].

The 1976 monograph presents many interesting results in support of this thesis. In the areas of classification (colored threads, geometric figures, and various objects), logical deduction, and self-evaluation (roughly equivalent to what is now referred to as metacognition), Luria observed that traditional people respond to his tasks in ways systematically different from their neighbors who have been involved in collective agriculture and/or schooling. Simplifying greatly, the following conclusions are most central. In the change from traditional agricultural life to collectivized labor in literature/industrialized circumstances:

(1) 'Direct graphical-functional thinking' is replaced by at least the rudiments of 'theoretical thinking'.

(2) 'The basic forms of cognitive activity go beyond fixation and reproduction of individual practical activity and cease to be purely concrete and situational', becoming a part of more general, abstractly coded systems of knowledge.

(3) These changes give rise not only to new forms of reasoning, restricted to logical premises free of immediate experience, but new forms of self-analysis and imagination as well.

It should be apparent that these conclusions leave ample opportunity for controversy about the way that cross-cultural differences in cognitive performance should be interpreted; the uncertainties of the 1920s have by no means disappeared by the 1970s! Controversy existed even at the level of basic fact. Koffka [in Luria, 1934] wrote three paragraphs summarizing the research on visual illusions, which Luria claimed were greatly reduced or absent in the most isolated groups of subjects. Koffka claimed that visual illusions were always obtained except

in cases in which the subjects believed that their mental ability was being tested. In these cases they became suspicious and gave alternative descriptions. Modern research on illusions has been kinder to Luria's interpretation [e.g., Wagner, 1982].

In fact, the terms in which Luria formulates the relation between ontogenetic and cultural/historic change have led such sensitive observers as Jahoda [1981, p. 126] to conclude that Luria's theory and Piaget's 'cannot be regarded as incompatible or even directly opposed'. Indeed, it is Cole and his colleagues who are taken to task by Jahoda for suggesting that the sociohistorical and genetic epistemological views are in conflict over the relation between history and cognition! [See Laboratory of Comparative Human Cognition, 1983, and Wertsch, 1985a, for additional discussions of the relationship between the ideas of Vygotsky and Luria in this domain.]

Without dwelling on the complexities involved, I can point to two features of Luria's cross-cultural research that fail to fulfill the methodological requirements of the sociohistorical school. First, as we have commented elsewhere [Cole and Griffin, 1980], Luria neither studied nor modelled in his experiments the practical activity systems of the Uzbeki and Kazaki people and the psychological processes associated with them; hence, his interpretations were not grounded in an analysis of culturally organized activities. Instead, for purposes of psychological diagnosis he introduced distinctly Western European activity systems, in the form of psychological tests and interviews, which did not model local reality, but served instead as measurements of generalized psychological tendencies for which there was a developmental interpretation in Western European societies.

Using this approach, Luria found that contact with European culture either through schooling or participation in Soviet-run collective enterprises increased the likelihood that traditional peasants would respond appropriately to his intellectual puzzles in Russian terms, but these results are basically silent with respect to possible analogues in indigenous practices. Such analogues might or might not exist, but the research Luria engaged in would not, in principle, be able to tell us which case fits reality.

The second, closely related problem, which becomes the focus when the sociocultural tradition is taken up in cross-cultural research by American investigators, is Luria's failure to restrict his conclusions to particular domains, instead appearing to claim that in general there is a change in the complexity of mediational mechanisms of cognition in the socioeconomic change from agricultural to industrial modes of production. Too often he seems to be concluding that the results he reports are independent of problem content and activity context, e.g., generalized cognitive changes. This kind of conclusion simultaneously undermines the well-established principles of the dependence of psychological process on living activity systems and renders adults who display such behaviors child-like in inappropriate terms. (There is no doubt that the principle of context specificity was known in theory, since it had been formulated by Vygotsky [1978, p. 53] prior to the work in Central Asia: '... the mind is not a complex network of *general* capacities, but a set of specific capabilities ... learning ... is the acquisition of many specialized capabilities').

