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MICHAEL COLE

Alexander Luria, Cultural Psychology, and the Resolution of the Crisis in Psychology

It is a great personal pleasure for me to address this conference, honoring the life and work of Alexander Romanovich Luria. I am not a Russian and I am acutely aware that I am speaking to you, so to speak, from afar, about a person you know very differently than I do. So, to begin with, I want to give a brief account of how I first encountered Alexander Romanovich and his work. I will then provide my interpretation of his contributions to psychology, in particular, the issue of creating a unified science of psychology.

Some personal background

I first came to Moscow with my wife thirty-five years ago, as a brand new post-doctoral student. I had obtained my degree one month earlier, specializing in a branch of American learning theory called "mathematical learning theory." I came because I had read an article by Alexander Romanovich and Olga Sergeevna Vinogradova about semantic reflexes. Reflexes were the basic units of analysis that underpinned the kind of learning theory that I was brought up on. The lineage went from Pavlov to Skinner to Estes, my graduate adviser. I had never taken a class in human development, but I had studied psychoacoustics and learning situations designed in terms of a basically Skinnerian framework. I had never heard of cultural psychology or the crisis in psychology.

Today I stand here with a lot less hair on my head and a lot different kinds of thoughts in my head. I cannot attribute the loss of hair to Alexander Romanovich. But without a doubt, the kinds of thoughts in my head were set on a radically different path by my experiences in Moscow.

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I say all of this because it is important that you understand the starting point from which I entered into interaction with Soviet-then-Russian psychology. I was an alien from the "other paradigm of psychology." I was treated very politely, and incorporated into Alexander Romanovich's collective at the Bourdenko Institute. Evgeniia Davydovna Khomskaia supervised a joint study of the development of semantic reflexes in people with different kinds of brain lesions. I spent several months working with Evgenii Nikolaevich Sokolov and Olga Vinogradova on orienting reflexes in rabbits and his then-student Nina Korzh on signal detection and memory among normal adults. I spent a month working with Boris Pokovich, Joseph Pressman, and Maria Varga in Asratyan's Institute of Nervous Activity. I visited Leningrad and saw the work of Chistovich and colleagues at the Pavlov Institute at Koltushi.

I came home having learned at lot more about Soviet reality than about Soviet psychology, but I also came home with a great admiration for Alexander Romanovich, and a feeling of obligation for the incredible amount of work he did to make our stay a productive and humane one.

My understanding of Soviet psychology, such as it is, grew very slowly. Following my return from Moscow, I began editing a volume of essays from the 1959 *Handbook of Soviet Psychology*. This task was carried out on Alexander Romanovich's initiative. The resulting book is a watered-down version of the Soviet original, filtered through a process of selection and deletion that depended primarily on my judgment about which chapters might possibly make sense to my American colleagues. Shortly thereafter, I became editor of the translation journal, *Soviet Psychology*, a chore I have fulfilled for the past twenty-nine years.

You might think that all this exposure to Soviet psychology would have provided me with a pretty good understanding of its basic principles. *Nichego podobnogo!* [Nothing of the sort!]. I tried as best I could, given the conditions of communication between the Soviet Union and the United States, to understand the debates I encountered in Soviet journals. But I found the arguments difficult to follow, and they often seemed more like local battles fought in the medium of ideological discourse. I could not use them as instruments with which to think.

My serious engagement with Luria's ideas began when I was sent to investigate the barriers faced by rural Liberian children who experienced great difficulty in learning the forms of mathematics that are part of the elementary school curriculum in Moscow and San Diego. My focus, reflecting the methodological background from which I came, was on creating a methodology that would make cross-cultural psychology a respectable discipline, based on sound scientific principles.

I did not approach the task of cross-cultural research in a sophisticated way.

I simply assumed that people would develop high levels skills in those domains of life that demanded that they do so. Consequently, I adopted an interdisciplinary methodology that sought to identify the occurrence of everyday local activities mediated by mathematics and to figure out how they were patterned as a part of the social heritage of the group called "culture." The applied goal of this work was to reorganize instruction to take account of local knowledge.

