

Microcredit Under the Microscope: What Have We Learned in the Past Two Decades, and What Do We Need to Know?

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Abstract

Research on microcredit is now two decades old. There has been enormous progress in understanding both what microcredit does and how. Yet a lot of what we have learned has raised new and often quite fundamental questions about its nature: Is microcredit primarily about investment, consumption, or savings? Why is it that the investments financed by microcredit do not always lead to income growth, and does this have to do with the structure of microlending? What are the roles of social capital, reputation, and group lending? This article attempts to take stock of this significant body of work and tries to identify the most important questions for future research.

1. INTRODUCTION

Interventions aimed at delivering affordable credit to poor borrowers are not new: The usury laws and Islamic prohibition on interest are clear and longstanding examples of public policy inspired by this goal. There have also been many attempts throughout history to set up institutions for directly supplying credit to the poor (Guinnane 1994, 2002). That said, modern microcredit, as an institutional mechanism for improving credit access for the poor, is unprecedented in its scale and visibility. In 2011, according to the Microcredit Summit, there were 195 million microcredit borrowers (<http://www.microcreditsummit.org>).

The basis of this expansion has been a combination of lower interest rates and a willingness to lend to people who have no previous connections to the formal financial system, which is most people in the developing world (Banerjee & Duflo 2011). In the absence of microcredit or some variant thereof, most poor people borrow either from friends and neighbors or from a professional moneylender. Whereas credit from friends and family can be quite inexpensive or even free—as Udry (1994) points out, there is an insurance component built into this kind of credit—available research suggests that moneylender credit is expensive, although it must be recognized that the data on this tend to be patchy and not necessarily representative. Robinson (2001) and Banerjee (2004) list moneylender interest rates that go from 4% per month (60% annual, 50% or so real) to simply astronomical rates such as 5% per day and above. Microcredit rates can also be high: In Mexico rates of over 100% are common, and 11% per month is standard in South Africa on small individual bank loans to poor borrowers. However, in the countries where microcredit has had the greatest success (e.g., Bangladesh, Bolivia, India, and Indonesia), interest rates are significantly lower than 30% per year. Few studies collect both moneylender interest rates and microcredit interest rates paid by the same households, but those that do find large differences: 3.84% per month (nearly 60% per year) charged by moneylenders versus 24% yearly rates for microfinance institutions (MFIs) in urban Hyderabad, India (Banerjee et al. 2010b), and 103% for moneylenders versus less than 30% for MFIs in 156 Bangladesh villages (Mallick 2012).

Moneylenders also tend to limit the set of people they lend to. Robinson (2001) reports that many moneylenders have only 15–20 clients. Based on a detailed survey of 14 moneylenders in rural Pakistan, Aleem (1990) reports that many of them will either not lend to someone they have no previous business dealings with in another capacity or refuse as many as three out of four potential new clients. By contrast, many MFIs lend to anyone who lives in their coverage area and meets a certain set of simple and explicit criteria.

Finally, like moneylenders, MFIs have very low loan default rates. Although default reporting is often somewhat murky (see, e.g., Morduch 1999a), default rates significantly below 10% are common, and those below 2% are seen often as well. These compare favorably with the previous experience of institutional lending to the poor in the developing world.

How is it that MFIs manage to provide credit at lower rates to a more diverse and less familiar group of borrowers than moneylenders, and more specifically, how do they control default without losing control over monitoring costs? This is the subject of Section 2. Section 3 then discusses the current evidence on the impacts of microcredit. Finally, Section 4 discusses interpretations of why the impacts are the way they are and concludes with a discussion of the role of microcredit in social policy. As is clear in this description, I make no attempt to go beyond the credit side of microfinance except in passing. Even though both microsavings and micro-insurance are potentially important parts of the overall agenda, the research on these services is still in its infancy.

This article is by no means the first attempt to review the microfinance literature, even within the academic economics discourse. Both Morduch (1999b) and Ghatak & Guinnane (1999)

provide excellent reviews of the literature around 1999 from very different perspectives, whereas Armendáriz & Morduch's (2010) textbook is a handy introduction to the state of knowledge at its time of writing. However, there was a burst of new research on the subject right after that book went to press (perhaps in some cases inspired by the book), including a large number of randomized control trials and other quasi-experimental work covering a wide range of issues that justifies another review now.¹

2. THE TECHNOLOGY OF MICROLENDING

To understand how microlenders may be able to do better than moneylenders, at least along some dimensions, one will find it worth revisiting the reasons why moneylenders charge so much, even under competitive conditions. Basically, lenders want borrowers to have enough “skin in the game” to have sufficient incentive to repay, and as a result, poor borrowers get small loans. However, the interest on that small loan still has to cover the fixed costs of lending (e.g., finding out where the borrower lives, recording his or her repayments). The interest rate therefore will have to be high. But a high interest rate distorts the borrower's incentives still further, making it necessary for the lender to either cut the loan size even more or spend additional resources on monitoring the borrower, either of which drive the interest rate higher. And the cycle begins again. This is what Banerjee (2004) describes as a credit multiplier (for a simple model that captures this, see Banerjee & Duflo 2010). A direct consequence of this multiplier is that small savings in lending costs can translate into large differences in the interest rate. From this perspective, microlending can be seen as an innovation in the technology of lending that leads to a large reduction in interest rates that result from this multiplier.²

2.1. Group Selection and Group Monitoring

One of the distinctive features of many early microlenders (e.g., Grameen Bank, BRAC, BancoSol, but not Bank Rakyat Indonesia) was group lending. Borrowers were formed into groups, and group members were made jointly liable for one another's loans—if one member failed to repay, the whole group would be held liable and face penalties unless they paid up for their delinquent colleague. This feature attracted a lot of attention in early theoretical research on microcredit: The basic idea was to study this as an innovation in mechanism design.

2.1.1. Insurance arguments. In a setting in which there is no asymmetry of information between the lenders and the borrower, and no voluntary default, the group can play an important insurance role. Suppose investment earnings are stochastic, but borrowers want to repay their loans and only default if they have no choice—this is essentially the setup in Besley & Coate (1995) when the default cost is very high. An individual loan contract in this setting has undesirable insurance properties; basically, the borrower ends up paying the lender even when his or her marginal utility of consuming is very high. A group loan in this setting can be a way to mitigate this: Each successful borrower chips in a little bit to help the less fortunate fellow group

¹Conning & Morduch (2011) provide another recent review of the literature, but their explicit intent is to locate microcredit within the broader finance literature.

²Alternately, it is a technology that allows financial institutions—which have a much lower cost of capital than moneylenders but usually cannot compete with them because of their much higher costs of lending—to become competitive in the market for tiny loans.

members meet their obligations without sacrificing too much consumption.³ The joint liability here ensures that, *ex post*, the successful borrowers do not simply walk away from their commitments and therefore enhances efficiency. As a byproduct, more loans get repaid and interest rates are lower.

Of course, the problem here is that the lenders are too powerful—the default cost is so high that people do not default even when it is efficient to do so. By and large, the literature has taken the view that the main problem is the opposite, that lenders are too weak to enforce repayment, although there are policy makers in India who have recently taken the opposite view (for the details, see the section on social norms below). Therefore, the focus has been on avoiding default.

2.1.2. Incentive arguments. The intuition is simple: Group members have better information about one another (or one another's actions) than does the financial institution; they are in a better position to observe both other borrower's types and their actions. They may also be better able to punish those who default, for example, by withdrawing social capital from those who misbehave; alternately, people pay up to avoid being exposed as a defaulter in front of the group. The challenge is to give the group incentives to use this information/enforcement ability in the interest of the lender (who may pass the gain from this back to them in equilibrium in the form of lowered interest rates). Group liability is potentially a way to achieve this goal.

The Banerjee et al. (1994) model is perhaps the simplest version of this idea. The economic environment has *ex ante* moral hazard—the choice of the investment project is noncontractable. Group members can monitor the actions of other group members much more cheaply than can the financial institution but have no money to lend. Moreover, monitoring itself is not contractable. The joint liability contract now becomes a way to induce group members to monitor the actions of their group to prevent default, as they are partially liable. It follows, however, that group liability contracts will discourage risk taking because group members have no stake in the upside from any risk taken by their fellow group members. In other words, the very high repayment rates many MFIs proudly report may actually reflect inefficiently low levels of risk taking among their clients.

Banerjee et al. (1994), however, do not allow the group members to collude. Why not claim that one will monitor, get the lower interest rate, but then, instead of monitoring, jointly choose precisely the actions the lender does not want the group to choose (and, if need be, make transfers so that the whole group comes out ahead)? As Stiglitz (1990) points out, joint liability itself can be an antidote against this kind of collusion. Higher levels of liability clearly impose a higher tax in expectation of the groups that collude on the high-risk option, conditional on at least one of them succeeding. But this effect is counteracted by the lower probability of at least one of them succeeding—as a result, the expected tax is not necessarily higher *ex ante*. However, there is a third effect: Higher liability allows the interest rate to be lower, and the lower interest rate benefits the low-risk action more because those who take the low-risk action pay interest more often. Stiglitz (1990) shows that the combination of the three effects can always be signed at zero liability and on net, increasing liability starting at zero while keeping the payoff to the low-risk action constant always lowers the payoff to high-risk action even if borrowers collude.

Although this shows that there exists a positive liability level that reduces inefficient risk taking, this is not true for arbitrary levels of liability. When the liability payment is so large that the safe payoff is not enough to cover it and repay the loan, the risky project will be preferred. In the real

³Udry (1990) articulates the view that efficient credit contracts should have some insurance built into them.

world, liability often tends to mean full liability for institutional reasons, so this is potentially a real concern.⁴

Turning to situations in which there are hidden types rather than hidden actions, a variant of Stiglitz's intuition also applies. Ghatak (2000) and Van Tassel (1999) analyze the case in which borrowers know one another's risk type but the lender does not (see also the related arguments in Ghatak 1999). The lender wants to charge the low-risk types a lower rate so that they do not exit the market; group liability offers a way to do so. The problem is that groups are allowed to collude—if the lender offers lower rates to groups claiming to be low risk, won't high-risk groups pretend to be low risk? A feature of the structure of joint liability keeps this from happening. For any fixed interest rate and liability, safe borrowers value safe partners more than risky borrowers because the latter succeed less often and therefore cover their partners less often. Hence, safe borrowers will partner with each other. Given that tendency, it is easy for the lender to sort between borrower groups by relying on what is effectively a version of Stiglitz's insight—low-risk groups benefit more from a combination of higher liability and lower interest rates than do high-risk groups. Joint liability contracts therefore aid sorting.

