(Measured) Profit is Not Welfare: Evidence from an Experiment on Bundling Microcredit and Insurance

Abhijit Banerjee, Esther Duflo, and Richard Hornbeck*

August 2014

Abstract

We analyze a randomized trial in which microfinance loans were bundled with an unpopular (but cheap) health insurance policy. In randomly assigned treatment villages, purchase of the insurance policy was made mandatory at the time of loan renewal. This requirement led to a 22 percentage point (or 31%) decline in loan renewal in treatment villages, compared to control villages where the insurance policy was not introduced. The insurance policy itself turned out to be useless, partly due to administrative failures in implementation. Therefore, non-renewing clients' valuation of microfinance is approximated by the modest fee to purchase the insurance; in the presence of any expected gains, the fee represents an upper bound. Comparing client businesses in treatment and control villages, however, the decline in loan renewal had negative impacts that were both economically substantial and statistically significant. Clients' decision to incur these losses, rather than pay the modest insurance premium, implies the substantial financial gains from microfinance are mostly dissipated by unmeasured costs of operating the small businesses. This result potentially reconciles the seemingly large returns to capital for microenterprises with the lack of growth and frequent business closure.

*Abhijit Banerjee: Department of Economics, Massachusetts Institute of Technology, banerjee@mit.edu. Esther Duflo: Department of Economics, Massachusetts Institute of Technology, eduflo@mit.edu. Richard Hornbeck: Department of Economics, Harvard University, hornbeck@fas.harvard.edu. This project received funding from the National Institutes of Health (grant PO1 HD061315-04), and received IRB approval from MIT (#0609001924) and Harvard (#F18180-104). We thank Aparna Krishnan and Prathap Kasina for outstanding management of this difficult project, and Mahvish Shaukat for tireless research assistance. For comments and suggestions, we thank David Cutler, Jishnu Das, Pascaline Dupas, Andrew Foster, Rachel Glennerster, Jon Gruber, Dilip Mookherjee, and Ben Olken.

Microfinance has spread around the world, and for many years microfinance organizations have argued that customers' enthusiasm for these loans constituted clear evidence of their positive impact. This presumed improvement in welfare has been so large that microfinance institutions have come to be seen as a potential channel through which other financial products, such as health insurance, might be delivered to the world's poor (see, e.g., ILO, 2013). The hope is that clients sufficiently value maintaining access to loans that bundling microfinance with health insurance would create a large pool of non-selected clients, even where there is very little initial demand for health insurance.

Until recently, however, there was little direct evidence on the effect of microfinance loans on small businesses and households' lives. This has recently changed, with the completion of several randomized evaluations of the impact of microfinance loans in various settings and countries (Crepon et al., forthcoming; Augsburg et al., forthcoming; Tarozzi, Desai and Johnson, forthcoming; Angelucci, Karlan and Zinman, forthcoming; Attanasia et al., forthcoming; Banerjee et al., forthcoming). All of these papers find that the availability of microfinance leads to larger investments in small businesses. Some of these papers, though not all, find statistically significant and large impacts on the profits of businesses that existed before the introduction of microfinance (Banerjee et al., forthcoming; Crepon et al., forthcoming; Augsburg et al., forthcoming). At the same time, however, all of these evaluations find very little evidence of significant changes in the lives of households who gain access to microfinance. In particular, households' overall consumption and income (when measured) seem to be unaffected. Even when businesses expand following the introduction of microfinance, they do not necessarily survive for long.

Further, a striking finding from all of these studies is that the take-up of microfinance tends to be low – even when the population is restricted to eligible households in demographics targeted by microfinance organizations, even when the effect on businesses are large and positive, and even in settings where there appears to be essentially no alternative source of credit.[1] Loan renewal rates are often low as well. It

[1]For example, in Morocco, Crepon et al. (forthcoming) estimate a 99% return to capital from microfinance loans (in terms of business profits), and yet the average take-up in villages was only

seems that the early rhetoric from microfinance organizations has been turned on its head: the demand for microfinance is low even in settings where there are seemingly large returns from their small business loans.

