

THE MICRO-ECONOMICS OF DEVELOPMENT: WHERE ARE WE? WHERE SHOULD WE GO?

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These are the questions that Wider invited a group of experts in international economic development to discuss on the occasion of the celebration of its 25th anniversary in Helsinki on May 13-14, 2010. The questions are indeed relevant. Development economics as a discipline that emerged in the post-WWII period had been largely dominated by concerns with growth strategies able to induce a catching up of the developing countries with the per capita incomes of the industrialized countries. Structural transformation away from agriculture and toward industry to create high productivity sources of employment had thus been the main concern, with little attention given to the micro-economics of development. This has changed drastically in the last 25 years. Why did it happen? Four reasons can be identified to answer the first question.

Where are we today with the micro-economics of development?

The first reason is the rapid growth in **data availability** at the level of individual economic agents. It started with the massive effort at data collection under the Living Standards Measurement Survey program with the objective of poverty measurement subsequently guided by Angus Deaton's book, *The Analysis of Household Surveys*. By now, data collection has been expanded with a multiplicity of surveys such as countries' household income and expenditure surveys, the release of sub-samples from population census data at the household level, and access to administrative data from private enterprises and development agencies. Expanding data availability has attracted the attention of many micro-economists, many previously working in the fields of labor economics and industrial organization.

The second reason is progress in the **methodological rigor** of empirical techniques. There has been a strong increase in our ability to rely on credible identification strategies, ranging from causal econometrics to the use of randomized control trials. This has allowed quantification to progress from associations (partial correlations) between variables to causality. Causality in turn greatly enhances the policy value of econometric results.

The third is the tremendous **interest in the field** of development as it seeks to answer hugely important questions about behavior, institutions, governance, and policies that could not be addressed in the past due to lack of data and deficient methodologies. This allows to research basic questions such as: What can help kids complete secondary school? What can keep politicians honest? How to link producers to dynamic markets? How to design incentives for service providers?

Answering these questions has large first-mover advantages for professional recognition still to be reaped. More than this, providing the right answers to these questions offers an opportunity to **make a difference** on issues of huge social significance.

Finally, there is **excitement** about the very process of research, with opportunities for interdisciplinary interactions, fieldwork experiences, access to significant budgets to run programs, work in partnership with the private sector, and links to governments and international agencies for policy analysis and program evaluation.

All of this has attracted a new generation of economists which is visible in the huge success of the micro-economics of development in academic programs.

A fruitful approach: an example

Progress with data and methods allows highly productive combinations of theory and empirical analyses based on natural experiments, randomized trials, and laboratory experiments, sometimes all combined into one research effort. This can be illustrated by our work on credit bureaus for microfinance institutions (MFI) done in close partnership with a large Micro Finance Institution in Guatemala. Question asked was what can be the expected impacts on microfinance lenders and their clients from entry of an MFI in a credit bureau.

Theory helps predict that a credit bureau should improve selection of clients by the lender and reduce moral hazard among clients, but could undermine the critical dynamic of group lending and its role in selection, enforcement, and insurance by reporting on client behavior. What is the net effect? How important are each of these elements? Answering these questions will indirectly provide evidence on residual adverse selection and moral hazard in group credit.

The research was able to take advantage of a natural experiment. Because the process of entry into the bureau required significant adjustment in the operational procedures of the MFI, there was staggered entry of its local branches into the bureau. This was done without telling clients by fear of undermining the relation of trust existing with the MFI's credit officers. It allowed to isolate the adverse selection value of a bureau, using standard panel data econometrics. What we found is that the bureau allows a large increase in the number of new clients selected by a credit officer, and an improvement in the quality of performance of the clients selected and retained.

This natural experiment could then be complemented by a randomized control trial. We organized an educational campaign to inform clients on the use of the credit bureau by lenders, how the bureau works (bad behavior is made public, leading to general loss of access to credit), and how they can take advantage of it (good public reputation opens access to outside loans). This allowed identification of the moral hazard value of a bureau. We found that clients in small solidarity groups improved their repayment performance, but that clients in large community banks indulged in additional borrowing with uneven success.

Finally, we turned to an experimental game to answer the question: do MFIs gain more from sharing information through a bureau on group or on individual performance? This was done using a variant on the "public goods" game allowing identification of the trade-off between stronger incentive against moral hazard coming from reporting individual information and preserving group incentives to control adverse selection coming from reporting group information. What we found is that it is better for the MFI to preserve the role of groups.

A comprehensive characterization of impact was thus obtained through the complementarity of results from these various approaches to micro-level research. There are of course plenty of other

examples. Banerjee and Duflo (2010) for instance give an excellent summary of research results on credit markets derived from the interplay of theory and experimental economics.