Overall, then, the Soviet experience with cross-cultural research in the service of

building a sociohistorical theory of psychological processes provides an uneven picture. On the one hand, the sociohistorical school is the only extant theoretical approach for which data about the cultural organization of activity and mind are essential. Such data provide a model of the unique structure of human psychological processes and provide a framework for the all-important study of psychological change or development. However, in moving from the realm of ontogeny to the realm of history, the school's essential insights about development as change in the interfunctional organization of elementary processes in real activity contexts tended to get lost. The crucial nature of culture as the unique medium of all human activity is obscured; its place is taken by a characterization of historical change based upon political economy in which cultural organization is subordinate and a form of uniformitarian stage theory appears in its stead, vitiating the power of the theory.

Enter a New Tradition Emphasizing Cultural Contexts

In the mid-1960s the resumption of exchanges between Soviet and American scientists produced the internationalization of cross-cultural research which Luria had worked to stimulate more than 30 years earlier. In 1962, while a postdoctoral fellow in Luria's laboratory, I learned of the expedition to Central Asia and was introduced to the ideas of the sociohistorical school. In 1964, when I was sent to do research on culture and thought in West Africa, I remembered enough about Luria's work to make arrangements to return to Moscow to study

his experience in the hopes of replicating his most important observations under very different historical and cultural circumstances.

In the summer of 1966, Luria and I had the opportunity to work through a large proportion of his copious notes on the Central Asian project, enabling him to prepare the materials for publication and enabling me and my colleagues to grasp enough of their content to undertake replications of specific experimental procedures in the following years [Cole et al., 1971; Scribner, 1975]. Simultaneously we began a process of self-education and translation of basic texts of the sociohistorical school as a means of understanding more deeply their approach to the problem of culture and development.

Initially our research was neither historical nor developmental in its orientation. As described elsewhere [Gay and Cole, 1967; Cole, 1978], we approached the problem of cultural variations in cognition from a pragmatic and relatively ahistorical perspective. The problem set for us was to understand the poor mathematic performance of children from a nonliterate, tribal society (the Kpelle of Liberia) in newly constructed American-style schools placed in their villages. We began our investigation by making two naive assumptions and one naive conclusion:

(1) Although tribal children might be lacking in particular experiences that we consider routine and necessary for children growing up in our own country, they were by no means lacking in learning experiences in general. As we phrased it at the time, 'we must know more about the indigenous mathematics so that we can build effective bridges to the new mathematics that we are trying to introduce' [Gay and Cole, 1967, p. 1].

(2) People become skilled in forms of activity that they have to engage in often.

(3) Therefore, cultural differences in mental processes will be domain- and content-specific.

These ideas ran counter to prevailing theoretical currents in psychology in one important respect. During the 1960s it was widely believed that the poor performance of America's ethnic minority children could be explained as a consequence of 'cultural deprivation', an amorphous concept roughly meaning that some cultural arrangements failed to stimulate intellectual growth. Our assumption was that cultures differ in the specific domains within which intellectual expertise is fostered and the means by which they arrange for mastery.

These ideas led us into a detailed investigation of Kpelle concepts and thought processes, grounded in an investigation in domains of practice, such as farming, weaving, and carpentry, where it seemed likely that the concepts would be richly represented. In our study of rice farming techniques, for example, we found that Kpelle people displayed an articulated mathematical system and accuracy in estimating volume superior to that of educated Americans [Gay and Cole, 1967], but that in other domains, no articulated mathematical concepts existed to guide behavior, which was inconsistent and inaccurate [Cole et al., 1968]. Systematic developmental studies were not a part of this first effort, which concentrated on knowledge and skills of adult members of the culture.

Between 1966 and 1969, we were permitted to carry out a second project, this one aimed at a broader portrait of cultural variations in cognitive processes. This research came closer in spirit to the strategy of Luria's Central Asian expeditions. It also included studies of syllogistic reasoning and classifica-

tion modelled on his work, along with a large variety of studies on memory, classification, and reasoning, modelled on procedures then prominent in the study of adult cognitive processes and cognitive development in the United States.

