Giving Credit Where it is Due

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Abstract

This paper shows how the productive interplay of theory and experimental work has furthered our understanding of credit markets in developing countries. Descriptive facts motivated a body of theory, which in turned motivated experiments designed to test it. Results from these experiments reveal both the success and the limits of the theory, prompting new work to refine it. We argue that the literature on credit can be a template research in other domains. *JEL: O16 C93 Keywords: Credit Markets; Field Experiments*

After a period of relative marginalization, development economics has now reemerged into the mainstream of most economics departments, attracting some of the brightest talents in the field. It is no longer the preserve of development "experts" who pronounce on the strange ways of the world outside high-income countries, but instead serves as a testing ground for fundamental economic theories and the source of exciting new ideas. There is, of course, nothing entirely new about this. Innovative theoretical ideas from people such as George Akerlof and Joseph Stiglitz were inspired by thinking about the developing world. Pranab Bardhan and T. N. Srinivasan, and, slightly later, Angus Deaton and Mark Rosenzweig also resisted the compartmentalization of the field into development and the rest. Nevertheless the extent to which, today, economists in many other fields routinely think about the application of their ideas and techniques in development contexts, seems unprecedented. This new centrality is excellent news for the field and, we venture to hope, for the world it studies.

We believe that one of the reasons for the field's vitality is the opportunity it offers to successfully integrate theoretical thinking and empirical testing, and the rich dialogue that can

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potentially take place between the two. The culture of development economics is particularly well-suited to this tight integration because of the emphasis on collecting one's own primary data based on the theories that are to be tested, a practice that goes back to at least Bardhan and Rudra (1983) and Bliss and Stern (1979) in the 1970s, and Udry in 1980s.

In the last few years, field experiments have emerged as an attractive new tool in this effort to cumulatively elaborate our understanding of economic issues relevant to poor countries and poor people. By enabling the researcher to precisely control the variation in the data, they allow the estimation of parameters, and testing of hypotheses, that would be very difficult to implement with observational data.[1] The results of this body of empirical work, in turn, have pushed theory in new directions.

Much of this paper illustrates the power of this interplay between experimental and theoretical thinking. Rather than discussing this in the abstract, we focus on one area where the recent empirical work has been particularly exciting and useful–credit. Imperfections in credit markets are a fundamental part of the way we think about development and underdevelopment. Credit markets in developing countries also offer up many facts and puzzles that lead us to build theories based on informational constraints, and psychological limitations. The empirical work inspired by these theories, in turn, has generated both support for the theories, which then influenced policy thinking, and new puzzles that have prompted new efforts to improve the theory. We see this substantive, two-way conversation taking place between theory and data around credit markets in developing economies as a promising template for the field.

1 Credit: Some Facts and Theoretical Explanations

1.1 Facts

There is a long tradition of high quality descriptive work on credit markets in developing countries. Within-country studies, such as Ghatak (1975, India), as well as cross-country surveys, such as Tun Wai (1957-58) and Bottomley (1963, 1975), report patterns that are confirmed by more detailed later studies of individual credit markets, such as Aleem (1989, Pakistan), Dasgupta (1989, India) and Ghate (1992, Thailand). Banerjee (2002) surveys this body of evidence.

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[1]Banerjee and Duflo (2009) develop this argument in much more detail.

Most people in the developing world have no access to formal credit and rely essentially on informal credit. In a survey of 13 developing countries, Banerjee and Duflo (2006) find that, with the exception of Indonesia (where there was a large expansion of government-sponsored microcredit), no more than 6 percent of the funds borrowed by the poor came from a formal source. The vast majority of the rest comes from money lenders, friends or merchants.

Informal credit markets are characterized by the following facts.

1. Lending rates can be very high relative to deposit rates within the same local area. Gaps of 30 or even 60 percentage points between these rates on an annual basis (in other words deposit rates of 10 to 20 percent and lending rates of 40 to 80 percent) are common.

2. Lending rates can vary widely within the same credit markets. Gaps of 50 percentage points or more between rates charged to different borrowers within the same local credit market are normal.

3. Richer people borrow more and pay lower (often much lower) interest rates. People with few assets often do not borrow.

4. These divergences in interest rates are not driven by the fact that a lot of these loans are not being repaid. The reason is that in this sector defaults are relatively rare. Both Aleem (1989) and Dasgupta (1989), who report default losses, tell us that 10 percent is very high and 1 to 4 percent is more common.

5. Monopoly power of the lenders over particular borrowers does not appear to cause the high levels of interest rates either. The data about high interest rates mostly comes from settings where there are a number of potential lenders available. Moreover, a number of studies, including Bottomley (1963), Aleem (1989), and Ghate (1992), find no evidence of supernormal profits amongst informal lenders.

1.2 Lending costs and the multiplier

Why are there such large wedges between the interest rates for depositing and lending in developing countries? Why would some people pay so much more than others? The stylized facts suggest that realized default or monopoly power are not going to go very far as explanations. The most natural remaining explanation within the existing framework is the cost of making sure that the loan gets repaid, in the presence of moral hazard or adverse selection. In the appendix,

we propose a simple model that captures this idea. Here, we summarize the main intuitions, which provide a useful framework for thinking about the empirical literature discussed below.

