

Introduction to the Hebrew edition of *Poor Economics* (Banerjee and Duflo).

More than a quarter of the world's population live in poverty, which is today defined by the World Bank as living on less than \$3.10 a day.¹ *Poor Economics* invites readers to step into the shoes of these people, and to think about poverty and economic development through the lens of their experiences and choices. The book, drawing upon research conducted in Asia, Africa and Latin America, presents a novel research approach to studying economic development. The authors' receipt of the 2019 Prize in Economic Sciences in Memory of Alfred Nobel was clear testimony to the tremendous impact that they have had on the discipline of development economics, and the fundamental debates their research has generated among both academics and policymakers. In this introduction to the Hebrew edition, I will situate the research presented in the book, in a broader context of the economic research on poverty, and examine what sets the approach represented here apart from previous work done. While the work has had significant impact on the field of development economics, it has also engendered important criticisms. We will look at new advances in research into development and poverty eradication of recent years, and the current directions taken since the book was first published. This brief review will also refer readers to economic research projects, many of them more recent than this book, for those who wish to further explore the issues.

Development economics is a research field that studies the causes of global poverty and how best to decrease it. The history of this subfield is probably as old as the history of the economics discipline itself. While trends have shifted over time, during the second half of the 20th century, research in development economics was dominated by a macro perspective, and theoretical models, often backed up by data patterns emerging from country comparisons, were used to determine why some countries are rich while others are poor. One important stream of such research uses growth models to explain the large discrepancies in productivity between rich and poor countries, while newer research highlights differences in the quality of formal institutions as a decisive factor behind the gaps. Common to much of the research until the 1990's was that it took a top-down perspective on development, with much of the research carried out by scholars in Western universities proposing models or methods detached from the realities of the poor.

Since the 1990's, the authors of this book have been leading a major shift in development economics, and the research presented here represents this newer strand of the discipline. It is now a growing field that attracts many young scholars and relies more and more on novel experimental methods. There has been a move away from the macro or country level perspective in favour of the micro perspective: much research in modern development economics emphasizes the individuals, households and small firm owners in low income settings, and strives to look at poverty through the everyday situations and challenges faced

¹ This is the international definition of "moderate poverty", while "extreme poverty" is defined as living on less than 1.90 per day and 10 % of the world population live below that lower level. These thresholds are based on the value of people's consumption, not their income, and take into account price differences between countries. In the 1990's when these concepts were launched, extreme poverty was defined as less than \$1 per day.

by these actors.² One might say that the authors of and the research presented in this book have helped change the focus of researchers, and to some extent also that of policy makers, to examine how the poor actually live.

This research has also had a significant input in the debate on the effectiveness of foreign aid and more broadly on “what works” in poverty reduction. This debate was for a long time divided between two camps. On one hand, proponents of foreign aid emphasize the human suffering related to poverty and advocate massive aid injections to stop poverty (see Sachs, 2005) and on the other, opponents of aid criticized poverty interventions that aim to fix immediate problems without solving the systemic political issues at their core, and argue that aid, at its best, does nothing to help alleviate poverty in the long term, and at its worst it is counterproductive for poverty alleviation (see Easterly, 2006 and Moyo, 2009). Both arguments are intuitive and theoretically plausible, so how can we determine which one is true? The authors of this book proposed a way forward: instead of attempting to answer the big question of how to end poverty, they break it down into many smaller and more manageable specific questions that can be answered through careful empirical measurement and observation.

A guiding philosophy of the authors, representative of the view of many modern development economists, is that the first step to understanding why some countries are poor is to empirically identify important sources of inefficiency and policies to address these sources. In *Poor Economics* the authors summarize a large set of microstudies on the causes of poverty, and draw lessons for a science-based approach to improving the health, schooling and incomes of the poor. The research projects discussed in this book are guided by microeconomic theory as well as the collection and use of microeconomic (individual level) data. They also draw inspiration from behavioural economics, a research field that connects insights from psychology with economic reasoning to better understand individual’s choices and preferences. Through telling real-life stories about individuals and families that they encountered during their field work, the authors exemplify central concepts in the economic theoretical toolbox in a simple and relatable way. The case studies show how an individual’s economic choices are guided by her incentives and the information available to her, how cost-benefit calculations influence decisions, and how an individual’s actions can have important effects on others, through “peer effects” or through positive or negative “externalities”. The notion of the poverty trap, which is often used in the policy debate without a clear definition, is connected here to basic economic modelling of the production function, to discuss under what conditions poverty traps are likely to exist and what are the implications for development aid.

An important and central aspect of modern development economics and one which Banerjee and Duflo have played an important role in promoting, is the increased use of experimental methods to measure the impact of development policies and better answer some of the

² The renewed focus on micro-data within development economists followed in the footsteps of the work of Angus Deaton (Nobel Laureate 2015) who advanced the notion that the measurement of the well-being of the poor, through detailed micro-level data, must be closely integrated into the fight against poverty.

