Book Reviews

Dean Karlan and Jacob Appel. *More than Good Intentions: How a New Economics Is Helping Solve Global Poverty*. New York: Dutton Adult, 2011. Pp. viii+308. $26.95 (cloth).

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Something new and exciting is definitely happening in development economics. The "new" departure has created a renewal of interest in the development profession, from academic economists to development practitioners, and we can feel it in how the field has galvanized a large cohort of talented young economists and spawned promising novel approaches. The book by Dean Karlan and Jacob Appel, *More than Good Intentions*, coming in the wake of Banerjee and Duflo's *Poor Economics* (2011), is a milestone in helping us better understand, and present to a broad popular audience, what is really happening and how a new development economics can help contribute to reduce global poverty, still humanity's major scourge. The authors have been major actors in this new departure, but their contributions are also part of a broader movement that has involved the profession in a massive way at many levels, along with inevitable and helpful heated controversies about the pros and the cons, the advantages and the limitations, of the approach.

The distinguishing feature of the Karlan and Appel book is that it is addressed to millions of well-meaning individual donors and small foundations that together contribute vast sums of money to foreign aid, estimated at some $200 billion annually. Yet, these small donors are typically more likely to respond to the tuggings at their heartstrings, to their emotions, and to ill-informed preconceptions than to allocate their money to programs that would give the biggest bang for the buck in reducing poverty. In that sense, "more than good intentions" is indeed needed to help better use this foreign aid. Careful assessment of what works, how it can be improved, and how it can be scaled up and sustained is necessary to maximize effectiveness of this remarkable flow of donations. The book brims with ideas, experiments, and examples showing how this can be done. It is an exciting read, written in a way that makes the new development ideas appealing to a broad audience. To fulfill its goal, it needs to be read very widely, beyond the choir of the already converted (a major challenge indeed).

There are three significant aspects to the authors' approach. First, is is deeply humanitarian, as it takes us back to looking at the poor as complete humans with complex real and vital decisions to make in order to survive under adverse conditions. Their behavior is loaded with ill-informed decisions and psychological quirks that make them not particularly different from all of us, except for the fact they operate in a context where the margin of error in resource use is slim, with potentially devastating consequences on survival and potential entrenchment in poverty traps. Behavioral psychology and economics has been a thriving field for some years, but the field's intersection with understanding poverty at the microlevel is new. The basic proposition here is that external actors cannot start helping agents whose behavior they do not understand and that this behavior is loaded with apparent irrationalities and is bewilderingly heterogeneous and shifting, requiring a purposeful effort to be understood. As a consequence, there is an emerging "poor economics," bringing the best of mainstream economics, behavioral theory, and experimental methods together to understand the poor as complete humans. And this has been highly fruitful. It is helping us understand patterns of behavior previously ignored or categorized as incomprehensible and design programs and institutions that are congruent with behavior, in particular helping people help themselves when they are inclined to act against their own best interest, think too fast, or fall into temptations that they will subsequently regret. An example is offering commitment contracts to help people willingly constrain their own behavior as opposed to having them rely on prohibitions or paternalistic guidance.

Second, the approach is deeply scientific, as it relies on rigorous evaluations of evidence as opposed to ideology, exaggerated claims, and unfounded "good intentions." For this, experimentation is necessary, often in the form of randomized control trials (RCT) more frequently used in hard science than in economics and requiring deep engagement in field research to set up experiments and observe what they do. Relegitimizing fieldwork as a major element of development research has indeed been a contribution of the new approach. In the 1960s and 1970s, students doing dissertations in development economics were typically going to the field, often as part of large bilateral exchange programs with foreign universities, but this subsequently was downgraded as "soft" by the broader economic profession. Today, we see major field platforms being set up, with engagement with local programs and counterpart institutions and collaborating local scholars as an essential aspect of the approach. Key in the scientific method is recognizing that we do not a priori know what works, where, when, and for whom because of extraordinary heterogeneity and changing conditions. We know that magic bullets are rare and that we need to be weary of defining the solution as part of the problem. Not that we start from scratch each time with new diagnostics. Accumulated experience counts. But the validity of the context in which an approach may work needs to be carefully established in each case, and the approach likely has to be extensively adapted to the context.

Finally, the approach is deeply committed to making a difference, motivated as it is by a widely shared objective, namely, reducing global poverty, the fundamental reason for the field's existence following MacNamara's famous 1973 Nairobi speech. Hence we have the search for what works, how what seems to work can be improved, and how it might be scaled up and sustained. Cumulating experience across a broad set of contexts—that is, seeking to establish the boundaries of external validity—is where Karlan's Innovation for Poverty Action is making a major contribution. It helps facilitate field experiments and replications across a broad array of settings that individual academic development economists would have a hard time managing and justifying in terms of career objectives.