Suppose that an individual, endowed with some wealth, seeks to borrow in order to start a project. The returns of the project are risk free, but the borrower can chose to default after the project is completed and the returns are realized, at a cost proportional to the sum invested (in other words, there is moral hazard). If the cost of default were lower than the interest payment, the borrower would always chose to default. Therefore the lender must insure that the borrower has enough "skin in the game," and borrowers will be credit constrained: they will only be able to borrow up to some multiple of their own wealth.

In addition, imagine that the cost of default to the borrower drops to zero unless the lender exercises due diligence. Think of this as a combination of what the lender has to do before he makes a loan (e.g., finding out where the borrower lives, what he does, what he owns, what type of person he is) and the cost of maintaining enough "enforcers" that the borrower knows that if he tries to default he will pay a price. Part of this cost is likely to be fixed, rather than proportional to the amount borrowed: A lot of basic information about the borrower (location, occupation, references) has to be collected irrespective of how much he borrows.

These fixed costs of administering a loan can explain why interest rates for small loans are so high, why they vary so much across borrowers, and why the poor pay higher interest rates. Since borrowers with little wealth must get small loans, the fixed administrative cost has to be covered by the interest payment, which pushes the interest rate up. But high interest rates exacerbate the problem of getting borrowers to repay. Total lending therefore shrinks further, pushing up interest rates even more, and so forth until the loan is small enough and the interest rate high enough to cover the fixed cost for even a small borrower. In other words, the presence of fixed costs introduces a multiplier into the process of determining the amount lent and the rate charged. As a result, small differences in the borrower's wealth, or in the cost of monitoring the borrower, can lead to very large interest rate differences. And if the borrowers are poor enough, or the fixed administrative cost is high enough, the interest rate could become infinite: these borrowers will be unable to borrow at all.

The emphasis on the role of fixed costs in credit markets is not new. For example, Bottomley's (1975) discussion of high interest rates for the poor makes explicit reference to fixed costs.

Banerjee (2002) takes a more complicated modeling approach, but the basic point is very similar. The same intuitions would hold even if default happens only through bad luck, *if borrowers know how likely they are to be affected by such luck, but lenders do not* (what is often called adverse selection). The reason is that borrowers who are less likely to repay are less affected by high interest rates, because they know that they may not pay in any case. Raising interest rates therefore tends to push out the borrowers who expect to repay but not the ones who have a high chance of defaulting, and, as a result, average default rates go up. Faced with adverse selection, lenders will tend to cut back on the amount they lend, which will tend to reduce default rates across the board. But this increases the administrative cost per dollar lent and pushes up interest rates, causing a credit contraction just as in the moral hazard case. Therefore, like in the moral hazard case, there can be substantial potential social benefits from even a small reduction in the administrative costs of lending.

Nevertheless, moral hazard and adverse selection have some quite different policy implications: for example, whereas a credit bureau (allowing lenders to find out if someone has defaulted on someone else's loan) would be an excellent intervention in a moral hazard world, the case would be less obvious in a world of adverse selection. In particular, by making the borrower's credit history public, it makes it harder for a lender to profit from his private experience with the borrower. But this means that lenders would be less keen to help new borrowers establish a public track record, because they know that once the track record has been established, the borrower would be free to move to other lenders. Thus, knowing whether we live in the adverse selection world or the moral hazard world clearly matters.

1.3   From theory to empirical questions

A growing body of experimental research has reacted, directly or indirectly, to this broad perspective on credit markets. Most of the rest of this paper will show how the empirical findings of this literature have interacted with theoretical models and predictions to build a deeper understanding of credit markets.

We start with the list of key questions.

First, is access to reasonably priced credit really a problem for the poor? The theory sketched above is a purely supply-side theory and assumes that demand is not a constraint. However,

it could be that people only borrow at the high interest rates we report for emergencies and otherwise these high rates are mostly irrelevant since most people do not consider borrowing at these very high rates.

Second, how seriously should we take the multiplier property? A testable implication of either moral hazard and adverse selection is that default should be interest-elastic. Do we see this?

Third, are moral hazard and adverse selection the right concepts for thinking about default? Can they be distinguished empirically?

Fourth, can the impressive success of the microcredit movement in lending to the poor be explained, at least in part, by its ability to generate reductions in monitoring costs, and the multiplier effects thereof? How do they actually reduce monitoring costs?

Finally, what are the consequences of making credit available to the poor? Are they able to start new businesses? Does it increase their standard of living? Does it provide a needed discipline device or, to the contrary, encourage reckless borrowing?

2   Are high interest rates discouraging the poor from borrowing?

There is no perfect way to answer this question–it all depends on what we call high interest rates. One way to benchmark is to use the rates that banks charge to those who they are willing to lend to–which often does not include the poor. In India, banks charge rates of rates of 20 percent or less, but, in a survey of a 120 slums we conducted in in Hyderabad, only 6 percent had a bank loan. On the other hand, 68 percent had loans (obtained from informal sources, such as moneylenders), and the average outstanding loan balance conditional on having a loan was over one thousand dollars. The average interest rate they were paying on these loans was 3.85 percent per month or about 57 percent per year. The average period for which they have been indebted with the current loan was 1.5 years. It does not seem that high interest borrowing is something that people only do in *extremis.*

However, it is still possible that these borrowings represent errors of judgment by the borrowers that they then have a hard time rectifying. A "cleaner" way to make this point is to look directly at the marginal product of capital in the firms owned by the poor and to compare

this with the market interest rates. The marginal product gives us a sense of high interest costs could go before choking off demand. Unfortunately, the marginal productivity of capital is very difficult to know from observational data, precisely because borrowing is a choice, which induces a correlation between investment level and rates of return to capital (Olley and Pakes, 1996).

