HOW MUCH DO EXISTING BORROWERS VALUE MICROFINANCE? EVIDENCE FROM
AN EXPERIMENT ON BUNDLING MICROCREDIT AND INSURANCE

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ABSTRACT

Several recent randomized controlled trials have found only modest effects of microfinance on consumption and income. However, these studies by design estimate impacts on new clients, so these modest effects may only have been lower bounds on the gains for more-experienced borrowers and the longer-run potential for microfinance. We examine the causal impacts of microfinance on experienced borrowers, and these clients' valuation of their ongoing microfinance relationship. Our research design uses an episode during which a microfinance institution modestly increased their clients' fees in randomly selected villages in exchange for a mandatory health insurance policy (which turned out to be entirely useless due to administrative failures). Our first result is that this modest increase in fees led to a 22 percentage point (or 30%) decline in loan renewal in treatment villages, compared to control villages where the policy was not introduced. Using this randomly generated variation in microfinance participation among established microfinance borrowers, we find impacts of microfinance that are strikingly similar to the previous literature: neither business outcomes nor household consumption outcomes were affected, on average, for the most part. Consistent with some previous studies, there were some declines in an index of business outcomes and declines in durable goods purchases, but only for those clients who had a business before microfinance entered the village. By contrast, businesses that started after microfinance had entered the villages were unaffected in terms of business outcomes but enjoyed an increase in non-durable goods consumption. This heterogeneity in effects is consistent with a simple model in which durable goods are lumpy purchases. The high drop-out from microfinance further suggests that the net gains from microfinance are small for a substantial share of borrowers. Strikingly, those who had a business before microfinance are as likely to exit as other borrowers, despite suffering large losses in business earnings as a result, which raises the possibility of substantial unmeasured costs from running microfinance-funded businesses.

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I Introduction

Several randomized evaluations of microfinance, from various settings and countries, find no evidence of a strong positive impact on household consumption, incomes, or social outcomes, such as female empowerment, health, and education (Crepon et al., 2015; Augsburg et al., 2015; Tarozzi, Desai and Johnson, 2015; Angelucci, Karlan and Zinman, 2015; Attanasio et al., 2015; Banerjee et al., 2015b).[1] Some have argued, however, that this could be because these studies generally focus on new clients. This focus on new clients is a consequence of these studies' general research design, which takes advantage of the randomized expansion phase of microfinance. Microfinance institutions (MFIs) usually resist the idea of denying credit to anyone who wants it once they are officially open for business in the area, and of course these institutions would be hesitant to randomly stop providing credit to existing clients. If established clients are systematically better at using microfinance than new clients, perhaps because it takes time to determine how best to use the credit, then the previous results could be misleadingly pessimistic about the potential long-run positive impacts from microfinance once it becomes established.[2]

This paper describes a randomized evaluation of the impact of microfinance on *established* microfinance borrowers. The paper takes advantage of an episode in which established microfinance clients, in randomly chosen villages in India, became obliged to purchase a health insurance policy upon renewal of their microfinance loans. As a consequence, many long-established clients left the microfinance institution and stopped borrowing. This episode therefore provides an unusual opportunity to examine whether the existing literature on new clients applies to a very different population of experienced microfinance borrowers. Further, this focus on established clients allows us to test a conjecture of the existing microfinance literature: that among those who joined microfinance to finance a business, the treatment effect of microfinance is very different for clients who joined to finance an existing self-employment activity as compared to the treatment effect for those who were induced to start a new activity.

In 2007, in rural Karnataka and Andhra Pradesh, one of India's leading microfinance organizations at the time, SKS Microfinance, began requiring all new and renewing clients to

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[1]Meager (2016a,b) provides meta-analysis across microfinance studies, and Banerjee, Karlan and Zinman (2015) summarizes the key findings.

[2]Breza and Kinnan (2016) estimate the impact of the loss of microfinance, at the district level, using the sudden retrenchment of microfinance in Andhra Pradesh, India. In contrast to the relatively modest impact on borrowers implied by the RCT literature, they find large negative impacts on district level outcomes. They note that there are two possible explanations for these difference. First, there could be general equilibrium effects affecting both borrowers and non-borrowers (see also Buera, Kaboski and Shin, 2014). Second, most of the RCT studies estimate impacts on new borrowers (and on borrowers who may be marginal in other senses as well).

purchase a health insurance policy that provided coverage for catastrophic events, hospital-ization, and maternal care. At the beginning of this initiative, for two districts in Northern Karnataka,[3] we coordinated with SKS to leave out randomly some villages from the health insurance expansion to enable the evaluation of this health insurance product. SKS had been operating in those districts for over two years, and microfinance was a known product in the area. We collected data at baseline (before the introduction of the health insurance requirement), endline, and at regular intervals on a randomly selected sample of existing SKS clients in 101 "treatment" villages (in which clients became required to purchase insurance) and 100 "control" villages (in which clients were not required to purchase insurance).[4]

To the surprise of SKS, the insurance product turned out to be extremely unpopular. There were anecdotal accounts of client complaints from the beginning. In the course of events, the insurance scheme was never properly implemented, the relationship between SKS and the third-party insurer (ICICI-Lombard) soured, and eventually the purchase of the insurance policy was made voluntary and then later discontinued.

Our first result is that the insurance requirement, and the associated fee, led to a large decline in loan renewal rates. Administrative data show that loan renewal rates declined by 22 percentage points (30 percent) in treatment villages compared to control villages where 75 percent renewed. Self-reported data from clients suggests that few of those who left SKS obtained microfinance loans from other organizations, even in villages where they were available, so this led to a net decline of participation in microfinance. The effect is large: the policy was inexpensive (Rs. 525, compared to a renewal loan size of around Rs. 9,600) and could be rolled into the loan so that it essentially represented a 5.5 percentage point increase in the interest rate on a base APR of 24%. Even if the clients assigned zero value to the insurance product, probably rightly, this was not a very large increase in borrowing costs. Historically in India, before interest rates were capped by the Reserve Bank, MFIs often charged rates in excess of 30%. The implied price elasticity of microfinance participation (1.4), however, is comparable to the participation elasticity estimates from Karlan and Zinman (2016) using experimental variation in the interest rate in Mexico.

Given the failures in the implementation of the insurance scheme mentioned above, and detailed below, we find unsurprisingly a very precisely estimated but very small impact of the provision of health insurance on utilization of health care, health care spending, or the financing of health care. We therefore treat this as a pure microfinance experiment, where the increased cost of the loan in some villages generated random variation in the continued

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[3]This region abuts Andhra Pradesh, the location of one of the previous RCTs and the location of the Breza and Kinnan study.

[4]We use the traditional nomenclature of "treatment" and "control" to indicate the new product, even though in a sense our intervention was to keep a group of villages untouched by the new product.

use of MFI loans.

Consistent with the previous literature on microfinance, we find little impact on income, consumption, social outcomes, or on whether SKS clients continued to own a business or started a new business. These results are not simply the mechanical consequence of the fact that these borrowers chose to drop out and so, by revealed preference, did not have much to gain from microfinance. While the net utility gain from a microfinance loan may well be small for those who drop out (more on this later), we might still expect changes in the observed outcomes. After all, those who are borrowing money are either investing the funds or consuming them and then repaying the loan, and so the loss of that loan should be reflected in the nature and timing of consumption and/or investment. For business owners, specifically, the loss of the loan should imply less investment, lower revenues, and less profits (gross of interest).[5]

Consistent with this prediction for business owners, we do see a decline in the scale of businesses and a significant decline in an index of business outcomes (that includes profit, sales, and employment). Interestingly, these results come entirely from the 80% or so of business owners at baseline who had a business before SKS started lending and for this group there is strong evidence for a negative effect on business outcomes. On the other hand, we find no effect of losing microfinance on those businesses that started after SKS started lending. Indeed, for this latter group of entrepreneurs, dropping out from microfinance had a substantial *positive* effect on consumption and particularly non-durable consumption (e.g., food). This is similar to the finding in several of the previous studies of microfinance, which also find positive impacts of microfinance on those whose businesses started before microfinance was available to them (Banerjee et al., 2015b; Crepon et al., 2015; Augsburg et al., 2015) even though they, like us, find little or no effect on the general population. Banerjee et al. (2015a) also compare the businesses that were started after microfinance was launched in treatment and control areas in Hyderabad and, like us, find no impact on business outcomes for them. However, this difference combines a possible negative selection effect (firms started with microfinance money may be less productive) with the potentially positive effect of getting an extra loan, so the net difference may be zero even if the loan by itself has a positive effect. By contrast, our estimate compares businesses started after microfinance both in treatment and control areas, so the selection is exactly the same. The absence of a treatment effect confirms that getting an extra loan does not make these households more productive.

