

Remembering Alexander Luria...

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Вспоминая Александра Лурию...

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It has been almost exactly 60 years since my first meeting with Alexander Romanovich Luria. I was a 24-year-old American psychologist with a PhD in mathematical learning theory and a participant in the recently formed post-doctoral exchange program between the USA and the USSR (pic. 1). He was a 60-year-old Soviet psychologist who had survived the Purges, survived World War II, and survived Stalinism. He was also an internationally influential psychologist specializing in neuropsychology. I had not the faintest idea of what to expect from this year abroad. I could not imagine that my post-doctoral year in Moscow would set in motion a sequence of experiences that would entangle my life with his, primarily as part of his biography but also of my own.

My essay is divided into two parts. The first is a narrative of how Luria came to have such a deep influence on my subsequent career. The second is a reflection on the complicated relationship between what I knew and what I could say given the historical circumstances at the time.

What a difference a year can make!

During that first year in Moscow, Luria arranged for me to participate in the research taking place at several different laboratories, each involved in the use of conditioned reflex methods for the study of learning. I also followed him on his grand rounds of the Burdenko Institute, where I participated in lab discussions and observed how he interacted with individual patients. He was familiar with the existing Anglo-American test



Fig. 1. Michael Cole in his dormitory of Moscow State University (1962)

methods for psycho-diagnosing brain injuries, but he did not hold them in high esteem. Trained as a physician, he had worked out methods for diagnosing brain injury that were derived from his theoretical framework. To me, largely ignorant of that theoretical framework, he seemed like a magician pulling rabbits out of a hat. For each case, his diagnostic procedures and strategy of rehabilitation were geared to the individual patient in a flexible, but clearly theory-driven way. He entered the field at a time when modern imaging methods were entirely absent; as a consequence, his diagnoses served as guides for the subsequent surgery.

It was a fascinating year in every respect. Living in a student dormitory at Moscow State University (MSU) provided a unique position from which to engage with Soviet society as represented by its academic elite. My

cohort and I made lifelong friendships that have survived a tumultuous half century. However, when we left Moscow, I was anxious to get my career back on track after a year away, a year that my peers considered a career-threatening diversion. Then, an event occurred that both changed the trajectory of my own life and reconnected me with Luria in a way that has continued to evolve ever since.

Not long after returning from Moscow, a committee of mathematics educators selected me to make a month-long trip to Liberia as part of an international project on mathematics education. They needed an experimental psychologist to lend support to the project head. I was the only candidate they had who could travel on short notice as I possessed a valid passport. With scant preparation, I found myself in the Liberian hinterland.

That first experience of a rural, non-literate, subsistence culture forced me to re-think a lot of my prior assumptions about the study of psychological processes. As a fresh young experimental psychologist, I had to somehow reeducate myself if I was going to take cultural context seriously in making claims about those processes. That re-education began with Luria.

Just before we left Moscow, AR told us a little about his project in Central Asia in the early 1930s. One finding in particular stuck out in my memory: The adults he studied appeared incapable of reasoning about logical syllogisms. I began to correspond with Luria to find out more about his project and how it related to the work he had introduced me to during my post-doc. Initially I got nowhere. He was busy writing about other aspects of his work and the data required further analysis.

Fortuitously, Luria requested that I return to Moscow in the summer of 1966, just as I was planning a second round of research in Liberia on the cognitive consequences of education. He asked that I work with the organizing committee of the upcoming International Congress of Psychology to provide assistance dealing with the larger than expected number of English speakers. In return, he offered to spend an hour a day with me going over his Central Asian data while I brought him up to date on recent research in the study of culture and development.

This convergence of my keen interest in the role of culture in human development with Luria's long-buried treasure trove of data provided one key to understanding Luria's enduring influence in my life. No less important was my more mature understanding of the overarching theoretical framework that he had been urging upon me from the beginning ("read Vygotsky"). That was the theoretical framework that created the bridge between the linked data from cross-cultural research on historical change and the Pavlovian study on the development of word meaning that had drawn me to Luria in the first place. He subsequently published this research, first in a small, specialized compendium of essays on history and psychology in Russia, then in a translation of that article for publication in the USA, and finally as a full monograph.

Our subsequent research incorporated a number of the tasks that he had used years before. He, in turn ar-

ranged for Peter Tulviste to carry out a new series of studies in a still-remote part of Siberia. Peter's work then influenced my own, both replicating earlier findings and extending them. At the same time, it forced me to reconcile my insistence on the primacy of cultural context in development (a relativist view) with the idea of cultural evolution and historical progress. At present, this view is referred to as Contextual Cultural Historical Psychology, or Cultural Historical Activity Theory.

