

Remembering Alexander Romanovich
Michael Cole
UC San Diego
July, 2022

It is almost exactly 60 years since my first meeting with Alexander Romanovich. I was a 24 year old American psychologist with a PhD in mathematical learning theory – a participant in the recently formed post-doctoral exchange program between the USA and the USSR. He was a 60-year-old Soviet psychologist who had survived the Purges, survived World War II, 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, both as part of his biography, and then in my own.

My essay is divided into two parts. The first is a narrative of how ARL 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. (I provide links at the end of this essay to relevant materials upon which these remarks have drawn).

What a difference a year can make!

During that first year in Moscow, A.R. arranged for me to participate in research in several different laboratories, each involved in the use of conditioned reflex methods for the study learning. I also followed him on grand rounds where I observed how he interacted with individual patients, and I participated in lab discussions. He was familiar with existing Anglo-American test 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 provided a unique position from which engage with Soviet society represented by its academic elite. We 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 plans and reconnected me with ARL 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 support the project's initiator in an ongoing project. I was the only candidate they had who could travel on short notice because I possessed an active passport. With little 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

process. As a newly minted experimental psychologist, I had to somehow reeducate myself if I was going to take cultural context seriously in making claims about psychological processes. That re-education began with ARL.

Just before we left Moscow, AR told us a little about his project in Central Asia in the early 1930's. One finding in particular stuck out in my memory: The adults in his research appeared incapable of reasoning about logical syllogisms. I began to correspond with A.R. 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 would need further analysis.

Fortuitously, ARL 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 requested that I work with the organizing committee of the upcoming International Congress of Psychology to deal with a larger than expected number of English speakers and the need for assistance. In return, he offered to spend an hour a day with me going over his Central Asian data and bringing him up to date on recent research on culture and development.

This convergence of my keen interest in the role of culture in human development with ARL's long-buried treasure trove of data provides one answer to the question of ARL'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"). It was the theoretical framework that created the bridge between the link data from cross-cultural research on historical change to the Pavlovian study of the development of word meaning that had drawn me to him in the first place. He subsequently published this research, first in a small, specialized compendium of essays on history and psychology in Russia, then translation of that article for publication in the USA, and finally a full monograph.

Our subsequent research incorporated a number of the tasks that he had used years before. He, in turn arranged for Peeter Tulviste to carry out a new series of studies in a still-remote part of Siberia. Peters' work then influenced my own, both replicating earlier findings and extending them. Simultaneously, 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 logic of my inquiry and my family life circumstances brought my empirical work in other countries to an end (I found it impossible to lead a normal family life in the USA and conduct proper cross-cultural work in many thousands of miles and away). My efforts to fuse psychology and anthropology had to be sought by other means. Further progress, I concluded, required me to conduct my research in a culture I knew well – my own.

This shift in circumstances made it possible to tackle a problem where social concerns in the USA coincided with our concerns about a basic methodological issue in psychology whenever consideration of culture enters the picture: the question of the ecological validity of psychological tests and experimental procedures. In the USA the scientific concern expressed itself as a critique of the use of IQ tests as measures of intelligence, interpreted as racial variations. In cultural historical theory it appears in endless arguments and

misunderstandings as the idea that concepts are higher than other, ‘everyday’ forms of thought and there their own society is more virtuous than the “Other.”

We were conducting research on variations in children’s problem solving depending upon the social context; to what extent is it possible to identify and compare the processes identified in psychological tests to determine if they are representative of processes that take place as a part of everyday life. In the course of that work we encountered a child clinically identified as learning disabled. One group of researchers observed and videotaped classroom activities and a set of specially selected tests, One group of researchers observed the child as he participated with his classmates in afterschool activities saturated with the need to read. The two groups deliberately did not discuss their findings 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 his ability to learn. To explore how this mismatch occurred, we rearranged subtle changes in group organization and took advantage of normally occurring variations. Now when we observed the video in a range of situation, 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 both connect with the special constraints of difficulty (even simple decoding was a chore) but separate them clearly from the idea of a general learning difficulty. And it made us wonder about his special ability as a drummer.

These observations motivated our first intervention efforts directly to combine our contextualist learning approaches and AR’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 6 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 markedly failing to acquire literacy. The specifics of the activity are not important in the current context. Two conclusions are worth emphasizing. Firstly, this work coincided closely with Tatiana Akhutina’s prescriptions for creating remedial activities for such children, indicating their common roots in AR’s ideas. Secondly, we realized that once we took up the challenge of teaching “the unteachable” children our social obligations to the subjects of our research were altered significantly. For the first time, we became responsible for the children’s welfare when they were in our hands. 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. ARL would have understood the difference.

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

In my view, to understand the theoretical importance of Luria’s version of a romantic science, it is important to realize that this mode research allowed him

to satisfy his lifelong ambition to resolve two central issues that had dogged psychology since its inception in the 19th century: how are we to reconcile natural science with the cultural nature of humans and how are we to reconcile nomothetic laws that apply to populations of humans with reality of individual, idiographic, lives?

I first encountered the idea of a romantic science in the early 1970's in the process of editing ARL'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 ARL arises from a confluence of his own distaste for writing about himself outside of his role as a scientist and his residence in USSR for his entire life. From his first autobiographical writing in the 1960's 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 autobiographical 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.

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

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

When I wrote the introduction to the English edition of his autobiography, I was well aware of ARL's aversion to including his personal circumstances in writing about his work. I had translated his earlier autobiographical essay. As a

matter of conscience, I felt obligated to follow his explicit wishes. Accordingly, I deliberately wrote an introductory essay on the historical context of his career in purely scientific terms- as he would have wanted. For the Epilogue, in which I described my year in Moscow and the early years of my involvement with his theoretical framework. 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 important projects that on the surface appeared to have very little to do with each other.

I was embarrassingly good at writing the historical introduction. I managed to write the entire account as a history of only the scientific historical context. The censors removed one reference (to Stalin and events in the early 1950's) that I assumed would be permissible 25 years later. But the epilogue was a different matter; all references to the massive social events that explain the AR's apparently random choice to study one topic or another had to be removed (no peasants in Uzbekistan, no twins, not developmental anomalous children, only pre-Vygotsky and post-Vygotsky. The ensuing argument brought 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. 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 ARL'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 re-print 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 [WEBPAGE URL](#).

Rather than repeat what and others have written about ARL I will repeat a discussion with Tatania Akhutina in discussing this essay. I complained that I had written about ARL 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 AR in the epilogue to his autobiography, I began with the following epigram ascribed to an Athenian bard, who earned his living by the patronage of important, wealthy men who praises he sang in return for his super.

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 in the continuing academic exchange programs that ended with the USSR, A.R. was not a flawless man. Rather, he was, as Karl used to say, "A decent man in an indecent situation." It was 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 life

threatening personal events in his life that allowed him to outlive Stalin. Would that I could have withstood the terror as a normal human being. Would you?