Part II: An anthropologist's Perspective on Education Research in the U.S.

Problems
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1. Provincialism/parochialism

At a time in the U.S. of increased calls for more and better education research there is decreasing interest in (and funding for) international/comparative education.

Why in the U.S. is scientific research in education a domestic science? Shouldn’t a science of education transcend national boundaries?

Why isn’t the U.S. more interested in what works, educationally speaking, in other countries?

2. International/comparative studies continue to emphasize a “league table” approach. Who is on top of the standings? What place are we in? No matter how well constructed, from an anthropologist’s point of view the tests used in these studies and the conclusions reached are inherently problematic—there are too many contextual variables to control.

3. This league tables approach to comparative education fuels the sense of crisis discussed above. And it leads to misleading correlational thinking: For example: If Japan is scoring higher than the U.S. in mathematics achievement, then Japanese mathematics teachers must be doing something better than American mathematics teachers. Therefore, researchers need to isolate what the Japanese are doing and copy it in the U.S. But the problem with this reasoning is that Japanese teachers do many things differently than do teachers in the U.S. and there are significant structural differences in the organization of schooling that makes it difficult to isolate causal factors.

4. A failure to pay attention to within country variation. If Japan has higher mathematic achievement scores than the US and also higher student achievement motivation, we shouldn’t make the mistake of jumping to the conclusion that the higher achievement motivation is a causal factor unless we can first show that within Japan those students who achieve the most have the highest achievement motivation. The causal inference must be supported first within the country before being used for cross-national inferences.

5. Decontextualization:

Scientific research in education calls for identifying “what works” and then “taking it to scale.”

The problem is that in so doing the cultural context is stripped. Only those educational practices that either don’t have a strong tie to their local context to begin with (e.g.
“teacher proof curricula” or whose cultural context can be stripped away can be easily exported.

Example: Reggio Emilia preschools.

Bruno Latour’s points about contextualized and decontextualized forms of knowledge and practice.

6. We should question the progressivism that is implicit in models of educational improvement.

Anthropologists are well known for their cultural relativism, the belief that cultures should not be rated one above another and that cultures should be understood using their own beliefs and concepts rather than by applying external categories to them.

I suggest that a similar logic should apply to claims about progress, and specifically about the idea that education keeps getting better and better.

Notions of educational progress and of educational crises do not make a lot of sense from the point of view of educational ethnography. By what criteria would we conclude that a country’s educational system is doing well or failing? Failure to pass on cultural knowledge from one generation to the next? A failure to reproduce the labor force?

7. How, then, should we use cross-cultural studies of education? As an alternative to league tables, correlational reasoning about differences, and decontextualized borrowing, I suggest a better use for cross-cultural/cross-national studies of education is to use ethnographic (“thick”) views of education in other countries/cultures to:

- question taken-for-granted assumptions
- expand the repertoire of the possible
- rehabilitate the disparaged/draw attention to the overlooked

We need an alternative model of how educational research impacts practice and vice versa. Rather than a linear model that can be easily sketched out on a flow chart but which has little connection with reality, we need to develop models for the connection between education research and classroom practices that are more complex and realistic. For example, to the degree my own comparative research on early childhood education in Japan, China, and the United States has had an impact on the field, I would argue it is not via a linear process of first persuading scholars with scientific evidence who then persuade policy makers and practitioners but instead by directly addressing practitioners with evidence that questions taken-for-granted assumptions; expands the repertoire of the possible; and rehabilitates disparaged practices and ideas.