De Mel, McKenzie and Woodruff (2009) implement an interesting experiment to measure the marginal product of capital. From a sample of 408 very small businesses (with less than $1,000 of fixed capital and an average turnover of $100 a month) in Sri Lanka, a randomly chosen subset received a grant worth about $100. Some of the grants were given in cash, while other were given in kind, in the form of a specific business asset chosen by the business owner. They then collected several waves of data on these firms, including detailed investment, sales, and cost data. The productivity of additional capital for these firms turns out to be very high. The average monthly profits of firms that received the $100 grants increased from $38.5 to $53, which corresponds to a marginal return to capital of 4.6 to 5.3 percent per month (55 to 63 percent per year). By comparison, banks charge 12 to 20 percent per year in Sri Lanka, but only 3 percent of the firms had a bank account for business us and only 11 percent of the firms got any money from any formal financial source to start their business. The pattern of these results is replicated for tiny retail firms in Mexico in McKenzie and Woodruff (2008), where they find even higher marginal productivities of capital (20 to 33 percent per month, and between 900 percent and 3000 percent per year).

Since the Sri Lanka experiment is focused on very small firms, it remains possible that larger firms are not subject to credit constraints, and correspondingly may have much lower marginal returns. It is difficult to imagine carrying out the same randomized experiment for larger firms that are already connected to the banking sector. However, Banerjee and Duflo (2008) take advantage of a natural experiment to estimate the effect of an inflow of credit on the investment and productivity of medium-sized firms in India. The study exploits a temporary policy change that increased the upper limit on fixed capital under which a firm was eligible for subsidized credit. After two years, the change was reversed. The firms affected by the policy were officially registered firms (they are not part of the informal economy) and fairly large by Indian standards, though not the largest corporate entities. Comparing firms that were always eligible to firms that became eligible for increased credit both before, during, and after the policy change, they

find that firms newly eligible for credit experienced a large increase in sales and profits. The results suggest a very large gap between the marginal product and the interest rate paid on the marginal dollar: the point estimate for the firms whose credit actually went up is that Rs. 100 more in loans increased profits by about 90 rupees per year, whereas firms pay around 16 percent per year in interest.

These results suggest that the high interest rates in developing countries are not the only reason why firms don't borrow. Both small Sri Lankan and Mexican firms and larger Indian firms have marginal returns to capital higher than the rates charged by banks, and would be happy to borrow much more if they were offered credit at those rates or even substantially higher rates. Indeed Mckenzie and Woodruff point out that the returns in Mexico are much higher than even the 120 percent rates that for-profit Mexican micro lenders such as Compartamos charge.

What these results are telling us is that there is enormous scope for improving intermediation— at least as far as the poor are concerned. They are able and willing to pay much higher rates than the banks are charging, and if there was some way to improve credit delivery models to allow these banks to lend more to the poor, both sides could be greatly benefitted.

3   Identifying the sources of inefficiency in credit markets

The obvious question that this poses is why banks simply do not set high enough interest rates to make lending to the poor remunerative. The theory we sketched above suggests one general class of answers: raising the interest rate makes the problem of getting borrowers to repay harder, and this is what generates the multiplier which ultimately drives lenders out of certain markets. This section reports on some recent work trying to assess whether these kinds of incentive problems are serious issues in the world.

The challenge in this comes from the fact that incentive problems are, by their very nature, unobservable (if we could observe them, we could directly stop them from happening). A very creative recent attempt to get at these issues by Karlan and Zinman (2010) involved an experiment where randomly selected former borrowers of a South African lending institution were sent letters offering them an opportunity to borrow. The rate at which they were invited to borrow (the offer rate) was also randomized so that different groups ended with different offer rates.

Then, among those who agreed to borrow at an initially high rate, they randomly selected half of the clients and surprised them with a lower rate of interest. They then compare default rates among these different sub-groups, some of whom faced the same offer rate but different actual rates and some of whom faced different offer rates but the same actual rate.

Comparing the repayment behavior amongst people who eventually borrowed at the same rate but who had initially consented to different offer rates isolates the effect of selection. The incentives of these borrowers, *ex post,* were identical, but when they chose to pursue the offer, they faced very different incentives. In particular, the adverse selection model mentioned in the previous section relies on the idea that high interest rates attract borrowers who are intrinsically more likely to default. Hence adverse selection would imply that the group that accepted the offer to borrow despite the high offer rate should have a higher default rate than the group attracted by the low offer rate.

Comparing the repayment behavior amongst people who initially consented to the same offer rate but ended up with different actual rates, keeps the selection effect constant and identifies the effect of higher interest rates on repayment. However, there are two reasons why high interest rates would generate more default. First there is moral hazard—high interest rates give greater reason to default, for those who have a choice. But there is also a possible more mechanical effect–high rates make the repayment burden higher, therefore making repayment more difficult.

