

Methodological Challenges and Continuing Social Concerns

A Reply to Serpell and Wertsch's Comments

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It is always interesting to discover how one is interpreted by one's peers in an institutionalized setting such as the pages of *Human Development*, and I am doubly grateful to the editor and the commentators for this opportunity to re-reflect on the issue of the developmental consequences of schooling. The fact that my commentators have generally picked different aspects of the overall presentation as the focus of their comments makes my job somewhat easier by allowing me to consider them more or less separately before commenting on a major shared concern.

Let me begin with the methodological issue that provided the major focus of a great deal of my own work over the years: How, given that institutionalized public basic schooling (IPBS in Serpell and Hatano's terminology) is a specially organized form of experience that is not shared by all developing humans, can we reach firm conclusions about its *general* cognitive consequences? The logical pre-requisites for reaching such judgments (given the logic of explanatory science) are that we compare people (generally, but not necessarily, children of different ages) who have, or have not, experienced IPBS for different amounts of time. In addition, such people should be chosen at random and the tests used as our proxies for cognitive consequences should be equally familiar to both groups.

The first pre-requisite, random assignment, could be achieved only under the most draconian social circumstances, so various proxies, 'natural experiments,' are used in their stead. The decision of external agencies such as AID to build roads along certain routes in Liberia in the 1950s and 1960s and place schools in towns along those routes, or the Mexican government's decisions to place schools in some but not in other communities in rural Yucatan provided such natural experiments. One had to be cautious and take into account possible contaminating factors (roads change more than the presence or absence of a school), but a rough approximation to random assignment could be achieved.

The second pre-requisite, finding dependent variables that were equally experienced by those who had been to school and those who had not, posed the problem that absorbed most of my concern. Historical analysis of the tasks used to

'measure' cognitive development indicated clearly their close relationship to schooling and development in European and American schools, which were themselves linked to historically specific forms of work and social life. When we found, as we often did, that increased performance on such tasks ('cognitive development') increased as a function of years of schooling, not age, how could we know the cause of such changes? Was it simply a matter of practice makes perfect in a very narrow sense (we gave the example of an apprentice carpenter learning to saw straight lines)? Why would we expect someone with no practice sawing to saw straight lines or someone who learned to saw straight lines to show generalized superiority in psychomotor development?

The answer, logically speaking, was obvious. Find cognitive tasks that schooled and non-schooled children from similar towns experienced with equal frequency as part of their common life and use those tasks to determine if schooling experience transformed the modes of thinking, remembering, etc., employed when these tasks were encountered. The difficulty, of course, was that outside of specially designed, social constrained, social engagements called 'cognitive tasks' it was extremely difficult to identify, let alone study, such tasks and the behavior of people who engaged in them. It is this recognition that led us into a decades-long study of the issue of the study of ecological validity and the nature of research on cognitive development which I mentioned, but did not pursue, in my paper.

And it was this same recognition that kindled my interest in the work of Le Vine and his colleagues because they solved the logical methodological problem in a manner I had not pursued: by tracing the effects of school attendance on the next generation. (I should mention here that another solution to this problem was the application of the 'school cutoff strategy' by Morrison and his colleagues, which took advantage of rules specifying the birthday of children relative to the start of first grade to provide an alternative solution, although one that could only be applied, strictly speaking, for one year. For a summary of this work see Cole, Cole, & Lightfoot, 2004).

It is in this context that I want to comment on Jim Wertsch and Robert Serpell's commentaries regarding various aspects of the consequences of schooling. Wertsch is certainly correct in commenting that it is useful to view those who participate in IPBS as engaged in a particular kind of language game that privileges practices that in turn promote the desire to use, if not the ability to use, new semiotic potentials or speech genres. In my focus on seeking to solve the methodological problem of treating schooling as an independent variable in the brave new developmental sciences, I neglected to emphasize that new forms of activity always require new functional systems of thought, and thereby promote a larger intellectual tool kit. In fact, my colleagues and I were at some pains to note that when schooled/non-schooled comparisons were made, results were not uniformly in favor of the schooled populations. Rather, they seemed to be related to more specific modes of transforming information, treating language in special ways that put aside common sense in favor of logical entailments 'in the words themselves' and seeing classes of problems as related to each other, such that experience with one set of problems promoted better performance on other problems 'of the same kind' (within the framework of the game of schooling, to be sure).

As a social practice that provides young people with intellectual resources for dealing with their lives in modern, industrialized states, IPBS carries with it special

uses of language and the ability to talk to bureaucrats, to read government forms, write responses to those forms as well as (a limited) ability to think about language are some of them. Moreover, learning to play the language game of using words to talk about hypothetical worlds, and perhaps to create systems of calculations on such hypothetical worlds as n-dimensional spaces and black holes has certainly become a powerful force in the world, a force that returns in other kinds of games, some of which could be considered development enhancing, others not so benign.

In this regard, Robert Serpell is incorrect in assuming that, 'In this paper, Cole acknowledges that in the larger scheme of human affairs, correlations, albeit loose, may be more significant than his earlier analyses conceded.' Quite the opposite, my major disciplinary interest in the work of Le Vine et al. was that it fulfilled the requirement my colleagues and I had identified as logically necessary for reaching conclusions about the developmental consequences of schooling. It identified a common task engaged in by schooled and non-schooled people alike (females in particular) and demonstrated differential consequences of their attendance at school in more or less identifiable cognitive tasks (such as the ways in which they talked and explained things to their children, dealt with health clinic personnel and procedures, etc.).