Laffont & N'Guessan (2000) study the same problem but in a setting in which there is no risk of adverse selection. They follow Armendáriz de Aghion & Gollier (2000) (who study the same problem under the assumption of no collusion) in assuming that the safe types also get higher payoffs when they succeed.⁵ Therefore, raising the interest rate or the liability payment pushes out only the worse types. In this setting, joint liability aids the extraction of rents from the safe/high types without losing the business of the risky/low types. Liability in the optimal contract should be negative to achieve this—borrowers repay less when others default—as the whole point is to subsidize groups of low types where there is a lot of default.

When there is ex post moral hazard—i.e., voluntary default by those who have the money to pay up—the problem is somewhat different. As Besley & Coate (1995) point out, group liability can exacerbate defaults. If some group members are bound to default because they have no money, the rest can avoid having to bail them out by opting to default. This compulsion is absent under individual liability. Of course, if people do pay for others, there can also be fewer defaults than under individual liability. Moreover, there can be multiple equilibria, in one of which no one repays only because no one else is repaying. However, in this context it is not clear if the group lending contract is optimal. For example, in a context in which both parties can pay, but may opt not to, Bond & Rai (2008) show that if output is deterministic, the voluntary default equilibrium can be eliminated by a slight redesign of the microcredit contract based on insights from implementation theory (basically, promise each group member that if the others default and he or she pays, that payment will be refunded). More generally, it is not clear why, when there are at least two players who both exactly know the state of the world ("X really cannot pay but Y can"), which is the assumption in Besley & Coate (1995), we would not make use of the powerful cross-reporting mechanisms common in implementation theory, in which one asks A about B's type and vice versa. For example, in Besley & Coate's environment, Rai & Sjöström (2004) show that a cross-reporting mechanism can implement the first best outcome as long as there is no collusion among the borrowers, but the joint liability mechanism cannot.

⁴Madajewicz (2011) makes the point that liability is usually set to be the same across all borrowers in a group, making it a bigger tax on richer borrowers who always have money to cover the losses of others, and as a result, richer borrowers may prefer individual liability.

⁵This contrasts with the classic Stiglitz & Weiss (1981) formulation of hidden information in credit markets adopted by Ghatak (2000) and Van Tassel (1999).

However, the assumption of no collusion is key; with enough collusion, Laffont (2003) argues, the microcredit contract may be the best one can do. That said, he shows this only for one specific adverse selection environment. In the Besley-Coate *ex post* moral hazard environment, Rai & Sjöström (2004) show that group liability with cross-reporting does better than just group liability, even if the group members can collude, as long as they cannot sign a binding contract before the state of the world has been realized. The source of the gain comes from cross-reporting, which allows the mechanism to treat voluntary defaults differently from forced defaults, whereas the standard group loan contract treats them exactly the same.

In practice, we do not see explicit cross-reporting mechanisms, although the loan officer does sometimes query other group members when someone defaults and may use that information to treat the default. It is also unusual for people to be rewarded for tattling on others, although there are obvious reasons why that may not be public. Informal discussions emphasize the loss of social capital from reporting on a fellow group member, but there are many subtler ways of conveying what one thinks (e.g., by just being silent when a fellow group member is being accused). Better understanding the use of cross-reporting mechanisms and the possible constraints on them remains an important direction for future research.

More broadly, much recent theoretical work on microcredit asks whether it is possible to improve on plain vanilla group liability by bringing in insights from mechanism design and implementation theory. Bond & Rai (2008) show that a contract in which only some people get the loan and the rest simply bear liability can sometimes improve on the symmetric group lending contract (in which all group members get loans and are liable for the others). The question is how to compensate the nonborrowing members. Perhaps the cosigner can be paid, potentially conditional on the outcome of the loan. Another issue is whether everyone needs to get the loan at the same time or whether loan access can be staggered to provide better incentives (see Chowdhury 2005, Aniket 2007).

2.1.3. Preference changes. A third possibility not really discussed in the theory literature is that being part of a group changes preferences. For example, people may feel more generous and optimistic about others they know well. Close ties could improve the functioning of microfinance groups if the core challenge were insurance, but their effect on incentives is ambiguous—for example, people may find it hard to punish or default by those they love. Depending on which effect dominates, personal relationships would also affect group formation: People may prefer to form groups either with individuals they are close to or with individuals they do not know.

2.1.4. Empirical evidence. Compared to the richness of the theoretical research on the subject, the body of empirical work is somewhat thin. There are two basic approaches. One is to compare the performance of group loans and individual loans, after doing whatever is possible to ensure comparability. This has the potential to give the right policy answer but does not tell us what is driving it. The other approach is to focus on the correlation between some reduced-form measures of the social capital and behavior of group members (e.g., primarily repayment, but also information). The danger here is that we may not find what we were looking for and may mistakenly conclude that it is not there—for example, because we are focused on the wrong measure of social capital or the wrong type of behavior.

Group loans versus individual loans. The clearest evidence that group membership matters comes from Giné et al. (2011). A Muslim community organization in India asked all their members to default on their microcredit loans. Because of strict joint liability, Hindus who were in groups with Muslims also had reason to default. Giné et al. (2011) show that the same Hindu household

that was in multiple groups was more likely to default on its loans from groups that had a higher proportion of Muslims. This does not, however, quite tell us that group liability is the problem, as the paper acknowledges. It is true that these groups had joint liability, but as we see below from Breza's (2012) closely related paper, group effects persist even when there is individual liability.

The comparison of individual and group liability loans in observational data is fraught with difficulty. To begin with, there is no guarantee that lenders who make group loans have the same incentives as those who make individual loans. Even the same lender might adopt a less commercial perspective when he or she makes group loans than when making individual loans. Moreover, if the loan officers believe in any of the theories discussed above, and assume that group loans are safer than individual loans even if there are no real differences, then they will adjust the target population as well as the size, terms, and monitoring of the loan. The terms and especially the monitoring are likely to be at least partially unobserved by the econometrician. Therefore, systematic differences in default rates on group versus individual loans would exist even if they were both group (or individual) loans. In particular, even if it were true that *ceteris paribus* group loans are safer, a profit-maximizing lender may design loan products in such a way that individual loans end up being safer than group loans.⁶ There is also selection on the borrower side. For example, a risk-taking, dynamic individual may prefer not to get involved with a group (e.g., so as not to have to waste time monitoring neighbors), and this by itself can generate a correlation between loan type and default. Group loans may also be offered in different areas and to different people.

Experiments and quasi-experiments offer a way around some of these problems by ensuring that group and individual loans are offered in similar populations. However, many of the problems stemming from the endogeneity of who actually gets offered a loan, the terms of the loan, and who takes it up are not easy to avoid, even in an experimental setting. Recently, Attanasio et al. (2011), for example, compare group and individual liabilities in Mongolia by randomizing locations where each will be offered. They find no difference in default rates (the point estimate on group liability is actually large and positive, although far from significant), but interestingly, there is more business creation and higher food consumption levels in group liability locations. They interpret this as evidence for greater monitoring of loan usage by other group members, but it could as easily be the effect of who gets offered or accepts an individual loan. As their data show, individual loans are substantially larger and are much more likely to be collateralized.

Giné & Karlan (2011) carried out a similar experiment some years before in the Philippines, although, unlike in Attanasio et al. (2011), individual liability borrowers were also formed into groups, and repayment was done in a group setting, ensuring that the public loss of face from defaulting remained. Like in Attanasio et al. (2011), individual and group liability borrowing locations had the same default rates. Once again, this could be because the lending pattern adjusts—in this case, individual loans tend to be smaller, and in the long run there is less individual lending overall.⁷

However, Giné & Karlan (2011) also report on another experiment with the same MFI partner that directly tackles the problem of endogeneity of lending patterns. They persuaded the MFI to take a randomly chosen set of groups that had been formed under the status quo assumption of

⁶This is because of the nonconvexities and complementarities in the lending technology. It may be, for example, that the lender prefers not to screen at all for group loans and therefore tolerates a certain amount of default, but with individual loans he or she feels there is a need for some screening, and once the fixed cost of screening has been paid, the lender can ensure that he or she lends only to safe borrowers.

⁷Unlike Attanasio et al. (2011), Giné & Karlan (2011) do not have data on business creation and consumption.

group liability and switch them to individual liability—so that borrower selection was unchanged. Members of these groups already had outstanding loans, the terms of which were not changed in any way. The data from this experiment reject anything more than a very small change in the default rate. The authors conclude that groups do not seem to play a monitoring role.

This is a clever design, but it has one obvious limitation. If the group is sufficiently good at screening, then the absence of an effect on defaults when the loan switches to individual liability might just reflect the initial selection of extraordinarily reliable borrowers under group liability. One way to get around this problem is to repeat the same experiment in the opposite direction: Let borrowers and terms be chosen under individual liability and then impose group liability. We should see a reduction in defaults if group liability has an effect on monitoring. Carpena et al. (2012) come close to implementing this design by taking advantage of a natural experiment. An Indian MFI switched from individual liability to group liability, and this change was phased in so that no one had to switch midcycle. To avoid any selection issues, the analysis is restricted to borrowers who had a loan before the change in regime and who eventually got another loan from the MFI. The paper compares the repayment behavior of borrowers who finished their cycle early and switched to group loans with that of those who were on a later cycle and therefore did not switch. It finds that the move to group liability reduces missed payments by between 8 and 18 percentage points, which is substantial.

Unfortunately, there is one respect in which this was not quite the ideal experiment: The terms of the loan changed when group loans were brought in—group loans are smaller but carry a higher interest rate. This raises the question of whether it was the group's vigilance that led to lower default or just a smaller loan size. Carpena et al. (2012) do find the same result when they control for loan size, but as loan size is an outcome, it is not clear that this solves the problem. The measure of default they use is also not ideal: Because they only consider people who got another loan and therefore had not defaulted on their previous individual loan, default is, by definition, impossible. They therefore focus on missed payments that are eventually made up. Perhaps loan officers tolerate late payments but the group does not; however, this scenario does not represent the kind of monitoring we are really interested in.

Turning to the role of groups in selecting members, the evidence is even weaker. Carpena et al. (2012) show that the groups that form after group liability is introduced tend to be assortatively matched on potential default rates as measured by missed savings contributions in the past (six months of savings contribution are required to borrow from this MFI). People with better contribution records end up matched with one another. The study does not, however, report on whether this matching reduces default (although because the default rate is below 0.5% under group liability, the effect cannot be large). Moreover, it does not tell us whether the groups would be different had group formation been required under individual liability. Nor does it present evidence that the loan officers are setting the terms of the loan to strategically exploit the self-selected nature of these groups as Ghatak (2000) suggests that they would—for example, do the potential borrowers with the worst histories drop out when there is group lending?