To distinguish between the repayment burden effect and moral hazard, they introduced yet another experimental group. A randomly chosen sub-group of the borrowers who were surprised by being offered a lower rate than what they had been offered, were in addition offered the option of holding onto that lower rate into the future, conditional on remaining eligible to borrow. This last condition made default much more costly for that group (because it automatically cancels the next loan) while keeping the repayment burden unchanged in the current period relative to those who had faced the same offer and actual rates, but were told that the surprise low rate was a one-off deal. It therefore picks up the pure moral hazard effect.

The results show little evidence of defaults induced by either repayment burden or adverse selection in this sample. On the other hand, there is clear evidence of moral hazard: the only group that has a substantially higher repayment rate than everyone else is the group offered a low interest rate with the assurance that they could keep it.

Of course, this result must be interpreted with care. The experiment takes place in a market which is in equilibrium. Thus, the lender may have already set the interest rate, the size of the loan, and the maturity of the loan and its own screening rules to minimize the risk of default on actual loans. The original interest rate may have been set to make repayment burden manageable, and discourage willful default. Since subsequent interest rates were either lowered or left the same, the borrowers should have been even more encouraged to repay. Moreover, the experiment sent letters only to borrowers who had previously borrowed successfully at the bank's preferred rate, the worst types may have been eliminated already, both through the bank's *ex ante* screening process and based on the borrowers' track records. This may explain why the repayment burden effect or the adverse selection was not larger. As for the moral hazard effect, it is not clear whether these are the borrowers who will eventually default and are just buying time, since now they have one more loan cycle to make that choice over. It would be very important to try to tease all these things out. An alternative experimental design that would get to that would be to offer loans at varying rates of interest to new clients, rather than former clients, and to propose a wider range of interest rates, including rates that are higher than market rates.

Nevertheless, this study is, rightfully, very influential as a template for good empirical research: it was designed to identify parameters which theory suggest should be important, and that would have been very difficult to identify without a sophisticated experimental design. Similar designs have been replicated in other context by Cohen and Dupas (2010) and Ashraf, Berry and Shapiro (2007) to identify the selection and (potential) treatment effect of prices of health goods.

From the point of view of the theoretical framework sketched above, the main take away from this exercise is that cutting interest rates (into the future, as it turned out) does affect default: When this is true the multiplier property discussed earlier suggests that if the lenders could reduce their administrative costs, the gains could be large, because now the multiplier effect would work to expand credit. Lower administrative costs would allow lower interest rates, which makes it easier to get borrowers to repay, and which further reduces administrative costs, etc. This is exactly what microcredit seems to have successfully done through a combination of innovations in the way lending is conducted. Experiments have been conducted to understand what part of the microcredit "package" really matters. This is important for three reasons: to

help microcredit practitioners in their practices; to potentially offer some guidance on what could be done in banks outside the microcredit movement; and to it further our understanding of the roles of different types of incentives in lending.

4   Microcredit: Reducing Costs of Lending

The term "microcredit" covers a spectrum of organizations and financial products, but a common defining feature is innovations that lower the administrative cost of making small loans. As discussed earlier, such reduction in the cost of lending can have large effects on access to credit and interest rates though the multiplier effect. We first discuss some of the ways in which microcredit reduces administrative costs, includingdynamic incentives, group liability, repayment frequency, and social capital.

4.1   Dynamic incentives

Microfinance contracts typically offer dynamic incentives: that is, the starting loans are small but increase with each cycle, so that a borrower who defaults on the current loan gives up the possibility of a larger loan in the future. This credit ramp should encourage repayment under moral hazard. As the Karlan and Zinman (2010) experiment demonstrates, defaults do respond to the promise of a future loan on favorable terms. However, as Bulow and Rogoff (1989) pointed out in the context of sovereign debt, long before microcredit became fashionable, dynamic incentives cannot work alone. By defaulting the borrower saves the entire principal and interest on the loan, whereas by repaying she can at best assure herself of a sequence of possibly larger loans, but these loans will need to be repaid with interest. To maintain incentives, Bulow-Rogoff show, loans need to grow at least as fast as the interest rate, and if they do grow at that rate they would soon become so large that everyone would default, not wanting a bigger loan. Furthermore, competition between microlenders might undermine dynamic incentives, because a borrower can default on one loan and borrow from another lender—although we see very little evidence that the rise of competition has increased default. On the other hand, as pointed out by Basu (2008) in a slightly different context, the Bulow-Rogoff argument relies on borrowers being able to save on their own after they default, which may not work if the consumers have

self-control problems. Fear of somehow eating into the capital and ending up destitute may be one reason people continue to repay. But overall, we suspect that other aspects of microcredit must bolster the effect of the dynamic incentives to assure repayment.

4.2   Group Liability

Probably the most commented upon feature of the standard microfinance contract is joint liability, in which each member of a group of borrowers is liable for the loans of the others. The precise sense in how members of the group are held liable for each other is often surprisingly unclear, but in general, the notion is to create a situation in which default by one borrower in a group makes it harder for other borrowers to get a loan. Theory argues that this gives clients strong incentives to both screen (Varian, 1989; Ghatak, 1999)) and to monitor (Stiglitz, 1990) each other. The effect of the first one would be to reduce adverse selection, and the second one to reduce moral hazard, both of which reduces administrative costs. The multiplier property means that this small reduction in cost can lead to a large reduction in interest rate, and open credit to others.