Following a decade and a half of cross-cultural research, the thrust of my inquiry and my family circumstances required changes (it is impossible to conduct proper cross-cultural work without a deeper immersion in the culture one is studying, which is incompatible with a normal family life). My goal to fuse psychology and anthropology had to be pursued through other means. Further progress, I concluded, required me to conduct my research in a culture I knew well — my own.

This shift made it possible to tackle a problem where-in social issues in the USA coincided with my concerns about a basic methodological problem in psychology that arises whenever consideration of culture enters the picture. That is, the ecological validity of psychological tests and experimental procedures. In the USA, this scientific concern expressed itself as a critique of the use of IQ tests as measures of intelligence and interpreted as evincing racial variations. In cultural-historical theory, this appears in endless arguments and misunderstandings concerning the idea that abstract concepts are higher than other, 'everyday' forms of thought and the conviction that one's own society is more virtuous than the Other.

To address this issue, we conducted research on variations in children's problem solving depending upon the social context; to what extent it is possible to identify and compare the processes identified in psychological tests to determine whether they are representative of processes that take place in everyday life. In the course of that research, we encountered a child clinically identified as learning disabled. One group of researchers observed and videotaped his participation in classroom activities and a set of specially selected tests. Another group of researchers observed the child as he participated with his classmates in afterschool activities which demanded constant reading. The two groups of researchers deliberately avoided discussing their findings with each other during the first few months of data collection.

In the friendly hurly burly of baking the cake, researchers had failed to notice anything unusual about the child's ability to learn. To explore how this disconnect arose, we rearranged subtle changes in group organization and took advantage of the normally occurring variations. Now, when we observed the video footage covering a range of situations, it became clear that the child had an excellent grasp of the overall task but struggled to read when the social circumstances left him no choice other than to struggle unsuccessfully in front of his peers. He was a master of inserting himself into the group activity in a strategic manner that obscured the source of his difficulty. Such results chimed both with our analysis of the child's specific difficulty in reading

(even simple decoding was a chore) but bore no correlation to the idea of a general learning difficulty.

These observations motivated our first intervention efforts to directly combine our contextualist learning approaches and Luria's cultural historical approach. We sought to create small group activities designed to serve both as a diagnostic procedure and a remedial procedure for children failing to acquire literacy in the first six years of schooling. As part of this activity, we included a combination of Vygotsky's concept of dual stimulation and Luria's combined motor method to create an after-school activity for children who were clearly failing to acquire literacy. The specifics of the activity are not important in the current context, but two conclusions are worth emphasizing. First, this work coincided closely with Tatiana Akhutina's prescriptions for creating remedial activities for such children, indicating their common roots in Luria's ideas. Second, we realized that once we took up the challenge of teaching "these unteachable" children, our social obligations to the subjects of our research were altered significantly. Suddenly, we became responsible for the children's welfare. Our roles as objective experimenters were fundamentally breached by our obligation to make a difference. Now we had to do more than make claims about zones of proximal development based on average differences between groups of children on some standardized measure. Luria would have understood the difference.

Luria ends his autobiography with a description of two case studies. These (one with a mnemonist, one with a brain injured engineer) were unlike his studies of Uzbeki peasant reasoning or the role of speech in the development of self-control, or even most patients he saw as a clinical neuropsychologist. Each case extended over many years and in each case, he acted as both diagnostician and therapist. It is through the mixing of these two roles that the form of psychological research he referred to as romantic science emerged.

In my view, to understand the theoretical importance of Luria's version of a romantic *science*, it is important to realize that this mode of research allowed him to satisfy his lifelong ambition to resolve two central issues that had dogged psychology since its inception in the 19th century. Those being, how are we to reconcile natural science with the cultural nature of humankind and how are we to reconcile nomothetic laws that apply to populations of humans with the reality of our individual, idiosyncratic, lives?

I first encountered the idea of romantic science in the early 1970s in the process of editing Luria's autobiography. In the following decades, this idea has come to describe my own attempts to combine psychology with anthropology, experiment with observation, the personal with the social, and theory with practice.