Nevertheless, this study does support the view that borrowers know something about the reliability of others, although what they know is also observable by loan officers, and not something loan officers cannot learn, which is the focus of the theory. Giné & Karlan (2011) also find that new members who join after the conversion to individual liability are less likely to know other members and that, under individual liability, group members know less about past repayment problems of others and are worse at predicting who will have repayment issues in the future. However, as noted above, because lending patterns also change, it is hard to conclude from these results that group liability induces people to have more information.

We still seem to be missing the right experiment here, and I am not entirely sure what that would be. Karlan & Zinman (2009) separate out selection from incentives in the credit market by offering people different terms *ex ante* but then surprising them and offering them the same terms *ex post*. Along the same lines, perhaps researchers can ask randomly chosen potential borrowers to form groups under group and individual liabilities and then invite them all to switch to the same individual liability contracts (adequately sweetened so that everyone wants to switch). The challenge here is in setting the terms of the loan so that the original individual and group loans are comparable, as a group loan that bears the same interest rate as an individual loan imposes higher interest costs on the borrowers (because of the group liability) but perhaps offers better insurance against default, therefore inducing a different selection even in the absence of the group liability. The group interest rate needs to be different from the individual rate, but by how much? This depends critically on a borrower's expectations about other borrowers, his or her risk aversion, and the default cost. There seems to be no way around this problem without, for example, imposing some structure on preferences and expectations.

This general problem—too many things changing at the same time when we move from group to individual liability—has encouraged some scholars to use lab experiments in the field to study microcredit. Giné et al. (2010) report on a series of microfinance games played by women who work in the microenterprise sector in Peru and are very familiar with microcredit. The basic structure is carefully modeled on microcredit: Each player gets a loan and is expected to invest it in a project, get the returns, and repay the loan. There is *ex ante* moral hazard, and borrowers can choose less or more risky projects knowing that if their projects fail, they will not be able to repay the loans. Finally, there is either individual liability or joint liability (with two member groups), and within joint liability, there is either perfect information (in which both group members talk and see what the other is doing) or imperfect information (in which there is only *ex post* information about the partner's choice).

The model is set up so that joint liability actually encourages risk taking because the liability eats up the entire surplus generated by the low-risk option. The results confirm that this force exists. Giné et al. (2010) show that when members observe each other's choice, there is much more risk taking overall than when the choices are not observed, presumably because the liability effect is stronger when the other person is taking the risky option. Finally, when allowed partner choice, people match assortatively based on risk aversion, and overall risk taking goes down.

Fischer (2011) carries out a similar exercise in India with potential and actual microcredit borrowers. The key innovation is the introduction of full monitoring by other group members so that all investment decisions are taken jointly. He shows that in the pure joint liability setting similar to Giné et al. (2010), the borrower gets the same result of increased risk taking for essentially the same reason. However, when joint decision making is introduced, risk taking goes down below individual liability levels, consistent with the prediction of Banerjee et al. (1994). He also finds this decline when moving from joint liability to a more profit-sharing-like arrangement.

There is, therefore, evidence of people reacting to joint liability as the theory would have us expect—for example, we see them being more informed, matching assortatively, and trying to minimize risk exposure from their coborrowers' projects when they can. What is missing is clear evidence that in real-world settings there is an effect on default rates. As pointed out above, this could be because loan size responds to liability or because enforcement is so effective that there is no default either way. However, it is entirely possible that the theory is missing something essential: Perhaps observing the actions of fellow group members without being able to control them actually encourages risk taking because it makes it easier to free ride on the other person's choice of a safe strategy. This is what both Giné et al. (2010) and Fischer (2011) find. Or, as mentioned as a possibility above, perhaps people do not like monitoring/reporting against other group members,

especially if they are close to them. Giné & Karlan (2011) find that the move to individual liability leads friends and relatives of members to join the groups and propose this exact reason for the shift. I now turn to the line of research that directly asks about how individuals react to having more people connected to them in their borrowing group.

Social capital. The basic theory here is that social capital is some preexisting connection between group members that is correlated with higher levels of information, enforcement, or just different behavior. The early evidence on the effect of social capital comes from correlations. Wydick (1999) studies 137 lending groups in Guatemala and finds no clear evidence of a correlation between higher levels of social capital within the group (e.g., all of them being female, being friends, or having known one another for a long time) and improved repayment rates. Sharma & Zeller (1997), who study credit groups in Bangladesh, and Ahlin & Townsend (2007), who study group loans given by BAAC in Thailand, find that groups with greater numbers of family members have higher default rates. The problem of course is that these high rates may be driven by the reasons people initially form groups with (or without) family members rather than by the effect of being in a group with family members.

Karlan (2007) studies the effects of quasi-experimental variation in the composition of borrower groups in Peru. FINCA, a large microlender in Peru, uses a simple first-come-first-serve rule to form groups. Because arrival is more or less random, some groups have a higher fraction of people who live in the same neighborhood than others and/or are ethnically more similar. Karlan shows that both these measures are strongly positively correlated with repayment rates. The former is also correlated with higher-than-required levels of savings by individuals in the group's account. Because these savings can be used to cover defaults by other members, more savings should mean greater faith that others will repay. More homogeneous groups, by either of these measures, also seem to know more about the financial status of other members, as measured by whether they know why members who left the group did so (specifically, did they leave after a default?). Most interestingly, more homogeneous groups are less likely to drop defaulters, which is what an optimal cross-reporting mechanism would suggest, if homogeneity leads to better monitoring and hence better insurance. Consistent with this, loan officers told Karlan about being lobbied by other group members to let someone off because the default was not their fault. However, this could also reflect greater sympathy for defaulters.⁸

Perhaps the most compelling evidence on the effect of social capital on borrower behavior comes from Feigenberg et al. (2010), who experimentally vary the level of social capital within groups. Their MFI partner in India randomly assigned some borrowing groups to have group meetings monthly rather than weekly. The extra weekly meetings clearly brought group members together. A year after the experiment ended, borrowers who had had weekly meetings in their first loan cycle saw each other outside group meetings 26% more often. Although there is no difference in loan default or renewal rates between the two groups in the first cycle, those in the monthly treatment were 3.5 times (7.8 percentage points) more likely to default on their second loan, even though by this time everyone was on a weekly schedule and these are individual liability loans. The authors interpret this as the effect of the greater social capital in these groups. They argue that the social capital reduces defaults because members can bail each other out—an insurance rather than an incentive effect. To support this, Feigenberg et al. have group members play a game in which

⁸Using default in a microfinance game developed by Abbink et al. (2006) (see below) as the outcome, Cassar et al. (2007) find mixed results on whether heterogeneity affects repayment. Unlike in Karlan (2007), ethnic/clan-based heterogeneity has no effect, but heterogeneity in the percent of life lived in the area does.

there is scope for risk sharing and show that the kinds of groups that meet more often and default less often also share risk better in the game, which of course does not rule out the possibility that an incentive effect is also there. To bolster the social capital interpretation, the authors show that the reduction in defaults happens mainly in the kinds of groups in which we also see more contact after the experiment is over.

Unfortunately, this insightful experiment was carried out only with individual liability groups; the transfers here were voluntary, and the givers could easily refuse to make them if they felt that they were being taken advantage of. This kind of implicit joint liability—in which the group context simply makes the members more aware of the potential for helping each other out—creates incentives that are quite different from those created by explicit joint liability.⁹ A similar experiment that simultaneously varies group and individual liabilities clearly has the potential to tell us a lot.

Neither of these studies looks at the selection aspect of group borrowing, as Feigenberg et al. (2010) randomize preselected groups, and Karlan's (2007) quasi-experiment is in a setting in which all groups are randomly assorted. Abbink et al. (2006) carry out a version of the Karlan study under the controlled conditions of a laboratory at Erfurt University to see if self-selected groups under joint liability are safer. The researchers design a game that replicates joint liability microcredit but find no clear difference in average loan repayment rates when the group is deliberately constructed of unconnected individuals versus when the group members are allowed to sign up together and therefore are likely to be connected.

What causal mechanism might be behind these effects (and the lack thereof) of social capital? Is it a more effective use of information or simply different preferences/norms, perhaps enforced by some grand supergame strategy that embraces multiple aspects of collective life? To look at this, another innovative study (Karlan 2005) had 397 pairs of FINCA group members play the classic trust game with each other. Karlan finds that greater cultural and geographic distance, both bilaterally between the players and between the players and the rest of the group, reduces trust. Trustworthy behavior is also decreasing with cultural distance to the group. The group effect is striking because it suggests that the collective context matters, but both these facts are consistent with an information as well as a preference interpretation, if we allow closeness of the group to affect bilateral preferences. Indeed, being more trusting is correlated with greater default, which inclines Karlan toward the preference view. The absence of a selection effect is also a strike against the pure information view. The evidence of better insurance that Feigenberg et al. (2010) find could, in principle, result from better information, but that they find better insurance also in laboratory games supports a preference interpretation.

2.1.5. Summary. It was the joint liability aspect of microcredit that first attracted the attention of economists (especially economic theorists), which led to a number of important theoretical insights. However, the empirical research—both on the effect of group liability and on the effect of social capital on repayment, risk taking, insurance, and other behavioral outcomes—although extremely creative, has not yet produced clear-cut results either for or against these theories. This is partly a result of the inherent difficulties of designing the right experiment and implementing it in a realistic setting. However, there are a number of interesting clues when we look directly at social capital effects, the most intriguing of which is the reaction of the loan officers to the introduction of individual liability—loan officers clearly seem to believe that the nature of liability matters for

⁹The distinction between explicit joint liability and implicit joint liability is from de Quidt et al. (2012a), who, however, have a slightly different definition.

repayment—and the evidence that closer connections between members help insurance. Given that, it seems premature to write off group liability as an idea, as Giné & Karlan (2011) seem to, notwithstanding that a number of MFIs, including the Grameen Bank, but by no means all of them, seem to have moved away from group liability. (My sense is that this is mostly because *ex post* they feel bad about enforcing it and thereby punishing potentially innocent borrowers.) In particular, we may want to think more seriously about what de Quidt et al. (2012a) call implicit joint liability.

2.2. Dynamic Incentives

Most microcredit contracts tend to be dynamic, with repayment in the current period tied to any future loan disbursement. The idea is obviously that the promise of a future loan secures the current one. Bulow & Rogoff (1989) explore this idea theoretically. Their key point from my perspective is elementary but important: A borrower can retain the money he or she would have repaid the lender and use it as the next loan. For repayment to dominate in this option, the rate of loan growth has to be higher than the interest rate.

