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Can the poor afford microcredit?

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The microfinance movement rests largely on one basic assertion: that poor households have high economic returns to capital.¹ Even a small bit of extra cash, it is argued, can transform money-starved, micro-scale businesses. The challenge for microlenders has been to figure out how to provide banking services in an efficient, long-term way.

The assumption of high returns to capital in poor communities justifies the expectation that, if it can be delivered, microfinance will bring critical social and economic impacts. The assumption also undergirds arguments that poor households can pay high interest rates—rates that are high enough to allow microlenders to sustain themselves without donor help. A recent survey of about 350 leading microfinance institutions finds most institutions charge interest rates and fees clustered roughly between 20 to 40 percent per year, after taking inflation into account (Cull et al, 2008). Some are lower, and some are higher—and, in the rare case, close to 100 percent per year. An expectation of high returns to capital is thus at the heart of both the social and economic logic of microfinance.

So it may be surprising that we in fact have very little direct evidence on the returns to capital of the poor. Indirect evidence, yes, but very little systematic, direct data on how access to capital translates into extra profits for “micro-entrepreneurs.”

We also lack data that can speak clearly to what is perhaps the biggest policy debate within the microfinance community – is microcredit an effective tool for the very poor (or should the focus be on households with incomes only slightly below poverty lines and above them)? Is it true, as Vijay Mahajan, the founder of BASIX in Hyderabad, India has said in summing up the early academic literature, that most microfinance borrowers starting below the poverty line “end up with less incremental income after getting a microloan,” and that borrowing “seems to do more harm than good to the poorest” (cited by Tripathi, 2006). Or can microcredit be a powerful tool to help the very poor, as long-argued by Muhammad Yunus and others?²

There is now good news from the research front. In the past few years, researchers have taken imaginative approaches to measuring returns to capital in poor communities, and the studies are beginning to generate cleaner results. Evidence from Mexico shows high returns to capital, for example, especially for smaller businesses. While an earlier study had showed returns to capital of about 15 percent per month, a follow-up study in Leon, in the state of Guanajuato, yields average returns of 20 to 33 percent per month for small, male-owned retail businesses with no employees other than the owner (McKenzie and Woodruff 2008). Businesses that are identified by their owners as being financially constrained, moreover, have estimated returns to capital of 70 to 79 percent a month—and these businesses are most likely to be run by poorer households. Assuming that financially constrained households do indeed tend to be poorer, the result suggests that poorer households have a greater ability to pay for capital than better-off households, and it makes interest rates of even 10 percent per month seem reasonable (though the result says nothing about female-owned businesses, nor small-scale enterprises engaged in services or manufacturing).

¹ A broader discussion of related issues can be found in Armendáriz and Morduch (2005), chapters 2 and 8.

² A working definition of the “very poor” has been adopted in U.S. legislation on microfinance. It is the population living on under \$1/day per person (at international prices) and people in the bottom half of the poor population as defined by national poverty lines.

Before leaping to conclusions about public policy and where to invest, though, it's important to note what the new papers are *not* saying—and what we still have to learn:

First, the definitions of enterprise profits do not account for the value of unpaid labor, which is expected to be the most important input into productions. Taking into account the value of unpaid labor would likely slash returns.

Second, the papers describe returns to capital but do not map the patterns by poverty levels explicitly.

Third, the focus has been on *average* returns to capital, but policy questions hinge on the distribution of the returns. How do the bottom quarter of entrepreneurs do? The middle half? The top?

This note describes what new research in Mexico, Sri Lanka, and Indonesia shows, identifies its limits, and describes what we need to know to resolve ongoing debates. Based on the evidence so far, the big debates are still far from being resolved. The evidence suggests that the poor are a diverse group. The question so far has been posed as whether or not the very poor can truly benefit from microcredit. A better question is: how many and to what degree?

Why Returns to Capital?

It's important to first step back to note that not all microcredit is used to fund small businesses. Poor households have a wide variety of financial needs that go beyond small businesses—for example, financing healthcare, paying for school fees, and facilitating purchases of consumer goods. Not all poor households run small businesses, and the poorest members of villages are often landless agricultural laborers. Business loans are not always a priority (Johnston and Morduch 2008).

So when we talk about the ability to repay loans, returns to capital are only one part of the equation. The question here is whether the available evidence supports the claim that when borrowers do fund small businesses, the profits are sufficient to justify the interest rates charged by microlenders. Do the estimated returns to business investment justify high microfinance interest rates?

Two Competing Ideas

The global microfinance movement rests on two ideas that, on close inspection, contradict each other. The first idea is that poor households can earn higher returns than richer households since poorer households get an especially big boost from the loosening of their financing constraints. The idea is a good place to start, and it lends heft to the claim that poor households can afford the high interest rates often charged by microfinance institutions. Richard Rosenberg (2002) has put forward the claim most sharply in a much-cited publication of the Consultative Group to Assist the Poor. The implication is that poor entrepreneurs can afford high-priced credit (perhaps even better than some richer customers), and that poor entrepreneurs can and should pay the fees required to cover costs, be they 20 percent or 40 percent per year or possibly higher.