“puzzles” related to poverty. Most of the research projects discussed in *Poor Economics* indeed consists in such experiments, although this is not always stated explicitly in the book. The idea builds on earlier field experiments in agriculture and clinical trials within medicine. The introduction of such methods to economics are part of what is referred to as “the credibility revolution” in empirical economics: a methodological shift that pushed economic research in several areas towards a stronger focus on estimating causal effects. Starting in labour economics and the economics of education in the 1980’s and 1990’s (see Angrist and Pischke, 2010) these new guiding principles have transformed economic research in the last three decades. From this time onwards, modern development economics has been at the forefront of this development, and it merits a brief explanation here.

When a researcher wants to assess the causal impact of a certain policy or program, she needs to answer counterfactual questions: How would individuals exposed to the program have fared in the absence of the program? Conversely, how would other individuals who were not exposed have fared, had they had the opportunity to participate? This is “the fundamental problem of causal inference” (Holland 1986): estimating the impact of a program on an individual at a given time is impossible because the individual was either exposed or not exposed to the program. However, it is possible to estimate the *average* impact of the program on a group of individuals by comparing them to a similar group of individuals who were not exposed to the program. If the researcher tries to answer the counterfactual question using observational data i.e. data created through a process that was not controlled by the researcher, there is a high chance that the results would be misleading because of “selection problems”: the circumstances of people who opt into receive a certain medical treatment are different from the circumstances of the people who will not seek out this medical treatment. When the researcher tries to compare the two groups, any difference observed between them will result from a combination of this pre-existing difference in circumstances (information, health etc.) between the groups, **and** the effect of the treatment. The fact that there is a systematic difference between the two groups before the treatment makes it difficult to isolate the effect of the treatment.

One method for solving the selection problem is to randomly assign individuals or units of analysis, which can also be households, villages or schools, to a treatment and a control group. If a unit belongs to one of these groups just as the result of a random draw, the only systematic differences across the groups arise through their exposure to the program. When correctly designed and implemented, a randomized controlled trial therefore allows researchers to estimate the causal impact of a certain program or policy in an unbiased way. Banerjee and Duflo are among the pioneers of the use of this particular set up in development economics (for more on this, see Banerjee and Duflo 2009, Duflo et al. 2007 and Duflo, 2017).

To use an example from Chapter 3 of the book, let’s suppose the goal is to eradicate malaria. Previously, the two well-known researchers mentioned above - William Easterly and Jeffrey Sachs - maintained opposite views on the issue of distributing mosquito nets for free. Easterly claimed that providing nets for free leads to the wrong incentives and that people would not value them, but instead end up using them for something else, such as fishing nets. Sachs, on the other hand, argued that distributing nets is a cheap and effective way to reduce malaria incidence and he supported the policy. To determine who is more correct, and determine whether subsidizing mosquito net is worthwhile, it is not useful simply to compare people

that received a bed net for free to people that paid for one, because these two groups are different to begin with: People who pay for a mosquito net are typically more educated, and have a better understanding of why they need a bed net; while those who got one for free might be poorer, which is why they were in contact with the NGO that gave out free nets in the first place. A better way to answer the question is by setting up an experiment. We can randomly select a number of individuals to receive a bed net for free, and another group of individuals that pay a given price for it. One year later, we can see whether those that got it for free, were actually less likely to use it. Such an RCT was carried out by Jessica Cohen and Pascaline Dupas in 2007. Their results showed that households who accepted the bednets when given for free were neither no less prone to use it nor less in need of it than the households who paid for nets. However, when small user fees were introduced, demand fell by 60%. They concluded that free distribution would save many more lives than a cost sharing model (Cohen and Dupas, 2010). This finding has since led a number of international organizations to eliminate user fees in the take-up of key preventive health products.

More generally most outcomes one may want to influence reflect purposeful choices made by individuals. The researcher's role is to understand what those choices are and what leads to them. For example, knowledge about how to stop deadly diseases such as malaria and diarrhea has previously been gathered through randomized controlled trials conducted by medical researchers. But despite established medical knowledge and availability of effective and cheap treatments, millions of children in low-income countries still die from such preventable diseases each year. Today's discussion about reducing child mortality in low-income countries therefore largely revolves around human behaviour. Why is inexpensive high-quality care not provided or demanded, even if the knowledge exists on how to do so? Why do service providers not perform? How could services be delivered in cost-effective ways? Such questions are at the heart of the experimental approach adopted by modern development economists.

While some of the proponents of aid often point out how little money is needed to stop poverty, critics argue that money and costly initiatives thrown at poor countries by foreign states and development organizations will never have a lasting effect on poverty, and by this view, the cost of aid is infinitely high. Impact evaluations, which are one type of randomized experiment, designed to measure and quantify the total impact of a policy or program, can help us make progress. The randomized setup, as explained above, allows researchers to isolate the effect of the program without confounding it with selection effects or economy-wide changes that occurred simultaneously with the program. It can therefore also help determine whether the gains of the program outweigh its costs, and by how much. A recent trend in development economics is long term follow-ups on past programs that were introduced through such an experimental setup, in order to more fully estimate the value of program impact. In one large project in Kenya (discussed in chapter 2 and 4 of the book), such a follow-up found that young adults who had been regularly treated for intestinal worms while in primary school had better health, jobs and incomes than the control group (who were treated for a much shorter period while in primary school) ten years later (Baird et al 2016). This finding gave reassurance that mass deworming of children in schools is an effective development program. Meanwhile, a recent study of the effects of access to finance in India

found that positive initial effects had tapered off after three years. This finding, contributed to a growing pessimism about microfinance as a development policy (Banerjee et al. 2015).³