This paper sheds light on this puzzle using an experiment in which microfinance clients in some areas of rural India became obliged to purchase a health insurance policy upon renewal of their microfinance loans. In rural Karnataka and Andhra Pradesh, one of India's leading microfinance organizations, SKS Microfinance, began requiring all new clients and renewing clients to purchase a health insurance policy, which provided coverage only for catastrophic events, hospitalization, and maternal care. At the beginning of this initiative, for two districts in Northern Karnataka, we coordinated with SKS to leave out randomly some villages from the health insurance expansion to enable the evaluation of this product. We collected data at baseline, endline, and regular intervals on a randomly selected sample of SKS clients in 101 treatment villages and 100 control villages.

The insurance product turned out to be extremely unpopular. There were anecdotal accounts of client complaints from the beginning. This was a surprise to SKS, as the organization had conducted intensive information campaigns beforehand. We estimate using administrative data that loan renewal rates declined by 22 percentage points (31 percent) in treatment villages compared to control villages. 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. 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 the purchase of the insurance policy was made voluntary and then later discontinued.

The observed willingness of clients to forego loans, however, allows us to bound their valuation of access to microfinance. Even if they assigned zero value to the insurance product (perhaps rightly), the policy was inexpensive (Rs. 525, compared

13.2%. In Hyderabad, the capital of Andhra Pradesh (India's hot spot for microfinance in the mid-2000s), 33% of eligible households had taken up a microfinance institution (MFI) loan in the sample tracked by Banerjee et al. (forthcoming) five years after several MFIs had started their operation in the city. Karlan and Zinman (2013) also find a large (above 1) price elasticity of the demand for microcredit.

to a renewal loan size of around Rs. 9,400) and could be rolled into the loan such that it represented a fairly small increase in the effective interest rate. To the extent that clients anticipated some benefit from insurance, the fee represents an upper bound on their valuation of microfinance. What seems clear and striking is that many clients were unwilling to pay even this small fee to continue to receive microfinance (and insurance). Further, most clients did not even return to SKS after the health insurance requirement was eliminated, consistent with little valuation of microfinance and moderate fixed costs of rejoining SKS and again changing their business practices.

Unsurprisingly, given failures in the implementation of the insurance scheme, we find little impact of the provision of health insurance on utilization of health care, health care spending, or the financing of health care. Somewhat puzzlingly, we find a large negative effect on self-reported health status, which could be consistent with the offer of health insurance causing people to become increasingly aware of unaddressed health problems (Zwane et al., 2011).

The dropout of SKS clients and the consequent loss of access to microfinance did substantially harm their businesses, however, consistent with the results from the evaluation of microfinance impacts in nearby Andhra Pradesh (Banerjee et al., forthcoming). There was little impact on whether people continued to own an enterprise, but SKS client businesses in treatment villages experienced large and significant declines in almost every measure we have: total sales, total profits, and the amounts spent on assets and workers. Decreased access to microfinance had few other impacts, such as impacts on household consumption. These results are strikingly consistent with previous research that finds impacts of microfinance on client businesses and little else.

Interpreting these results in combination, a large fraction of microfinance clients seem to place little value on the financial gains in their businesses that come as a result of access to microfinance. Clients report substantially lower profits when losing access to microfinance, but prefer to take those losses and avoid paying a small fee amortized over the loan period. The actual welfare gains to these microfinance borrowers must be lower than the small fee, after including all unmeasured costs (e.g., labor costs associated with higher entrepreneurial effort, stress from risk-taking, etc.).

For those clients that remain, the gains from microfinance are presumably higher.