The Said and the Unsaid in Biographical Narratives

A special challenge in writing about Luria arises from a confluence of his own distaste for writing about

himself outside of his role as a scientist and his life-long residence in the USSR. From his first autobiographical writing in the early 1970s, he insisted that

It certainly does not seem essential that a participant in the volume *A History of Psychology in Autobiography* write autobiographical notes on the assumption that he must recount all the events of his life. This would be not only insufficiently modest but also beside the point. A series of such auto biographical sketches would not be likely to result in a true picture of the history of science. ... Individual people come and go, contributing some, to them insufficiently distinctive, bits of knowledge to the general enterprise. (p. 253)

To emphasize the irrelevance of his personal autobiography in the history of science, he followed these declarations with a barebones history of his family origins, his scientific accomplishments, and the honors he had received.

Only then did he turn to a description his own research program.

He focused the narrative almost entirely on the research connected with the development of Vygotskian theory, mentioning his cross-cultural research only in passing. He describes the social context of his research only in the following general terms.

The scientific atmosphere of Soviet Russia in the twentieth century, as many authors have noted, was very unusual, not to say unique. The greatest social revolution ever to take place had just occurred. It had occurred in an economically backward country but one which possessed strong intellectual traditions (p. 255).

Luria's 1979 autobiography provides a greatly expanded account of his scientific life. But it contains virtually no mention of the social or personal context, other than to emphasize the enormous opportunities that the Revolution opened up for his generation. As a consequence, the reader is left with no understanding of the logic connecting his different projects, other than his meeting with Vygotsky and the development of cultural-historical psychology. I travelled to Moscow specifically to discuss the manuscript with him, but he deflected my questions.

When I wrote the introduction to the English edition of his autobiography, I was well aware of Luria's aversion to discussing his personal circumstances in any writing about his work. I had translated his earlier autobiographical essay. As a matter of conscience, I felt obligated to adhere to such an explicit wish. Accordingly, I deliberately wrote an introductory essay on the historical context of his career in purely scientific terms, as he would have wanted. In the epilogue, I described my year in Moscow and the early years of my involvement with his theoretical framework. For that, I allowed myself to provide sufficient information about the circumstances of his life for the average American reader to get at least a glimpse of the common logic underlying his important projects which, on the surface, appeared to have very little to do with one another.

I was almost embarrassed at how well the historical introduction turned out. I managed to write strictly an account of the scientific historical context. The censors

removed only one reference (to Stalin and events in the early 1950s) that I had assumed would be permissible 25 years later. But the epilogue was a different matter; all references to the massive social events that provide context for Luria's apparently random choice to study one topic or another had to be removed (no peasants in Uzbekistan, no twins, no developmentally anomalous children, only pre-Vygotsky and post-Vygotsky).

The ensuing argument brought the publication of the book to a halt. The Luria family insisted that my epilogue be printed as it was. After a year of negotiations, Elena Luria asked Vladimir Zinchenko, himself a prestigious cultural historical psychologist and friend of the Luria family, to intervene. The latter, Volodya, minimized the omissions so artfully that any Russian reader would be able to fill in the blanks, but only the most informed and careful American reader could glean a rough idea of the dramatic circumstances of Luria's life and their relationship to his work.

Following the demise of the USSR, I began to collaborate with Karl Levitin, a prominent science journalist who wrote extensively about Vygotskian psychology and was a friend of the Luria family. We arranged to reprint the original autobiography and my two essays, this time adding our own contemporary understanding of what I had written at the time. I am not going to repeat our account of the confluence of events. Those who wish to read it may find it at luria.ucsd.edu

In recent decades, several scholars have written their own accounts of Luria's life and career. Rather than repeat what others have written I will repeat a discussion

with Tatania Akhutina in preparing this essay. I complained that I had written about Luria too often and had nothing new to offer. She replied by saying, "But we have been good students, haven't we?" We have certainly tried.

* * *

When I set out to write about Luria in the epilogue to his autobiography, I began with the following epigram ascribed to an Athenian bard, who earned his living from the patronage of important, wealthy men whose praises he sang in return for his supper.

So I shall never waste my life-span in a vain useless hope, seeking what cannot be, a flawless man among us all who feed on the fruits of the broad earth. But I praise and love every man who does nothing base from free will. Against necessity, even gods do not fight. -Simonides

As I learned from the many visits I made to Moscow for the subsequent academic exchange programs that came to an end along with the USSR itself, Luria was not a flawless man. Rather, he was, as Karl used to say, "A decent man in an indecent situation," a high complement.

I want to end these remarks with the following invitation. Think back over this essay. Note that I have provided this account without any details of the harrowing personal events in Luria's life that allowed him to outlive Stalin. I'd like to think that I too could have withstood such terror and remained a normal human being. Could you?

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