Consistent with the American tradition of empirical research with its focus on method, we were particularly concerned with the logical basis upon which psychologists' conclusions about cultural variation in thought were based. In particular, we were anxious to provide a systematic alternative to cultural deprivation theories, which struck us as incorrect in principle and pernicious in practice.

Our concerns about method led in three productive directions. First, they led us to consider the developmental origins of performance differences in ontogeny. In addition to including people of different ages ranging from young children to adults, we included systematic comparisons between schooled and nonschooled populations, as a way to assess the degree to which European claims for major cognitive changes between 5 and 7 years of age were confounded with the influence of schooling. Simultaneously, we varied familiarity with task content and procedures in an effort to prevent confusion of performance differences that might be interpreted as basic differences in cognitive competence with differences arising from superficial aspects of experimental procedures and cultural emphases on different content domains.

Second, they led us into the study of domains of everyday practice that were the sources of local expertise. This line of investigation had two interconnected purposes. First, it was necessary for narrow reasons of experimental method to seek differences in

stimulus familiarity for the objects and procedures used in our experiments. Second, it was necessary if we were to test thoroughly the importance of everyday practice as a source of cognitive expertise. These efforts were never as thoroughgoing as we would have liked, but they gave us wide experience with the methods and theories of microsociology and cognitive anthropology.

Third, as our Soviet colleagues might have anticipated, our concerns with method eventually brought us face to face with broader issues of theory and methodology that we were ignorant of at the outset. However, our theoretical efforts took a direction somewhat different from that of our Soviet colleagues. We too were forced to return to examine the origins of psychology as a science as a means of resolving the theoretical contradictions raised by our empirical work. But instead of seeking a resolution completely within psychology, we were led to seek realignment of the disciplines of anthropology, psychology, sociology, and linguistics. For a while we referred to this enterprise as 'experimental anthropology and ethnographic psychology' as a way of pointing at the area where we believed reformulation was necessary.

The results of our second cross-cultural project, in certain respects, supported the sociohistorical thesis that involvement in modern socioeconomic life leads to a change in mental performance. In particular, we replicated to an astonishing degree Luria's observations on syllogistic reasoning and classification. However, there were some important differences both in the style and content of the work, and in the way we interpreted the results.

One such difference was the amount of effort we put into identifying indigenous

forms of practical activity which could serve as models for experimental studies of cognitive skills. Observational studies were carried out on widely known skills such as house building, specialties such as bonesetting and blacksmithing, legal disputes, games involving specialized skills or knowledge, and social negotiations involved in the management of secrecy. While we were often unable to construct experiments on the basis of such observations, they served as a criterion of socially organized cognitive complexity against which to evaluate the results we obtained from experimental studies.

We also adopted a very different way of treating the results of experimental observations. Realizing that the very structure of most experimental procedures in cognitive psychology derives from the structure of school practice, we adopted a strategy of systematically varying aspects of experiments in an attempt to elicit variations in performance. It was especially important to discover conditions under which poor performance could be eliminated as a means of finding its sources when Western-style procedures were implemented without significant modification. Over a range of experimental topics we demonstrated that seemingly minor variations in content or procedures in many (albeit not all) cognitive tasks could lead to large quantitative and qualitative differences in performance. It appeared that, according to the logic and methodological postulates of developmental psychological theories, our African tribal subjects were in different stages of development, characterized by different rules of learning, at the same time!

The presence of significant variations across different implementations of a single experimental procedure led us to take a far

more skeptical view of the presumed consequences of education than was fashionable among either our American or Soviet colleagues, since the oft-observed superior performance of educated people depended strongly on the particular contents and procedures. Simultaneously, we discovered that a vast number of developmental-psychological studies appearing in American journals were assessing relatively specific consequences of schooling rather than general laws of cognitive development [Cole et al., 1971; Sharp et al., 1979].

