

RESPONSE

The Nobel Winners in Economics Are On the Right Track

Randomized controlled trials aren't perfect, but a new generation of development economists is building on the work of the Nobel laureates and pushing the field in ambitious new directions.

BY AHMED MUSHFIQ MOBARAK, C. AUSTIN DAVIS | DECEMBER 9, 2019, 6:25 PM

In a recent *Foreign Policy* article, Sanjay G. Reddy critiqued the approach to development economics embodied by the work of the recent Nobel Prize winners Abhijit Banerjee, Esther Duflo, and Michael Kremer. We see the strengths and weaknesses of this approach quite differently and propose a constructive direction for the field.

A generation ago, development economics had a problem. The theories proposed to explain economic growth appeared disconnected from the economic lives of individuals in developing countries. A dominant mode of empirical analysis at the time, known as cross-country regressions, lacked credibility. That mode of analysis purports to establish the effects of policy on economic growth by comparing the growth of countries that share some characteristic or enact some policy to those that do not.

A primary concern about this approach is that policies are implemented strategically; policies that actually enhance growth may appear to do the opposite if governments implement them to mitigate a recession. Another concern is aggregation. Country-level outcomes are the result of decisions undertaken by individuals, households, firms, and governments. Correctly identifying the effects of a policy change requires understanding and measuring the behavior of each of those agents.

An initial set of innovations in development economics research involved the systematic observation of the behavior, characteristics, and circumstances of those residing in poor countries. These microdata were used to develop and test theories of poverty. Early examples include the work of Mark Rosenzweig, who tested specific theories of household behavior such as how families who have more children invest in each child. Christopher Udry embarked on a research agenda that combined in-depth interviews and structured data collection in West Africa to develop theories of

investment and technology adoption in agriculture. Robert Townsend followed the same households in Thailand over time to understand how the poor manage risk. Angus Deaton made fundamental contributions on careful measurement.

This year's Nobel in economic sciences was awarded to Banerjee, Duflo, and Kremer for the next significant step: setting new standards for making credible inferences from such data. After all, even relationships observed in the microdata may suffer from confounding effects. For example, microloans have been promoted as a ladder out of poverty by allowing profitable businesses to grow. Indeed, economists often observe that people receiving microloans see their profits grow faster than those who do not receive loans. But, of course, loans often go to the entrepreneurs with the best prospects. These entrepreneurs may have found other ways to succeed even without microloans, and a naive analysis could incorrectly attribute their success to microcredit.

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The work of Banerjee, Duflo, and Kremer has pushed the field to identify causal relationships in creative and rigorous ways. Randomization was added to the development economist's toolkit. The appeal is simple and powerful. If you randomly split a large sample of individuals into groups labeled "treatment" and "control," then those two groups will look the same, on average, across any characteristic you care to measure. Now offer microloans to the treatment group. Any differences in average profits between the two groups can be attributed to the microloans rather than preexisting business prospects. This is a sound and straightforward measure of causal relationships.

The rigor exemplified by the laureates' work represents a generational advance for the field's scientific and humanitarian goals, but—as they would be the first to admit—there is more to do. Reddy's recent article made several critiques of randomized controlled trials, principally that they do not tell us anything new; they only tell us whether something worked, missing the more important question of how or why; that they are not subject to ethical oversight; and that practitioners have at best a superficial understanding of the environments they study.

It is a strawman argument, because Reddy inaccurately caricatures randomized controlled trials and then attacks that caricature. Still, it is worth discussing some of the

meaningful limitations of randomized controlled trials and to constructively chart a path forward by building on their rigor and credibility.

The key issue is that evidence from such trials is not guaranteed to provide forward-looking, generalizable policy guidance. While the effects of an intervention may vary according to the characteristics of beneficiaries or features of the environment, the trials estimate the effects on a particular sample in a particular setting. The intervention itself embodies potentially consequential choices: What interest rates should be charged on microloans? What will the repayment term be? Thus, the results of a randomized controlled trial may have limited predictive value if the beneficiaries, environment, or intervention depart from the original experiment.

Programs are often evaluated when they are piloted, but complexities may arise when an intervention is implemented at the scale of a government policy. Spillovers are one example. A small-scale randomized controlled trial might show large benefits to households receiving a productive asset such as a cow. These benefits may diminish at scale because, say, the relative abundance of cows lowers the price of milk. Positive spillovers **exist as well**. Other complexities of scale include political reactions and macroeconomic effects. A large-scale policy will attract the attention of politicians and constituents, potentially altering incentives, public goods provision, and political accountability.

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A final challenge facing randomized controlled trials is their purported inability to address big questions. Critics argue that practical and ethical barriers to randomization have turned researchers away from the things that are likely to be transformational, such as electricity; roads; piped water and sanitation; a clean, disease-free environment; and strong institutions to protect property rights, resolve disputes, and encourage healthy competition. Such big questions are also within reach. Of all things, physical infrastructure such as bridges might seem the least likely candidate for randomization, but young, ambitious researchers are **building an agenda** around it.

The next generation of development economics research—of which Banerjee, Duflo, and Kremer are very much a part—is more ambitious in research design, in scale, and in combining randomized controlled trials with other methods to draw generalizable lessons. Some economists recently worked with the state government of Andhra

Pradesh, India, to conduct such a **trial** of government service delivery to 19 million people. That experiment was large enough to induce **changes** in market wages. Others have used two-stage so-called saturation designs to measure spillover effects and market-level changes, allowing them to study the effects of microloans to farmers on **seasonal fluctuations** in market food prices. Such a strategy can be used to study how subsidies for toilet construction can affect **social norms** around open defecation at a large scale, or induce **reactions from local politicians**. Other scholars have **predicted** the performance of interventions when transferred to new settings.

These are just a few examples of what the path forward for development economics could look like. Economists should not compromise on analytical rigor but build on the Nobel laureates' work by becoming more ambitious and creative.

To make the most of randomized controlled trials—which have high fixed costs arising from partnership development, data collection, and implementation expenses—economists may want to combine that data with statistical or theoretical models that improve the ability to credibly extrapolate policy lessons. The trials could also be enhanced by using coordinated multisite trials to improve the efficiency, completeness, and coherence of the resulting evidence. By studying similar interventions in different places or over time, we can learn how the effects of the program change with the conditions. Together, these features will offer a holistic view of a program so that policy decisions, scaling, and implementation can be truly evidence-based.

The economics Nobel committee gave due recognition to an important step in the right direction, but it is incumbent on the rest of us in the field of economics to build on that innovation with new creativity and rigor to address the valid concerns raised by the critics of this movement.

Ahmed Mushfiq Mobarak is an economics professor at Yale University, where he directs the Yale Research Initiative on Innovation and Scale. Twitter: [@mushfiqmobarak](https://twitter.com/mushfiqmobarak)

C. Austin Davis is an assistant professor at the School of International Service at American University.

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