The remainder of the paper proceeds as follows. In section II, we lay out a simple model

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[5]Something similar should also be true if they sought out other more expensive sources of borrowing after the loss of microfinance.

that highlights why the impact of losing access to microfinance may vary systematically across different kinds of households. In section III, we describe the empirical setting. In section IV, we describe the experimental research design and data collection. Section V lays out the empirical methodology. We report the results in section VI, which we interpret further in section VII. Section VIII concludes.

II Potential Impacts of Exit from Microfinance

In this section we sketch a simple model of consumption and investment choice for households that are credit constrained. The purpose is to highlight the different responses to the tightening of the credit constraint among households who have been in business for some time, as compared to those households who are relatively new to business. An important advantage to our empirical application is our ability to differentiate between these two groups.

II.A Basic Model

Each consumer "lives" for 2 periods. At the beginning of each period, the consumer can spend money on two goods that we will call consumption goods and capital goods. Consumption, denoted by c, is fully divisible and purchases are consumed during the period in which they are bought. The per-period utility function is given by $u(c)$. We assume that it is defined only on the positive orthant and that $u'(c) \to \infty$ as $c \to 0$. There is no discounting.

The capital good comes embodied in two available technologies. Technology 1 is purely linear: the consumer can invest an amount k in technology 1 in any period and get a return ak at the end of that period. Technology 2 involves a one-time fixed cost f to be paid in period 1. If the consumer pays that fixed cost in period 1, and then invests an amount k in technology 2 in any subsequent period (including the period where in which the fixed cost is paid) then the consumer receives a return $Ak > ak$ at the end of that period.

Given the increasing returns technology, the model is only well-defined if there is a credit constraint. Assume that the consumer starts period 1 with wealth zero but can borrow up to a limit b at the beginning of each period at interest rate $r < a$ which is repaid at the end of the period. At the end of the first period, the consumer can also save and therefore begins the next period with wealth $w \geq 0$.

II.B Analysis of the Model

Given the above model setup, the consumer has to decide whether to invest in technology 1 or technology 2. In the former case, the utility will be $v(b) = max_w u(b(1-\frac{r}{a}) - \frac{w}{a}) + u(b(1-\frac{r}{a})+w)$. In the latter case, the utility will be $V(b) = max_w u(b(1-\frac{r}{A}) - \frac{w}{A} - f) + u(b(1-\frac{r}{A})+w)$. We can verify that:

Lemma 1: Regardless of the choice of technology (technology 1 or technology 2), it is

optimal to set $w = 0$

Using this lemma, it follows that: for f large enough, i.e., for $b(1 - \frac{r}{A}) - f$ close enough to zero, $V'(b) > v'(b)$. This gives us our first result:

Result 1: as long as f is large enough, given a reduction in b, consumers that are in the first period of their life will tend to switch from technology 2 to technology 1. When this happens the consumer's first period consumption will jump up and second period consumption will go down.

However, not all consumers will make this switch. Those consumers that have high enough b to start with will continue to invest in technology 2 and just cut back their first period consumption. Likewise, consumers that had a low b to start with, and therefore were never going to choose technology 2, will cut back period 1 consumption.

Result 2: when b drops, consumers in the first period of their life with either very low or very high initial values b will see a drop in their period 1 consumption.

This result tells us what to expect for the consumers who started their businesses after SKS started lending in their village. Period 1 consumption may go up or down on average for these consumers depending on the initial distribution of b (or, if we allowed their initial wealth to vary, the average impact would also depend on the initial distribution of wealth).

For the households that already had a business before SKS started lending, we assume that the cut in borrowing happens in the second period of their life. For these households the only effect is a reduction in k and a resulting fall in second period revenues and consumption. The reduction will be larger for those households that had adopted technology 2.

Result 3: a reduction in b leads to reduction in investment, business earnings, and consumption for consumers that are in the second period of their lives.

II.C   Discussion

This model is, of course, a vast oversimplification. By ending the story in period 2, the model rules out the possibility that consumers in the second period of their lives may cut back their consumption to rebuild their capital stock. This effect would depress their consumption even further. On the other hand, consumers may borrow to buy indivisible consumption goods. In this case, dropping microfinance might lead them to substitute divisible consumption for non-divisible consumption. While our data do not directly distinguish between these two categories, it is reasonable to assume that most durable goods are more indivisible and most non-durable goods are more divisible. There is one important exception, however, which is weddings and other celebrations and which tend to be somewhat indivisible without being durable.

The model also rules out all selection effects. It is plausible that those who started their

business before SKS arrived, when relatively inexpensive credit became available, are on average self-selected on being more productive and/or more committed to being in business. If this is the case, then the effect of dropping microfinance may be stronger among the pre-SKS businesses even if there is no indivisibility in production because the marginal product of capital is higher for those people. However, if there is no indivisibility then we should not see a jump up in consumption in either period in treatment villages that experience decreased microfinance borrowing.

III   The Context: Bundling of Credit and Insurance

In 2006, SKS Microfinance decided that it should offer health insurance to its clients. At that time, SKS was the largest MFI in India and sought to leverage its administrative advantage in dealing with low-income clients spread across rural areas of India. While ICICI-Lombard would provide the back-end insurance, SKS would administer enrollment and the initial processing of claims.

In June 2007, SKS began requiring loan clients to purchase health insurance across most of their area of operation. We persuaded them to use the expansion for a randomized evaluation of the insurance product in 201 candidate villages with SKS presence in two districts of Northern Karnataka.[6] In 100 randomly selected villages (the control group), they continued with business as usual. In the remaining 101 villages (the treatment group) insurance subscription would become mandatory for clients at the time of loan renewal. The typical health insurance policy cost Rs. 525 (approximately \$13 at 2007 exchange rates), which was loaded into the amount of the loan and paid in weekly installments along with the loan payments. By way of comparison, the average renewal loan size was Rs. 9,600.[7] The insurance premium thus represented a 5.5 percentage point increase in the interest rate, which was roughly 24% APR at the time. The health insurance policy was intended to be actuarially fair, though SKS was prepared to lose money initially on administrative costs.

The launch of the insurance product did not go smoothly. SKS initially planned to make the purchase of insurance mandatory for all existing clients. Faced with rebellion by its clients, SKS decided to make it mandatory only for new clients and for existing clients when renewing their loans. Still, discontent with the policy and resulting client drop-out led SKS to make the insurance voluntary starting in October 2008. This unilateral change to the insurance product, and anecdotal accounts of adverse selection and outright fraud, led to a breakdown of relations between SKS and ICICI-Lombard, and insurance enrollment was

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[6]The two districts are Bidar and Gulbarga, which are a few hours' drive from Hyderabad, the capital of Andhra Pradesh and the location of SKS's headquarters.

[7]This number reflects the average loan size upon renewal in control villages following the roll-out in treatment villages.

discontinued in March 2009. Thus, by the time of our endline survey, clients had become free to rejoin SKS without purchasing the insurance policy.

As it turned out, SKS clients were correct in not wanting to purchase this particular health insurance policy. In principle, the policy covered hospitalization and maternity expenses, and clients had the option of going to approved health facilities to get cashless treatment or paying out of pocket for treatment at other facilities and submitting a claim for reimbursement. In practice, however, the implementation was badly managed by the partnership of SKS and ICICI-Lombard. Reimbursements were difficult for clients to file, and often went unprocessed. In an attempt to deal with this problem, the focus of the program was shifted to upfront cashless treatment, but the number of hospitals that were networked for this service was inadequate, and in any case many SKS clients did not receive the required insurance cards. As a result, the cashless approach was also ineffective. Below, we show that obtaining insurance had no impact on the way SKS clients handled major health events or on their health status and expenditures.

IV Randomization and Data Collection

SKS Microfinance originally identified 201 villages where it was currently running its microfinance program and was interested in evaluating its health insurance program. SKS operations were organized by center, with multiple centers in a village. To minimize the risk of spillovers between treatment areas and control areas, however, centers were grouped by village such that all centers in close proximity would receive the same treatment/control status.