These findings led us to conclude that cultural differences in cognition reside more in the contexts within which cognitive processes manifest themselves than in the existence of a particular process (such as logical memory or theoretical responses to syllogisms) in one culture and its absence in another.

Uniting the Two Traditions

We can see that the two traditions of cross-cultural work identified with the sociohistorical school began from very different starting points. The Soviet tradition began with a deep theoretical-methodological critique of existing schools of psychology which dictated cultural development as a fundamental psychological category. It emphasized broad historical changes in the nature of mind somewhat at the expense of synchronic variability arising from differences across concrete activity settings, leading Wertsch [1985b] to comment that Luria's cross-cultural research was really cross-historical in conception. Empirical research came late in the experience of the Soviet sociohistorical scholars, and that research,

when it at last became possible, followed the early tendency to concentrate on major historical shifts in political and economic formations in place of detailed studies of particular activity systems and the functional psychological systems to which they give rise.

The American tradition began from an applied empirical demand to explain synchronic, culturally conditioned differences in quite specific domains of cognition, in connection with equally specific domains of sociocultural practice; it generated a great deal of research with relatively shallow, ahistorical, and eclectic theoretical underpinnings, but a strong methodological, interdisciplinary base as a warrant for claims about the factors controlling different levels of performance across contexts within cultural groups.

During the 1970s these two traditions enjoyed a period of expanded interaction in which American psychologists came to a deeper understanding of the Soviet tradition, while Soviet scholars were made familiar with the empirical results of the American tradition and its grounding in disciplines other than experimental psychology. It was during this period that a deeper appreciation of Vygotsky's thought was achieved by Americans through the publication of heretofore inaccessible seminal works such as Vygotsky [1978], Leontiev's [1981] basic monograph on development, Luria's [1976] descriptions of his cross-cultural investigations. These were supplemented later by important synoptic treatments [Kozulin, 1983; Wertsch, 1985a, b]. It became possible, for the first time, to develop a principled account of the relationship between the two approaches.

Significantly, during this period the American experience of cross-cultural re-

search in the sociohistorical tradition, which continued to be expanded and deepened [Scribner and Cole, 1981], was complemented by developmental research within the United States. This 'return home' provided an essential basis for understanding that cross-cultural research is only one part of the broader program of research on the cultural development of mind, as the founders of the sociohistorical school had argued half a century earlier [Cole and Bruner, 1971; Cole et al., 1987].

This new phase in the development of the research program is still in progress, so a final evaluation must certainly await the passage of time and the sifting of results. However, I will hazard some suggestions about the future of this line of work as a means of eliciting constructive criticism.

Looking to the Future

Overall, I see current progress on the development of the sociohistorical school growing out of its cross-cultural research program as a process of combining the American emphasis on cultural context and the study of concrete activity systems with the Soviet emphasis on the mediated structure of higher psychological functions and the importance of history and political economy. This combination seems to be in complete agreement with the spirit of the approach outlined by the founders of the sociohistorical school during the 1920s, but imperfectly implemented in their research.

At a conceptual level, the compatibility between the context-based and mediational approaches can be appreciated by reconsideration of the theoretical links between the basic terms. These connections can be illu-

minated by a closer examination of two key concepts, tool use and context. It is our basic contention that tool use implies both mediation and context specificity, while context-dependence implies that mental processes are historical phenomena.

First, consider the way in which the tool-mediated nature of psychological functions implies context specificity. In their quite correct insistence on the mediated nature of mind and the instrumental aspect of mediators, embodied in the notion of psychological tools, the founders of the sociohistorical school neglected the cardinal fact that there is no universal, context-free tool. Rather, all tools embody simultaneously a theory of the activity they have been designed to fulfill and a theory of the human beings who must carry out the activity. Tools vary from highly specialized to relatively general with respect to the tasks they can fulfill [see LCHC, 1983, and Wertsch, 1985b, for discussions of the sociohistorical school's approach to the problem of transcending local context]. But the dream of a context-free tool which is currently being pursued in the realm of artificial intelligence, as critics such as the Dreyfus brothers in the USA [Dreyfus and Dreyfus, 1986] and Zinchenko [1987] in the USSR have emphasized, completely misinterprets the relationship between human beings and the world, denying the mediated and the always incomplete nature of human knowledge. (These same remarks apply equally to human language, of course, as Bakhtin [1981] has consistently emphasized in terms quite compatible with those of the sociohistorical school).