Examples showing that the approach can work already abound. They show that well-defined small actions can have large payoffs in using aid better. They apply to such fields as agriculture, education, health, reproductive practices, financial services, and enterprise development. Interesting results include the social value of subsidizing mosquito nets to reduce malaria, the importance of a schoolwide approach to the deworming of children, installing chlorine dispensers for safe water at the community level, the yield effect of providing small discounts to the early purchase of fertilizers, the role of a wide array of nudges in creating incentives to save, the help provided by default options when there is a temptation to postpone decisions about insurance or savings, using the discipline of group lending as a transition to individual loans, providing personalized information on expected benefits from investing in education, helping cool off decision making under pressure, and providing commitment devices to avoid procrastination and regrettable temptations (e.g., in quitting bad health habits and risky sexual practices).

Yet, will the approach make a significant dent in global poverty? The simple answer is that it is necessary but not sufficient. It can address poverty at the microlevel, but it does not address the broader structural determinants of poverty, such as lack of access to assets like land, entry into overcrowded fields of economic activity that bring a low return, use of the assets in contexts that are not supportive of efficient returns, lack of broader social protection to assist risk taking and avoid the irreversibility of exposure to uninsured shocks, and governance failures that bias the playing field against the poor (i.e., the political economy of poverty). For this, the profession needs to move from the small game of localized actions to the big game of policy reforms and their political economy. Yet, it is likely that the small game has to be the channel through which the big game will eventually be achieved, particularly in the poorest countries. This is due to a fundamental asymmetry between the developed and the developing country contexts.

In developed countries, where behavior is better understood, institutions are more complete, and the political process is relatively more open, macropolicies can be relied on to influence microbehavior. A top-down approach has a greater likelihood of success. We can then organize rather generic workfare programs that can be effective in reducing poverty and creating a ladder back into the labor force. We can offer commitment contracts that can be managed on a website. In developing countries, poor economics is largely to be discovered, institutions are distinct and with large gaps, and weak governance is the norm. How to make governments work for development is a largely unresolved challenge. Under these conditions, a bottom-up approach will need to be pursued for a long time, using people's behavior to influence governments and politicians to change macropolicies and steer the sate toward development functions. Effective external donors can help. However, small actions should be used strategically to achieve this broader purpose. And this may be where the new approach has not yet reached sufficient maturity in targeting minor interventions not only in harvesting the low-hanging fruits with potential large payoffs but also in choosing those fields of interventions with larger spillover effects on the construction of stronger institutions and more open political processes. In a Hirschmanian perspective of unbalanced growth and strategic choices for long-term poverty reduction, the well-intended bottom-up approach needs to be strategically used to accelerate the transition toward addressing the bigger game issues of global poverty reduction. To be more effective for global poverty reduction, small-game playing needs to be part of a big-game vision.

Let me finish with seven recommendations to the profession in closing this review of the new approach to global poverty reduction as presented in the Karlan and Appel book. The first is that impact results are often deflating donor expectations more than warranted, and this has created sometime unjustified conflicts with practitioners. This is because we are typically measuring marginal local treatment effects in deviation from an existing intervention that is already doing part of the job. Hence, reported impact will often be small, even if the approach does work overall. The second is that, in using RCTs, we typically measure short-run effects, when it is well known that "development takes time." The short-run effect may well be reduced consumption to accelerate asset accumulation, with long-term welfare benefits observable only at a later date. The third is that we need to look beyond often mechanical RCTs toward broader opportunities to use information from natural experiments, providing impact evaluations of policy reforms of meso- and macromagnitudes. What has been learned about microlevel identification strategies needs to permeate into broader cross-regional and cross-country evaluations of big-game issues. Economic historians have here regained a place in rigorous evaluations of big-game reforms that is welcome. The fourth is that there is still a long way to go in enlisting development agencies and donors to endorse a results-based approach to investing in aid. In this, we have a major role to play in deciding when each approach is best suited, often deflating claims that an RCT approach is the only trusted methodology. Tools must not take precedence over hypothesis formulation and the corresponding choice of identification strategies. The fifth is that we still have a lot to explore in adapting institutional constructs to traditional community organizations, with their specificities of local information, interlinked transactions, social capital, and power relations. There is much to be gained here from interacting with anthropology in devising, for example, commitment contracts that can make use of the specificity of traditional institutions, instead of designing new institutions with a narrow Western eye. This is an exciting field that requires ability to work across disciplines, with potentially large payoffs. The sixth is that the Karlan and Appel book addresses foreign aid but that ultimately the goal of development is unlikely to be reached through aid but rather through the generation of autonomous incomes in response to new widely shared opportunities. Think, for example, of China. And, finally, like the poor, small donors are locked up into their own bad emotional habits, preferring the satisfaction of warm-glow feelings inflated by exaggeration biases propagated by opportunistic nongovernmental organizations to the reality of results-based hard evidence on what works. There is nothing harder to overcome than rational cognitive dissonance. In that sense, this book, with its ambition of making the new development ideas accessible to a popular audience, still faces an uphill battle in reaching beyond the choir of the converted, a worthy cause to which we all have to contribute.

Reference

Banerjee, Abhijit, and Esther Duflo. 2011. *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty.* New York: Public Affairs.