But group liability also imposes costs. The incentive of group participants is always to reduce the risk taken by their fellow members, since participants do not benefit from the upside of any risky investment, but are on the hook for the downside (Banerjee, Besley and Guinane, 1994).[2] For this reason, members of a group may impose excessive risk-aversion on members.

To test the effect of group liability *per se*, and distinguish its impact on moral hazard and adverse selection, Gine and Karlan (2009) perform two field experiments in partnership with Microfinance Institution (MFI) in the Philippines. In the first, they randomly selected 56 groups who had a joint liability contract, and informed them that they were no longer jointly liable for their loans, though they still had to meet every week to repay their loans. This treatment eliminates the monitoring effect of being in a group (moral hazard) but not the screening effects (adverse selection), since the selection happened before the change in rules was announced. In the second experiment, the bank introduced individual liability from the beginning in randomly selected areas. In this case as well, clients continued to meet weekly and reimburse in group.

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[2]Fischer (2009) finds evidence of this kind of behavior in laboratory experiments that he carried out among microcredit clients.

Taken together, these experiments are meant to allow us to identify separately the selection and monitoring effects of joint liability. Surprisingly, the default rate was the same in all the groups: those who were initially formed as joined liability and stayed that way, those who were initially formed as joined liability groups and were converted, and those who were always individual liability groups. Moreover, clients were less likely to drop out in individual liability groups: joint liability did discourage some clients from staying part of the group. However it is true that this is a context where default rates are very low to start with, so that the other incentives that the MFI provided (monitoring, etc.) may have been so effective that group liability was not needed. It remains possible that group liability would matter in contexts where there is otherwise a larger temptation to default.

While this seeming redundancy of group liability may be surprising to those of us who had spent time thinking about why joint liability was such a important innovation, it corresponds well to events in the microfinance world over the last few years. Starting with Bangladesh's well-known Grameen Bank (under the Grameen Two model), a global pioneer of microfinance, most microfinance institutions are quietly abandoning joint liability. Like the bank in the Gine-Karlan experiment, however, they do not abandon weekly *meetings.* We will see below why these weekly meetings may be desirable in and of themselves.

4.3   Repayment Frequency and Social Interactions

The standard microfinance product is a yearly loan, and interest and capital are repaid in weekly installments of the same size. Economic theory suggests the possible advantages of tailoring the repayment to the cash flows generated by the investment; for example, a cow does not typically start to produce milk right after it is purchased. However, microfinance institutions tend to consider weekly repayment a way to build credit discipline and thereby avoid default.

Field and Pande (2008) and their collaborators have worked with a microfinance institution in Calcutta, India, called Village Welfare Society, to evaluate the effect of weekly repayment plans on repayment and use of the loan. The groups of borrowers were randomly assigned to one of three conditions: usual weekly reimbursement starting immediately after loan disbursement, monthly reimbursement, or weekly reimbursement starting a few weeks after the loan started.

The results suggest that some modifications to the standard microfinance model may allow

microfinance loans to be more productive. In the first few cycles, monthly reimbursement clients were no more likely to default, and seemed much happier with their loans. Clients required to begin weekly repayments immediately appeared concerned about their ability to repay, and were conservative in their investment strategy as a result. The difference in risk-taking is particularly striking between the two weekly groups. The group which was given a gap of a few weeks was more likely to start a business, and when they started a business they were more likely to make a bigger investment and one that only started paying after a while (Field, Pande and Papp, 2009). For example, women were less likely to buy saris for resale and more likely to acquire a sewing machine. This supports the idea that the nature of the standard microfinance contract leads to insufficient risk-taking.

However, after three loan cycles, default rates started to be higher in the group with monthly repayment than in the group with weekly repayment. One possible interpretation is that this difference emerges because weekly meetings help clients to get to know each other better, which makes it easier for clients to monitor each other, to trust each other, and even to shame each other if necessary. The importance of social capital and trust in microcredit was theorized in an early paper by Besley and Coate (1995). The reason why group liability works, in this view, is not because of the formal structure of liability (which may be hard to enforce *ex post* in any case) but because after being together for a while, people start to value their relationships with other members, and this means either that people are more willing to help those who are in risk of defaulting or they feel embarrassed about being exposed to the rest of the group as a defaulter. This would explain why Karlan and Gine, who have groups that continue to meet as a group despite not having joint liability, see no effect.

The insight that frequent meetings can contribute to social capital and/or trust was nicely brought out by another element in the Village Welfare Society experiment (Feigenberg, Field and Pande, 2009). It turned out that the clients who repaid weekly, and hence met every week, were much more likely to know each other well. After five months, they were 90 percent more likely to have visited each other's homes and to know the family members of other borrowers by name. This induced them to act differently in a trust game that the researchers had them play. Lottery tickets were distributed to some group members, who also were given the option to give more tickets to members of their group. By giving these extra tickets away, a woman

increases the probability that someone in their group wins, but decreases the likelihood of her own victory. If she trusts that other group members would share the proceeds of the lottery, she should distribute the tickets. If not, she should keep them for herself. The results suggest that members who met every week are more likely to give each other tickets; furthermore, this is true even for clients in their first loan cycle, who don't yet know each other well.