Similarly, to say that higher psychological functions are context-bound raises the central question of the historical origin of the context in question. The agricultural prac-

tices of the Kpelle of Liberia or the animal husbandry of the Uzbeki peasant are not universal forms of activity. Nor is print-mediated education. To understand the cognitive implications of such practices we need to know both their internal organization and the way this organization is shaped by the historical circumstances that gave rise to them [see LCHC, 1983, for an extended discussion of these issues].

I believe that when an appreciation of the context-boundedness of cognition is combined with a historical appreciation of the origins of leading contexts and their interconnections, the basis exists for realizing the theoretical aspirations of the founders of the sociohistorical school. I will illustrate this kind of synthesis with respect to Luria's research in Central Asia, that being the one major cross-cultural study to grow out of the Soviet tradition.

A reinterpretation of Luria's research in context-specific terms does not deny his basic contention that there are important changes in the organization of activity and modes of cognitive functioning associated with the political-economic change from pre-literate agricultural societies to literate, industrial societies. However, it challenges his claim that traditional cultures are devoid of theoretical thinking and questions the generality of the 'higher' forms reported for those people who have moved into modern forms of labor and experienced education. With respect to the characterization of thought processes in traditional societies:

(1) Ethnographic evidence makes it a virtual certainty that people in traditional non-literate societies build theories about their world that bear great similarities to the activities of schooled populations [Goody, 1977; Horton, 1967; Jahoda, 1982; Levi-Strauss,

1966]. What differs are the specific problems to which such thinking is applied and the mediational means for solving them. Logical reasoning is exhibited, for example, in Trobriand land disputes [Hutchins, 1980].

(2) The idea that nonliterate peoples are restricted to concrete thinking is incorrect. As a single example, Micronesian navigators employ a complex, abstract representation of heavenly bodies, combining it with both totally abstract models of geographical landmarks and quite specific wind and water conditions to guide their navigation [Hutchins, 1983].

(3) A great number of studies demonstrate the use of taxonomic hierarchies in the classificatory behavior of nonliterate peoples, although such organizing principles are likely to be manifested only when special care is made to use familiar materials and to make the structure of the task clear [LCHC, 1983; Rogoff, 1981; Sharp et al., 1979].

With respect to schooled populations, the following qualifications of Luria's conclusions seem to be required:

(1) While it is quite true that a few years of education produces a marked change in response to simple syllogisms, even highly schooled individuals are susceptible to the influence of the content of logical problems [D'Andrade, 1982; Shweder, 1977]. The same principle applies to the other cognitive domains studied by Luria; in all cases, it is possible to show that the performance of educated populations is a mixture of the two modes Luria posits [see LCHC, 1983, for a review].

(2) Formal schooling involves a new kind of institutionalized activity system in which a qualitatively new kind of activity, 'educational activity' [Davydov, 1972], is extremely important. It may well be the case

that specialized cognitive practices arise in connection with this form of activity, as Vygotsky [1962], Luria [1976] and Tulviste [1979] suggest. But it is an error to imagine that the changes coincident with formal schooling and modern industrialized labor have a uniform impact on all life contexts. The very distinctiveness of school contexts is a major reason for the lack of transfer of knowledge between school and other settings.