The main contribution of this paper is therefore to provide a (very low) upper bound on the welfare gains from microfinance, as revealed by clients' decision-making, for a substantial group of borrowers. This conclusion obviously depends on the (standard) assumption that microfinance clients make borrowing decisions in their best interests. An advantage of our "reverse experiment," relative to studies of obtaining microfinance loans, is that current microfinance borrowers are presumably more aware of the impacts of microfinance. It is possible that the requirement to purchase health insurance created an irrational distaste for purchasing the bundle of services, but our estimates also come from after the health insurance requirement was lifted and therefore would require that irrationality to persist.

Such welfare bounds, with all their limitations, are extremely useful in assessing the claims of the microfinance movement regarding support and subsidies and in framing regulatory attitudes towards microfinance institutions.[2] To our knowledge, this is the first paper that attempts to estimate the welfare gains from microfinance, even though claims about welfare gains (and losses) abound in the public conversation on this subject.

From a less normative point of view, the conclusion that the net surplus from access to microcredit is small for many borrowers can help resolve the puzzle of why the apparent success of microfinance in raising profits does not translate into a greater demand for loans, and why access to microfinance does not lead to sustained growth among these microenterprises (many of which stop growing very early and/or shut down). In our simple story, financial gains for these clients are largely counterbalanced by unmeasured costs, such as the effort associated with running a business, but also possibly the added stress from the new financial obligations, working two jobs, or juggling family life with running an enterprise (Karlan and Zinman, 2010). One could object that this is simply a matter of requiring better measurement, though we draw on other evidence to show that accounting for the reduction in work hours

[2]The limitations are of course well understood, in particular from the literature on the value of a statistical life. Both of the problems we face here, that the compliers may not be representative of the average microfinance client and that the decision-maker may be irrational, also arise in that literature (Ashenfelter, 2006).

after leaving microfinance would not come close to making up for the loss in measured profits. Many of the unmeasured costs are probably impossible to measure, which makes the revealed preference approach we take here particularly useful.

I Bundling of Health Insurance and Microfinance

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.

SKS began requiring loan clients to purchase health insurance at the time of renewing their loan in 101 pilot villages, which we randomly selected out of 201 candidate villages with SKS presence in two districts of Northern Karnataka.[3] Around this time, in June 2007, SKS also rolled out the health insurance program in a non-experimental fashion across most of its areas of operation. 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. 9,400.[4] The insurance premium thus represented a 6 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 existing clients renewing 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

[3]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.

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

selection and outright fraud, led to a breakdown of relations between SKS and ICICI-Lombard, and insurance enrollment was 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. It 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. However, the implementation was poorly 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 almost no impact on the way SKS clients handled major health events or on their health status and expenditures.

II Randomization and Data Collection

SKS Microfinance originally identified 201 villages where it was currently running its microfinance program and was interested in piloting its health insurance program. In most of these villages, SKS had begun operations recently so most clients were in their first loan cycles when the insurance requirement was introduced. SKS operations were organized by center, with multiple centers in a village. To minimize the risk of spillovers between treatment and control, 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 ran-

domization was performed by the Principal Investigators using the Stata random number generator after stratification by branch and number of microfinance clients.[5] 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:

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 are available for download.[6] 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.[7] 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.

[5]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.

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

[7]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.

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.[8]

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.

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 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 col-

[8]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.

lected 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.[9] 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.[10]

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 how long 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, along with basic information about the provider, what treatment was received and at what cost, and the amount of lost income due to the inability to work of this person and family caregivers. For expenses incurred, the person was asked what sources these funds were provided through, such as savings, borrowing, or sale of assets. Information was collected on whether and how this person used insurance and other finances to pay for the

[9]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.

[10]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.

treatment expenses as well as the person's expectations for receiving reimbursement.

III Methodology

The empirical analysis compares client outcomes in treatment villages to client outcomes in control villages. 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):

$$Y_{ivs} = \beta T_v + \alpha_s + \epsilon_{ivs}. \tag{1}$$

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.