This set of results builds upon Karlan's earlier work on the causes and effects of trust in microcredit. Karlan conducted two projects in collaboration with FINCA, a large microfinance institution in Peru. Karlan (2005) finds that behavior of group members toward each other in trust games predicts future savings and repayment behavior. And, using the fact that FINCA Peru forms groups in a quasi-random way (on a first come-first served basis), Karlan (2007) finds that people who live closer to each other or who share cultural affinities also save more in microfinance groups, and are less likely to default on loans to each other.

However, as Besley and Coate (1995) pointed out a long time ago, social capital can potentially have the opposite effect–it could help the group members in colluding in defaulting, making it harder for the MFI to punish them. Why this has not been observed in the world remains a theoretical puzzle.

4.4 Reengineering loan collection

While it is true that weekly meetings might help build social capital and reduce default, the striking fact about microcredit loans is how low default rates remain even in the worst-case scenarios–where there are no weekly meetings and no group liability. This points to one key element behind the success of microcredit that the theoretical literature seems to have almost entirely missed. A part of the the cost advantage of microcredit is quite mechanical. The fact that all loans from a particular group get repaid on the same schedule, that the repayment amount is fixed for each person, and that the group leader typically is in charge of collecting it for the loan agent, reduces the time the loan officer spends collecting. As a result, a loan officer may be able to collect payments from a very large number of borrowers every day. Simplifying the technology of lending also opens the profession of credit officer to a whole new set of potential hires, who need very little education or qualifications, and can be paid little, made to work extremely hard, and given strong financial incentives to avoid default and enroll new borrowers. This plays a big

role in reducing the administrative costs per loans.

In this sense, the group lending and rigid repayment structure that characterizes the microcredit model may actually be less important for incentive reasons than for what they require of the technology of loan collection. To further our understanding, it thus seems that the next step should be to perform experiments where loan officers, rather than clients, would be the main subject of study.

5 The Benefits of Microcredit: Credit Constraints, Temptation, and Self-Control

Taken together, microcredit organizations deliver credit to more than 150 million poor people at interest rates that are much lower than what was on offer before, while continuing to be financially viable (Karlan and Morduch, 2010). The average interest rate paid by borrowers in the slums of the Indian city of Hyderabad was about 4 percent per month before microcredit showed up there, charging less than 2 percent a month and still making money. But is this expansion of credit ultimately good for borrowers? Expanding credit access would always be good if borrowers were fully rational: it would only increase their options, and they would not borrow at high interest rates if they did not have a sufficiently good project. But perhaps borrowers have self control problems that are exacerbated by access to easy credit.

A recent study by Banerjee, Duflo, Glennerster and Kinnan (2009) helps allay some of these concerns, while also providing some evidence on perhaps unexpected benefits of microfinance. The study was done in collaboration with Spandana, a fast growing microfinance institution in South India. It was conducted in Hyderabad, where over 100 slums were randomly assigned to either treatment or control: in treatment slums, Spandana opened an office and started operations; in control slums, they did not.

The study provided evidence that microfinance institutions expands the pool of those receiving credit and help new businesses to get started. Overall, 27 percent of eligible families are borrowing from any microfinance institution in the slums where Spandana started their operations (compared to 19 percent in the other slums). One new microfinance loan in five generates a new business that would not otherwise have been created. Households who are predicted, on

the basis of their baseline characteristics, as being highly likely to start a business, are indeed the ones who are most likely to start a business when microfinance becomes available, and they reduce their non-durable household consumption when they get the loan, presumably to put that extra money into the new business. Those who are the least likely to start a business, on the other hand, increase their non-durable consumption.

Interestingly, in addition to these expected results, we find that, on average, households in treatment slums, irrespective of whether they borrow and what they do with the money, increase their purchase of durable goods for their home or their business (televisions, bicycles, refrigerators, etc...), and reduce their consumption of what they define as "temptation goods." Spandana gives no direction to households on how they should spend their money. However, Padmaja Reddy, Spandana's CEO, ventured a guess that after a year or two, the main effects of microcredit should be to help people plan their spending, because they develop a sense that with the ability to get a loan at a reasonable rate of interest, durable goods, such as a television, a refrigerator, or a cart for their business, are now actually within their grasp.

These results fit with the theoretical predication of the behavioral economics literature. Models of self-control and inconsistent time-preferences (e.g. Laibson, 1997, O'Donoghue and Rabin, 1999) suggest that microfinance borrowing could be a way for the poor to commit themselves to a savings plan Banerjee and Mullainathan (2009) propose a model with regular consumption goods and "temptation goods" that are valued by the consumer in the current period, but not as things to look forward to. They show that, if the marginal utility of the temptation goods falls faster than that of the goods that one looks forward to, there will be a demand for "savings transformation." Consumers would be happy to give up some of their daily spending on temptations to be able to buy something that they look forward to, such as a refrigerator or a television. In effect, microfinance borrowing could be a way for the poor to commit themselves to a savings plan.

These conclusions, based on one study, are obviously quite tentative. But research on the impacts of microcredit is accumulating fast, and further evidence should become available in the next two or three years.