However, based on the few prominent MFIs for which these data are easy to find, the standard increment from the first cycle to the second is substantially below the interest rate. This means that these loans must be secured by something in addition to the promise of another loan, which might just be the borrower's conscience. Given this additional cost of defaulting (whatever it might be), it is possible to ask what happens to borrower behavior when the dynamic incentives become stronger. Clearly, willful default should go down, which is a good thing, but it must also affect *ex ante* risk-taking behavior and repayment behavior conditional on ending up with no money. From both these points of view, the efficiency of canceling credit whenever the loan is not repaid is questionable, as it leads to poor insurance and loss of productive investment opportunities. This is reinforced by the fact that with microcredit, there may be other group members who have information about why the person defaulted, and cutting off credit to the whole group whenever there is a default is not making use of this information.¹⁰ From this point of view, it may be better to make the members of a group in which one person defaults pay something immediately, irrespective of whether the default was justified, but not cut off credit to the nondefaulting members. However, I am not aware of a proper theoretical treatment of this problem.

There is a limited amount of empirical work on the effect of strengthening dynamic incentives, but reassuringly, it all points in the same direction. Karlan & Zinman (2009) offer a new loan at lower than the market interest rate to a randomized selection of microborrowers in South Africa who each currently have a short maturity, individual liability loan. They find that the promise of a second loan substantially reduces default on the first one. In their paper on microfinance-based lab experiments in the field discussed above, Giné et al. (2010) find that the threat of credit denial reduces risk taking and default. Finally, Giné et al. (2012) report on a study in Malawi in which they randomized who gets fingerprinted before getting a loan, thereby making it more difficult to get a second loan if they default. They find no significant overall effect but a massive effect on the quintile in the borrower population predicted to have the lowest repayment rate: Borrowers from this group show a 31.7 percentage point increase in the probability of partial repayment and a 39.6 percentage point increase in the likelihood of full repayment. The study shows that this is partly a result of the borrowers asking for smaller loans and partly a result of diverting less of the loan away from the cash crops in which it is supposed to go.

¹⁰Tedeschi (2006) makes the point that it does not always make sense to permanently deny loans to defaulting microborrowers.

2.3. Transaction Design

An interesting but less discussed aspect of microcredit is the careful design of transactions. Most of the biggest MFIs insist that loan collection take place in group meetings even when the loan has individual liability.¹¹ These meetings are always held at a fixed time at a fixed place and tend to be short. This has two advantages. First, it saves the loan officer time. Having everyone come to the same spot every week creates a routine that makes it easier for the borrowers to be there on time; moreover, they often come together, which acts a reminder. Typically, groups also end up having one or more leaders, and these leaders usually collect everyone's loan payments to hasten the process and hand it over to the loan officer. The group also helps mitigate the inflexibility that results from fixing the time of the meeting—if a borrower cannot come to a particular meeting, he or she can always give the money to another group member to take in. Second, if someone fails to make a payment in the group meeting, that automatically gets publicized within the group and, in all likelihood, well beyond.

The amount of loan officer time saved by this process, compared to the alternative model in which every borrower repays at a separate but designated time, is difficult to determine because I am not aware of an experiment that varies just this dimension. However, it may be considerable—Giné & Karlan (2011) find that when they compare group and individual liabilities, the time devoted to loan repayment is 90 minutes higher per week per center under individual liability, which is about half the total time spent by a loan officer for everything he or she has to do for an average community-based center visit. It is not obvious what is going on here (the authors do not offer a theory) as the collection itself happens in a group meeting in both cases (the difference obviously would be much larger if the loan officer went door to door). One possibility is that the collection in this case was individual by individual rather than mediated by the group leader.

Another key feature of microcredit is frequent and small repayments. Mohammad Yunus, who founded Grameen Bank, explains the motivation behind this policy: “It is hard to take a huge wad of bills out of one's pocket and pay the lender. There is enormous temptation from one's family to use that money to meet immediate consumption needs. ... Borrowers find this incremental process easier than having to accumulate money to pay a lump sum because their lives are always under strain, always difficult” (Yunus & Jolis 2003).

Fischer & Ghatak (2010) formalize this intuition in a recent theoretical paper on the repayment decision of hyperbolic discounters.¹² Splitting the one final repayment into multiple payments throughout the life of the loan obviously makes that final payment much less onerous and therefore much less likely to be defaulted on. However, the first loan payment is now further away from the reward of repayment (a new loan), and therefore default is more tempting. Finally, borrowers who are sophisticated hyperbolic discounters know that by making a small payment today, they can get all future selves engaged in the project of repaying the loan and securing the next loan (if the first self repays, all future selves will as well because they are closer to the benefit). In other words, they can pass on a substantial part of the burden of repayment to future selves. So borrowers who are significantly hyperbolic but not too impatient (so that they care enough about the long-term benefit) will be more likely to repay if the repayment is split into multiple tranches. It should be noted that because this works off the current self strategizing against future selves, there is no

¹¹BRI in Indonesia is one major microlender that does not have a group structure in its lending, although that might have something to do with the fact that it does not lend to the poorest sections of the population.

¹²The intuitions that are developed in this paper are closely related to those developed by Basu (2011), who studies the repayment decisions of sophisticated ROSCA participants with hyperbolic discounting. Heidhues & Koszegi (2010) discuss the repayment decisions of naive consumers with self-control problems.

guarantee that welfare measured by the preferences of some long-run self who does not share the present bias of the period-by-period selves goes up as a result. In particular, the increase in the loan size resulting from splitting the payments and the resulting increased willingness to repay might benefit the first-period self at the cost of future selves.

However, as long as borrowers can save at the same rate as they pay on their loans, this intuition applies only when the hyperbolic factor is strong enough. For people who can save and who are not hyperbolic discounters, splitting the payment is just constraining their choices because they can always set money aside if they want to make an early payment. Yet, as Fischer & Ghatak (2010) note, if borrowers can save only at much lower rates than what they pay on the loan, then offering them the option of early repayment benefits them directly with a superior savings opportunity. Because the poor rarely have rewarding savings opportunities (Rutherford 2001), this may very well be as important a reason for the frequent payments, as in the behavioral argument suggested by Yunus.

Indeed, focusing on savings offers an entirely different perspective on microcredit. This is the subject of the next subsection. Returning to transactions frequency, there are also other potential psychological effects. The requirement of making a small payment every week, proponents argue, takes the stress out of making a payment because it is a small amount, always within reach by just cutting back on something easy to sacrifice. In contrast, if borrowers have to make monthly payments and the amount is sizable, and they have somehow spent the money already, there is no easy way to get there. Weekly payments are therefore good for everyone: less worry for the borrowers and better repayment for the lenders. A standard economic model, of course, says exactly the opposite—as Fischer & Ghatak (2010) note, more frequent payments just mean less flexibility.

Unfortunately, the empirical evidence seems on the whole rather negative on frequent repayments. Armendáriz & Morduch (2010) talk about the experience of the giant Bangladeshi MFI BRAC, which abandoned a potential move from weekly to biweekly repayments when a pilot program showed increased delinquencies. Conversely, McIntosh (2008) studies a natural experiment in Uganda in which joint liability microcredit groups in some areas were given the option of switching to biweekly payments. He shows that those who were given the option end up defaulting less and drop out less. This suggests that for some people (those who choose this option), biweekly payment reduces stress because dropouts decrease. But the rest might find weekly payment less stressful (which may be why they stick to it). Field et al. (2012) use a field experiment in India to ask what happens if the weekly payment option is removed. They compare individual liability loans with weekly and monthly repayments and show that the latter is associated with a 51% reduction in feeling “worried, tense, or anxious” about repaying and a 54% increase in feeling confident about repaying as well as higher business investment and income. There is also no change in the default rate.¹³

Field et al. (2011) investigate a different dimension of flexibility in microcredit. Most microcredit contracts allow little or no flexibility in the time structure of payments. Installments typically start a week after loan disbursement. This means that if the household is to use the returns on the investment on the loan to be the source of repayment, it cannot afford to invest in anything that will take more than a week to start generating returns. In this experiment, with the same MFI in India, some randomly chosen groups were offered the option of starting their loan payment after a two-month grace period. Once started, the payments were exactly the same in both cases. They

¹³Field and Pande are coauthors on another paper mentioned above in which they also find that weekly and monthly repayments generate the same default rate (Feigenberg et al. 2010). It is true that they do find that the reduced contact due to less frequent meetings decreased social capital within the group and led to increased default in the future, but this may have been avoided by continuing the meetings without the repayment obligation.

find that this more than doubles new business creation and increases profits by over 50% and total earnings by nearly 20% but also quadruples the default rate (from a very low base). Detailed observation of the businesses of these two sets of households suggests that the shops that grace-period households run (retailing is the main occupation here) have a wider range of products and are more willing to extend credit to their clients, which is obviously risky but attracts more clients. The authors argue that the requirement of starting to repay right away discourages any kind of risk taking or innovation because there is no time to make a mistake and recover from it.

2.4. Microcredit for Savings

Paradoxical as it might sound, there is plenty of anecdotal evidence that microcredit is used as a way to save. Rutherford (2001), who first pointed to this phenomenon (to the best of my knowledge), argues that this can be a way for the poor to spend a large one-time sum that they would not be otherwise able to. Accumulating that much money at home is nearly impossible for many of these borrowers because of theft, self-control issues, or demands from family members. Therefore, the only way they reach their goal of buying a television or a bicycle is by borrowing the money, spending it first, and subsequently paying down the loan by saving a little each week.

Moreover, when the borrower has commitment issues, the loan officer's efforts to collect on the loan actually help the borrower achieve his or her long-term objectives. For instance, a woman who is under pressure from her husband to give him money for a drink can refuse him on the grounds that the loan officer will come after them if she does not save the money and pay him.¹⁴ In that sense, a small increase in collection efforts may have large effects on repayment because it convinces the borrower that she will repay in the future and therefore effectively increases her discount factor. Moreover, if the borrower cannot easily save, the Bulow & Rogoff (1989) argument no longer necessarily applies, and it is possible to get borrowers to repay without promising them fast loan growth.

From the point of view of understanding what MFIs do, a basic question is whether traditional moneylenders could provide the same commitment service. There are two possible arguments why they may not be able to, both of which come down to forbearance, but of different sorts. One worry is that if moneylenders are given the power to extract money from borrowers when they falsely claim to have none, what is to stop the moneylenders from using that power when the borrowers genuinely have so little that it would make sense to let them off? Perhaps the MFI will be less harsh in this situation because part of its mission is social and it is more invested in the well-being of the borrowers, because it cares about its reputation, or simply because it is so much bigger than the moneylender and can afford to take some losses. The presence of groups, which serve an insurance function (as discussed above), might also make this easier for MFIs. The other concern is that moneylenders will provide the commitment so effectively that the current self of a present-biased borrower would be able to borrow and consume much more than the long-run self would want him or her to, and perhaps the MFI, being less effective at enforcing repayment (for any of the reasons mentioned above), will not allow the present self that much profligacy.¹⁵

Unfortunately, there is little quality empirical research in this important area. One notable exception is provided by Bauer et al. (2012), who show that among women in rural Karnataka

¹⁴Gugerty (2007) makes a similar argument for why ROSCAs are so popular in Africa.