In December 2006, using SKS's list of villages, our research team randomly selected 101 villages for SKS to pilot the health insurance product. The remaining 100 villages formed the control group, in which health insurance was not offered through SKS (although some clients had insurance through other sources). The randomization was performed by the Principal Investigators using the Stata random number generator after stratification by branch and number of microfinance clients.[8] The stratification ensured an even geographic distribution of treatment villages and control villages, as well as a similar number of clients in treatment and control.

SKS introduced the insurance requirement on a rolling basis, whereby the first village was reached in June 2007 and the last in November 2007. Once insurance was introduced in a village, its purchase became mandatory upon loan renewal for all microfinance clients within the village.

We draw on four sources of data for the analysis:

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[8]SKS operation across villages is grouped within branches, of which there are seven in our sample. Within each branch, we also stratified by whether a village had more or fewer clients than the branch median.

First, we collected detailed baseline data from a random sample of SKS client households: 29 households per village, on average, in all treatment and control villages. We collected data from December 2006 through March 2007, and the survey instruments and data are available for download.[9] A household survey module was administered to the household head in sampled households, and an adult module was administered to each adult found in the household.[10] The household survey identifies a number of household characteristics, including: household composition, economic status and assets, means of livelihood, and household expenses. The adult survey covered the adult's means of livelihood, income, educational background, expenses, health status, and medical treatment patterns. For rarer health events, the household survey covered the household's experience with major health events in the previous year: all events in which a household member died, gave birth, experienced an injury or illness that prevented them from performing their normal daily activities for more than a week, had any other health problem that required hospitalization, or otherwise spent more than Rs. 300 ($7) to treat a health event. For each of these health events, the survey records basic information on its type, the way it was handled, and how the household paid for it.

In the baseline data, we see similar client characteristics in treatment and control villages (Table 1). For the subsample of clients who report owning a business at the time of the baseline survey, Panel A reports average business outcomes over the previous year. Following Kling, Liebman and Katz (2007), we also pool these four outcomes into a single index of business performance.[11]

Second, we collected similar survey data at endline, which came after insurance enrollment had been discontinued and clients had the opportunity to rejoin SKS without purchasing insurance. From 2009 through 2010, approximately two years after clients had faced enrollment decisions, we collected detailed data on the same households. Of the baseline households surveyed, only 1.3% were not found for the endline survey and this attrition was not differential by treatment status.[12]

Third, we draw on administrative data provided by SKS, which can be merged to our detailed surveys through SKS's client identification numbers. The SKS administrative data

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[9]The surveys can be downloaded at http://dx.doi.org/10.7910/DVN/25890

[10]Surveyors visited households multiple times to interview each adult (over the age of 14), though in some cases they did not find all adults reported to be in the household.

[11]Following Kling, Liebman and Katz (2007), we create each index in the paper by calculating an equally weighted average across the component characteristics' z-scores. The z-score itself is calculated by subtracting that characteristic's mean in the control group and dividing by the standard deviation in the control group, orienting the sign of each z-score to be in the same conceptual direction (e.g., a larger business). Differences in the index then reflect an average difference in the standard deviation across each component characteristic.

[12]We attribute this low attrition rate to the relative stability of these households, and our ability to find households with the help of prominent village members.

comes in two main forms. First, SKS provided loan histories for its entire client base in our research areas, including when clients took out past loans and the amounts received. This gives us detailed information on clients' previous loan activities, as well as the ability to calculate the effect of the requirement to purchase health insurance on loan renewal. In a previous paper (Banerjee, Duflo and Hornbeck, 2014), we combined this data with our baseline sample to show that there was no adverse selection in client sign-up: less healthy households were not disproportionately likely to renew their loan in treatment areas compared to control. Second, SKS maintained a database of everyone who was enrolled in insurance and all requested and processed insurance claims. This database provides information on who used the cashless facility and who received reimbursement for health expenses at other facilities.

Finally, to identify the effects of relatively uncommon major health shocks, we collected detailed data on health events and the way households handled them through the "Major Health Events Survey." A "major" health event is defined to be any health event that substantially disrupted a person's ability to perform normal daily activities for more than one week.[13] This survey was conducted on a continuous basis, from April 2008 to December 2009, and covers 25,000 major events that happened to 7,000 unique households. The survey was conducted in two stages.

At the first stage, a survey monitor accompanied the SKS loan officer to multiple meetings and asked the clients about any major health events in their household. At the center meeting, the surveyor recorded the name of the person who was affected, the category of health problem (sickness, accident, birth, other), the relationship between the affected person and the head of household, and whether the person went to a hospital.[14]

At the second stage, the full survey was conducted with the SKS client who had been identified at the first stage, generally in the presence of the person affected by the health event. The full survey began with verification of the information collected at the microfinance center meeting, and included a brief description of the event, when it began, and the timing of treatment received. The person categorized the seriousness of the event, along with the length of time for which it caused an inability to perform normal daily activities. The person also provided a list of symptoms, which allows us to further characterize the seriousness of the problem. The surveyor then collected information on all health providers the person visited,

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[13]We experimented with several definitions, but found this one to be most successful at identifying the major health events that we were most interested in and that might be underrepresented in the baseline and endline surveys.

[14]Though at the beginning we asked about all major health events since January 2008, in July we switched to asking about all major health events in the last 30 days, in order to improve recall ability of clients and to allow us to visit villages more frequently.

along with basic information about the provider, what treatment was received and at what cost, and the amount of lost income for this person and family caregivers resulting from this episode. For expenses incurred, the person was asked about how they were covered, including by saving, borrowing, or the sale of assets. Information was also collected on whether and how this person used insurance and other finances to pay for the treatment expenses as well as the person's expectations for receiving reimbursement.

V   Methodology

The empirical analysis compares client outcomes in treatment villages to client outcomes in control villages. We focus on existing clients who had loans by June 2007, the date of the roll-out of the health insurance requirement in the first village in the sample. The roll-out took place progressively at different villages (from June to November).

For each client i in village v and randomization strata s, we regress each outcome (Y) on an indicator variable for treatment village (T) and randomization strata fixed effects (α):

$$(1) \qquad\qquad Y_{ivs} = \beta T_v + \alpha_s + \epsilon_{ivs}.$$

The coefficient of interest β indicates the average impact from the requirement to purchase health insurance. For all regressions, the standard errors are adjusted for heteroskedasticity and clustered by village to adjust for local geographic correlation.

We begin by considering impacts on clients' SKS loan take-up decisions using administrative data from SKS. Given the troubled implementation of the health insurance program, we then verify the expected absence of impacts on healthcare utilization and expenses on healthcare using both the major health event survey and the endline survey. Whatever impact we find on other outcomes is therefore presumably unrelated to the provision of health insurance.

In looking at household and business outcomes, to avoid the potential for specification search, we simply follow the template for analysis that the randomized evaluations of microfinance adopted in the 2015 Microfinance Issue of the *American Economic Journal: Applied Economics*. Following this template we classify the outcomes into consumption effects, business effects, and social effects.[15] To avoid misleading inference due to multiple inference, we compute an index of outcomes for each category and regress that index on treatment (Kling, Liebman and Katz, 2007). Further, we verify the estimated p-value on the business outcome index using a Hochberg correction for multiple hypothesis testing across total consumption

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[15]Unfortunately, we do not have data on labor supplied to the household business, though in the interpretation section we draw on estimates from Banerjee et al. (2015*b*).

and an index of social effects (Hochberg, 1988).[16]

We also separately report estimated impacts on business outcomes for the entire sample and for households that had a business at the time of our baseline, following previous literature. Further, we split the sample of businesses that existed at the time of the baseline survey into two subgroups: those businesses that were started before SKS started operating in the village, and those businesses that were started after SKS began operating in the village (but before our baseline survey). The idea is to look for heterogeneous treatment effects based on the theory that the availability of cheaper credit changes the nature of self-selection into entrepreneurship: the post-microfinance entrepreneurs that are the focus of most existing research may be less gifted or less committed. Previous studies of microfinance (Banerjee et al., 2015a,b; Banerjee, Karlan and Zinman, 2015) have attempted to get at this distinction by separately estimating treatment effects for those households that started their businesses before the introduction of microfinance and for those households that started their businesses after the introduction of microfinance. This separation is imperfect, however, as those households that start businesses after microfinance are necessarily not the same in treatment and control because of differential self-selection and therefore the estimated effect of microfinance on these businesses is potentially biased downwards. A nice feature of our experiment is that it allows us to separate the treatment effect from the selection effect because the businesses we compare are the same pool of post-microfinance firms both in treatment and control areas, unlike in previous work where the control firms are necessarily pre-microfinance.