(3) The distinctiveness of schooling and the close association between the methods of cognitive psychology and schooling have vital methodological implications that must be addressed. While we labored in our cross-cultural work to represent a wide variety of cultural practices, we were often unsuccessful, falling back on systematic variation of procedures derived from educational activity as a hedge against erroneous interpretations of cultural variations. In research by Cole et al. [1978], it was shown that classroom instruction is so specialized that while standard methods of experimental psychology could be adapted to analyze cognitive performance in school, these methods did not generalize to afterschool activities which presumably bore a close relationship to schooling. These methodological problems have by no means been solved in a definitive manner [Rogoff and Lave, 1984].

(4) In so far as schooling and modern conditions of labor create qualitatively distinctive contexts within which qualitatively distinctive forms of activity occur, the functional cognitive systems associated with this activity require specialized skills for their mastery, skills that are often modelled by standard psychological instruments. An appreciation of the cognitive changes associated with sociocultural change requires si-

multaneous analysis of the special morphology of activity that arises in new contexts and an appreciation of the distribution of those contexts within the society and the way they are interwoven to create an entire life space for an individual [LCHC, 1983; Wertsch et al., 1984].

(5) Using the principles of an activity-based, context-sensitive sociohistorical approach, it is possible to go well beyond idealized characterizations of educational activity and standardized psychological tests to model a great variety of activity systems and their attendant psychological processes. This is the path being followed, for example, by Scribner [1984] or Lave [1988] in their studies of labor activity, by Au [1985], Engestrom [1986], Mehan [1979], Newman et al. [1984] in their studies of educational practices, and by Griffin et al. [1986] in their systems of remediation.

A description of the new line of research and theory emerging from the combination of Soviet and American traditions of sociohistorical psychology goes well beyond the confines of this paper [see Bruner, 1986, for one stimulating example]. I therefore close by noting a pleasant irony that I perceive in the events I have recounted. According to the principles of the sociohistorical school, a major way in which new stages of development arise is when two or more streams of history come together, and out of their interweaving emerges a qualitatively new kind of psychological structure. So it has been with the sociohistorical school itself. True to the ideals and insights of its founders, it has entered a qualitatively new stage owing to the interweaving of the Soviet and American traditions of thinking about the role of cross-cultural research. Nor is the interweaving of ideas restricted to Soviet and American

scholars; rather, essential threads of knowledge are being contributed by representatives of many national traditions, providing the foundation for creating a new pattern of scientific understanding with which to address the problems facing humankind in the late 20th century.

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Commentary

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Michael Cole has given an engrossing account of the development of the sociohistorical approach in our century. At the center of the discussion are works by Soviet advocates of this approach – L.S. Vygotsky, A.N. Leontiev, and especially A.R. Luria, who undertook a famous expedition to Central Asia in order to obtain empirical data on the possibility of cultural-historical influence on the organization of psychological processes. Cole's article is valuable for its detailed critical analysis of works from that early period. Indeed, today one can state that Luria himself as a researcher could not avoid being influenced by the context of genetic epistemology that was paradigmatic in the middle of the 20th century. He considered the intercultural differences he obtained in the spirit of the juxtaposition of concrete and abstract-logical thinking. Other possibilities for interpretation remained unnoted. It is perfectly possible, for example, that the subjects tried to avoid answers that might have seemed socially unacceptable to them (see the notes of Kofka on visual-geometrical illusions). Furthermore, their reasoning was probably shaped in full accordance with the syllogism of Aristotle, in which the initial starting point could have been something

like the following assertion: 'Don't say too much, especially about something that you haven't seen yourself.' Then, indeed, any reference to Novaya Zemlya, Murmansk, or Kolyma (far northern regions of European Russia and Siberia) should have elicited the responses, 'I don't know, I've never been there', and so on. There are other possible explanations for the results obtained, each one of which is a hypothesis. These hypotheses must be tested through experimental research, which, in our view, is still largely a matter of the future (despite brilliant research in recent years by Cole, Scribner, Tulviste and others). We should note, incidentally, Luria's initial interpretation should not be completely disregarded. In part, it seems that his suppositions are confirmed by research on characteristics of thinking during the selective suppression of the functioning of the left or right hemisphere (Chernigovskaya).

