



Poverty, power and RCTs

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In awarding the 2019 Nobel Prize for economic science to Abijit Banerjee, Esther Duflo, and Michael Kremer for their work adapting randomized control trials (RCTs) to the field of development, the jury emphasized that the technique has “considerably improved our ability to fight global poverty” and “transformed development economics”.

The Nobel Committee’s recognition that RCTs have transformed the field of development economics is noteworthy. Although the technique itself is neither new nor particularly sophisticated in principle (but much more so in practice), its analytical simplicity in trying to establish causal inference is part of its attraction. And it has certainly transformed the field of development economics, as part of a larger “credibility revolution”, with its emphasis on identification strategies and causal inference underlying empirical work (Imbens, 2018). RCTs have also led to a greater emphasis on fieldwork in economics, resulting in researchers spending time in the field, again a positive contribution.

The most wide-ranging critique of the professed “gold standard” causal inference status of RCTs has come from Deaton and Cartwright (2018). More broadly, critics have questioned RCTs claims regarding what constitutes evidence, the distinction between evidence and understanding, and the long winding road from evidence to policy. However, here my focus is on the relationship between power and ideas and its implications for economic development writ large.

1. The “crowding out effects of RCTs on development research and practice

RCTs epitomize the turn to micro-foundations in the social sciences. The dominance of RCTs in the context of ideas about development is part of a longer trend. From planning in the 1950s and 1960s, to poverty and rural development in the 1970s, to structural adjustment in the 1980s and 1990s, to cross-country regressions and most recently RCTs, the life cycle of ideas on development have had two constants. One, their half-life is between one and two decades. And two, the ideas that become dominant are championed by a select set of institutions, some in the U.K. but primarily in the U.S.

The “evidence hierarchy” of RCTs that privileges it over other forms of evidence does not simply reflect the power of already privileged institutions – it amplifies that power. It is not a coincidence that the institutional locus of the most dominant inferential tool on poverty reduction is in the richest universities of the world. The sheer costs of doing RCTs has sharply raised the financial barriers to entry – even for researchers in the less elite institutions in developed countries – making it even harder for developing country researchers.

Moreover, and in sharp contrast to the natural sciences, where all members of a group participating in an experiment get credit in any papers published, experiments in the social sciences such as RCTs give short shrift to the many people involved in their implementation. Only already privileged academics get credit, while all those involved in execution at best get acknowledged in a footnote. The noble cause of poverty reduction has amplified inequality in the production of ideas regarding development, replicating growing inequalities world-wide.

2. Have RCTs “considerably improved our ability to fight global poverty?”

Ironically the Nobel citation’s claim cannot be tested using the very gold standard that proponents of RCTs have claimed for it. Can one do a RCT to test the two key propositions underlying this claim? First how considerable is “considerable”? For a technique that claims quantitative precision, this claim is intrinsically qualitative, not just because RCTs cannot be used to test the claim, but also because there is no evaluation of the opportunity costs of the large financial and human resources consumed by RCTs – which if deployed to improve growth drivers might have a greater impact on poverty.

The opportunity costs of this dominance have been (to borrow from the Nobel citation), “considerable”. Development thinking’s pivot to RCTs in elite institutions is giving short shrift to major questions that affect investment and production that are at the heart of the development process, be it integration into the global economy, the balance between the state and market, improving regulation and public administration, public finance, industrial policy and the like. Poverty programs, no matter how well designed, will go nowhere without resources, which will not be forthcoming without growth. Where will that growth come from? What new ideas do we have given that the ideas of 1980s have run their course?

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When a methodology that can intrinsically be applied to only a narrow set of questions becomes the gatekeeper for journals and research funding (and hence shapes overall incentives for young researchers), its negative externalities are substantial. The reality is that by virtue of being institutionalized in the most prestigious and wealthy institutions, the success of RCTs owes much to “media and donor appeal, and academic and financial returns” (Bédécarrats, Guérin, & Roubaud, 2019). The result is crowding-out of research on the many other factors that affect economic development and sustainable poverty reduction in the long term and the reality that fewer researchers are being trained on these other important issues than would otherwise be the case.

The second claim (in the Nobel citation) regarding the contribution of RCTs “to fight global poverty” also requires a non-RCT evaluation. Just how much have RCTs improved our ability to fight poverty relative to other factors? The most spectacular reduction of poverty in recent times has been in East Asia, first the “Asian Tigers” and then China (and to a lesser extent India). Importantly, this reduction occurred with negligible inputs from RCTs informing poverty reduction policies in these countries and was the result of growth enhancing development policies and strategies that complemented macro and sectoral policies with ground-level implementation.

Indeed, if one examines the case of India by way of example, a country which has seen the largest decline of poverty in recent years (after China), and has also seen many expensive RCTs, one might well ask if there was any causal relationship between the two. When queried if RCTs “had moved the policy needle in India, Arvind Subramanian, Chief Economic Advisor, GOI, was hard pressed to find a single one that had been helpful to him in addressing the dozens of pressing policy questions that came across his table” (Kapur, 2018). Researchers’ insistence on gold standards might well be necessary to run the publications gauntlet, but for most policy purposes having timely and good quality data, underpinned by a degree of specific knowledge and engagement, has much greater payoffs for policy formulation.

A similar story could be said not just for the recent stories of “development” but for all stories of development. All countries make policy mistakes, including industrialized countries, ranging from policies that led to the great depression in the 1930s or financial deregulation that led to the global financial crisis of 2008. One could, of course, counter that the point is not that other countries developed without RCTs, but that knowledge from RCTs may have helped avoid some bad choices if policy makers had been open to better evidence. While that is certainly possible, it assumes a) that evidence, rather than ideas, ideologies and politics, is what drives

policy and b) that evidence produced through RCTs would have been the relevant evidence.

This, however, elides the reality that policies that have been shown to have had major impacts on growth and development – import-substitution industrialization, trade liberalization, state-owned enterprises, exchange rate and monetary policies, the Cultural Revolution in China or demonetization in India, tax systems, etc. – cannot be addressed through RCTs. While public service delivery programs have been the most amenable to RCTs, unfortunately they are but one aspect of development.

3. Conclusion

RCTs are a classic example where the best intentions of researchers have become the enemy of the good as regards development policies. There can be little doubt on the importance of RCTs to improve public service delivery but in a world where poor countries are beset by macro-forces, be it climate change, technological transformations or trade-wars, there is increasing urgency for researchers to find ways to address these challenges as well. To do so, researchers need a wide repertoire of research methodologies, including RCTs, but without privileging them. For instance, Chinese reforms usually originate in spatially concentrated policy experiments, and if successful, expand to cover increasing parts of the country. Poor countries may do better by following the Chinese adage of crossing the river by feeling the stones, rather than blindly following yet another development idea that will inevitably be displaced sooner rather than later.

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