But if microcredit is understood as a form of making a commitment to save, then it is not the only way (and possibly not the best way) to offer a commitment for saving. After all, saving

in a more direct form, rather than repaying a loan, involves receiving interest rather than paying it. Several recent experimental studies suggest some possibilities. Ashraf, Karlan and Yin (2006) show that some prospective customers are willing to commit to a binding savings goal (duration or amount to be saved), and those given the option to commit save more on average. Dupas and Robinson (2009) study the effect on micro-businesses owners of offering access to a savings account with steep withdrawal fees, a form of commitment savings product, and find increases in business investment, a reduced sensitivity to shocks, and an increase in overall expenditure per capita as a result. Duflo, Kremer, and Robinson (2009) find large effect on fertilizer adoption in a program that helps farmer commit early to buy fertilizer later in the seasons.

6 Conclusion: Theory and Experiment in Development Economics

The experimental literature on credit markets in developing countries shows the powerful connections between empirical research and theory in development economics. Empirical work in this area has drawn directly on a well-developed body of theoretical research, testing and finding support for some theories, but not others, and has gone on to generate new puzzles that the theory needs to explain. For example, we do not yet have a good theory of why people repay their micro-credit loans that does not rely on people's innate desire to repay. However, as pointed out above, the idea that microcredit may be as a commitment product may offer a possible way out: the service that microcredit provides is something that the borrower will want in the future as well, because the money he saves by defaulting will be gone by then.

Although we have focused in this paper on the credit markets, better integration between theory and empirical practice is very much the trend in all areas of development economics. Nevertheless, there are some areas where theory seems to lag behind the experimental work. Education is one such area, where the vitality of the empirical work contrasts with the relative paucity of suitable theory to explain its results. Unlike in the credit literature, there is simply 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.

The implicit assumption in this formulation is that while there many inputs that money can buy, the allocation of money between them is unproblematic and is carried out efficiently at the ground level. Yet, a large number of field experiments have tested the effect of various interventions on learning, and have offered some surprising discoveries. For example, Glewwe, Kremer and Moulin (2009), based on a randomized experiment carried out in Kenya in the mid 1990s, discovered that access to textbooks has no effect on the test scores of the average child. Only the best children benefitted. Banerjee, et al. (2007) evaluate a specific remedial education program in government schools in India, which combines several features: it acts as a pull-out program to reduces class size; it focuses on basic skills for the children who are lagging behind; and it relies on a group of highly motivated, closely monitored young teachers. The program was found to be remarkably effective, and was later replicated in rural areas (Banerjee, et al. 2010). In contrast, the same paper found no effect of the reduction in the student-teacher ratio when government teachers were teaching, suggesting that returns on different inputs were probably not equalized.

This mixture of empirical results suggests that we need to develop a more sophisticated theory of the education production function. Why might adding certain inputs to the educational process have little or no effect, while others have a very substantial effect? One possible reason, suggested both by the effect of textbooks on the best children, and by the surprisingly large effect of remedial education, is that the existing system is elite-focused and therefore inputs are used to maximize the impact on the best performing children.

Duflo, Dupas, and Kremer (2009) offer a parsimonious theory of pedagogy built in part on this intuition: that students indirectly affect both the overall level of teacher effort and teachers' choice of the level at which to target instruction. Choices made by teachers depend on the distribution of students' test scores in the class, as well as on whether the teachers' rewards are tied to test scores. Under some plausible conditions, this model has a number of testable predictions, which vary according to whether teachers' incentives push them to care more about test score improvements at the top of the distribution, throughout the distribution, or at the bottom of the distribution. Specifically when (as in many developing countries), teachers care more improvement at the top, they will tend to focus their attention to the top of any classroom distribution. This means that students at the bottom may be not learn much (or anything) from

the teacher in large, heterogeneous classes. Tracking will force teachers to focus their attention on the students they have in the classroom, and may thus benefit both students at the top and students at the bottom. Furthermore, the median students may not lose from being affected in the bottom track versus the top track: in the low track, he will benefit from teaching much more adjusted to his need than in the top track. These predictions (and other ancillary predictions from the model) where tested in Kenya in settings were schools were allocated an extra teacher for grade 1 who were randomly assigned to either track the students by prior achievement when dividing the class, or to randomly assign them to either section. The results were consistent with the model: both students at the top and students at the bottom benefited from tracking. Moreover, students originally at the median benefited just as much in the top track as in the bottom track. Finally, in non-tracking schools, students at the top and students at the bottom benefit from an increase in average peer quality, but this did not benefit students at the middle. This is also consistent with a model where both direct effect of peer and indirect effects through teacher effort and teacher choice of the level at which to teach coincide.

Many other interesting theories of pedagogy are waiting to be developed. For example, why might it be true that teachers care primarily about the highest performing students? Possible answers include the difficulty of measuring the performance of poorly performing students, teachers' beliefs about which children are more "teachable," or parents' beliefs about which children are more teachable. Part of the theoretical challenge here would be to explain how such beliefs could be sustained. These theories are certain to have testable implications, and each one comes with a very different policy response. But at least at present, such theories do not exist, at least in forms developed enough to test.