Cole's approach to understanding the contextual specificities of psychological processes are positive and new. Here one could also look at the influence of the paradigm of parallel distributed processing that is fashionable today, or the modular organization of the mind described by Fodor. Moreover,

Cole notes that this conception was known theoretically to Vygotsky, who devoted one of his best later works to it, *On Psychological Systems*. Later, a crucially important addition was made by Leontiev, who associated the formulation of specialized psychological mechanisms with the context of one or another object-based activity. From here it is only a short step to the idea of domain specificity. However, because Soviet psychological investigations have been weakly equipped methodologically, this step was independently taken in American studies by Chi, Resnick, and others.

This concept of a 'functional system' has received a detailed theoretical elaboration. 'Functional system' refers to a complex of psychological mechanisms, formulated during life, concerned with the resolution of several specific everyday tasks. The leading Soviet psychologists A.A. Ukhtomsky, P.K. Anokhin, and N.A. Bernshtein contributed to the elaboration of this concept. It should be noted that this concept was also central to the neurological studies of Luria. In our view there is great similarity between the conception of functional systems and the principle of domain specificity.

A serious difficulty in the contemporary formulation of the tasks and principles of the sociohistorical approach lies in the absence of a universally accepted model of levels of cognitive organization. Take, for example, the classical differentiation of 'natural' and 'culturally mediated' higher-order psychological functions. This differentiation has always been problematic, and it was not worked out conceptually. As a result, Soviet followers of Vygotsky gradually transformed the 'natural' basis into an infinitesimal quantity. Numerous publications confirm the mediation of language in the structuring of per-

ception, memory, and so forth. On the other hand, experimental data, especially the research of Gibson's followers, testify that a significant part of perceptual mechanisms are part of the genetics of routinized means of activity. Furthermore, even several mechanisms of the normal functioning of language turn out to be innate. This is shown for the perception of phonemes (Eimas), although for a long time the assimilation of the phoneme grid of one's native language was considered to be a process that provides specifically human forms of perception. One should not ignore the genetically given component in the case of significantly more complex types of activity as well, particularly such types that traditional cultural anthropology would have put solely in the sphere of sociohistorical laws and normative rules (Bishof). In the face of these data, the extraordinary similarity of the concepts 'contextual specificity', 'functional system', 'modular processing', and so on, are obvious. On various levels of psychological organization, these phenomena and processes should reveal several specific characteristic features.

In particular, in the area of higher-order symbolic coordination, relating – but not identical – to the semantics and syntax of speech, the contextual dependence of conceptual structures is completely different than, say, the specialized mechanisms of spatial perception. The multiple (recursive) embedding of mental contexts in one another is typical of the symbolic means of structuring the linguistic, sign-based model of the world. This condition makes theoretical analysis difficult, but at the same time it opens up an interesting possibility for making contextual dependence relative and overcoming it. For example, the fixed (rigid) relation of the individual to him- or herself may be overcome

by creating series of contexts in which 'I' acts as an invariant in relations with other people who have meaning for him or her. Recently this idea has had interesting development in the framework of research on mediation with a computer in the organization of children's joint activity during the learning process (Rubtsov). In this regard, the work of Cole himself and his colleagues is extraordinarily interesting. They are working in the direction of remediating (by means of manipulating contexts) children who are developmentally delayed because they have acquired an incorrect structuring of learning of such higher-order psychological functions as reading.

The next and final thought engendered by Cole's article involves the recognition that another aspect of the sociohistorical approach has been inadequately elaborated. Emphasizing the role of the cultural determinants of human activity, this approach in its orthodox variant does not leave open the possibility of free and creative choice in space, determined by sociocultural matrices. Thus, the better-known attempt to transform the position of the Soviet cultural-historical school into a technology of instruction – P. Ya. Galpern's theory of the stepwise formation of mental activities – is distinguished by the fact that it nullifies discrete differences between individuals, thereby eliminating