Granted this methodological point, which I take to be important in understanding both developmental, cognitive psychology and the developmental consequences of schooling, let me go on to comment on other, related issues raised by my commentators.

Wertsch is certainly correct that in pointing to the ways that modes of discourse learned in school might help women deal more effectively with bureaucratic state institutions I was pointing at one of the language genres which, evidently, IPBS promotes. I worry, however, when the language games employed in IPBS are characterized as the ability to 'operate in the realm of abstract, decontextualized word meanings.' Here my tendency to slip into a mode of inquiry that assumes that non-educated people are capable of intellectual actions that they do not manifest in situations ordinarily used by developmentalists to measure intellectual development has a down side: it is indeed vulnerable to the interpretation that there are mental abilities 'inside' all biologically normal people by virtue of common experiences in the world, but which are differentially assembled according to culture-specific circumstances. If IPBS is not a universally encountered circumstance, there is every reason to believe that the intellectual abilities it fosters will not be universal. The issue, as Wertsch correctly points out, is to identify what is unique about such activities, and the games they foster. I apparently did not make this point clearly.

But the mirror image problem (perhaps fostered by IPBS?) is to assume that there is such a thing as 'abstract, decontextualized word meanings.' In other circumstances, inspired by both Wittgenstein and Rommetveit, Wertsch has warned us to distinguish between mediational means that can be used in a wide variety of settings and decontextualized meanings. One can appreciate that the ability to write or to analyse events on the basis of language, alone, outside of the relevant referential context, are semiotic practices that can be used in a variety of settings, and in this sense, these semiotic means are (relatively) decontextualized. But this is very different from the claim that word meanings can exist outside of a context of use which I believe (and I believe Wertsch believes) to be, in principle, impossible. The irreducible tension between active agents and their cultural tools brings with it the

parallel assurance that all meaning is constituted by, as it constitutes, the context in which it is made manifest. Caught in the language game of scholarly discourse about language, we all have to be careful lest we break our own rules, context-bound as they are.

Turning to another point that is a shared concern of both commentators and myself, I think Serpell overstates the extent to which I believe that IPBS mobilizes progressive change in third world countries. In fact, a point that unites the original article and both commentaries is our shared concern with the manichaeian nature of IPBS. Wertsch's invocation of the professor as the 'doctorate d'état' and the idea that IPBS has displaced the monopoly of legitimate violence (pencils do the work of stilettos) echoes many of my concerns and those expressed by Robert Serpell in his own writing on schooling. Yes, the IPBS model 'confers advantages', as Serpell states and the work of Le Vine and his colleagues documents. It provides access to, and perhaps the motivation to engage in, language about language that provides purchase on understanding the welter of our experience. I take it to be a good thing when children lead longer, healthier lives, and if maternal schooling furthers that end, it is a fact to be celebrated. But as many have commented, failure is *constitutive* of schooling in the model of IPBS. So correlations showing improved health, longevity, and school success, based as they are on *distributions of scores* are equally about the mothers who drop out of school, who do not get health care, and could not find clean water for their babies to drink even if they knew all about the germ theory of disease. Even those mothers who have made it through 6 years of schooling do not, by standards acceptable to any readers of these words, provide lives for their children that could be considered the consequence of a 'virtuous cycle.' Rather, many, perhaps a majority, live lives of privation and fear of violence. A statistical difference of several IQ points and a lowered infant mortality rate do not put a chicken in every pot or clean water in one's local water supply.

Here the issue of loose correlations does indeed come into play. A decade ago Americans envied the Japanese their high test scores in mathematics and low unemployment. Today Japanese test scores have not gone down, but unemployment has certainly gone up and so has teenage prostitution, as well as violence against peers and parents.

The relation between research results and social policies, let alone social outcomes, is indeed a loose one. Here I confess myself less optimistic than Serpell, perhaps because I live in a country where public education has been steadily commodified and manifest inequalities are routinely rationalized away. I am particularly doubtful of real causal links between research results and national policies. I do not believe it was the arguments of Frederick Douglas, Mahatma Gandhi, and the other great individuals he names that brought about change through their influence on decision-makers. I believe, instead, that it was the unbearable suffering of the underclasses made manifest in the streets and the recognition of the middle classes and those in power that change was inevitable at the risk of their own lives that created the conditions for change.

I would ask anyone who thinks that educational reform that manifestly promotes human development will be easy to look back at figure 1. It is more than a pun to realize that the birthplace of IPBS was in a city like Ur. Ur, the society where schooling first made its appearance was, a Soviet archeologist once told me

(a person who knew the phenomenon up close and personal from his own personal experience), one of the most thoroughgoing totalitarian states in world history.

That realization makes Serpell's experimentation with new modes of education 'on the periphery' all the more important. As the late Steven J. Gould reminded us, new species often get their start in small, obscure niches. But it worries me that the current leaders of those currently peripheral countries were themselves educated in the IPBS model and often more convinced than the doctorat who taught them of the future good of the state via a more efficient application of the IPBS model, so that no country, like no child, will be left behind.

Meantime, like Serpell, I search for my own niches – de-centralized, embedded in the community, and on the periphery of the legitimate source of violence, whether from the local police department or the school. I am not sure that in so doing I am providing an alternative set of practices that may, in conjunction with concomitant social forces, bring about changes in any structures of power. But at least they are humane places for my own development, and if Serpell is correct, and in a modest way, for the development of others as well.

Reference

Cole, M., Cole, S. & Lightfoot, C. (2004). *The development of children* (5th ed). New York: Freeman.