Still, the idea is not without problems. First, poor households may have fewer inputs to complement capital (e.g., less education, fewer business connections, limited political clout, etc.) and thus their returns to capital may remain low. Second, households with more capital may be able to reap returns to scale unavailable to poorer households. Those caveats make the second claim more plausible, which is that many poorer households are in fact relatively *weak* prospects

for loans, and that they can take better advantage of other interventions (schools, health clinics, savings accounts, insurance, and the like). Marguerite Robinson (2001) has, for example, drawn this conclusion in her sweeping assessment of the “microfinance revolution”, and it drives Dale Adams’ wariness of microfinance as a poverty reduction tool (e.g., Adams and von Pischke, 1992). So while microfinance advocates like Muhammad Yunus see credit as a human right (Yunus, 2006), others argue that poorer households tend to have such low returns that expanding credit access to the poorest might only create a heavy debt burden.

Much of where one stands on ongoing microfinance policy controversies – Should credit be targeted to the poorest? Are there better interventions for donor dollars? Should interest rates be subsidized for the poorest? Is there a trade-off between financial sustainability and depth of outreach? – is thus bound up with what one believes about patterns of returns to capital. Unfortunately, the true picture cannot be sorted out by recourse to theory or first principles: they are fundamentally empirical matters.

And, though tempting, they are not questions that can be fully answered by simply looking at whether poor households *do* pay high interest rates. First, this kind of “market test” gives no sense of the *level* of gain that households experience. To see the point, consider the case in which microcredit is priced so that loans are only just worth taking. The interest rate, for example, might be 40 percent while the expected return to capital is 45 percent. The 5 percentage point gain is an important incremental gain (and will keep customers coming back for loans), but it is not a transformative change—and not the kind of gains asserted by Yunus. Second, households may be caught in debt traps, paying interest but falling deeper into a hole.

More important, the market test tells us whether *some* people can pay high interest rates, but it doesn’t tell us anything about people who are not borrowing. Are they not borrowing because they can’t afford to? Or because they have no desire to (but could afford to if necessary)? Household surveys that look at a broad population are needed to see the bigger picture.

What Have We Learned So Far?

Researchers measuring returns to capital run into the same difficulties that make impact evaluations so challenging. The biggest hurdle is to disentangle the pure return to capital (i.e., the improvement in profit that occurs relative to a situation where all else is the same, but the business owner has less capital) from the effect of qualities and conditions correlated with having capital. People with better access to capital tend also to have better access to other resources like labor and markets. They may also be more entrepreneurial, less risk averse, and higher skilled. So when we see that people with more capital have higher profits, it doesn’t necessarily mean that having more capital caused the higher profits. The gains may be due to the other attributes.

New work attempts to address this problem by creating interventions that randomly distribute capital in poor communities. In these interventions, some people get larger transfers, some smaller, depending on a decision formula that leaves an important part of the allocation to chance. De Mel, McKenzie and Woodruff (2008), for example, study 408 small firms in Sri Lanka and offer them a range of cash or in-kind prizes (the in-kind grants are either equipment or inventories, selected by the business owners). The prizes (worth either roughly \$100 or \$200) were large enough to make a difference to the businesses, all of which functioned with capital investments under about \$1000. The researchers picked winners and losers using computer-generated random numbers. The random element (which means that people get access to capital independent of whether they are more talented, more connected, etc.) turns out to provide a key to estimating the pure return to capital.

The real returns to the capital infusions in Sri Lanka turned out to be about 60 percent per year—an impressive return, especially given that nominal interest rates on loans are 12-18 percent per year. As in Mexico, for this population at least, the microcredit story seems to hold.

What Do *Average Returns* Tell Us?

These are averages, though, and the researchers find a wide variance of impacts. Not surprisingly, more able individuals have higher returns.

But, very surprisingly, the average impact when female-owned businesses got more capital was “not different from zero.” About 40 percent of female owners had returns that exceeded the market interest rate of 12-18 percent per year, but almost 60 percent did not—and about half appear to have *negative* returns. Men did better on average, but about a fifth of male owners generated returns below market interest rates.

The study measures returns to capital for a sample of micro-entrepreneurs, not for a sample of microfinance borrowers. The challenge for microfinance institutions is to identify the most promising households in a given location and find financial products that match their needs. This is the hope for microfinance.

My work with Donald Johnston shows how this matters. We asked loan officers employed by Bank Rakyat Indonesia, a pioneering microfinance bank, to assess the creditworthiness of a nationally-representative sample in Indonesia (Johnston and Morduch 2008). As we expected, households with incomes above the poverty line were deemed far more likely to be creditworthy than poor households. Still, the loan officers identified 38 percent of poor households as being ready and able to borrow from Bank Rakyat Indonesia with existing financial products. We found that the right question isn't the one that has generated debate: Are the poor and very poor as a group creditworthy? Rather, the key question is: How many? And can they be cost-effectively identified and served?

Where Do We Stand?

Attention is turning to measuring returns to capital, and new results are becoming available. The work from Mexico and Sri Lanka shows how randomized evaluation methods can be used to estimate clean impacts. The resulting estimates show that financially-constrained businesses can gain much from access to capital, but averages conceal widely varying impacts.

We do not yet have the information—either in quality or quantity—to resolve the big debate: how much are the poorest households likely to benefit from improved financial access? But we have a credible beginning – and that is much more than was available a few years ago. We also know that the most useful studies will tell us about the distribution of impacts, not just their means.

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