VI   Results

VI.A   Impacts on Loan Renewal

The requirement to purchase health insurance substantially lowered SKS clients' loan renewal rates. Table 2, column 1, reports that clients in treatment villages were 22 percentage points (or 30%) less likely to take out an annual loan within one year after the pilot began. Specifically, clients were less likely to take out a new loan between June 7, 2007 and July 3, 2008.[17] Since the roll-out took place between June and November, these estimates are intent-to-treat estimates because not all of those who were renewing were facing the health insurance requirement. We estimate that 73% of clients in treatment villages renewing during the experimental period actually faced the health insurance requirement in order to renew, so these intent-to-treat estimates might be scaled up by a factor of 1.37 to get a sense of the

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[16]Effectively, this correction multiplies the business outcome index p-value by a factor of 3, given that its p-value is the lowest among the three outcome indices.

[17]Clients' annual loans are repaid over 50 weeks, so our clients would have been eligible to review between June 7 2007 and June 2008, since they all had a loan as of June 21, 2007. Since renewal can take place within a short grace period, we have included a six-week period for clients to renew their loan.

magnitude of the impact on those facing the requirement to buy insurance.[18]

Interestingly, this difference in loan renewal persisted after the health insurance require-
ment had been eliminated. At the time of the endline survey, SKS clients in treatment
villages remained substantially less likely to have an SKS loan. Based on administrative
data, clients in treatment village were 16 percentage points (30%) less likely to have an
outstanding SKS loan (column 2). This difference is smaller based on self-reported data
(column 3), which may reflect measurement error because many clients report having an
SKS loan when these do not appear in the administrative data. While both our survey
data and the administrative data may contain errors, we suspect the administrative data is
more accurate than the self-reports. Average loan renewal rates should decline over time,
as previous clients naturally drop out from SKS, and the self-reported mean renewal rate in
control villages is higher than would be expected. Table 2, Panel B, reports corresponding
estimates when restricting the sample to clients who report owning a business in the base-
line survey. Of particular use later, Panels C and D split this sample into those baseline
business owners who report their business starting before SKS entered the village (Panel C)
and those baseline business owners who report their business starting after SKS entered the
village (Panel D). Panel E reports estimates for households that did not include a surveyed
business at baseline. Table 3 reports the accompanying declines in SKS loan sizes, where
non-renewing clients' loan size is set to zero. Outstanding loan sizes decline, mostly due to
changes on the extensive margin (i.e. whether or not someone has a loan).

Some of the villages where the experiment took place had another microfinance organi-
zation, so part of the flight from SKS may have been compensated for by borrowing from
another provider. Column 4, of Tables 2 and 3, reports the estimated impact of treatment
on whether the household reports having a loan from another MFI. With the caveat that
these data are self-reported and may underestimate actual borrowing, we find little impact
of the treatment on borrowing from these alternative sources. In general, there is very little
reported borrowing from other MFIs at endline by current SKS clients (1.0%) or former SKS
clients (1.5%).

The characteristics of those clients who leave SKS are discussed in a previous paper
(Banerjee, Duflo and Hornbeck, 2014). In general, we found that clients who drop out
are similar to those who remain. In particular, we found no evidence of adverse selection

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[18]Based on clients' previous loan expiration dates and the dates of pilot roll-out, we calculate the fraction
of clients who would have faced the health insurance requirement when their previous loan expired. If clients'
renewal decisions are only affected when the health insurance requirement is binding at the time of their
first opportunity for renewal, then the implicit first-stage impact of the treatment is 0.73. We do not observe
roll-out dates for 20 villages, but make the conservative assumption that roll-out was immediate in these
villages. Clients whose previous loan expired prior to June 2007 are assumed not to face the health insurance
requirement.

based on health characteristics and that extends to health events that are fairly predictable (e.g., propensity to have a child). We found little difference in the households' economic characteristics, including the propensity to own a business.

VI.B Impacts on Health Status and Health Expenditures

For the surveyed "major health events," Table 4 reports impacts on insurance usage. People in treatment villages are 51 percentage points more likely to report having health insurance at the time of the health event (Panel A, column 1). However, they are only 0.3 percentage points more likely to receive insurance benefits (column 2). This number includes both the use of a cashless facility and reimbursements, either of which taken separately show very small increases (columns 3 and 4). The major health event survey was generally conducted shortly after the event, however, and in 4.5 percent of the cases of responders say that they expect to receive reimbursement (column 5). While these differences are statistically significant due to the large sample size and near absence of insurance in control villages, the magnitudes are all very small.

We see the same pattern when we group the health event survey data by client (Panel B). People in treatment villages were 68 percentage points more likely to report ever having insurance for a major health event and more likely to report ever having received insurance benefits (1 percent), or expecting reimbursement (9 percent), but the magnitudes remain small.

For this sample of clients who report a major health event, we can also use administrative claims data to see whether they appear to have ever used insurance. Column 1 reports that 84% appear in administrative data as being enrolled in the insurance program at any point.[19] In terms of these clients receiving insurance benefits at any time: 7.4% receive some benefit, of which 2.6% used a cashless facility and 5.2% received some reimbursement. This number lie between the rate or reimbursement observed in panel B (1%), and those that were expected (9%). Since this is *conditional* on an eligible event occurring, these are very low numbers.

Given that insurance benefits were rarely availed of, it is unsurprising that we see no meaningful difference in how households responded to a major health event (Table 5). Following one of these events, there is no significant impact on whether the person stayed overnight in a hospital, the total cost of health care (including lost income), or the financing of associated costs. The point estimates and standard errors are small, suggesting that the lack of a significant finding is not driven by noise.

In the endline survey, there is also no meaningful impact on clients' health and their health care usage in the previous year (Table 6). Specifically, we find no impact on health

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[19]Across all control villages, only one client is reported to be enrolled in the insurance program (and is not reported to receive any insurance benefit).

care expenditures (column 1), whether clients borrowed for health care expenses (column 2), how much they borrowed for health care expenses (column 3), the number of serious health events (column 4), or the probability of staying overnight in a hospital (column 5). The absence of impacts on health-care utilization parallels recent estimates from Nicaragua (Thornton et al., 2010). There is also no impact on the ability of individuals to perform basic activities in daily life (column 6).[20] Curiously, households have significantly *worse* self-reported health (column 7), which may reflect the insurance information campaign leading clients to focus more on catastrophic health events or their health more generally.[21] Overall life satisfaction, however, is not substantially affected (column 8).

The health insurance product had no direct effect on the impacts it sought to achieve: health status, health care usage, and the financing of health care expenditures. While many people in these areas did pay the health insurance premium and enroll, very few received insurance benefits following major health events. For whatever reason (failure to communicate to households, failure of SKS field officers to effectively intermediate between the clients and ICICI-Lombard, clients' lack of understanding, etc.), the product turned out to be mostly useless, and anecdotal evidence suggests that clients found this out fairly quickly. The requirement to purchase insurance did inadvertently lead to a significant decline in microfinance borrowing, however, and the following sections explore how this impacted households.

VI.C Impacts on Client Businesses

Table 7 reports the impact on clients' businesses resulting from the requirement to purchase health insurance. For the full sample of clients (Panel A) or the sample of clients who owned a business at baseline (Panel B), there was no substantial or statistically significant impact on whether they owned a business at endline (column 1). Columns 2 through 5 report impacts on endline business outcomes for those with businesses at baseline, including zeros for those who do not report owning a business at endline.[22] All the point estimates suggest they invested less in their businesses and generated less profit, though only expenditures on

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[20]We ask each adult about their difficulty in performing 15 daily activities, rated on a 5-point scale. We create an index for each adult, averaging across the responses by activity (each normalized to have a mean of zero and a standard deviation of one), and assign an index for each household by averaging across the adult member indices.

[21]Dow et al. (1997) find a similar effect, in reverse, in Indonesia: an increase in health facility fees led to an increase in self-reported health status, as people were less likely to visit the hospital. In Zwane et al. (2011), we found that asking people a long series of baseline survey questions on health tended to make them more likely to buy health insurance, perhaps because it made them aware of the risks. The information campaign could have had the same effect.

[22]Note that we asked businesses owners about profits directly, rather than calculating the difference between reported revenues and reported costs, so the outcome in column 5 contains additional information compared to the previous columns.

workers is individually statistically significant. Column 6 reports the estimated impact on an index of business outcomes, drawing on the outcomes in columns 2 to 5, which is negative and statistically significant at the 10% level, though not with a Hochberg correction for multiple hypothesis testing across all three categories of household outcomes (business, consumption, and social outcomes).

