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A research agenda for development economics¹

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Abstract

Development economics has grown tremendously in the last fifteen years. It can continue to grow and improve in the next decades by focusing on three areas: First, revitalizing the tradition of applied theory which transformed development economics in the 1980s and 1990s, by giving us a better understanding of how poverty shapes individual options. A new wave of applied theoretical work is needed, to incorporate recent empirical findings that have revealed the limits of the earlier theoretical framework. Second, continue expanding and improving empirical work, in particular experimental work. More ambitious, potentially more expensive experiments, should be conducted. Third, expanding theoretical and empirical work on the aggregate consequences of micro-level distortions, themselves identified by the new theoretical and the empirical work to be done under the first and second areas of focus.

¹ Most of these thoughts are the product of years of joint work with Abhijit Banerjee. In particular, some of these ideas have been introduced (and are developed in more details) in Banerjee and Duflo (2005, 2009, 2010).

Development economics has seen tremendous growth in the last fifteen years. The initial advances were theoretical. These advances in turn opened a new empirical agenda, and the bulk of recent work in the field has been empirical. In particular, they have been at the forefront of the use of field experiments, considerably refining and improving the method in the process. They have used field experiments to test specific theories. The empirical findings have validated part of the theoretical model that served as an orienting framework for the “new birth” of development economics, but they have also shown the model’s limitations, and spawned new questions.

In this paper, I will argue that, to continue growing and to improve in the next decade, development economics should focus on three big challenges: First, developing a new testable theoretical framework to orient research. Second, test the empirical relevance of this model. The rigorous, creative, microeconometrics and experimental work needs to continue, to test theories developed under this new framework. Third, incorporating microeconomic models and findings into a coherent macroeconomics model.

1) Developing a testable theoretical framework

With the phrase “poor but efficient,” Ted Schultz captured an entire research philosophy, namely, there is no reason to think that the poor are any more irrational or ignorant than the rich. They, too, are doing as best as they can given their limited resources.

Schultz’s principle that our working attitude should be that the poor are no different from other human beings represented an essential shift, away from a position where “culture”, or “tradition” were seen as key constraints. To understand the poor, economists should just apply their standard analytical tools.

We can preserve this principle and still ask whether poverty itself changes both the opportunities and decision-making processes of the poor. The realization, from the work of Joe Stiglitz , Debraj Ray, Abhijit Banerjee, and others, that poverty changes the set of options available to individuals, even if there is nothing else that is different about them, ushered in a new era for development economics. Poverty affects behavior even if the decision maker is “neoclassical”. He is more likely to be prevented from borrowing, for example, because he cannot pledge much collateral. He is also more likely to be risk-averse if he has limited access to insurance, since shocks are particularly painful if one has very little to buffer them. He may thus not be an “efficient” farmer, because the efficient choice would be too risky. He may also never learn what techniques work the best, because experimenting is risky, and he may want to wait for his neighbor to do it for him.

This new paradigm helped structure a vision of the world, and proved fertile ground for the growth of development economics, first with an emphasis on applied theory, then with a shift towards empirical work to test these theories.

This empirical work has substantiated the theoretical models in many ways. To cite a few examples, some studies showed clear evidence on credit constraints and others showed that the availability of insurance does change choices. An experiment provided suggestive evidence of the presence of moral hazard in credit markets. There is now considerable convincing evidence on social learning.

At the same time, in other areas, the empirical work was either ahead of the theory or exposed some of its limits. An area where the empirical work is ahead of the theory is education. There is a large body of empirical work on the determinant of school performance. The vitality of the empirical work contrasts with the relative paucity of suitable theory to explain its results. There is no theory that has anything to say about many questions of interest. Specifically, answering the key policy question of how best to spend money to promote education requires us to move beyond a theory that thinks of a human capital production function based on two inputs, time and money, to a formulation that recognizes incentives problems in education.

Traditionally, development economists have shown no interest in these questions. The presumption was that the quality of education would improve when the demand for good quality education increased sufficiently. There has been no effort to develop a theoretical models similar to those that have given structure to the work on credit and insurance, and this needs to change.

