

Thoughts on Relevance

by

Robert C. Granger, Ed.D.
William T. Grant Foundation

This document is a précis of the keynote address given at the Annual Meeting of the Society for Research in Educational Effectiveness, in Alexandria, Virginia, on March 1, 2009.

I presented the keynote address at the 2009 Annual Meeting of the Society for Research on Educational Effectiveness. The address was delivered with a slide presentation, and I am now in the process of turning the remarks into a paper. In the interim, I am releasing the slides (attached) along with the following summary of my remarks.

The structure of the summary is a bit different than the keynote I delivered, as I subsequently improved the clarity of my thoughts and argument for a version of this presentation I gave to the Board of the William T. Grant Foundation on March 26, 2009. I would welcome any reactions that might be helpful in continuing that process. Please email your comments.

Sincerely,

Bob Granger
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bgranger@wtgrantfdn.org

Thoughts on Relevance

For the past 100 years, there have been passionate debates in the education research community about the appropriate relationship among research designs, methods, and questions. The debates have led to improvements in the validity of findings from individual studies and syntheses of studies. However, there is a lingering concern that the research community will deliver strong evidence on questions that are irrelevant to the concerns of most policymakers and practitioners. In these remarks, I argue for two things that need to change in order to make research relevant. First, studies need to include an examination of what occurs in the everyday environments of young people, such as classrooms or family life, in order to identify the practices in these settings that shape youth outcomes. Further, researchers need to focus on practices that occur with some frequency as opposed to identifying low-frequency practices that would need to be “scaled up.”

Much current work is meant to estimate the effects of changes in policy and practice on important student outcomes such as achievement, course completion, and high school graduation. While policymakers and practitioners unquestionably need such information, too few of the current studies are focused on explicating the classroom-level phenomena that cause such outcomes to improve. This creates two major problems. Teachers do not know what it is that they need to do to help students succeed. Further, policymakers and administrators do not know how to change policies or organizational practices in ways that will productively improve classroom effects.

In addition, the current approach—in which theories and interventions are explored under limited, controlled circumstances before they are scaled up—has not produced effective change at scale. I am a champion of large-scale field experiments, conducted well in the pursuit of questions about the effects of changes in policy and practice. Given my interest in the phenomena that distinguish effective from ineffective classrooms, I am especially focused on experiments that use classrooms as the unit of randomization. Such designs produce a useful contrast, given what policymakers and practitioners want to understand. However, experimentation and moving from small to large-scale are not necessarily linked, and the “scale-up” strategy has not succeeded in education or other human services. To make this argument, I drew on the mixed record of replicating “phase one” trials in medicine and the dismal record of scaling up efficacious reforms in both foundation-funded education reforms and publicly funded intervention/prevention work.

As an alternative to starting small with the plan to subsequently scale up, I drew on the ideas of Donald Campbell and Julian Stanley. Somewhat surprisingly given their seminal writing on experimental and quasi-experimental designs, Campbell and Stanley were rightfully concerned that research that was too far from practice would stay that way. Instead of assuming that the role of research is to discover innovations that could be used by practitioners, Campbell and Stanley argued that the rightful role of education research is to separate out widely implemented and useful practices from those that happen at

scale but are ineffective.

This view that research needs to improve the “sifting mechanisms” used by policymakers and practitioners draws more on the traditions of epidemiology and engineering than it does on drug trials, and it can be seen currently in several research programs. To illustrate, I used the work of Bob Pianta and colleagues at the University of Virginia on classroom practices and Reed Larson and colleagues on the practices of staff in out-of-school youth programs. Both teams are studying widely replicated practices in multiple settings, theorizing about which of those practices are causally related to youth outcomes, and then intervening to induce change(s) in the practices (or in Larson’s case, planning to intervene) to increase the warrant for causal claims.

The presentation closed with a call for continued theorizing and empirical work on the in-school experiences of students and staff; an assertion that interventions are better seen as attempts to induce change and advance fundamental knowledge, than as a vehicle for evaluating a branded product; and an explanation of the need for better measures of classroom phenomena.

The following list contains the main references I used to develop the ideas in the presentation:

Ioannidis, J. P. A. (2005). Contradicted and initially stronger effects in highly cited clinical research. *JAMA*, 294(2), 218–228.

Campbell, D. T., & Stanley, J. C. (1963). *Experimental and Quasi-Experimental Designs for Research*. Boston: Houghton Mifflin Company.

A foundation goes to school: The Ford Foundation comprehensive school improvement program 1960–1970. (1972). New York: Ford Foundation.

O’Connell, M. E., Boat, T., & Warner, K. E. (Eds.). (in press). *Preventing mental, emotional, and behavioral disorders among young people: Progress and possibilities*. Washington, D.C.: The National Academies Press.