

The problem with research in education and other such fields

: 5/11/2022



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4 min read . Updated: 11 May 2022, 10:25 PM IST [Anurag Behar](#)

Even well-intended studies tend to be woefully out of touch with the reality of actual field practice

A recent paper by well-known authors at a multilateral institution claimed that extreme poverty in India had been practically eliminated by 2020. Many soon offered rebuttals, though these may not have been necessary, given how outlandish the claim is. That such a paper was published raises questions about the global research ecosystem.

Many years ago, in our naivete, we committed to an elaborate randomized control trial (RCT), lasting years. One of the key conclusions of that research project, after five years or so, was that children learn better if there are two teachers for any group of children than if there is one teacher for a similar group of children. Shorn of spin, that is something anyone in education, or out of it, has known since education began. It is like conducting a 5-year RCT to prove that when an apple falls from a tree, it hits the ground.

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Some years after that unnecessary RCT, I participated in what can only be called the Kumbh Mela of education research. It had a few thousand serious education researchers in hundreds of parallel sessions, scores of sub-plenary sessions, and a grand daily one. The list of all the papers at the conference was truly as thick as a telephone book of yesteryears. None of the listed research papers was as unhinged from reality as to claim that poverty had been eliminated, but a close careful look at the list could hardly locate anything that people working in education would find useful.

The only test of good research in education should be whether it is useful to the practice of education or not. In disciplines like physics or psychology, usefulness to any kind of practice or application is not the only test for good research. In such disciplines, a valid description and/or explanation of nature and people would be counted as good research. The reason is fairly simple. Education is by its very nature a field of practice; it is not an effort at explaining the world. Public health, livelihoods, or water management are similar fields. So, good research is that which helps improve the practice; explanations and descriptions are insufficient. But too much of the research in education (and other fields) doesn't pass this litmus test.

Rigorous methodology is inadequate. What matters most in such fields is whether the issues being explored and questions being asked are such that these are useful to improve outcomes for real people. Like the RCT mentioned earlier, there is much research that rediscovers the obvious.

Then there is research that focuses on 'particular' matters, such as how a particular set of teachers mobilize and interact with the local community to gainfully engage them in school. Such a project doesn't lead to anything very useful, other than being an inspirational story. The principles of any such engagement of teachers with the community are so commonplace that they don't need to be

rediscovered, while the details are so particular—dependent on individuals, local relationships and dynamics—that they have no translatability outside this milieu. In research jargon, this means that there is very little external validity to such studies of the ‘particular’.

Then there is research that focuses on the trivial, for example how boundary walls effect the performance of children in schools. You need to stretch your imagination to even hypothesize that this matter has much bearing.

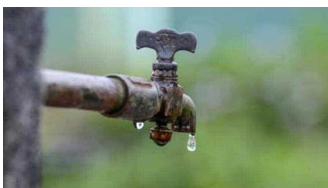
These are not neat categories. Research could be on matters that are obvious, particular and trivial, or a sub-set of these, all together. The most problematic are those which are a combination of these and have obvious negative effects. Say, if you were to put cameras in classrooms to monitor teachers, do we need to really conduct an experiment to find out what is going to happen? Cameras demonstrate a deep mistrust of teachers; some will swallow the insult, most will not, and over time, it will create significant disgruntlement.

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So why is it that so much of research in education is on matters that are obvious, particular, or trivial, and therefore not useful? The explanation is not difficult. The research ecosystem in education and many such fields is disconnected from the actual field of practice. In education, especially, practitioners rarely have the power to ask questions and get funding for such research. These things are in the hands of policymakers and academics at a distance from the heat-and-dust of school reality. Despite their best intentions, they prioritize research that they think is important, which rarely weighs the challenges and needs of people on the frontlines.

Practitioners rarely get a chance to ask questions that are relevant to them and even less support to explore answers. While this matter of a research ecosystem disconnect from domain reality is true in many fields, it is deeply problematic in fields like education, public health, livelihood generation and economics, which directly affect the lives of people.

There is no doubt that research is an important dimension on which we must improve, as a society; but for this, we need to fundamentally change the power equations in our research ecosystems, with those on the frontline of a practice actually determining what should be researched.

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