This line of approach led quickly to an apparent contradiction. From an analysis of test scores in cross-cultural experiments, it seemed evident that "primitives think like children." Both scored poorly. But any reasonably informed ethnographic account of the complexities of everyday social interactions and activities (and my own personal observations) indicated that only highly intelligent, culturally knowledgeable, persons could deal with the complexities of everyday existence. Psychological test data seemed clearly in conflict with my everyday interactions with the people who were subjects of my experiments. The great ease with which they outwitted me stood in sharp contrast to their "childlike behavior" in my experiments. In addition, my colleagues and I found that when we modified our experimental procedures, people's performance was also modified. Repeatedly we demonstrated that what others took to be universal cultural achievements that were attained to a lesser degree, were in fact intimately related to the distortions of reality introduced by experimental procedures. It was this paradox that led me back to Alexander Romanovich, and through him, to Lev Vygotsky and Alexsei Leontiev. I had discovered one modern manifestation of the "crisis in psychology," but I did not know this at the time.

In the summer of 1966, I returned to Moscow with my wife and baby daughter. I had made a bargain with Alexander Romanovich: If he would spend an hour a day explaining his research in Central Asia to me, I would work seven hours a day to help organize for the International Congress of Psychology. The congress is a blur in my memory. But I clearly remember our discussions about issues of methodology and theory with Alexander Romanovich several days a week. Each morning we went over his old data and he tried to explain to me how his data followed from a very general theoretical position that was articulated by Lev Vygotsky in the middle of the 1920s.

Two years later, I was able to begin to apply what I had learned because I received a grant from the U.S. National Science foundation to follow up on our earlier cross-cultural work. I was able to create an American, 1970s version of Luria's and Vygotsky's research in Central Asia, but with people from rural Liberia as a new "test case." I often found it difficult to reconcile their ideas about the way that socioeconomic-cultural transformations lead to a new form of thought, "theoretical" thinking modeled on normative literate practices of industrialized countries with evidence from my own research and that

of many others that even nonliterate people think "theoretically" in cultural practices where it is required by circumstances, and even highly educated people do not think theoretically outside of a restricted range of cultural practices.

It is important to my story to realize that the book we published on *The Cultural Context of Learning and Thinking* in 1971 contained only one reference to Vygotsky. It came at the beginning of a chapter on classification and focused on the Vygotsky-Sakharov procedure. The theoretical underpinnings of the procedure were not discussed. Rather, a superficial interpretation of the experiment linked it to American studies of concept formation.

At this point, in the early 1970s, the "theoretical side" of my lessons in cultural-historical psychology began to improve because Luria and his friend, the American publisher, Arthur Rosenthal, used me as an intermediary in the publication of a series of books, which included essays by Vygotsky and Luria's autobiography. As "designated editor" of these volumes, I was forced to go back and read the sources to which Vygotsky and Luria referred in their writings.

Initially, I had great difficulty understanding their focus on psychologists who lived around the turn of the century. I did not know anything about the ideas of people such as Brentano, Hoffding, Dilthey, Rickert, or Windelband. Wundt's ideas about a *Volkerpsychologie* were temporarily forgotten by the dominant paradigm of methodological behaviorism. Dilthey's search for a *reale Psychologie* simply was not a part of my intellectual training. So, I spent a long time reading the work of people completely foreign to my own training.

There, in a capsule, is the starting point for judging what I say in the remainder of this talk. It is my view of a part of your history. It is a view reflected through another national tradition, and it is likely to seem wrong, or at least not insufficiently subtle to you.

The current interpretation of Luria in Europe and the United States

In the United States, and perhaps in Russia, too, Luria is honored as a founder of neuropsychology, a major subdiscipline within psychology. His ideas about the functional organization of brain systems remain influential in many fields of medicine and defectology, as well as the new discipline of cognitive science. Many talks at this conference on Luria, the neuropsychologist, certainly attest to his continuing international influence in this area despite the great advances in brain-imaging techniques that have so enriched neuropsychology in the past two decades.