The point estimates of the scaling down of businesses are large, despite the churn in the number of businesses. Only 32% of all self-reported business owners at baseline continue to own a business at endline, so we have many zeros in the data.[23] For example, the point estimates in panel B imply a reduction of 55% in expenditures on workers for existing businesses, a 61% in reduction in asset expenditure, and 12% reduction in sales.

In panels C and D, we separate the existing businesses according to whether they are reported to have been started before SKS entered the village (Panel C) or started after SKS entered the village (Panel D).

The negative impact of losing microfinance access is entirely focused on the older businesses (Panel C), which tend to be about twice as large in terms of sales and profit as those businesses that were started after SKS entered the village. For these older businesses, the effects are even larger: we find a reduction of 82% in asset expenditure, 58% in expenditure on workers (significant at the 10% and 5% level respectively), and a 0.063 standard deviation decline in the index of business outcomes that is significant at the 5% level (and 13% with the Hochberg correction).

In panel D, we find no impact on businesses that were started more recently, after SKS had entered the village. This is a smaller sample of clients, and the standard errors are correspondingly larger, but the point estimates are also small and often have the opposite sign. Thus, it appears that either the microfinance funds were not invested in the business (consistent with the absence of a decline in asset expenditure), or that the marginal product of capital for these businesses is close to zero.

Panel E reports estimated impacts on business outcomes for clients that did not own a business at baseline. We see negative impacts on these clients' business outcomes, smaller in magnitude than the effect for business stared before SKS started. About 9 percent of them do have a business at endline, so in terms of magnitude, the impact per active business is comparable to the impact for the pre-SKS business owner. This makes sense: those businesses started after SKS became less attractive, so they would be selected to be motivated

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[23]If the impacts on business outcomes were driven solely by impacts on clients that owned a business at endline, then the estimates might be scaled up by a factor of 5.6 (for Panel A) and 3.2 (for Panel B). In fact, this is what we find when we restrict the sample to businesses that are still in existence at endline (a potentially endogenous outcome), in which case the estimated impacts on business outcomes are also all statistically significant.

businesses.

VI.D Impacts on Other Household Outcomes

Recent randomized evaluations of microfinance have also found some impacts on household businesses, but little impact of access to microfinance on overall consumption and social outcomes. We analyze data for these other outcomes, following the same template as those papers, and find very similar baseline non-results from our "reverse" experiment (the loss of microfinance). We then extend this analysis to consider differential impacts for households with businesses started prior to SKS and households with businesses started after SKS.

Table 8 reports estimated impacts on household consumption at endline.[24] For the sample as a whole (Panel A) or the sample of baseline business owners (Panel B), we see little impact on households' consumption, either total annualized consumption or within particular categories. There is some decline in durable goods consumption, which aligns with other estimates on the impacts of gaining access to microfinance (with the opposite sign, as expected).[25] There is also some evidence of increased non-durable goods consumption, which we explore further below.

Focusing on business owners at baseline, a very interesting pattern emerges when we separate the older businesses and those who started after SKS started its operation. Households that own older businesses spend significantly less on durable goods when they lose access to microfinance (we also saw that they reduced business durable expenses). Households that own newer businesses *increase* their total consumption substantially, and this estimate is statistically significant at the 1% level even after correcting for multiple hypothesis testing. This consumption increase is focused on non-durables (food, in particular). This same pattern emerges in per capita terms, adjusting for the number of adult equivalent household members (Table 9).

Finally, focusing on the household who did not have a business at baseline (the vast majority of them still don't have one), we find significant increases in food and temptation good consumption, and insignificant decline in durable consumption and celebrations.

Column 9 reports no increase in the household's adult labor income from salaried jobs or day labor, which was reported in the adult modules of the household survey. Households do not appear to have compensated for declining business by working harder. Indeed, all the point estimates are negative and statistically insignificant. Thus we do not see an increase

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[24]Table 9 reports corresponding estimates that are expressed in per capita terms that reflect the number of adult equivalent household members based on conversion factors used by Townsend (1994) for rural Andhra Pradesh and Maharashtra.

[25]We do not estimate greater impacts on consumption after a longer period of time, comparing households surveyed further after being exposed to the treatment (after 27 months) to those households surveyed more recently after the treatment rollout (within 27 months).

in another source of revenue that compensate the loss of business income for older business owners. For more recent businesses, that then raises the question of how the consumption increase could have been financed. We return to this question below.

Table 10 shows no systematic impacts on social outcomes, consistent with findings across other studies of microfinance. There is little change in the share of younger or older children in school and, while there is some impact on adolescent girls' reported "progressive" attitudes,[26] an index across all outcomes in this category is statistically insignificant.

VII Interpretation

Most of the baseline results are in line with the existing experimental literature on the impacts of microfinance. For non-business-owning households, there is little systematic impact on overall consumption. This is perhaps not surprising, as with microfinance they may be borrowing more to finance some form of consumption and then cut back other forms of consumption to pay for the loan. However, we find an increase in food consumption and temptation goods consumption that suggests that the loan may have played a disciplining device. The remaining question is why their consumption does not actually decline, as they pay interest on the microfinance loan, though perhaps they are substituting away from more-expensive loans (e.g., from a moneylender) or working a little more in a manner that we do not detect in the data. Notably, the effect on durable goods consumption is negative, but insignificant and not very large in magnitude (compared to the size of the loan), which suggests they are not using the money to finance additional durable goods purchases that might be lumpier.

For business owners, we find results that are broadly consistent with the simple model described in section II, and with much of the previous literature on microfinance. As predicted for those in period 2 of their "life" in the model, older business owners invest less in working capital (payments to workers) as well as fixed capital (business assets). We also see a reduction in profits that is large (the implied treatment on treated effect is nearly 10,000 rupees per business per year) but imprecisely estimated (because of the substantial churn in business ownership and the large number of zeros) and this is perhaps reflected in lower durable consumption (although we do not see a decline in non-durable consumption). For newer business owners, who correspond to those who are in period 1 of their "life" in the model, we see the jump up in consumption predicted for those who switch away from making a lumpy investment. The increase in consumption is over 9,500 rupees per house-

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[26]We define "progressive attitudes" by creating an index across girls' responses to five questions concerning: whether men should be more educated than women, whether men should eat before women, the ideal age of marriage for women, whether women should have children immediately after marriage, and the ideal number of children.

hold. Assuming this increase is only due to the 22% of them who stopped borrowing due to the treatment, this is an increase in consumption of more than 43,000 rupees per year for those households that stopped borrowing. This is much larger than the 11,900 rupees that a borrowing household repays in a year (Rs 9,600 in principal and Rs 2,300 in interest). This suggests that the indivisible item that these households were buying costs much more than the amount of the loan and they were paying for it by cutting consumption as well as by borrowing.

However, we do not see in the data the predicted corresponding decline in the acquisition of business assets among treatment households (which is why the control households were borrowing in the first place). One way to square this with the model is to think of the first period of the model as consisting of multiple years. At the beginning of this period the household takes the microfinance loan but also borrows from other sources, some of which are more expensive, to finance a single investment (say it builds a room for a shop). In each subsequent year (all part of the first period of our model) the household then both saves and takes additional microfinance loans to repay the expensive loans it is carrying until its obligations are discharged. When that is done they use the microfinance loan to pay for additional working capital for their business (this is what business owners in the second period of their lives do in our model). What happens in our model is that a certain fraction of households who were at the cusp of making this investment decide to not make the investment after all, due to the imposition of this new requirement. Therefore, they do not make the large financial commitment and do not buy the asset. This means that their consumption can be higher since they are not servicing these loans.

A similar story can be told about these households borrowing from a moneylender to finance an expensive wedding and then using microfinance money and savings to finance the moneylender loan over multiple years. The loss of microfinance makes people unwilling to have such expensive weddings, which is why they are richer now. This would also help explain why we see no impact on their business earnings from the fact that they gave up their loans, though it might also reflect borrowers paying down their loans before they build up enough working capital to make proper use of their assets.

VIII   Conclusion

SKS's attempt to bundle health insurance with microfinance was clearly a failure. The requirement to purchase insurance led to substantial drop-out from the organization, which led to abrupt changes in the program rules that further undermined the insurance product and led to its rapid demise.