Due to administrative constraints, SKS decided to gradually roll out the health insurance requirement to villages between June 2007 and November 2007. As we did not randomly select village roll-out dates, we define a sample of clients who had loans prior to June 2007 and focus on intent-to-treat estimates of the impact on loan renewal by SKS clients in treatment villages after June 2007.

Among the possible outcomes of interest, 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 health care utilization and expenses using both the major health event survey and the endline survey. Whatever impact we find on the business outcomes is therefore unrelated to the provision of health insurance.

The rest of the paper then focuses on clients' business outcomes, which the existing literature on microfinance suggests are likely to be affected by the loss of microfinance. We define a sample of business owners in three possible ways: everyone who has a business at endline, everyone who has a business at baseline and endline, and everyone who has a business at baseline regardless of their status at endline. The first and second samples suffer from potentially endogenous selection into having a business, although we will show there is no impact of the treatment on having a business at endline. By focusing more narrowly on current business owners,

however, those samples give more precise results because, for reasons that appear to be unrelated to the treatment, people seem to go in and out of business over short periods of time.

The loss of access to microcredit might affect a range of other household outcomes as well. In looking at household outcomes, to avoid the potential for specification search, we simply follow the template for analysis that the randomized evaluations of microfinance adopted in the forthcoming Microfinance Issue of the *American Economic Journal: Applied Economics*. Following this template we classify the outcomes into categories of consumption and social effects.[11] 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 and an index of social effects (Hochberg, 1988).[12]

IV Results

IV.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 31%) 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. Clients' annual loans are repaid over 50 weeks, and we have included a six-week period for clients to renew their loan. The pilot's gradual roll-out implies that approximately 73% of clients in treatment villages would have faced the health insurance requirement at the time when their previous loan expired, so these intent-to-treat estimates might be scaled up by a factor of 1.37 to get a sense of the magnitude of the impact on those facing the requirement to buy insurance.[13]

[11] Unfortunately, we do not have data on labor supply, though in the interpretation section we draw on estimates from Banerjee et al. (forthcoming).

[12] 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.

[13] 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

Interestingly, this difference in loan renewal persisted after the health insurance requirement 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. Both our survey data and the administrative data may contain errors, though 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 similar estimates when restricting the sample to clients who report owning a business in the endline survey. For this sample, the impact on loan renewal is similar or slightly larger. Panels C and D report 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 of having a loan.

Some of the villages where the experiment took place had another microfinance organization, so part of the flight from SKS may have been compensated for by borrowing from another provider. Column 4 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 drop out are discussed in a previous paper (Banerjee, Duflo and Hornbeck, 2014). In general, we found that clients who drop

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.

out are similar to those who remain. In particular, we found no evidence of adverse selection 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.

IV.B Impacts on Health Status and Health Expenditures

For the surveyed "major health events," Table 3 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 an additional 4.5 percent 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 quite 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, or expecting reimbursement, but the magnitudes remain very small.

For this sample of clients who report a major health event, we can 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 some time.[14] In terms of these clients receiving insurance benefits at any time: 7.4% receive any benefit, of which 2.6% used a cashless facility and 5.2% received some reimbursement.

Given that insurance benefits were so rarely availed of, it is unsurprising that we

[14]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).

see no meaningful difference in how households responded to a major health event (Table 4). 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 (Table 5), there is also no meaningful impact on clients' health and their health care usage in the previous year. Specifically, we find no impact on health 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 is similar to 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).[15] 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.[16] Overall life satisfaction, however, is not substantially affected (column 8).

The health insurance product had no direct impact 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

[15]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.

[16]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.

understanding, etc.), the product turned out to be 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.