The drama of the mnemonist and the brain-injured patient became very

well know in the United States. But they were treated as oddities, tours de force. Few American readers took seriously the theoretically motivated foundations of Luria's writings. They did not know the history of research, begun in the 1920s, that led to these "sudden innovations" in the 1950s and 1960s. Although they were translated and affected experts, books such as *Traumatic Aphasia* were not linked to Luria's work in the 1920s and early 1930s when he and his colleagues were struggling to formulate a comprehensive new approach to psychology. The theory underpinning studies with children, with the brain injured, and the mentally retarded, was not seen as a unified whole. Rather each line of research was interpreted as a set of relatively unconnected studies of unusual interest.

I see things differently. My key thesis is that from the earliest days of his career, well before he met Vygotsky, Luria was working to solve the crisis in psychology—and that he succeeded.

The crisis in psychology

In seeking to understand the full scope of Luria's contributions to psychology it is important to take seriously the fact that Alexander Romanovich began his career at a time when modern psychology was just beginning to take shape. Experimental approaches to psychological processes were in the ascendancy, but there were those who opposed the idea that psychology could be or should be, a "positive" science, in the model of methodological behaviorism. The opponents of the dominant paradigm were themselves divided by a series of interlocking disputes about what kind of science psychology could be.

- Should it be an experimental science, modeled on the natural sciences, or a descriptive science, modeled on history and the humane sciences?
- Were psychological laws restricted to "nomothetic" generalizations applying only to populations or do they include "idiographic" laws that could illuminate the causal dynamics of individual human minds?
- Is it necessary to choose between subjective and objective approaches to research?
- Should psychology be restricted to a laboratory science, or could it be expanded to apply to people's everyday lives and serve as a basis for promoting social progress?

It is this set of issues that was being widely debated in psychology in the early decades of this century, that Vygotsky analyzed in his monograph on the crisis in psychology. As van der Veer and Valsiner (1991) remark in their comprehensive monograph, it is not clear whether ever Vygotsky resolved the crisis, even in his own terms. Vygotsky, you will recall, wrote that insofar as the

two kinds of psychology can be reconciled, unification would come about through uniting theory with practice:

> The most complex contradictions of psychology's methodology are brought to the field of practice and can only be resolved there. Here the dispute stops being sterile, it comes to an end. . . . That is why practice transforms the whole of scientific methodology. (Van der Veer & Valsiner, 1991, p. 150)[1]

Vygotsky criticized eclectic, atheoretical approaches to practice, arguing instead for a principled methodology of theory-driven practice that he called "psychotechnics." Luria also argued for a psychology built on theory-driven practice. Early in his life, the combined motor method provided one methodology. Later in life, he evolved a new methodology he called "romantic science." Both were solutions to the crisis in psychology.

The combined motor method

By the testimony of his autobiography and his early published writings, Alexander Romanovich was interested in the issues that made up the crisis in psychology for several years before he met Vygotsky. While still a student he read widely in German and American psychology. He was very sympathetic to German act-psychology and at the same time he was fascinated by Freud and Jung. This combination may seem odd, but it reflected his search for a theory of motivation and some way to address questions of emotional conflicts and the method of free associations. As he summarized his notions about psychoanalysis:

> Here, I thought, was a scientific approach that combined a strongly deterministic explanation of concrete, individual behavior with an explanation of the origins of complex human needs in terms of natural science. Perhaps psychoanalysis could serve as the basis for a scientific *reale Psichologie,* one that could overcome the nomothetic-idiographic distinction. (Luria, 1979, p. 23)

In emulation of the psychoanalytical writers, he conducted clinical research on free associations, but he mistrusted the results of such efforts, feeling that any conclusions he tried to reach about the flow of his subjects' thoughts were insufficiently grounded. As he wrote in his autobiography, "While I was able to fill notebooks with [a patient's] free associations, I was in no position to carry out my plan to use such data to capture the concrete reality of the flow of ideas" (1979, p. 24).