This does not suggest to us that the empirical research in this area should stay put. Part of the value of experimental studies in development, as well as of purely descriptive research, is to keep generating interesting facts. As we saw, this is, in some ways, where the literature in credit got started. In some cases, such as the experiments in credit markets, researchers were able to draw on economic theory that was already fairly well-developed, and then test and sharpen that theory. In other cases, such as experiments in education, the empirical results are ahead of the theory, which has made it more difficult to derive general lessons from specific projects, as theory needs to catch up.

We firmly believe that the rest of development economics can follow the same trajectory as the credit literature. The constraint, if there is one, is the trend in theory, which has somewhat shifted away from the simple, illustrative models that were popular in the 1980s and 90s and provided the basis for empirical research in the more recent period (not just in development economics). But it is hard to imagine that this trend will not correct itself soon–there are so many interesting facts to explain, and a parsimonious theory which fits them will certainly find many people ready to go out in the field and design the experiment to test it.

This is, after all, the main reason why we are excited about working in this field. Very few other areas in economics are blessed with questions of first-order importance, challenging puzzles, and an ever-growing ability to collect the data one needs to explore them.

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7 Appendix: A simple model of the credit market

This model present the intuition of section 2 in a simple mathematical form.

7.1 Model set up

Assume that there is a borrower with wealth w who wants to invest an amount $k > w$ in a technology $f(k)$ with a decreasing marginal product of capital. To do so he needs to borrow the rest, $(k - w)$. There is a competitive lending sector where the cost of capital is ρ. Assume that the loans are offered at an interest rate r.

We focus on the problem that the borrower can default. By expending a cost $\eta k, \eta < \rho$ he can avoid having to pay, in which case his payoff is $f(k) - \eta k$. If, instead, he chooses to respect the contract his payoff is $f(k) - r(k - w)$. The lender will not lend if the borrower has no incentive to repay. To avoid this possibility, lenders set k so that

$$f(k) - r(k - w) \geq f(k) - \eta k,$$

which[3] is equivalent to the condition that

$$k \leq \frac{r}{r - \eta} w.$$

The lender will not lend any more than this, because it knows that the borrower will default if he is given any more. The borrower, however, may very well want to borrow more. That will be the case when the borrower borrows the maximum allowed but his total investment k is such that the marginal product of capital is still greater than the interest rate: $f'(k) > r$ When this holds we say that the borrower is credit constrained. Obviously if the borrower happens to want less than $\frac{r}{r-\eta} w$, he is not going to be credit constrained. Notice that when r goes up, the credit limit k for a constrained borrower will go down. A higher interest rate creates greater incentive to default, which requires that the borrower be reined in even further.

To figure out what r will clear the market, we need to say something about the lender's costs. We already assumed that the cost of capital is ρ. In addition, assume that there is a fixed administrative cost associated with making a loan, which we will denote by ϕ. If the lender

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[3]This equation makes it clear why we need that $\eta < \rho$. Otherwise the lender can always set $r = \rho$ and then the borrower would never want to default.

does not pay this cost, the borrower will act as if η were zero and always default. Furthermore, assume that this cost does not vary with the size of the loan. Adding some costs that increase with loan size (the lender may want two references for someone who is borrowing a lot rather than one) will not change the spirit of our conclusions.

Given these assumptions, and the fact that competitive lenders are not meant to make profits, r will be determined by the requirement that the lender's revenues equal his cost,

$$r(k - w) = \rho(k - w) + \rho\phi.$$

We assume here that the lender spends the fixed cost at the time the loan is made, and so needs to cover the interest cost on that expense.

7.2   Model Implications

Now consider a credit constrained borrower who wants to borrow as much as he can. For him $k = \frac{r}{r - \eta}w$. Using this, a bit of algebra gives us,

$$r = \eta + \frac{\rho - \eta}{1 - \frac{\rho\phi}{\eta w}}$$

A number of observations follow from this equation.

First, when wealth w increases, the interest rate r clearly goes down. And the maximum size that can be invested (k) increase, for two reasons: One is the effect of being able to post more collateral. The other is that the fall in the interest rate allows the borrower to borrow more. When wealth w is so low that $\frac{\rho\phi}{\eta w}$ is close to 1, then the interest rate can be extremely large.

Second, for those who have so little of their own wealth that $\frac{\rho\phi}{\eta w}$ is greater than 1, no lending is possible: some people with low wealth will not be able to borrow at all.

Third, when wealth is low, or the cost of capital or the cost of administering the loan are high, and $\frac{\rho\phi}{\eta w}$ is close to 1, small changes in η or ϕ, which measure how easy it is to monitor the borrower, will generate large shifts in in the interest rate.

The key insight of this model is the *multiplier property*, which contracts the supply of credit: Borrowers with little wealth must get smaller loans, so the fixed administrative cost has to be covered by the interest payment on a small loan, which pushes the interest rate up. But high interest rates exacerbate the problem of getting borrowers to repay. Total lending therefore

shrinks further, pushing up interest rates even more until the loan is small enough and interest rate high enough to cover the fixed cost.

The predictions of this model fit the facts we saw above. The lending rate r can be much higher than deposit rate ρ when the borrowers are poor. As the borrower gets richer, he can borrow more and pays a lower rate, while the poorest borrower will face complete exclusion from the credit market. Small differences among poor borrowers, in terms of how easy it is to get information about them or to bully them, may turn into large differences in the interest rate that they face. In this sense interest rates are likely to be most variable when they are high, which is also consistent with what we see.