Where should we go with the micro-economics of development?

We propose six issues worth considering in developing a micro-economics of development research agenda.

1. Imbalance in progress between social assistance and income generation

It is clear that there has been a lot of good micro-economics research in the fields of health, education, infrastructure (sanitation, housing), and social assistance programs (conditional cash transfers, guaranteed employment). Much less attention has been given to issues affecting income generation such as investment, enterprise startups, and employment creation. Why has this been the case? In part it is because it is harder to do in practice: poverty reduction through transfer programs is much easier to achieve than through programs that aim at increasing earned incomes. In part because it is harder to research: earned incomes are affected by a multiplicity of complementary causal factors, and firms are less willing to share information and respond to surveys than households. While social assistance is essential, earned income has to be the main instrument in moving out of poverty. For 600 million Chinese, it was self-employment in agriculture (the household responsibility system) and employment in industry, not social protection that did it.

2. Imbalance in attention between self-employment and wage employment

Within the domain of income generation, more attention seems to have been given to self-employment than to employment generation. The first includes concern with such well-known themes as smallholder farming, micro-enterprise development, technology adoption, gains for households and micro-enterprises from reducing market failures in financial services (microfinance, micro-insurance), and the role of producer organizations. The second includes such issues as job creation in medium and large enterprises, and workers' ability to access remunerative employment. This bias toward self-employment has likely been due to undercurrents of populist ideology in the development profession, intellectual attractiveness of studies of behavior (risk aversion, time inconsistencies, coordination problems, etc.), and empirical issues related to working with larger firms (fewer large firms and more difficult to affect their behavior than that of micro-entrepreneurs).

3. Imbalance in focus between individual behavior and institutions

We have seen many studies on the effects of providing services (such as credit, information, training, and subsidies) to individual households. By contrast, much less attention has been given to improving farmers' organization, microfinance institutions, lobbies, and local governments, i.e., to institutions. This is not for lack of interest, of novel ideas, and of theories. But the empirical analysis of institutions faces major difficulties due to the number of units of observation and the cost of data collection. It is easier to observe thousands of kids and to analyze thousands of households. It is much harder to collect information on hundreds of villages, and impossible to find large numbers of comparable producer organizations, micro-finance institutions, or value chains. Focusing on understanding institutions is thus still a task that remains largely incomplete.

4. Imbalance between empiricism and theory

Of the four dimensions of research mentioned above—theory, natural experiments, randomized trials, and laboratory experiments—, the first may be the most demanding. Because of this, we see much reduced form impact analysis: a specific program, a specific set of reforms, a crisis,

or a violent event. Each program/reform/crisis/violence/reform is one of a kind, combining elements in an idiosyncratic way. There is insufficient learning on the elements or the channels involved. Results only speak to the particular event being analyzed. Insufficient attention is given to the two-way link between theory and empiricism that could reveal more fundamental behavioral parameters or a decomposition of channels, for example decomposing the price and income effects of a conditional cash transfer. Clearly, the profession needs to give more importance to theory and to anchor its empiricism more strongly in models from which the estimated equations are derived.

5. Impact evaluation concepts and methods: Imbalance between rigor and importance

Good econometrics is difficult to do and rare. Bad econometrics can be highly damaging. In that perspective, the concepts and methods of impact evaluation that have come to dominate the micro-economics of development (and go beyond randomized control trials!) have played an important role in serving as an effective disciplinary device. They often focus on the construction of a sample that allows identification of a causal effect with standard econometric techniques such as ordinary least squares, panel estimation methods, instrumental variables, and the use of fixed-effects. This is an important contribution, but it has its dangers. One is that sampling is usually specific to the particular issue analyzed, and hence can rarely address more than one issue. Another is that rigorous use of the impact evaluation methods is demanding, leading the profession to focus on the issues that are easier to identify, even if small and secondary, rather than on larger and more important problems. Finally, no method (except a perfectly implemented randomized control trial with an infinite number of observations) will provide absolutely rigorous causality, without some additional assumptions. For that reason, like in any good econometric analysis, the name of the game in using impact evaluation methods is (1) to support the validity of the underlying assumptions with statistical regularities, and (2) to engage in robustness checks to dismiss confounding factors and check plausible channels of influence.

6. Imbalance between precision and external validity

Finally, rigorous identification is typically established for a specific and narrowly defined context. This raises the issue of how to broaden the external validity of these results. There are two approaches to this. One is to use repetitions in different settings to establish broader validity. This will typically be done by development agencies interested in applications of the innovation rather than by academics interested in first-mover rewards. The second is use of more structural analysis to understand (the expectedly generic) underlying channels of causation. This is definitely harder to do, but is this not the role of economists?