¹⁵Interestingly, Jain & Mansuri (2003) make the argument that MFIs actually try to free ride on the superior enforcement ability of moneylenders. According to the authors, the reason for the inflexible and high-frequency payments that the MFIs demand is to force borrowers to borrow from moneylenders to meet the gaps in their cash flows. The moneylender then monitors the borrowers to ensure repayment, which also benefits the MFI.

state in India who borrow, those who are more present biased are more likely to borrow more from an MFI. While recognizing that there could be other reasons driving these correlations, this is certainly suggestive. Another suggestive piece of evidence comes from Banerjee et al. (2010b), who, as a part of the baseline for an impact evaluation of microcredit, had asked potential borrowers to name some things that they currently spend money on but would prefer not to. The borrowers listed tea, coffee, snacks, cigarettes, and alcohol. Eighteen months after the loans were disbursed, comparing loan-eligible households in randomly chosen loan-eligible neighborhoods with the rest, the authors find a significant reduction in the consumption of these temptation goods. However, treatment households own much more in durables. Their interpretation is that the household is engaged in what Banerjee & Mullainathan (2010) call consumption transformation, turning the small amounts that they would rather resist spending into what they want but cannot get (televisions and DVD players). If this interpretation is correct, microcredit does help solve a self-control problem; indeed, Banerjee & Mullainathan argue that the structure of microcredit—the borrower gets one lump sum loan and then pays it down in small installments—is ideal from the point of view of dealing with this particular self-control problem.

Turning to the issue of controlling the influence of others on consumption and savings, Anderson & Baland (2002) make the case that women in Kibera, Kenya, join rotating savings and credit associations (ROSCAs) to protect their savings from their husbands.¹⁶ To support this narrative, the authors point out that women living as a part of a couple are much more likely to join ROSCAs than women living alone, and this remains true if we look only at women who are working and earning. Moreover, the relation between ROSCA participation and the woman's share of total family income is an inverted U-shape, which is what it would be if women joined a ROSCA to have more control over the money (a ROSCA cannot help a woman who earns nothing, and a woman who is the primary earner does not need a ROSCA to get her own way).

Schaner (2012) asks a related question in the context of savings account selection in Kenya.¹⁷ She carries out a field experiment in which members of couples were given the choice of a joint account with their spouse or an individual account and shows that couples who in the baseline show wide divergences in time preference (i.e., one is much more impatient than the other) often choose an individual account, even when it is dominated by the joint account in terms of interest rates. This is strong evidence that keeping savings out of the spouse's reach is a major issue in this population, although in Schaner's study both genders show these kinds of preferences.

None of this proves that spouse control is a major issue for microcredit. However, it would be interesting to look at how involving both spouses in the decision to take out the loan (e.g., by requiring cosigning) affects loan use and repayment. As far as using microcredit to ward off unwanted demands from relatives, I have yet to see a way to credibly identify this effect.

2.5. The Role of Reputation

The idea that reputation may be an important part of what distinguishes microcredit from traditional moneylending has already been introduced. Certainly, MFIs invest effort into signaling that they are different from moneylenders. Every MFI talks about its social mission on its website and in all its public presentations, and many run schools, offer health services, and even provide

¹⁶ROSCAs are not a form of microcredit in the sense that I used the term in this review, but it is worth making an exception and covering this paper here given that there is really very little else.

¹⁷Once again, savings accounts do not count as microcredit.

income support to the poorest members of the community.¹⁸ Although some of this is no doubt for the benefit of potential donors, the seriousness with which they do it suggests there is something more.

I have already suggested several reasons why MFIs caring about social (as well as monetary) outcomes, or at least caring about having a reputation for doing so, may be valuable from the point of view of borrowers. Basically, borrowers may want a lender who can put some pressure on them when they are tempted to default, but not too much, and an MFI, who cares about being seen as just and committed to social objectives, may be more or less in the right position to do so. Of course, that such a record makes borrowing more attractive ultimately also benefits the lender.

There may also be a more direct benefit to the lender. Much of the enforcement of loan repayment happens through public shaming of defaulters, but it is not clear why that works. After all, if the lender is seen as the exploiter and a public enemy, then there is no shame in defaulting. The lender's reputation must be sufficiently positive for there to be some stigma attached to not paying.

Of course, a reputation for being just or caring may also be valuable to the MFI if it serves as a way to attract donor money. How these two reputational incentives interact is an interesting theoretical question. For example, does the lure of donor money make MFIs too soft, or does it actually make it easier for them to build a reputation? And do the preferences of the donor play a role here? From the point of view of building the right kind of reputation, is it better to have social businesses rather than aid givers as the main funders because social businesses value toughness, or is it worse because they push the MFIs to be too harsh?

Another aspect of reputation that is also important for MFIs is durability. As we see above, borrowers are much more likely to repay when they expect to get another loan if they do. Therefore, it is important that the MFI is expected to stay in business.

I am not aware of any direct test of these theories, but numerous observations are consistent with them. First, they would explain a lot about the recent crisis in Indian microfinance. Although the government of Andhra Pradesh, which led the attack on microcredit that culminated in it basically asking everyone to default, had its own political reasons, one key precipitating factor seems to have been the initial public offering by the biggest MFI, SKS, which made it apparent that microfinance was very profitable. This went against the image of MFIs as being socially minded (although no one bothered to ask how much money they make per dollar lent). For the same reason, press reports of farmers purportedly driven to suicide by microlenders (would they have fared better if they borrowed from the village moneylender instead?) made it easier for the government to step in. There is an even more interesting story involving Spandana, one of the largest MFIs at the time. One of Spandana's competitors circulated copies of a false news story that claimed that the founder of Spandana, Padmaja Reddy, had murdered her husband and was either in jail or absconding. This story combined two key elements—a suggestion that the end of Spandana was near (and therefore there was no reason to repay) and an undermining of Spandana's moral legitimacy. To counteract this message, Reddy had to tour all over the state, meeting borrowers and assuring them that she was a free and innocent woman and that Spandana was not about to fold.

Second, these theories would explain why there is so much emphasis on repayment performance—why MFIs are so reluctant to trade off some default for more flexibility for their

¹⁸An example is the ultra-poor program pioneered by BRAC in which the MFI gives an asset to those it considers too poor to be included under microcredit. Banerjee et al. (2010a) evaluate a version of this program implemented by the Indian MFI Bandhan and find large positive effects.

borrowers, larger loans, or higher interest rates. After the study on flexible repayment described above, Field et al. (2011) tried to persuade the MFI that the increase in default was worth the increased profitability, perhaps with a higher interest rate, but the lenders were quite adamant that their priority was default.¹⁹ In the first round of the battle between the Andhra Pradesh state and the MFIs in 2006, the Krishna district administration ordered borrowers to default; once the order was rescinded, Spandana went back to defaulters and offered them a new loan—but only as long they paid the installments due on their previous loan when they defaulted. This was probably less efficient than just writing off the loans because it caused much delay in relending, but the symbolism of insisting on “full” repayment was important to Spandana.

One way to rationalize such behavior is to recognize that, for an MFI, dealing with default is potentially a minefield. Once default has happened, ignoring it would invite more default, yet going after the defaulters always risks crossing the line into unacceptable coercion and loss of reputation. MFIs therefore want to avoid default.

Third, these theories provide a rationale for why repayment behavior tends to be correlated even when there is no group liability. Breza (2012) studies Spandana’s efforts, mentioned above, to reclaim its borrowers after the default. She notes that under these rules, some borrowers could have gotten a new loan just by making one or two payments to complete their cycle, while others would have had to make up to 50 payments. She shows that this difference does indeed predict repayment. More remarkably, controlling for people’s own position in the cycle, members of centers in which most other people were close to the end of the cycle and therefore more likely to repay repaid more often. People repay when others repay. One potential explanation for this is that the act of repaying legitimizes the organization and therefore makes it easier for others to also repay. (Another is that people like imitating others.)

None of this, however, constitutes proof that reputation matters. But it should be clear that if it does, policy has a potentially important role in helping MFIs build reputation. In particular, MFIs may benefit from certain types of regulation that protect them from their own greed and make it easier for them to sustain a reputation.

3. THE IMPACTS OF MICROCREDIT

3.1. What Should We Expect?

Improved access to credit can affect either investment or consumption behavior (or both). It allows for higher levels of investment without cutting back consumption and for higher consumption today at the cost of lowered future consumption. If the potential investment opportunity is lumpy, the increase in investment might be accompanied by a cut in current consumption (presumably for the sake of increased future consumption). There is potentially a similar consumption effect—credit allows the household to undertake lumpy consumption spending (e.g., buying a fridge), which might come at the expense of current nonlumpy consumption and future consumption.

Sustained access to credit, which microcredit usually provides, can have an additional effect through its effect on savings. For example, a mother who might have been saving for her child’s future school fees might now be more relaxed about saving because she knows that she will have a chance to get a new loan before that date. This would lead to a one-time increase in consumption. There is potentially also a similar effect on precautionary savings, although the standard MFI loan

¹⁹Field et al. (2011) point out that this may be rational for the lender if moral hazard goes up enough when the interest rate is raised to compensate for the default rate.

with its fixed cycle is not ideal for dealing with emergencies (but many MFIs do offer an emergency loan product).

As long as the borrower has time-consistent preferences and makes choices rationally, the welfare effect of access to credit through any of these channels has to be positive. This changes, of course, if borrowers are time inconsistent. It is obvious that credit could make time-inconsistent borrowers worse off by allowing them to consume too much too soon. More interestingly, however, time inconsistency or any other reason for not being able to save (e.g., spouse control issues or just the risk of theft) could increase the benefit of being able to borrow. Consumers who have trouble saving may never be able to save enough to pay for things they really want, like a television or a really nice wedding for their daughter. This reduces their welfare and may discourage them from taking up productive investment or employment opportunities. In other words, being able to consume in the way they want can stimulate investment and effort and simultaneously raise earnings.²⁰

3.2. Empirical Evidence

The earliest serious attempt to empirically assess the impact of microcredit is by Pitt & Khandker (1998), who find substantial positive effects of the Grameen Bank by comparing the difference in outcomes for those eligible for microloans and those not, in villages where Grameen Bank had entered and where it had not. Morduch (1998) critiques their empirical strategy as relying on an eligibility rule that does not appear to have been followed in the data. Pitt (1999) responds to Morduch, arguing that many of Morduch's criticisms were misplaced. Without going into the merits of these two views, it seems clear that the underlying identification assumption, that the Grameen Bank entered villages without regard to whether the eligible people in that village wanted or needed loans, is hard to feel confident about.