The approach to economic research in general, and to poverty more specifically, promoted by the authors has also received criticism. “Randomistas”, i.e. economics researchers that employ randomized field experiments and carry out impact evaluations, are criticized for letting their methods guide them too much in choosing their research questions, and thus shying away from the big and important questions that cannot be answered with a randomized experiment (Ravallion, 2018; Rodrik, 2008). Randomized controlled trials have also been criticized on the basis of external validity: Finding out that something works in Kenyan villages may not imply that the finding extends to other contexts. Finally, some critics argue that randomized *field* experiments (more than e.g. clinical trials) are ethically problematic since they affect the lives of individuals as part of the research, and since, during an experiment period, resources are not allocated according to greatest need, but based on randomization, in order to ensure a comparable control group.

All of these criticisms raise valid points that are vital to keep in mind. Indeed, the criticisms are currently being addressed within the field of modern development economics. In the last chapter of the book, the authors partially address the claim that the “randomistas” only scratch the surface of poverty alleviation, without addressing the underlying political structures and bad institutions that are the root causes of slow economic development. They show though a series of examples from the political economy of development that the experimental approach and applied microeconomic research also has something to say about this side of economic development, and in particular about informal institutions such as trust and norms, and how these matter for how formal institutions are perceived and function – a question that is getting increased attention in both low income countries and richer economies during the COVID-19 pandemic. The authors argue that politics can be improved at the margin, and that paying careful attention to the details of how people make decisions is as important for understanding political participation as it is in other areas like health and education. In addition, responses to the challenge of external validity have started to emerge more recently from within the field of empirical microeconomics, with meta-studies that use statistical methods to draw conclusions from a larger set of randomized controlled studies on a given subject, such as the effectiveness of microfinance (Meager, 2019) or develop general rules for carrying out such analysis (Vivalt, 2020).

Ethical concerns are not specific to development economics or field experiments, but present throughout the economics discipline, as policies proposed on the basis of economics research have real consequences for people. There is an active debate within development economics about ethics. An ethical argument *in favour* of randomized control trials for evaluation is that they help us learn what works, and consequently how best to use limited resources (be it aid money or state budgets) efficiently. One could also claim that it would be unethical for policy makers to implement policy on a large scale that has not first been rigorously tested in the field. An untested policy may end up having unforeseen negative side effects, or waste resources that could have been put to better use. In addition, the economist carrying out field experiments must adhere to the same ethics standards as any social scientist working with human subjects, relating to data privacy, research transparency and ensuring no harm to

³ See also Barak-Weekes and Stryjan (2019) for more discussion in Hebrew on microfinance.

research subjects. Moreover, researchers in development economics are currently leading the path towards more stringent norms in research transparency and ethics in the economics discipline (Christensen and Miguel, 2018). For more discussion on ethical considerations related to field work and randomized control trials, see Singer et al. (2019).

In the introduction of the book the authors call to “abandon the habit of reducing the poor to cartoon characters and take the time to really understand their lives, in all their complexity and richness.” While the questions at the core of this book are all related to poverty, the scope of the book extends well beyond international aid. The book is really about policy: how should public service systems such as the educational system or the health system be organized, given the information held by, and the motivations of the people to be served by these systems? How can small businesses be supported to achieve sustainable business growth? These are questions central for policy making in low- and high-income countries alike, and since people can respond to policy in unexpected ways, they are always complex. Yet, to inhabitants of richer countries – and to many early development economists – the problems and challenges of low-income countries and their inhabitants are often reduced to be primarily about lack of money. Perhaps the most important contribution of the research presented in *Poor Economics* is its insistence that we take the lives of the poor, and their choices, incentives and motivations, as seriously as we do those of the rich.

The Prize Committee that awarded Abhijit Banerjee and Esther Duflo, together with Michael Kremer, whose research projects within education and health are also discussed in the book, with the 2019 Prize in Economic Sciences in Memory of Alfred Nobel recognized that while the authors had a transformational impact on the field of development economics, many other researchers are also active in this field. Several scholars have contributed to the new focus on careful measurement of causal relationships in the evaluation of aid programs and policies. *Poor Economics* offers us an accessible summary of this vast field of earlier research conducted by many scholars across the field. Each chapter has a reference list for readers that wish to learn more about specific research projects discussed in the chapter. Even though the book is ten years old, it still provides a solid and relevant introduction to the research field of modern development economics, as well as to thinking about the key questions in development policy and poverty alleviation with a perspective that places the circumstances, actions and choices of the poor in the centre.

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This text was written by Miri Stryjan for the Hebrew edition of:

Banerjee A.V., Duflo, E. (2011). *Poor economics: a radical rethinking of the way to fight global poverty*. New York, Public Affairs.