There are also many examples where the empirical work set up to test some of the standard theories, exposed its limits. For example, many studies found very high price elasticity for preventive healthcare goods that are known to have large effects on health, even when the price is very low, or even negative. How can this be squared with a standard human capital model? Farmers were found not to invest in fertilizer, even though it is highly profitable, and available in small quantities, and even after they had an opportunity to experiment with it on their field. However, free delivery when farmers have cash has a large impact on the take up of fertilizer. How can this be reconciled with a model where the only limits to fertilizer adoption are information, credit, or concerns about risk and insurance? These and other results have pointed towards the importance of the limitations of human psychology in explaining some of the behavior of the poor. There is an active literature seeking to incorporate the insights of behavioral economics into development economics, but it is mostly theoretical. Building theoretical models of how poverty affect decision-making of less-than-perfectly-rational agents is an important area of possible progress. An example of this kind of work is Banerjee and Mullainathan (2010), who show how the existence of “temptation goods” (the consumption of which we value only in the present) can lead to poverty traps. This warrants more research.

These theories, if developed, will provide a guide for a new round of empirical work. The theory does not need to be particularly sophisticated. But it needs to provide a framework for delineating interesting questions and for interpreting empirical results.

2) Testing the models: developing empirical work and methods

One of the most important methodological development in development economics in the last decade has been the explosion of the use of experiments, both to evaluate policy and to test programs and theories.

The debate that ensued (sparked by Deaton (2010)) clarified the role that experiments have played and will continue to play in development economics. Much of the criticism about experiments is about the difficulty of generalizing from the evaluation of one particular program to predicting what would happen to this program in a different context. Clearly, without theory to guide us on why a result extends from a context to another, it is difficult to jump directly from one result to a policy conclusion. However, when experiments are motivated by a theory, the results of experiments (not only on the final outcome, but on the entire chain of intermediate outcomes that led to the endpoint of interest) serve as a test of some of the implications of that theory. The combination of data points then eventually provides sufficient evidence to make policy recommendations.

Relating a particular experiment to a theory of change is of course easier when the researchers had the opportunity to choose the experiment. In development, very quickly, experiments were not used only to evaluate the impact of a specific program, but to design and test programs with some theories in mind. Researchers have moved from the role of the evaluator to the role of a co-experimenter. In other words, the researcher was now being offered the option of defining the question to be answered, thus drawing upon his knowledge of what else was known and the received theory.

Thus, by the time experiments started being criticized for being too a-theoretical, they had already moved to being quite theory-driven. Experiments remain one of the most promising ways to make progress on gaining better understanding of the micro-structure of development. It is important to note that they require a level of funding which is higher than typical empirical research, since existing data can rarely be used for the purpose of assessing the effects of experiments.

A related objective should be to improve them, and to better integrate them with theoretical work. They can become more sophisticated, with multiple treatments motivated by an explicit model. (Of course, for ethical and practical reasons, since they are field experiments, they will need to continue to be motivated with at least some hope to improve the lives of the subject.) They can be combined with structural estimation, to allow the estimation of parameters even when experimental variation to identify them cannot be obtained. They can be integrated with statistical systems in, to ensure that long run outcomes can be collected. Some progress on all these fronts is already going on, but hopefully there will be further improvements in the next decade.

3) What all this should mean: towards a non-aggregative growth and development theory

Finally, the third challenge is to incorporate the microeconomic models and findings into a coherent macroeconomic model, to get a sense of the impact of each of these mechanisms on growth and distribution. One key finding of both theoretical and empirical development economics is that there are many reasons why, in developing countries, resources are not put to their best use. This implies that the standard aggregation theorems, which allow us to treat the “capital stock” and the “human capital” in an economy as aggregate construct, break down. We pointed that out in Banerjee and Duflo (2005), and proposed very preliminary way to start thinking about this problem. Since then, this has been an active area of research, both theoretically and empirically. However, much more progress remain to be made in understanding the aggregate behavior of poor economies with market imperfections.

Different types of distortions (credit constraints, imperfect insurance, failure to accumulate human capital optimally), are likely to have different impacts on the evolution of the economy, so work is needed on each of them separately. For example, the psychological constraints we mentioned may have very different impact on aggregate growth than credit constraint.

The next objective should be to bring these models to the data. This could be done through calibration (along the lines of the work carried out by Robert Townsend), or possibly, through econometric or even experimental work, using source of variation that are large enough to affect entire markets.

Further references:

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