However, this failure tells us something very interesting about the nature of microfinance:

the resistance that the proposed introduction of insurance generated suggests that many microfinance clients receive little surplus from their borrowing relationship. SKS certainly did not anticipate this. While they did worry that clients may not understand the value of what they are getting, at least initially, they were convinced that the benefits their clients derive from microfinance were sufficiently large that it would not lead to any significant loss of demand for their product. This is in fact while they took on the risk to introduce a new product.

One reason for this misperception might be the fact, suggested by casual observation and supported by more rigorous research, that what we have called "old" businesses do benefit substantially from the availability of microfinance. That these impacts do not necessarily imply a large gain in welfare would explain SKS's mistaken expectations, and indeed the mistaken expectations of many microfinance organizations worldwide and their supporters. Many remain convinced that microfinance has the potential to cause improvements in business outcomes, and *hence* in welfare. Our results show that the first part is right: as with other microfinance institutions that have been evaluated, SKS loans do contribute to better reported business outcomes. The second part is potentially wrong: many households that gain in terms of business outcomes behave as if they receive very little in terms of welfare. These results are discouraging to the view of microfinance businesses as an engine for clients to escape poverty and, more generally, to the many hopes pinned on microenterprises as a way for large numbers of people to improve their lives. There are other indicators that point in this same direction: the absence of impacts on household outcomes generally from microfinance, the failure of microfinance businesses to grow, and the frequent closure of these businesses. It appears that we must look elsewhere to find opportunities for supporting economic growth in developing contexts.

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Table 1. Baseline Client Characteristics, by Treatment and Control Villages

	All Villages	Treatment Villages	Control Villages	Difference: (2) - (3)	Number of Clients
	(1)	(2)	(3)	(4)	(5)
Panel A. SKS Client Businesses					
Expenditures on assets,	4568	4707	4410	347	2118
previous year	[22929]	[16951]	[28211]	(1095)	
Expenditures on workers,	1560	1700	1401	313	2112
previous year	[8118]	[9266]	[6579]	(382)	
Total sales,	36339	35386	37411	-1270	1968
previous year	[65433]	[56969]	[73823]	(3558)	
Total profits,	14558	13536	15761	-2404	1581
previous year	[23351]	[22683]	[24074]	(1587)	
Index of business outcomes	-0.005	-0.01	0.001	-0.007	2136
	[0.656]	[0.633]	[0.681]	(0.034)	
Panel B. SKS loan activity					
SKS loan,	0.893	0.895	0.892	-0.002	5366
at time of baseline survey	[0.309]	[0.307]	[0.31]	(0.024)	
SKS loan amount,	7619	7604	7635	-43	4794
at time of baseline survey	[2625]	[2634]	[2615]	(160)	

Notes: Column 1 reports average household characteristics from the baseline survey, with standard deviations reported in brackets. Columns 2 and 3 report average characteristics for households in randomly-assigned treatment villages and control villages, respectively. Column 4 reports the estimated difference between treatment and control households, controlling for the randomization stratification groups (SKS branch and above/below median number of clients within branch). Robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 2. Estimated Impacts of Treatment on Loan Renewal (Extensive Margin)

| | First Year After Treatment: | | At Time of Endline Survey: | |
| | Administrative Data | | Self-Reported | Other MFI Loan |
	(1)	(2)	(3)	(4)
Panel A. Loan Renewal, Full Sample				
Treatment	-0.220***	-0.162***	-0.076***	0.006
	(0.029)	(0.029)	(0.024)	(0.004)
Control Group Mean	0.724	0.541	0.717	0.011
Number of Clients	5353	5353	5219	5353
Panel B. Loan Renewal, Business Owners at Baseline				
Treatment	-0.255***	-0.220***	-0.054**	0.005
	(0.034)	(0.033)	(0.027)	(0.006)
Control Group Mean	0.778	0.596	0.722	0.014
Number of Clients	2149	2149	2102	2149
Panel C. Loan Renewal, Business Owners at Baseline, Business Started Before SKS Entry				
Treatment	-0.257***	-0.219***	-0.051*	-0.001
	(0.037)	(0.035)	(0.030)	(0.006)
Control Group Mean	0.787	0.609	0.721	0.016
Number of Clients	1715	1715	1676	1715
Panel D. Loan Renewal, Business Owners at Baseline and Endline, Business Started after SKS Entry				
Treatment	-0.217***	-0.187***	-0.057	0.026*
	(0.055)	(0.057)	(0.048)	(0.014)
Control Group Mean	0.731	0.527	0.717	0.006
Number of Clients	392	392	384	392
Panel E. Loan Renewal, Non Business Owners at Baseline				
Treatment	-0.201***	-0.133***	-0.091***	0.005
	(0.033)	(0.032)	(0.028)	(0.005)
Control Group Mean	0.687	0.510	0.713	0.009
Number of Clients	3071	3071	2984	3071

Notes: Column 1 reports the estimated impact of treatment (imposing the insurance requirement) on whether clients took out a new SKS loan in the first year after treatment (by the end of June 2008, for the SKS clients in our baseline and endline surveys who had an annual loan prior to June 2007). Column 2 reports the impact on whether baseline SKS clients had a loan at the time of the endline survey, continuing to use SKS administrative data, whereas Column 3 uses clients' self-reported loan data. Column 4 reports the impact on whether clients self-report having a microfinance loan from a non-SKS MFI, at the time of the endline survey. Panel B restricts the sample to business owners at baseline, panel C to restricts the sample to business owners at baseline whose businesses started before the entry of SKS in the business owner's village (using the date of the first loan as reported in the administrative data), and panel D restricts the sample to business owners at baseline whose business started after the entry of SKS. Panel E restricts the sample to non-business owners at baseline. All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 3. Estimated Impacts of Treatment on Loan Renewal (Extensive and Intensive Margins)

	First Year After Treatment:		At Time of Endline Survey:	
	Administrative Data		Self-Reported	Other MFI Loan
	(1)	(2)	(3)	(4)
Panel F. Loan Amount, Full Sample				
Treatment	-2075***	-1995***	-	72
	(340)	(438)		(45)
Control Group Mean	7485	7195		63
Number of Clients	5352	5353		5353
Panel G. Loan Amount, Business Owners at Baseline				
Treatment	-2713***	-2869***	-	110
	(418)	(516)		(75)
Control Group Mean	8505	8343		64
Number of Clients	2148	2149		2149
Panel H. Loan Amount, Business Owners at Baseline, Business Started Before SKS Entry				
Treatment	-2843***	-2724***	-	97
	(445)	(562)		(96)
Control Group Mean	8669	8505		78
Number of Clients	1714	1715		1715
Panel I. Loan Amount, Business Owners at Baseline, Business Started After SKS Entry				
Treatment	-1902***	-2868***	-	191*
	(664)	(837)		(113)
Control Group Mean	7725	7461		0
Number of Clients	392	392		392
Panel J. Loan Amount, Non Business Owners at Baseline				
Treatment	-1728***	-1552***	-	29
	(361)	(464)		(44)
Control Group Mean	6802	6513		65
Number of Clients	3071	3071		3071

Notes: The reported estimates correspond to those reported in Table 2, but report impacts on the amount of the loan (including zeros for non-renewal). All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 4. Estimated Impacts of Treatment on Insurance Usage

	Have Insurance (1)	Received Insurance Benefits (2)	Used Cashless Facility (3)	Received Reimbursement (4)	Expect To Receive Reimbursement (5)
Panel A: For Each Major Health Event (Self-Reported)					
Treatment	0.510***	0.0030***	0.0016***	0.0018***	0.045***
	(0.035)	(0.0006)	(0.0004)	(0.0005)	(0.012)
Control Group Mean	0.057	0.0003	0.0001	0.0002	0.012
Number of Events	25072	25072	25072	25072	25072
Panel B: For Each Client (Self-Reported)					
Treatment	0.683***	0.0106***	0.0055***	0.0062***	0.091***
	(0.0402)	(0.0019)	(0.0014)	(0.0016)	(0.0231)
Control Group Mean	0.0573	0.0003	0.0001	0.0002	0.0119
Number of Clients	6941	6941	6941	6941	6941
Panel C: For Each Client (Administrative Data)					
Treatment	0.842***	0.074***	0.026***	0.052***	
	(0.018)	(0.008)	(0.004)	(0.007)	
Control Group Mean	0	0	0	0	
Number of Clients	6941	6941	6941	6941	