IV.C Impacts on Client Businesses

Table 6 reports the impact on clients' businesses resulting from the requirement to purchase health insurance. For the sample of clients who owned a business at baseline (Panel A), there was no substantial or statistically significant impact on whether they owned a business at endline (column 1). Note that only 32% of all self-reported business owners at baseline continue to own a business at endline, which reflects substantial churn in self-reported business ownership and operation, but this appears to be unrelated to the decision to leave the program due to insurance. 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.[17] All of the point estimates suggest they have invested less in their businesses and generated less profit, though only expenditures on 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 with a standard p-value, though not with Hochberg correction for multiple hypothesis testing across all three categories of household outcomes (business, consumption, and social outcomes).

More importantly, the estimates in Panel A are dampened considerably by the presence of a substantial number of zeros (68%), reflecting baseline businesses that are not reported at endline. As the probability of having a business is not affected by the treatment, we focus in Panel B on the baseline businesses that continue to exist at endline. The estimated impacts on business outcomes are now larger and statistically significant for all variables except assets. In Column 6, the treatment reduces the index of business outcomes and the estimate is statistically significant even

[17]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.

after adjusting for multiple hypothesis testing across the three families of household outcomes.

The probability of owning a business at endline is not affected by the treatment: on the full sample of 5358 clients, the estimated impact on the probability of owning a business is 0.007 with a standard error of 0.014. In Panel C, we expand the sample from Panel B to include all businesses that exist at endline, including new ones. The results are similar to those in Panel B. Businesses that are active at endline spend significantly less on worker salary (Rs. 1,879 or 67% of the control group mean), have lower sales (Rs. 9,492 or 14%), and have lower profits (Rs. 5,431 or 16%). Overall, the estimated decline in the business index implies that each business characteristic declined, on average, by 0.12 standard deviations in the group that was compelled to purchase insurance to maintain a loan from SKS. This estimate is highly significant, even after adjusting the p-value for multiple hypothesis testing across the three categories of outcomes. These results are robust to controlling for the corresponding outcome for these businesses at the baseline (when observed), and also to analyzing the log of business outcomes at endline instead of the level.[18]

Households do not appear to have compensated for declining business income by taking on additional work outside of the household. Table 6, column 7, reports impacts on wages from salaried jobs or day labor, summed across adults in the household. There is no indication of higher income from these outside activities and, if anything, some evidence for decreased labor income from outside activities.

IV.D Impacts on Other Household Outcomes

Recent randomized evaluations of microfinance have also found 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 non-results from our "reverse" experiment (the loss of microcredit).

Appendix Table 1 reports estimated impacts on household consumption at end-

[18]The estimates are also not sensitive to excluding outlier values in the business characteristics, dropping the top and bottom 1% of observations.

line.[19] We see little impact on per capita consumption, either total annualized consumption or within particular categories. There is a marginally significant decline in durable goods consumption (and insignificant increases in food and temptation goods), which aligns with what we know about the impacts of gaining access to microfinance (with the opposite sign).

Appendix Table 2 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,[20] an index across all outcomes in this category is statistically insignificant (particularly when adjusting for multiple hypothesis testing).

V Interpretation

When we compare the impact on business owners from giving up microfinance against the insurance premium they would have had to pay to keep their loan (Rs. 525), the numbers are striking. To calculate the implied loss for business owners, we assume there is no direct impact on business outcomes from the offer to purchase insurance. The results in Tables 2 and 6 (Panel C) then imply that losing access to microfinance reduces business owners' profits, on average, by Rs. 20,808 (5431/0.261) or 62% of the control group mean. This means that the ratio of the decline in business profit over the initial reduction in credit was 2.04,[21] which is high but not out of range compared to previous estimates of the impact of microfinance borrowing on business profits. Crepon et al. (forthcoming) report that actually borrowing from a MFI

[19]Following the practice in recent papers on microfinance, the outcomes 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.

[20]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.