Kaboski & Townsend (2011, 2012) use Thailand's Million Baht program as a natural experiment to look at the impact of microcredit. In 2001 and 2002, Thailand implemented a program that gave every village 1 million baht (approximately \$24,000) to create a village bank. The bank was governed by a committee of villagers and was tasked to make loans within the village (unlike in some microfinance organizations, there was no gender restriction). Every village got the same amount of money even though some were ten times larger than others, and Kaboski & Townsend exploit this fact to generate village-level variation in how much credit access improved. They show that village size is uncorrelated with pretrends so that it is plausible that this source of variation is exogenous. They find evidence that both consumption and incomes go up when the program is started but then converge back to trend, while asset growth slows down at first and then returns to trend. They argue that the consumption and asset accumulation pattern is consistent with a model in which households, recognizing that they will have access to the loans when they need money, adjusted their stock of precautionary balances downward. The magnitudes of the consumption increases are very large, but almost all take the form of household and vehicle repairs, both of which have large durable components and tend to be lumpy, so the usual consumption-smoothing motives do not apply. The income increases are more mysterious because we do not see more business startups or greater business investment. The authors argue this could be either the result of better allocation of investment (those who cannot run their businesses sell them to those who can run them better, now that those buyers can afford the capital outlay) or just the consequence of a few people expanding their businesses a lot, which the data will not pick up.

²⁰Banerjee & Mullainathan (2010) develop a theoretical framework to capture these kinds of intuitions.

Banerjee et al. (2010b) report on a randomized control trial of the classic microcredit model. As mentioned above, they evaluated Spandana's microlending program in Hyderabad city. The program was characterized by minimal screening of applicants, group-based lending,²¹ small loans (approximately \$250), exclusively female borrowers, and relatively low interest rates (24%). The randomization was at the level of the neighborhoods where Spandana gave out its loans. Even though other MFIs did move into the neighborhoods left vacant by Spandana, the authors do find a significant difference in microloan access and total borrowing between the two sets of neighborhoods. They first look at outcomes 18 months after the loans were disbursed. At this point, Spandana entered the control neighborhoods as well so that the difference in being a microcredit client vanishes, but it still remains that those in the treatment neighborhoods have a bigger total stock of past and current loans and can be compared with those in the control neighborhoods. The study collected a second round of data 36 months from the original loan disbursement.

Based on both rounds of data, some patterns stand out. First, there is no impact on total nondurable consumption or on food consumption either in the short or longer run. Nor do we see clear evidence of increased human capital investment. Conversely, business creation goes up when the loans are first given out, as do business assets, hours worked in self-employed activities, and profits, mainly for those who had existing businesses. Families also purchase more consumer durables. In the longer run, business assets go up considerably more, but the increase in profits is no longer significant, although it is still substantial in magnitude; however, we still see no effect on nondurable consumption, and the effect on durable consumption is also gone.

Crepon et al. (2011) report on another trial, this time in rural Morocco. The authors randomized 81 matched pairs of villages so that one village in the pair got microcredit and the other did not. The loan product was quite similar to the Hyderabad study: group-based lending, relatively low levels of ex ante screening of borrowers, and low interest rates (between 11.5% and 13.5%). The loan size was larger than in Hyderabad—between \$124 and \$1,855—but Morocco is a substantially richer country than India. However, the lending environment was quite different from that studied in Banerjee et al. (2010b): both because, unlike in Hyderabad, there was very little credit access before Al Amana (the MFI) arrived and because most people were involved in agriculture and animal husbandry. There was also no restriction on lending to men. Yet the results are in many ways very similar: There is no average effect on consumption, and although there is also no effect on starting new businesses, fewer existing activities are discontinued. Additionally, the scale of activity both in agriculture and in livestock rearing goes up in the villages where microcredit is available, and there is a cutback in outside wage work (the authors do not find clear evidence that this is because the participants are working harder at home, but perhaps they are doing more management tasks, which are harder to measure). Families that get credit end up with higher levels of assets, mainly in the form of livestock. There is also evidence of a small cutback in consumption, at least among those who already were in agriculture or livestock rearing.²² Unlike in the Hyderabad study, there is also a small positive effect on school participation.

The study by Attanasio et al. (2011) in Mongolia is discussed above in the context of group versus individual liability. The group liability loans in this study were quite similar to the ones in

²¹However, the group was almost never held formally liable.

²²Crepon et al. (2011) emphasize the heterogeneity in responses between this group and the group that was not involved in agriculture and livestock rearing in the baseline, but the difference is really in significance levels rather than in the point estimate. It is true that the first group shows a small decline in consumption and the second shows some evidence of a small increase, but the data are certainly consistent with most of the people with a business using the money to expand their businesses and cutting back a little in consumption to make that happen and a small group of those without any businesses using the money mainly for consumption purposes.

India: very limited screening, small loans (\$279 on average), relatively low interest rates (1.5–2% per month), and female borrowers. The authors also find an impact of business creation and increased ownership of various consumer durables and business assets, although the latter is only among the less-educated borrowers. For this group of borrowers, we also see evidence of asset decumulation (or at least reduced asset accumulation)—ownership of *gers* (residential tents), land, and vehicles all go down. Interestingly, they do find evidence of an increase in food consumption. As reported above, the study also looks at a parallel individual lending program; the results there are weaker but similar in direction.

Giné & Mansuri (2011) discuss an individual loan program in Pakistan in which existing male and female clients of a microcredit program were offered a chance to enter a loan lottery that offered them the option of applying for larger loans (they could get up to 100,000 rupees when the standard loan amount was capped at 10,000 rupees in the first cycle and increased by 5,000 every cycle). Program interest rates were 20% on a declining balance, and because loan offers were made based on a lottery after screening of borrowers, this was effectively a low-screening environment. Interestingly, the actual increase in loan amounts was much smaller than the maximum number of people applying for them: The average difference between lottery winners and losers in terms of loan amounts was just 13.5%, which, given that more lottery winners took up the loan offer than losers (four out of five winners took up the offer compared to three out of five losers), suggests that the winners did not have substantially larger loans than losers (in part because most of them did not want larger loans). So loan amounts were mostly less than \$500, and the average increment seems to have been at most \$100. Perhaps unsurprisingly, the effect of the program echoes that of many of the previously mentioned low-interest rate, small-loan size, low-screening studies: Business assets go up and the business has more secured buyers; although consumption is higher, it is far from significantly so; and there is also no effect on profits and revenues.

Augsburg et al. (2012) study an individual lending program in Bosnia-Herzegovina. This program also had relatively low interest rates (22% per year) and small loans, at least relative to per-capita consumption of these households. However, there was a lot more screening than in the classic microfinance product, and there was no gender restriction. The borrowers in the study were deliberately chosen to be marginal borrowers based on the scoring model used by the loan officers, but then the selection of who could be offered a loan involved an interview as well. The randomization was within the group that survived the interview. The program led to the creation of more businesses and an increase in self-employment. Business inventories went up, but there were no effects on durable purchases and there was a cutback in consumption among the less-educated/poorer group, suggesting that the loan was not quite large enough to cover their expansion needs. The richer/more-educated group reduced savings instead, presumably for the same reason. Young men in the 16–19 age group dropped out of school, probably to work in the family business. One of the striking features of this program was the high level of delinquency—40% of the borrowers were late and 20% defaulted.

In an early paper, Karlan & Zinman (2011) focus on a similar individual lending model, in this case, via a bank in the Philippines that made individual three-month loans to the poor based on a credit-scoring model. The bank's interest rates were relatively high (60% per year), and it tolerated, by microcredit standards, very high levels of delinquency (one-third of the loans showed late payments at some point). The study is also randomized at the individual level: Of the male and female applicants for a loan who were marginally eligible based on the credit-scoring model, some were denied and others were granted loans, based on a lottery. As in the previous study, a comparison of these two sets of households shows no effect on consumption but, more surprisingly, suggests that getting the loan led to less businesses being created (or more being shut down). The authors also find higher levels of stress among loan recipients (significant for men).

Finally, they show evidence of less insurance being purchased by the recipient households and suggest that this is because the households can now rely on credit instead. However, it is not clear why access to a three-month loan that is not guaranteed in the future should affect demand for insurance. A more plausible explanation is that these clients borrowed because they felt they had an urgent need, which is why the high interest rates do not discourage them. Having borrowed, they find it difficult to service the loan—hence the elevated stress levels—and have to shrink both their business outlays (presumably the businesses that close are the ones that are not currently profitable) and insurance purchases to stay afloat.

In another early study, Karlan & Zinman (2010) look at a microloan product that is even more unlike classical microcredit—an individual loan in South Africa at 200% APR, given after a significant amount of screening. The randomization involved offering a loan to a fraction of those who had been marginally rejected for screening based on their loan applications. Both male and female clients of the bank were eligible. The loan was used for repaying other loans and paying for transportation, education, social events, home repairs, and food. There was almost no mention of business expenses. Interestingly, this is the one loan product that generated a clear increase in income, mostly because it seems to have helped the loan recipients remain employed over the study period. Experience of hunger went down, and loan recipients ended up with a more positive outlook on their prospects and position but were also more subject to depression and stress.

3.3. Summary of Results

Taking this body of work together, some patterns stand out. First, there is clear evidence that as long as the credit is reasonably priced, it leads to business creation and/or some amount of expansion. The one exception to this is the Thailand study, but that is the only place where the process of selecting borrowers is almost entirely a black box. It is possible that those who needed money for vehicle or home repair (the main forms of spending) were given priority. Most studies also see an increase in ownership of consumer durables and business assets, especially if home repair and livestock ownership (both of which provide services into the future) count as durables, although the relative importance of consumer durables and business assets varies (indeed, in many cases it is hard to tell which is which—is a cow a consumer durable because it gives milk that the household consumes or is it a business durable because in principle the household could sell the milk?).

What is also striking is the lack of strong evidence linking this business creation to increases in consumption. Indeed, there is no evidence of large sustained consumption or income gains as a result of access to microcredit. In the short run, this is what we might expect if all borrowers are investing in businesses, the investment is lumpy, and we see some evidence of cutbacks in non-durable consumption or reduced savings, but eventually consumption should go up in this case. The Hyderabad study tracks borrowers for three years and still finds no evidence of such a consumption rebound. Indeed, one obvious prediction of this lumpy investment theory is that consumption growth should be faster for those who have already made the sacrifice and invested than for those who wait to invest. But the evidence is, if anything, the opposite: Consumption growth between the first end line and the second was slower for the treatment group than for the control (who presumably started investing later), and the effect is almost significant. There is also no evidence of substantial gains along other dimensions of welfare, such as education and health. At least in the one- to three-year horizon, we see no evidence of microcredit transforming the lives of its beneficiaries.