Notes: For each "major health event" surveyed, panel A reports the estimated impact of treatment (imposing the insurance requirement) on clients' self-reported insurance usage for that event: whether clients self-report the affected person having insurance at the time of the event (Column 1); whether clients self-report having received any insurance benefits from that event, either through the affected person using a cashless facility or receiving reimbursement (Column 2); whether clients self-report the affected person used a cashless facility (Column 3) or they received reimbursement (Column 4); or whether clients self-report expecting to receive reimbursement from the insurance policy (Column 5). Panel B aggregates the "major health events" by client, indicating the impact of treatment on whether clients self-report any affected person in their household having: had insurance for any surveyed event (Column 1); received insurance benefits for any surveyed event (Column 2); used a cashless facility for any surveyed event (Column 3); received reimbursement for any surveyed event (Column 4); or expect to receive reimbursement for any surveyed event (Column 5). Panel C uses administrative claims data, merged to clients ever surveyed on a major health event, to report the impact of treatment on whether the client or persons covered under the client's insurance policy: were ever enrolled in insurance (Column 1); ever received insurance benefits (Column 2); ever used a cashless facility (Column 3); or ever received reimbursement (Column 4). All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 5. Estimated Impacts of Treatment on Major Health Events

	Overnight Hospitalization	Health Event Expenses	Borrowed from any MFI	Borrowed from family/friends	Borrowed from moneylender
	(1)	(2)	(3)	(4)	(5)
Treatment	0.014	-80	0.002	0.021	0.009
	(0.011)	(151)	(0.005)	(0.029)	(0.031)
Control Group Mean	0.067	1662	0.022	0.437	0.288
Number of Clients	25072	25072	25072	25072	25072

Notes: Each column reports the estimated impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the "major health events" survey. The outcome variables are: whether the affected person stayed overnight in a hospital for that health event (Column 1); total health expenses for that event, including lost income (Column 2); and whether these expenses were partly paid by borrowing from a microfinance organization (Column 3), family or friends (Column 4), or a moneylender (Column 5). All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 6. Estimated Impacts of Treatment on Health Outcomes at Endline

	Health Expenses	Borrowed for Health Expenses	Amount Borrowed	Number of Health Events	Overnight Hospitalization	ADL index	Self-Reported Health	Overall Life Satisfaction
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-18	0.009	-234	0.020	0.011	-0.010	-0.208***	-0.005
	(277)	(0.018)	(236)	(0.034)	(0.015)	(0.013)	(0.065)	(0.020)
Control Group Mean	2747	0.337	1462	1.691	0.271	0.004	6.865	3.556
Number of Clients	5353	5353	5353	5353	5353	5353	5353	5353

Notes: Each column reports the estimated impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the endline survey. The outcome variables are: expenses on health care for major events over the previous year (Column 1); whether the household reports having borrowed money to pay some of those health expenses (Column 2); the amount borrowed to pay health expenses (Column 3); the number of major health events experienced by the household over the previous year (Column 4); whether a household member was hospitalized overnight in the previous year (Column 5); an index reflecting adults' self-reported ability to perform 15 typical daily activities, averaged across adults in the household (Column 6); a self-reported index of health, averaged across adults in the household (Column 7); and a self-reported index of overall life satisfaction, averaged across adults in the household (Column 8). All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 7. Estimated Impacts of Treatment on Business Outcomes at Endline

| | Business Outcomes, Summing Over the Previous Year (in Rs.): | | | | | |
| | Owns Business | Spent on Assets | Spent on Workers | Total Sales | Total Profits | Index of (2) to (5) |
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full Sample						
Treatment	0.008	-278	-333**	-1359	-783	-0.038*
	(0.014)	(171)	(159)	(1258)	(701)	(0.020)
Control Group Mean	0.177	437	507	12142	6120	0
Number of Clients	5353	5353	5353	5353	5353	5353
Hochberg p-value						0.191
Panel B: Business Owners at Baseline						
Treatment	0.006	-308	-537*	-2554	-1470	-0.050*
	(0.025)	(192)	(311)	(2335)	(1269)	(0.028)
Control Group Mean	0.316	503	973	21681	11030	0
Number of Clients	2149	2149	2149	2149	2149	2149
Hochberg p-value						0.213
Panel C: Business Owners at Baseline, Business Started Before SKS Entry						
Treatment	-0.008	-408**	-658*	-3706	-1963	-0.063**
	(0.028)	(204)	(373)	(2777)	(1528)	(0.031)
Control Group Mean	0.335	532	1141	23599	11709	0
Number of Clients	1715	1715	1715	1715	1715	1715
Hochberg p-value						0.134
Panel D: Business Owners at Baseline, Business Started After SKS Entry						
Treatment	0.065	191	-66	1707	-282	0.018
	(0.041)	(399)	(197)	(3038)	(1721)	(0.060)
Control Group Mean	0.222	410	234	12860	7951	0
Number of Clients	392	392	392	392	392	392
Hochberg p-value						0.760
Panel E: Non Business Owners at Baseline						
Treatment	-0.003	-231	-187*	-1111	-677	-0.045*
	(0.012)	(249)	(102)	(892)	(435)	(0.024)
Control Group Mean	0.094	410	217	5788	2981	0
Number of Clients	3071	3071	3071	3071	3071	3071
Hochberg p-value						0.174

Notes: Each column reports the estimated impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the endline survey. Panel B limits the sample to business owners at baseline, panel C to restricts the sample to business owners at baseline whose businesses started before the entry of SKS in the business owner's village (using the date of the first loan as reported in the administrative data), panel D restricts the sample to business owners at baseline whose business started after the entry of SKS, and panel E restricts the sample to non-business owners at baseline. The outcome variables are: whether the household earns money from owning a business at endline (Column 1); total amount spent on business assets, over the previous year (Column 2); total amount spent on hiring labor for the business, over the previous year (Column 3); total sales over the previous year (Column 4); and a direct measure of self-reported total profits over the previous year (Column 5). When there is no business reported at endline (in Panels A and B), zero values are assigned for the outcome variables in Columns 2 to 5. In Column 6, the outcome variable is an index reflecting the equal-weighted average of the component variables in Columns 2 to 5 (each normalized to have a mean of zero and a standard deviation of one). All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. The Hochberg p-value reflects the statistical significance of the treatment effect on the index, adjusting for multiple hypothesis testing across the three summary outcomes (business, consumption, social impacts) in Tables 7, 8, and 10.

Table 8. Estimated Impacts of Treatment on Annual Consumption and Household Labor Income

| | Total | Consumption Catgories | | | | | Temptation Goods | Festivals and Celebrations | Other Labor Income |
| | | Durables | Non-Durables | Food | Health | Education | | | |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
Treatment	1957	-202	2159*	1667*	-13	-91	152**	-312	-344
	(1206)	(127)	(1170)	(851)	(46)	(102)	(73)	(561)	(1035)
Control Group Mean	50728	1175	49552	28969	1092	1241	1345	9694	28253
Number of Clients	5353	5353	5353	5353	5353	5353	5353	5353	5353
Hochberg p-value	0.212								
Panel B: Business Owners at Baseline									
Treatment	2458	-313	2771*	1340	-17	-101	170*	-33	-1138
	(1671)	(218)	(1614)	(1084)	(51)	(155)	(92)	(701)	(1523)
Control Group Mean	51624	1329	50295	29729	1093	1262	1321	9608	26386
Number of Clients	2149	2149	2149	2149	2149	2149	2149	2149	2149
Hochberg p-value	0.286								
Panel C: Business Owners at Baseline, Business Started Before SKS Entry									
Treatment	478	-555**	1033	-65	20	-59	160	67	-687
	(1650)	(241)	(1588)	(1007)	(54)	(121)	(100)	(775)	(1670)
Control Group Mean	51935	1369	50566	30033	1070	1188	1339	9381	25836
Number of Clients	1715	1715	1715	1715	1715	1715	1715	1715	1715
Hochberg p-value	0.772								
Panel D: Business Owners at Baseline, Business Started After SKS Entry									
Treatment	9516***	605	8911***	5718***	-52	-190	331*	-21	-3397
	(3071)	(463)	(3011)	(2085)	(110)	(623)	(169)	(1968)	(3664)
Control Group Mean	49822	1188	48634	28350	1130	1691	1089	10913	29463
Number of Clients	392	392	392	392	392	392	392	392	392
Hochberg p-value	0.007								
Panel E: Non Business Owners at Baseline									
Treatment	955	-220	1174	1594*	-30	-122	171*	-823	514
	(1341)	(155)	(1305)	(958)	(61)	(129)	(102)	(759)	(1251)
Control Group Mean	50473	1097	49376	28572	1091	1250	1349	9792	29734
Number of Clients	3071	3071	3071	3071	3071	3071	3071	3071	3071
Hochberg p-value	0.693								

Notes: Each column reports the estimated impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the endline survey. Panel B limits the sample to business owners at baseline, panel C to restricts the sample to business owners at baseline whose businesses started before the entry of SKS in the business owner's village (using the date of the first loan as reported in the administrative data), panel D restricts the sample to business owners at baseline whose business started after the entry of SKS, and panel E restricts the sample to non-business owners at baseline. The outcome variables reflect total household consumption over the previous year, where Columns 4 to 7 are monthly measures multiplied by 12. Other labor income in Column 9 sums earnings from salaried jobs and daily labor. All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. The Hochberg p-value reflects the statistical significance of the treatment effect on total consumption, adjusting for multiple hypothesis testing across the three summary outcomes (business, consumption, social impacts) in Tables 7, 8, and 10.