In response to this dissatisfaction he created a methodology designed to embody a psychodynamic theory of mind in an objective set of laboratory

procedures. The centerpiece of this methodology was an experimental technique that he called the combined motor method, which, he hoped, would provide a way of rendering Freud's clinical methods accessible to experimental treatment.

The fullest existing description of this work is contained in a monograph published in English in 1932 under the title *The Nature of Human Conflicts: Or Emotion, Conflict and Will.* Unfortunately, this fascinating book is not yet available in Russian; rather, Russian papers describing this experiment provided only a partial glimpse of the overall project. The book also provides a window on how Luria's early work was taken up and transformed under the influence of Vygotsky, and why Vygotsky would find in Luria such a useful colleague.

In the first chapter, Luria outlines his basic presuppositions and his experimental strategy. He explicitly rejects mechanical determinism, declaring: "The structure of the organism presupposes not an accidental mosaic, but a complex organization of separate system . . . [that] unite as very definite parts into an integrated functional structure" (Luria, 1932, pp. 6–7).

Insofar as this structure is the consequence of a long, complicated development, both ontogenetically and cultural-historically, and because the parts are integrated into a whole functional system, how can it be possible to isolate elements in this system for purposes of psychological analysis? Phrased differently, insofar as no two people are constructed alike, how could one possibly obtain valid evidence abut the thought processes of another person?

The answer that Luria provided was that other people's thoughts could not be observed *directly.* But they could be revealed *indirectly* insofar as they could be reflected in a publicly displayable, voluntary behavior. He phases his strategy as follows:

> We should on the one hand, produce the central process of the disorganization of behavior; on the other hand, we should try to reflect this process in some [other] system accessible and suitable for examination. The motor function is such a systematic, objectively reflected structure of the neurodynamic processes concealed from immediate examination. And there lies before us the use of the motor function as a system of reflected structure of hidden psychological processes. Thus we proceed along the path we call the combined motor method. (Luria, 1932, p. 18)

Work on the combined motor method is concentrated in the first two sections of *The Nature of Human Conflict,* which was published in 1932 in English, but contains a record of research from 1923 to 1930. I find the book fascinating reading in part because I know that at the start of this period, Alexander Romanovich was intent upon creating the combined motor method

as a model system for the study of the psychodynamics of individual thought. In doing so, he had achieved one model for resolving the crisis in psychology. By the end of this period, he was engaged with Vygotsky and Leontiev (who had participated in some of the early experiments on the combined motor method) in creating a new school of psychology founded on the principle that the mind is mediated through culture. From this perspective, the combined motor method sets up a mini "cultural system" and it is within the confines of this microcosm that one can study how culture serves as the medium of propagating ideas from one person to another. As Luria expressed this relationship at the end of *The Nature of Human Conflict*, "The analysis of complex cultural mechanisms is the key to the understanding of the simple neurodynamical processes" (1932, p. 428).

One sees many variations on this method in Luria's later work such as his studies on the development of self-control in normal and abnormal children; unfortunately, this work was carried out at a time when it was required to communicate in terms that sounded like Pavlovian theory, so the continuity between the early and later work is difficult to notice, or at least, it was for me.

Luria's romantic science

I do not desire to rehearse with you the many transformations that Alexander Romanovich and his whole generation underwent in the half century between the writing of *The Nature of Human Conflicts* and the publication of his autobiography. As chronicled in his autobiography, Alexander Romanovich worked in many areas of psychology, completed medical school, worked through the war as a neuropsychologist-therapist, went on to do basic research in the neurophysiology of brain functions, and wrote two small books chronicling long-term case studies that he had carried out with two remarkable men.

Luria (1979) began his autobiography with a discussion of the science of psychology he inherited, and how he had, from his earliest research, sought a way to combine the two psychologies, one experimental/generalizing, one descriptive/particularizing. He called this approach romantic science, which he contrasted with Classical Science.