The high-interest rate loans seem to be quite different, perhaps because the selectivity on both the borrower side and the lender side is much higher. Borrowers seem to take these loans to deal with some urgent need. In the Philippines paper, we are not told what the borrowers do with the money, but the evidence from South Africa fits with this view. People borrow at these high rates because they have a loan that they cannot repay but also cannot easily default on, a broken vehicle that makes it impossible to get to work, or just no money for food. Dealing with these problems might force them to take actions that will hurt their long-term prospects—run away to avoid the loan collector, miss work because the car is broken, or move back to their mother's home in a different city to feed the family. The benefit of this kind of credit is precisely that it allows people to deal with such emergencies. The cost is that they now have a high-interest rate loan to pay back, and this may be the reason for the increased stress.

3.4. Why Is the Impact of Microcredit So Limited?

What do we learn from these studies? There are a number of hypotheses suggested by this evidence. Let us discuss them one by one.

3.4.1. Borrowers are not credit constrained. One reason the impact of microcredit is muted may be that borrowers do not need microcredit. In that case, the effect on their choices as well as outcomes will be limited. It is clear that many MFIs overestimate the demand for their product. The power calculations for the original experimental design used for the Spandana evaluation were based on the MFI's claim that more than 50% of the people offered the loan products would take them up. In practice, only 19% did (and only 26% borrowed from any MFI), which meant that the data collection in the end line had to be entirely rethought. Although this was in urban Hyderabad, where credit access may be relatively good (certainly a large fraction of the population had at least one loan, usually from a nonformal source), the same thing happened in rural Morocco, where, contrary to the MFI's expectation, only 16% took a loan from them, even though only 2% had any loans in the baseline.

However, there is direct evidence that small business owners have a marginal product that is much higher than the microcredit interest rate. Based on an experiment in which a random sample of tiny firms in Sri Lanka were given either \$250 or \$500, de Mel et al. (2008) show that the marginal product of capital is approximately 5% per month.

McKenzie & Woodruff (2008) carry out a similar experiment in Mexico and find returns that are even higher—indeed almost implausibly high (20–33% per month). In Ghana, Fafchamps et al. (2012) conduct an intervention in which they offer both in-cash and in-kind grants and find that 150 Ghanaian cedis (\$133) worth of cash increases profits by between 10 and 14 cedis per month, whereas the same amount of in-kind grants increases profits by between 37 and 39 cedis per month. Even the lower in-cash numbers suggest that the firms have very high returns on investment.

However, not every capital drop on microentrepreneurs works. Karlan et al. (2012) study another similar-sized capital drop in Ghana (200 cedis) and find that although it has large investment effects right after the fact (investment goes up by 178 cedis), there is subsequent disinvestment, and eventually the grant recipients end up with a lower income than the control group. They interpret this result as evidence that the firms are indeed credit constrained (otherwise, why would they invest the money right when they get it?) but are searching for their optimal investment plan. The grant allows them to explore some possibilities that mostly turn out not to work for them.

Berge et al. (2011) gave a randomly selected group of microentrepreneurs in Tanzania a capital drop of approximately 80 dollars each and find that it has no effect on investment and insignificant positive effects on profits and revenue. They see this as evidence that these firms are not credit constrained, but the firms could equally be even more constrained in consumption than in production. Therefore, using the grant for consumption purposes may be the optimal use of the money.

There is also a lot of nonexperimental evidence that supports the view that borrowers are credit constrained, including that the poor borrow at the much higher rates that moneylenders offer (Banerjee 2004). However, once again, the evidence does not support a presumption that they are necessarily borrowing for production. If so many people are credit constrained, why don't more people want microloans? And how do these high returns square with the relative lack of success of microcredit in promoting income growth? I explore a number of possible answers to these questions in the subsections below.

3.4.2. People borrow to consume. Some of the puzzles vanish if we think the main purpose of borrowing is to relax a constraint on consumption (although not one concerning the lack of demand for microcredit). Many people who are targeted by microfinance have a job or housework to do and do not wish to be in business. However, even people like those in the McKenzie-Woodruff studies, who own a business that would be profitable to expand, may not always want to expand it, for example, because there are already enough demands on their time or because they think they have already exhausted most of the local demand for their product. They might invest if they get a grant but be unwilling to borrow and pay interest in order to invest.

For these kinds of people, the primary impetus for borrowing must come from some unmet consumption need, but they may then be willing to invest some of the money to generate just enough income to pay back the loan. They will not try to grow the business or necessarily put a lot of effort into it, and indeed, their business might shrink or even shut down once the loan is repaid. One would therefore observe no impact on their medium- to long-run consumption (or may even see a negative effect if they need to cut back a bit to repay the loan), beyond the one-time blip in consumption that was the goal of the loan (the wedding or the television purchase).

3.4.3. S-shaped production function. A related possibility is that borrowers do not want to invest even though the marginal returns would be high if they did. This could be the case if the production function is S-shaped so that returns fall very fast after a certain scale is achieved and only recover with a much larger investment, out of reach of most microentrepreneurs given the small size of the microloans (Banerjee & Duflo 2011). Within the range of investments that they are able to make, the return per dollar is very high, but because the returns fall very fast, the total return is too small to justify the effort and psychological costs involved in making the investment.

Perhaps the same idea applies to consumption as well. Given the available range of credit, the household members may value only a few specific expenses (e.g., a television, a new roof, a daughter's wedding), and when their specific priority items are paid for, or considered out of reach, they do not borrow. This would explain why so many people do not take up the offer of a loan.

The S-shaped function can help explain several other facts as well. First, in the Sri Lanka experiment mentioned above, those who (randomly) received \$500 instead of \$250 did not invest any more into their business, despite the very high returns from investment. There seems to have been clear agreement on what the businesses needed, and that was approximately \$250. It would also explain why Karlan & Zinman (2008), based on a field experiment in which they got a bank to randomize the interest rate it was offering its past clients, find a very low elasticity of -0.1 for the

loan size (conditional on borrowing) with respect to the interest. Basically, borrowers had a clear idea of what they needed the money for, and they did not want more just because it was available cheaper.²³

That does not mean that they would not borrow more if a much bigger loan were available so that they could undertake a very different kind of purchase or investment. But, within the range of what was possible, the borrowers were happy to stick to their initial choice. This seems to be true over quite a range. As mentioned above, Giné & Mansuri (2011) also look at what happens when borrowers are offered somewhat larger loans (e.g., 30,000 Pakistani rupees rather 10,000 or 15,000). They find that most borrowers were not interested in the larger loans, at least over this range.

In their paper, Karlan & Zinman (2008) also randomize the loan maturity that was being suggested to different borrowers and find that borrowing was much more responsive to that than the interest rate. This suggests that borrowers do not expect to get their loans automatically refinanced. To them, a shorter maturity loan means higher monthly payments that have to be extracted from their budgets by cutting back on something. (As seen above, microcredit borrowers do often need to cut back on consumption.) Hence, longer maturity loans are more attractive.

To summarize, one reading of the evidence is that the relatively weak demand for microcredit in many contexts is a reflection of how constrained borrowers are, and not the opposite—the small loan sizes and fixed maturities make these loans useful only for specific purposes. However, it must be admitted that this is pure speculation. Because even the largest loans we see in experiments are for less than \$1,000, we have no way of checking whether there is a larger loan size that would be attractive to these borrowers or whether most of them believe that all loans beyond a few hundred dollars are too risky to touch.

3.4.4. The terms of the loans limit: what they can be used for. Above we discuss loan size and maturities, and another key feature of traditional microcredit is the emphasis on loan discipline—payments start as soon as the loan is given out, and there is a payment due every week or month. As mentioned above, Field et al. (2011) find that just a two-month gap before repayment starts had very large effects on incomes and profits. The authors argue that the requirement of starting to repay right away discourages any kind of risk taking or innovation because there is no time to make a mistake and recover from it.

Another characteristic of conventional microcredit is group liability or, its weaker version, individual lending in a group setting. Above we discuss how these might help secure loans but, depending on how much control group members have over each other, can discourage risk taking, even when it is efficient, as Fischer (2011) demonstrates experimentally.

Karlan et al. (2012) make the interesting point that one important way grants impact production outcomes is by moving mass to the right tail of the distribution of returns. They do a meta-analysis of a number of grant (and other) programs and find that a fair number of them do have this kind of impact, although it is not emphasized in the papers. If MFI loans discourage risk taking as discussed above, then loans could have a very different effect than grants by eliminating these very profitable extreme outcomes. However, both the Hyderabad and the Morocco impact evaluation studies also find that the positive effects on profits are concentrated in the right tail of the distribution. So perhaps microcredit is not as constraining as we might have suspected.

²³Karlan & Zinman (2008) also find a low elasticity of applying for a loan. This probably reflects that, on short-maturity loans, a change in the interest rate does not change the monthly payment by much.

There is also a separate concern, which has to do with flexibility. Microcredit imposes an inflexible repayment schedule, unlike moneylenders. This makes it less useful for people like farmers and those who do contract work, whose earnings tend to be bunched and unpredictable, especially in the absence of reliable ways to save. Also, microcredit is generally not suitable for dealing with emergencies, although many MFIs now have special programs for emergency loans. This may be why moneylenders seem to have survived the expansion of microcredit.

To summarize, the structure of the microcredit contract may be an important part of why these loans have not generated very large income or consumption gains and have captured only part of the market, but we clearly need much more evidence on this point.

3.4.5. Microcredit borrowers are not particularly good at growing businesses. Microcredit, according to its proponents, has the potential to transform lives by allowing the poor to take the entrepreneurial route out of poverty. Dr. Yunus, founder of Grameen Bank, has emphasized the idea that the poor are natural entrepreneurs in order to suggest that this route is available to a substantial fraction of those in need. However, a number of facts go against this view.

First, there is no *a priori* reason why it would be true. We would expect the poor to have more difficulty tolerating risk just by virtue of being closer to the margin of survival; they also lack the human capital and the connections that help build successful businesses.

There is also no evidence that they want to be entrepreneurs. In a survey in India that asks parents to articulate what kind of occupation they hope their children to fill, approximately 80% say a government job. No one mentions starting a business (Banerjee & Duflo 2011).

That the poor are often self-employed seems to be more a result of not being able to find a suitable job than a reflection of their life goals. A theoretical paper by Emran et al. (2007) argues that the value of microcredit is precisely because it allows the household to make full use of its labor endowment in a setting with imperfect labor markets, and for that reason, we should not expect the household enterprise to grow beyond the size dictated by the amount of unused labor in the household. This is consistent with a calculation reported in Banerjee & Duflo (2008) showing that if labor were valued at the minimum wage, the average business in the slums of Hyderabad would lose money.