Table 9. Estimated Impacts of Treatment on Annual Consumption (per capita, adult equivalent) and Household Labor Income

	Total (1)	Durables (2)	Non-Durables (3)	Food (4)	Health (5)	Education (6)	Temptation Goods (7)	Festivals and Celebrations (8)	Other Labor Income (9)
Panel A: Full Sample									
Treatment	3	-58*	61	111	-14	-24	23	-152	-344
	(268)	(33)	(256)	(181)	(12)	(22)	(18)	(106)	(1035)
Control Group Mean	11365	261	11105	6508	249	258	299	2105	28253
Number of Clients	5353	5353	5353	5353	5353	5353	5353	5353	5353
Hochberg p-value	0.991								
Panel B: Business Owners at Baseline									
Treatment	177	-105	282	127	-14	-17	14	-113	-1138
	(338)	(68)	(317)	(217)	(12)	(36)	(21)	(130)	(1523)
Control Group Mean	11345	309	11037	6526	244	248	296	2062	26386
Number of Clients	2149	2149	2149	2149	2149	2149	2149	2149	2149
Hochberg p-value	0.601								
Panel C: Business Owners at Baseline, Business Started Before SKS Entry									
Treatment	-164	-159**	-5	-142	-5	-2	14	-65	-687
	(330)	(79)	(306)	(193)	(12)	(19)	(24)	(148)	(1670)
Control Group Mean	11383	327	11056	6573	239	228	297	2008	25836
Number of Clients	1715	1715	1715	1715	1715	1715	1715	1715	1715
Hochberg p-value	0.620								
Panel D: Business Owners at Baseline, Business Started After SKS Entry									
Treatment	1260**	102	1158*	854*	-33	-54	49	-290	-3397
	(590)	(85)	(587)	(448)	(23)	(160)	(38)	(334)	(3664)
Control Group Mean	11257	233	11024	6417	257	364	264	2385	29463
Number of Clients	392	392	392	392	392	392	392	392	392
Hochberg p-value	0.103								
Panel E: Non Business Owners at Baseline									
Treatment	-219	-48	-171	59	-19	-38	36	-253*	514
	(304)	(34)	(295)	(207)	(15)	(26)	(26)	(152)	(1251)
Control Group Mean	11395	233	11162	6488	252	268	297	2140	29734
Number of Clients	3071	3071	3071	3071	3071	3071	3071	3071	3071
Hochberg p-value	0.693								

Notes: Each column reports the estimated impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the endline survey.   Panel B limits the sample to business owners at baseline, panel C to restricts the sample to business owners at baseline whose businesses started before the entry of SKS in the business owner's village (using the date of the first loan as reported in the administrative data), panel D restricts the sample to business owners at baseline whose business started after the entry of SKS, and panel E restricts the sample to non-business owners at baseline. The outcome variables reflect per capita household consumption over the previous year, where Columns 4 to 7 are monthly measures multiplied by 12. Per capita consumption is calculated per adult equivalent, following the conversion to adult equivalents used by Townsend (1994) for rural Andhra Pradesh and Maharashtra (the weights are: 1.0 for adult males, 0.9 for adult females, 0.94 for males and 0.83 for females aged 13 to 18, 0.67 for all children aged 7 to 12, 0.52 for all children aged 4 to 6, 0.32 for all toddlers aged 1 to 3, and 0.05 for all infants). Other labor income in Column 9 sums earnings from salaried jobs and daily labor. All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. The Hochberg p-value reflects the statistical significance of the treatment effect on total consumption, adjusting for multiple hypothesis testing across the three summary outcomes (business, consumption, social impacts) in Tables 7, 9, and 10.

Table 10. Estimated Impacts of Treatment on Social Outcomes

	Share of children in school, aged 5-15		Share of teenagers in school, aged 16-20		Index of Women's Attitudes	Index of (1) to (5)
	Girls	Boys	Girls	Boys		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full Sample	0.009	-0.028*	0.002	-0.02	-0.075**	-0.021
Treatment	(0.019)	(0.015)	(0.020)	(0.024)	(0.037)	(0.030)
Control Group Mean	0.68	0.732	0.166	0.279	0.034	-0.042
Number of Clients	3001	3155	1677	1627	1444	4761
Hochberg p-value						0.986
Panel B: Business Owners at Baseline						
Treatment	-0.039	-0.002	0.001	-0.029	-0.088	-0.031
	(0.029)	(0.023)	(0.030)	(0.037)	(0.054)	(0.042)
Control Group Mean	0.705	0.715	0.168	0.312	0.026	-0.035
Number of Clients	1201	1222	657	630	561	1906
Hochberg p-value						0.601
Panel C: Business Owners at Baseline, Business Started Before SKS Entry						
Treatment	-0.040	-0.019	-0.009	-0.001	-0.081	-0.042
	(0.030)	(0.025)	(0.035)	(0.040)	(0.064)	(0.047)
Control Group Mean	0.700	0.720	0.174	0.308	0.022	-0.032
Number of Clients	963	972	505	507	444	1517
Hochberg p-value						0.620
Panel D: Business Owners at Baseline, Business Started After SKS Entry						
Treatment	-0.035	0.091*	0.064	-0.107	-0.095	0.052
	(0.064)	(0.055)	(0.062)	(0.099)	(0.108)	(0.086)
Control Group Mean	0.716	0.672	0.115	0.380	-0.014	-0.076
Number of Clients	216	226	134	107	101	349
Hochberg p-value						0.760
Panel E: Non Business Owners at Baseline						
Treatment	0.043*	-0.047**	-0.006	-0.025	-0.056	-0.015
	(0.022)	(0.019)	(0.026)	(0.028)	(0.048)	(0.039)
Control Group Mean	0.664	0.740	0.171	0.263	0.035	-0.047
Number of Clients	1719	1857	980	970	857	2739
Hochberg p-value						0.693

Notes: Each column reports the impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the endline survey. Panel B limits the sample to business owners at baseline, panel C to restricts the sample to business owners at baseline whose businesses started before the entry of SKS in the business owner's village (using the date of the first loan as reported in the administrative data), panel D restricts the sample to business owners at baseline whose business started after the entry of SKS, and panel E restricts the sample to non-business owners at baseline. In Columns 1 to 4, the outcome variables are the share of household children that are in school (by age and gender). In Column 5, the outcome variable is an index of adolescent girls' self-reported attitudes concerning: whether men should be more educated than women, whether men should eat before women, the ideal age of marriage for women, whether women should have children immediately after marriage, and the ideal number of children. The index in Column 5 reflects an equal-weighted average across responses to each question, after the responses are normalized to have a mean of zero, standard deviation of one, and the sign of the response oriented toward a more positive number reflecting more "progressive attitudes" (e.g., for the questions above: "No", "No", older ages, "No", fewer children). In Column 6, the outcome variable is an index reflecting the equal-weighted average of the component variables in Columns 1 to 5 (each normalized to have a mean of zero and a standard deviation of one). In Columns 1 and 2, the sample is restricted to households with children between the ages of 5 and 15. In Columns 3 and 4, the sample is restricted to households with children between the ages of 16 and 20. In Column 5, the sample is restricted to households with girls between the ages of 14 and 19. All regressions control for the randomization stratification groups (SKS branch and above/below median number of clients within branch), and robust standard errors clustered by village are reported in parentheses. *** denotes statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. The Hochberg p-value reflects the statistical significance of the treatment effect on the index in Column 6, adjusting for multiple hypothesis testing across the three summary outcomes (business, consumption, social impacts) in Tables 7, 8, and 10.