> Classical scholars are those who look upon events in terms of their constituent parts. Step by step, they single out important units and elements until they can formulate abstract, general laws. . . . Romantic scholars' traits, attitudes, and strategies are just the opposite. They do not follow the path of reductionism, which is the leading philosophy of the classical group. Romantics in science want neither to split living reality into its elementary components nor to represent the wealth of life's concrete events as abstract models that lose the properties of the phenomena themselves. (1979, p. 174)

In writing about romantic science, Luria quoted a line from Goethe's *Faust* in which Mephistopheles tells the eager student, "Grey is every theory, ever green the tree of life," expressing his skepticism for the golden promises of theory. While editing Luria's autobiography, I read *Faust* to see what other insights into Luria's ideas I could discover there. One such passage struck me forcefully for its power to contrast the two sciences whose interactions have been a major topic of this book. The passage occurs elsewhere in the same act from the play. Mephistopheles advises an eager student on his future career, describing the consequences of following the path of science.

The conservation begins with Mephistopheles admiring the work of weavers, who create patterns, a process in which "A single treadle governs many a thread, And at a stroke a thousand strands are wed." Quite different is the scientists' approach, and quite different the result. In light of my discussion to this point, it would not be amiss to think of the scientist as a psychologist who pursues experimental, causal, explanatory, psychology and a weaver who follows the path of cultural-historical, observational-clinical, descriptive psychology.

> And so philosophers step in
> To weave a proof that things begin,
> Past question, with an origin.
> With first and second well rehearsed,
> Our third and fourth can be deduced.
> And if no second were or first,
> No third or fourth could be produced.
> As weavers though, they don't amount to much.
> To docket living things past any doubt
> You cancel first the living spirit out;
> The parts lie in the hollow of your hand,
> You only lack the living link you banned.
> (Goethe, 1988, p. 9)

Here we encounter yet another formulation of the basic issue underlying the crisis in psychology; two different logics of inquiry. Goethe highlights precisely the issue that Alexander Romanovich sought to resolve by formulating an approach that made the two methodologies moments in, parts of, a methodology that combines theory and practice through deep involvement in the lives of individuals over time. The important idea that when we are talking about human life processes, declarations about "firsts," the ultimate causes from which consequences flow, must always be suspect. This criticism is at the heart of Dewey's century-old criticism of the reflex theory of thinking. It is a position expressed in different ways by philosophers as apparently diverse as Bakthin, Dewey, and Mead, that "in the beginning was the act."

For the same reasons, romantic science as formulated by Luria does not allow a simple formulation such as "purposive psychology is concerned with goal formation, causal psychology is concerned with problem solution." Goal formation, no less than theory and practice, are two different moments of one and the same psychological act. Analysis must seek to include both moments and their dynamics.

Luria's fusion of the two different world views, or orders of reality, is clearly in conformity with Vygotsky's views about the conditions necessary to overcome the crisis of psychology summarized earlier: it is the field of the real life circumstances of real human beings that serves as the anvil on which the improvements in theory must be tested.

It is their success in bringing together the two ways of knowing that make Luria's longitudinal case studies significant beyond the fields of neuropsychology or cognitive psychology, any of the subbranches of psychology one cares to name. So, for example, Luria is able to use his knowledge of the general laws of memory to help Sharashevskii develop a way to "erase" memories of number sets when he was earning his living as a mnemonist who conducted several shows a night. He provides the brain-injured Zasetskii with cultural-psychological tools to remediate the way in which his own brain works to that he can read and write again. Such "helpful hints" were far more than the idiosyncratic, creative insights of a lucky investigator. They were the application of general psychological principles to the real lives of real people. Had he lived to have read them, Vygotsky would almost certainly have agreed that they represent demonstration proofs that the crisis in psychology can be overcome.