manifestation of the creative resolution of tasks, such as sudden insight. Furthermore, the linguistic (sign-based) means of structuring a culturally specific model of the world does not exhaust the list of internal means of organization of activity. There are means that permit structuring models of hypothetical, contrafactual, fantastical and absurd worlds. On this level of activity, 'contextual specificity' acts entirely differently than in the case of the specialized models of perceptual processing or the structures of semantic memory organized around several object areas. This refers to metaprocedural structures similar to those that stand out in research on poetics and rhetoric. This mention of the creative components of organizing activity takes us beyond the limits of the explicit contents of the article in question. We won't forget, however, that Cole has left most interesting questions to be dealt with in future work. I have no doubt that the approach developed by him and his colleagues will truly bring us to the creation of a more powerful theory, capable of giving us solution to the problems of the end of the 20th century.

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Commentary

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Why should one do cross-cultural research? There is no simple answer to this question. Historically, the motive was usually curiosity about strange and exotic peoples, sometimes regarded as a mirror of our own past. When mental tests became available, it was widely believed that these provided a means of assessing the genetically determined mental abilities of various 'races'. After such notions had become discredited, there came a period when the mere search for 'differences' became fashionable, though these were called 'cultural' rather than 'racial'. In due course, the futility of reporting psychological 'differences', whose nature was not understood, became apparent; whatever tests or tasks are employed, one can nearly always find 'differences' between populations.

The result was a radical shift in objectives. First, it came to be realized that invariances across populations are apt to be more informative than blindly gathered differences. Hence, one of the declared aims of contemporary cross-cultural psychologists is the search for psychological 'universals'; it can only be mentioned here that this is also somewhat problematic, for there is little consensus as to what constitutes a 'universal'.

Secondly, the cross-cultural field was hailed as a potential testing ground for theories initially elaborated in Europe or America. Most such theories, including that of Piaget in its original formulation and even some by social psychologists (who ought to have known better), implicitly conceptualize humans essentially as psychobiological organisms, claiming pan-human validity. Hence one of the commonly stated functions of cross-cultural research is that of testing the range of applicability of theories in other cultures.

Without wishing to deny the potential usefulness of such work, which I have myself undertaken, it is necessary to point out some snags. The almost limitless scope for testing theories is not really an advantage, this being a labor of Sisyphus. Moreover, in my experience the authors of a theory shown to be, with a high degree of probability, narrowly culture-bound, mostly feel free to continue to publish as though the conflicting findings did not exist. There are two main reasons for this, one being that cross-cultural research is as a rule not widely disseminated and thus easy to ignore. The second reason provides a better justification: If a theory is framed in universalistic terms, it is often very hard in

practice to demonstrate its cultural relativity convincingly; this is particularly so if it does not deal with some limited aspect of human functioning such as perception, but with a highly complex one like cognitive development. In that kind of case, it is usually possible to argue that the evidence was not conclusive since, for instance, the adaptation of the tasks to the cultural context might not have been adequately done. There can rarely, if ever, be a crucial test!

The way out of this dilemma is to have theories incorporating cultural variations as an integral part. There are very few of these, the sociohistorical school being perhaps *the* outstanding example. One of the merits of that school is the fact that it is reflexive, the merging of the Soviet and American traditions described by Cole being itself a cultural phenomenon. Cole provides a valuable account of the dialectical process whereby cross-cultural research resulted from and in turn modified the theory. This of course is not to say that all the difficult problems have

been resolved – in particular the elusive issue of specificity versus generality of psychological processes continues to perplex. Cole emphasizes context specificity, yet rightly denies the claim that non-literate peoples are incapable of abstract thinking. While such issues may be debated among cross-cultural psychologists, they will all agree that psychological functioning must be studied in its cultural context; and the sociohistorical tradition, propagated in America and Europe chiefly by Cole, has powerfully contributed to this consensus. Unfortunately, it cannot be said that we have as yet been able to persuade psychologists at large who, like fish in water, remain largely unwilling to concede the importance of other elements ‘outwith their ken’ (as we say in Scotland).

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