For the past forty years, anyone wishing to be present when I was conducting research was most likely to find me in some sort of after-school setting playing with kids of various ages and their undergraduate buddies from UCSD. Whatever the setting, a Boys and Girls Club, or a housing project learning center, or a school-room made available once classes were over, I was likely to be found moving around the room, kibitzing with the children and their undergraduate partners, or watching anxiously as a 6 year old measured out the ingredients as we started to make a cake. To a newcomer, the comings and goings usually seem somewhat chaotic and noisy. It doesn’t look like a classroom and it seems impossible to measure cognitive or social development in such circumstances. In fact, it doesn’t look like research at all! Instead it looks for all the world like John Dewey standing in the kitchen of the Lab School in Chicago creating a science curriculum.

Over the years, people have wondered at my seeming shift in careers, from experimental, mathematical learning theorist to mushy developmentalist who seems to ignore the most common rules of how to conduct research on learning and development. How did Cole come to this sorry pass? I call the kind of research I do a form of romantic science, and this essay is the explanation of how I came to adopt it.

To begin in the middle, I found myself at Indiana University where I conducted experimental studies of learning with Bill Estes. My subjects were rats and college sophomores. Developmental psychology was not yet on the American psychological radar, and the thought of studying the process of education had never occurred to me.

To obtain a Ph.D in psychology at the time I was required to have an outside major and to pass two foreign languages. Luckily, there was support for grad students who wanted to learning Russian and the historical links between Russian and American Psychology (Pavlov being the leading figure) made that topic of natural interest. While learning Russian and persuing my Soviet Studies minor, I encountered an article by Alexander Luria that brought together my behaviorist background with the study of language, in particular, the acquisition of word meaning. In my eyes, Luria was studying learning, he was using clearly interpretable experimental techniques, and the results were exciting.

Luria was the hook. A post-doctoral year in Moscow set in motion the odd sequence of experiences that would lead me into a wholly different way to conduct empirical research on learning and development.

While in Moscow, I conducted my scientific experiment on semantic conditioning among patients with temporal lobe lesions. It produced only mud. Other experiments, all
within a psychophysical/experimental tradition, were published in Russia, a rarity at the time. But I also followed Luria on grand rounds and observed how he interacted with individual patients. He was familiar with existing Anglo-American test methods of psychodiagnosis of brain injuries, but he did not hold them in high esteem. Trained as a physician, he had worked out some simple diagnostic methods concerning brain injury that were derived from his Vygotskian theoretical background. But he did not use them in a rigid way, rather, he tailored how he carried out his diagnoses to the individual patient. To me he seemed like a magician pulling rabbits out of a hat. Each time his diagnostic procedures and strategy of rehabilitation were geared to the individual patient. At family tea before we left Moscow, I learned that once upon a time he had conducted research with peasants in far off Uzbekistan.

Not long after returning from Moscow, Pat Suppes and Jerry Bruner threw me into the Liberian hinterland, active passport in hand, because of my presumed knowledge of mathematics and they were rolling out the new mathematics. That first experience of a rural, non-literate, subsistence culture was a jolt. As a newly minted professional, it forced me to re-think the modifications to scientifically accepted experimentation that had to be made if one was going to take cultural context seriously in making claims about psychological processes. And in pursuing this question, I was forced to rethink the nature of psychological experimentation in general. I had fallen into the pit of contextualism and the sticky problem of ecological validity.

Simultaneously, I was being pushed to think more and more deeply about the overall intellectual enterprise that Luria had engaged in. For the first time I began to take the historical origins of modern schooling seriously along with a concern that data suggesting a generalized cognitive advance as a consequence of school was badly over-generalized and misleading. My increasing concern with culture as an historical phenomenon then got mixed together with Luria’s neuropsychology. I became engrossed in his work on rehabilitation of injured brain functions when my colleagues and I began to study clearly diverse children all of whom fit a social category called “learning disabled.”

A next central event in my evolution was the program of research organized by my colleague Peg Griffin. Peg invented a variety of Luria’s method of dual stimulation to create an after-school activities for children who were markedly failing to acquire literacy. These activities were carefully scripted “plays” in which children and adults used theoretically selected materials to work out the meanings of written paragraphs. The specifics of the activity are not important in the current context. What is important is that we had become responsible for the children’s welfare when they were in our hands. Our roles as objective experimenters were fundamentally subverted. 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. We were obligated to demonstrate what it means to create a zone of proximal development, how the process works in circumstances beyond the ordinary dyads of experimental studies.

And now romantic science makes its entrance into my work in a serious way. 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 he acted as both diagnostician and therapist. It is in the mixing of last two roles that romantic science emerges.

In my view, to understand the importance of Luria’s approach, which Oliver Sacks referred to as “the dream of a novelist and a scientist combined,” it is important to realize that this research allowed him to satisfy a life long ambition to resolve a central issue that has 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 to individual, idiographic, lives. For Luria, romantic science meant “both/and,” the complementarity of analytic “timeless” science that murders to dissect and a synthetic, time-bound, human biography to give it life.

Which begins to explain my rationale for hanging out and participating in after-school settings with kids of different ages including college students in settings where there is lots of interesting stuff to do. My professional rationale for my activities is that my colleagues and I are engaged in “cultural-historical design experimentation.” Fittingly, our first such undertaking involved Ann Brown and Joe Campione who bravely withstood the chaos of invention and went on to do their own, seminal, form of design experimentation.

What perhaps distinguishes our manner of conducting experiments by design is our adoption of a “life course” approach to this form of design research. By analogy with Vygotsky and Luria’s insistence that the study of human development should encompass both periods of growthand decline during ontogeny, we believe that the entire life course of the designed activity is important to study in order to learn its properties, which are constantly changing, however permanent they may have seemed. This means, of course, that in so far as a designed activity turns out to be robust, one needs to continue to search for the sources of its continued development in every shifting ecology.

Which more or less explains how I got hooked on Romantic Science as a mode of research into the study of human development, and that peculiar institution called Modern Schooling. It welcomes “evidence based” research, but it thinks reflexively about the shortcomings of its data. Such reflexivity comes from directly participating in some version of the designed activity so that you, the experimenter, can feel the flow of the activity you have helped to create.

Large societies are difficult to govern and social scientists-as-technicians cannot avoid being caught up in the construction of instruments of governmentality. But as part of designing an activity for someone else’s own good, its scientifically useful to get into the middle of things so you can feel the pains as well as the gains.