Moreover, if we look at what businesses the poor go into, the lack of diversification is striking. Among urban borrowers, a small number of occupations dominate, such as selling fruits and vegetables on the streets, running a small grocery store, selling prepared food, and tailoring. In rural areas, it is again nonspecialized retail, plus livestock rearing and growing some cash crops. Given that everyone else is in the exact same business in the same neighborhood, the possibility of earning very large profits is obviously limited.

Finally, microcredit often targets women. There is, however, no credible evaluation of whether women are better or worse than men at setting up businesses with their microcredit loans, but there is an arm of the Sri Lanka-based capital-drop study mentioned above (de Mel et al. 2008), reported in de Mel et al. (2009), in which the same \$250 or \$500 was given to women instead of men. Remarkably, the evidence suggests that their profits did not go up at all, while that of their male counterparts went up by a lot. The same gender difference is also reported by Fafchamps et al. (2012).

This gender gap could be a result of differences in human capital, however; de Mel et al. (2009) find the same difference after controlling for human capital (as well as risk aversion and family wealth). Or perhaps it reflects the structure of poor women's lives in many developing countries. They are expected to take care of most household activities (e.g., child care, cooking, cleaning, washing, and getting water) in addition to anything they do for their businesses. Indeed, they may go into business precisely because they can conduct it while doing their household chores, but this

may be exactly the reason why they put less effort into the business and generate lower returns. Fafchamps et al. (2012) propose another interpretation of why women are different: They are less able to turn grants into investment because of other claims on their resources (i.e., they are unable to resist the family tax).

However, before we conclude that microcredit would be fine if it stopped focusing on women, note that most impact studies mentioned in this article are from MFIs that lend to both men and women. Moreover, giving a grant to a woman entrepreneur is very different from giving a loan to a woman; because Spandana, for example, does not stipulate that she is the primary business owner, the money could just as easily be used for her husband's business.

3.4.6. Microcredit borrowers have poor financial judgment. Many potential and actual microcredit borrowers also take loans at exorbitant rates. Fruit vendors in Chennai, India, routinely pay 5% a day to finance their working capital. This raises concerns of whether they understand interest rates and financial calculations more generally, because if they do, they have to be extraordinarily impatient to do this on a sustained basis (see Banerjee & Mullainathan 2010 for a discussion of this point). The same concern arises when we observe that even after three years, the recipients of the Spandana loans are not consuming more, even though their revenues are much higher (Banerjee et al. 2010b). Do they realize that their costs are also so much higher that the profits did not go up at all? Maybe their accounting is not sufficiently sophisticated to pick up this rather key fact. Perhaps the results are not better because loan recipients are running the wrong businesses.

This is an important policy concern because a lot of the controversy around microcredit has to do with whether borrowers know what they are doing or whether they are responding to temptations and driving themselves into a poverty trap. Given that we do not know what the borrowers want, this is a hard question to answer.

One worrying piece of evidence comes from the study mentioned above by Fafchamps et al. (2012). They find that in-kind grants are much more effective in increasing profits than cash grants, mainly because the in-kind grants ensure that the resource gets invested rather than spent on other things. This is not necessarily a reason to be concerned—after all, the household could have priorities that do not involve investing—but the authors show that an index of self-control effectively predicts the profits from the cash treatment. Indeed, the profits of the people with the highest self-control who receive the cash grant are no different from the average effect of the grant in kind.

However, there is also one piece of positive news: As mentioned above, one result of taking a microcredit loan in the Hyderabad study is a large cutback in spending on temptation goods. The other studies do not explicitly ask the borrowers the temptation good question, but we see a significant cut in spending on cigarettes and alcohol in Bosnia-Herzegovina and a not-quite-significant cut in cigarette spending in Mongolia. The effect on cigarettes in Morocco is negative but small, but there is a significant and substantial fall in social spending, which may be another temptation good. Banerjee & Mullainathan (2010) argue that this process of being able to turn small immediate pleasures into a larger and more durable source of pleasure, such as a nicer house or a television, has dynamic consequences: People become more forward looking and interested in raising their earnings.

Of course, it is still the case, no doubt, that some borrowers are borrowing more than they should. But there seems enough ambiguity in the evidence to warrant much more research (e.g., how does the impact of microcredit differ by level of financial sophistication?).

A different concern is whether the investments are worthwhile. As mentioned above, Karlan et al. (2012) raise the possibility that microentrepreneurs may be using the initial investment to

explore options, but the study's actual finding is that most of them fail and give up. This is not inconsistent with the exploration hypothesis as the few who do not give up may win big, but how do we know that it is not possible to identify the potential winners without going through this rather expensive detour? Perhaps most people have no chance of succeeding and either learn it the hard way or never learn at all. Karlan et al. (2012) also randomly assign business training to some of their clients. Their finding that this training leads to disinvestment (the effect is large although not significant) is suggestive in this regard: Perhaps business training is useful precisely because it makes it easier to know when to get out.

So business training might foster the foresight not to invest. The problem with this view is that it is difficult to test rigorously. The value of information is necessarily in the future while the costs are paid now—how do we know that people are making the right trade-off?

4. DIRECTIONS FOR FUTURE RESEARCH

Many, indeed most, of the key issues and ideas brought up in this article are only partially resolved at best. We still do not know whether group liability really matters (and in which direction) and why microcredit does not do more to transform the lives of its participants, although in both cases we have a number of useful hints.

There are also several areas in which exploration is only beginning. On the theoretical side, the idea that behavioral issues are key to understanding borrower and lender behavior is in the air, but much of the focus is on hyperbolic discounting, and even for that case, we do not yet have a characterization of the optimal dynamic credit contract. Moreover, other deviations from the standard model of borrower behavior—inattention, inability to understand complicated contracts, good-specific discount factors—have been studied less.²⁴

We also briefly touch on another important area for theoretical research: optimal lending policy in a setting in which the lender's reputation is important in making the credit contract work well. I argue informally that this could lead the lender to set contracts that are perhaps excessively safe, but the subject deserves proper theoretical treatment. In particular, would a commercial lender choose a different contract from a nonprofit, and which one would promote efficiency? This question of how nonprofit and commercial lenders differ has animated the recent debate on microcredit. De Quidt et al. (2012b) analyze this question: They ask whether, for example, joint liability lending can be a tool of rent extraction in the hands of a for-profit lender, in other words, whether the social capital that MFIs are thought to leverage in order to extend credit to collateral-poor borrowers might also be a resource that a lender can exploit. In an ex post moral hazard model, the authors show that a monopoly lender seeking to maximize profits is less likely to require joint liability over individual liability than a nonprofit and that forcing the monopolist to make joint liability loans would increase welfare. They then ask whether monopoly lending can be justified on the grounds that competition undermines repayment incentives and conclude, based on quantitative estimates using parameters estimated from the MIX Market data set (<http://www.mixmarket.org/>) and existing research, that it cannot be—the gains from competition remain very large. Thinking of other reasons why commercial lenders and nonprofits may behave differently—because of different incentives for reputation building, for example, as discussed above—clearly remains an important area of future research.

²⁴Banerjee & Mullainathan (2010) provide a partial exception: As mentioned above, they argue that the structure of the microcredit loan has some advantages when the source of the problem is good-specific discounting but do not characterize the optimal credit contract in this environment.

There are also important questions about the market equilibrium implications of the expansion of microcredit. Bardsley & Meager (2012) already point to conceptual difficulty analyzing such markets. They use a dynamic model to study competition between a traditional lender that relies on direct punishments to control default and a microlender that lends based on the borrower's reputation. The interaction between the two comes from the fact that a change in the strategy of the nonreputational lender affects the value of the reputation. They show that as a result, there may not be a pure strategy equilibrium. Buera et al. (2012) highlight a different aspect of competition: They demonstrate that the provision of microcredit can lead to an efficiency-enhancing reallocation of production across firms. However, as we see in the data above, microcredit has, at best, a rather limited effect on production patterns, which might limit the relevance of the Buera et al. (2012) argument.

One market equilibrium issue with immediate policy implications is competition between lenders. There are a number of reasons why this is unlike other forms of competition. First, there is a significant amount of learning involved in the credit relationship: The first loan reveals a lot of information about the borrower that can be useful to all subsequent lenders. There is therefore clearly a potential externality here, especially if the usual reasons that lead to credit rationing prevent the first lender(s) from raising the interest rate on the first loan to capture the value of the information that loan generates. Alternatively, tying a borrower permanently to a lender generates other inefficiencies. How to balance these conflicting incentives is an important regulatory concern, especially as credit registries expand and there is discussion of imposing mandatory disclosures on lenders.

There are other, less understood, but potentially more important problems with excessive competition. Borrowers can borrow from one MFI to repay another, and on and on, and eventually end up defaulting on the last member of the chain. Indeed, MFIs can adjust their repayment structure to encourage their potential defaulters to default on other lenders. At a less cynical level, borrowers who are offered multiple loans might take them all without really understanding what they are getting into and end up defaulting on all of them. Mandatory disclosure of all MFI loans would help with some of these problems (like the last one) but may make it easier, for example, for some MFIs to prey on others. The one empirical study that I am aware of (Luoto et al. 2007) does find that defaults went down as a result of the introduction of a credit bureau.

On the purely empirical side, two questions stand out. First, is it possible to help borrowers make better use of the loan? Second, is it possible to create a mechanism that makes loans that are an order of magnitude larger than microcredit loans (e.g., \$10,000 instead of \$250) and still assure high repayment rates? Related to this second question, how does one identify people who are suitable for larger loans, both in the sense that they will make good use of the money and that they will repay? Increased loan size is the obvious direction to go, if one of the main reasons why microcredit has a limited impact is that the loans are too small, as argued above.

As of now, there are essentially no clear positive answers to either of these sets of questions. On the first—of how to improve loan use—there have been a number of experiments, but with no evidence of clear impact. Karlan & Valdivia (2011) find an effect of business training on what entrepreneurs know but no effect on revenues, profits, or number of employees. Karlan et al. (2012), as mentioned above, also offer business training and find that it does lead to changes in business practices, but their net effect seems to be to reduce profits, and they are eventually abandoned. Bruhn & Zia (2011) also find that training changes practices but reduces profits (although the effect is not significant). More recently, we do see some studies that get some positive effects: Giné & Mansuri (2011), in the Pakistan study discussed above, also have a business training component and find evidence of changes in business practices, but also greater borrowing as a result of the training (which makes it harder to unpack the results). They find no effect on

profits or revenues but a positive effect on income. Finally, Berge et al. (2011) find a positive impact on both business practices and business profits. The divergent results of these studies definitely need unpacking; one possibility is that much of the training is too sophisticated, as suggested by Drexler et al. (2010), who try different types of training and find that only the most mundane instructions help.

On the question of whether bigger loans would make a big difference, there are no experiments that I am aware of (perhaps because they are too expensive), but some nonexperimental evidence suggests that the returns to successfully identifying and lending to such borrowers may be very high.

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