Generalizing Luria's approach to romantic science

In recent years, the best known champion of romantic science in the United States has been Oliver Sacks, who will be presenting his ideas to us later in this meeting. Oliver's deep involvement with his patients over a period of time is strongly reminiscent of Alexander Romanovich's approach and adds importantly to the range of abnormal brain-behavior relationships we can use to develop a more powerful theory of mind. According to Sacks, central to romantic science is that it treats analytic science and the synthetic biography of the individual case as essentially complementary, "The dream of a novelist and a scientist combined" (Sacks, 1987, p. xii). Equally important in my view is the fact that both Luria and Sacks are therapists who engaged their patients as human beings over long periods of time and attempted to demonstrate through practical amelioration of suffering the truth of the basic premises of their theories.

My own involvement in romantic science represents a generalization of Luria's views that retains a focus on individuals, but more directly addresses connections between individual human development and cultural-historical development.

A number of years ago, I began to engage in work with children in after-school clubs. There are many reasons for this choice, among them the fact that the United States is undergoing a crisis in the after-school care of children associated with the swelling number of mothers who have small children and who must work and no existing national or regional system of after-school care. But also important is the fact that by escaping the clutches of the school and entering onto the ground ordinarily inhabited by "the community," I could mix leading activities in ways that might just increase the prevalence of genuine developmental activity.

Some of the children with whom we work experience difficulty in school, others do not. Some come from homes where English is not spoken and educational experience of parents is limited. Others are said to be hyperactive or to suffer learning disabilities. I described the early stages of this work in an article on culture and development that was published last year in *Voprosy psikhologii.* There I presented an example of a specially organized reading activity for elementary school, which engaged several children, a teacher, and a teacher's aide. The procedure is a direct generalization of the combined motor method, and, at the same time, it creates a zone of proximal development for children's reading development.

In the course of doing that work, my colleagues and I began to experiment with the use of microcomputers as tools for creating educational activities for children. Subsequently, the success of that effort has resulted in a situation where I not only study the creation of alternative activity systems for diagnosing and remediating reading difficulties, but I also study the development of children over time while, at the same time, studying the development of the activity systems themselves. Our research has shown that every such system quickly constructs an "idioculture" that gives it a distinct form of being and has distinctive kinds of impacts on its participants. This approach to experimenting through the design of activity systems allows us to add a cultural-historical dimension to the romantic science methodology, while retaining the power to create "unimagined portraits" of both the children and the cultural-historically constituted activity systems of which they are a part.

For more than a decade, now, the after-school activity systems we have created have proved a fascinating medium for my generalization of romantic science. They have proved so beneficial to university students who participate in them, as well as to the community's children, that our approach

has been adopted by all branches of the University of California, several universities in Mexico, Sweden, and elsewhere. So I can safely report that in at least one of its guises, romantic science is alive and well in many parts of the world.

Alexander Romanovich ends his autobiography by commenting that the only "imaginary portrait" that remained for him to write about was himself, but that such a book would be different than all the others because

> There is no subject with exceptional abilities—I have none. Nor is there a specific capacity or a specific disaster. . . . People come and go, but the creative sources of great historical events and the important ideas and deeds remain. (1979, pp. 187–88)

I want to dispute this description. Alexander Romanovich had several exceptional abilities and specific capacities and he experienced a series of specific disasters, along with his whole generation. His special talents were the ability to combine an inquisitive and all-embracing scientific mind with a deep intuition about how to know another person's thoughts and feelings. He was an internationalist in science at a time and in ways that put him at risk. He was a model scientific mentor, giving of his time for tasks that were certain to be unrewarded. He created a laboratory within which people did not have to be afraid to interact with a foreigner in a society where such fear was normal. And he successfully planted his ideas so that they survived at least one journey of many years over many countries, to grow again in North America.

I may have come to Moscow in 1962 a methodological behaviorist. But I return here today, thanks to Alexander Romanovich, a cultural-historical activity theorist . . . for better and for worse.

Note

1. Vygotsky appears to draw heavily on Münsterberg's ideas about unification in practices as the requirement for uniting the two kinds of psychology Münsterberg (1914): "Unification is reached in applied psychology which speaks of the practical application of mental facts in the service of human purposes. The selection of these purposes is a matter of purposive psychology, the mental effects to be used a matter of causal psychology. They are thus joined in that practical part which comes nearest to real life" (p. 16).

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