[21]These numbers are potentially an overstatement, as those who do not drop out might also experience a decline in business profit if they consider the insurance premium an additional business expense. This loss is no more than Rs. 525, however, and adjusting for this loss implies decreased profits from losing microfinance of Rs. 19,394 ((5431 - 525*0.74)/0.26) or 57% of the control mean. The ratio of decline in profit over the decline in funds borrowed would then be 1.89.

increases self-employment profit by 130% of the control mean, and that the ratio of increase in profit over amount borrowed is 1.44, while the estimates of Banerjee et al. (forthcoming) would imply even larger impacts on those who borrow, though the authors do not report IV estimates due to potential spillover effects on non-borrowers.

Thus, to avoid paying a Rs. 525 fee, a quarter of business owners preferred to accept a loss in profit of approximately Rs. 20,000. While the financial impact of microfinance is known to be heterogeneous across borrowers, this financial loss reflects average losses for the large group of "compliers" who decided to drop out rather than pay the insurance premium. Some compliers may have had smaller financial losses than the insurance premium, though on average the financial losses were much greater.

The simplest and most natural explanation is that the estimated financial impacts of microfinance do not reflect real economic gains to those business owners due to unmeasured costs of business operation. The opportunity cost of their own labor is just one of the often unmeasured disadvantages, as is the cost of worrying about risk and other problems of keeping a business going. As such, the measured gains in "profits" may well be illusory. Note that we asked businesses owners about profits directly, rather than calculating the difference between reported revenues and reported costs (although these answers are consistent with the general scaling back of the business we observe by looking at revenues and cost), so "unmeasured costs" in our context refers to "unconsidered costs" from the business owner's perspective, not basic measurement error by the survey team. Furthermore, this wedge is unlikely to be entirely accounted for by the failure of the owners to take into account the financial value of their own time, although that is clearly part of it. First, we do not find that when businesses shrink the households substitute by working harder outside their own enterprise, as measured by an increase in business-owning households' earnings from salaried or day labor (Table 6, column 7). If anything, we see the opposite effect. Second, although our data do not include time spent on the business, a back-of-the-envelope calculation based on estimates from recent microfinance papers suggests that even if the owners had in fact completely ignored the value of their own

time when they answered the profit question, accounting for it would not erase the substantial decline in profits.[22] In other words, there is no obvious "fix" that would make the profits data immediately consistent with the revealed preference number.

So, given how difficult it is to directly measure welfare (both because of measurement issues and because we do not observe people's utility functions), we find it appealing to use clients' choices to recover the (surprisingly limited) welfare losses from giving up their access to microfinance.

This interpretation has the additional advantage of being consistent with a number of supplemental features of the data and general context. First, households appear to have been generally unaffected along other dimensions, such as their consumption level or overall life satisfaction, consistent with the financial losses being counterbalanced elsewhere. Indeed, for business owners only, there is no impact on consumption or overall life satisfaction.[23] Second, this interpretation is consistent with the frequent closure of microfinance-funded businesses and the absence of sustained growth of microfinance-funded businesses. Third, SKS clients' failure to rejoin the organization after the health insurance requirement was eliminated is consistent with clients' low valuation of microfinance (combined with some cost associated with re-joining and changing business practices).

[22]Drawing on the papers in the forthcoming Microfinance Issue of the *American Economic Journal: Applied Economics*, and assuming the same treatment effect on the treated in our setting, the median estimated impact of microfinance borrowing on labor supply implies that households in our setting would save approximately 72 hours per year. Valued at an average daily wage of Rs. 70 for casual labor in rural Andra Pradesh (National Sample Survey Office, 2010), and assuming eight hours of labor per day, the saved hours are worth approximately Rs. 630 to households. These numbers are approximate, but the shadow labor costs are much smaller than the observed Rs. 5,400 impact on profit, . The assumed hourly wage is also fairly generous for our setting: among adults in our sample who only report income from outside labor activities, nine rupees per hour is at the 67th centile of male and the 90th centile of female hourly earnings (dividing annual reported income by 2000 hours).

[23]Moreover, there is some indication that clients become stressed by changes in access to microfinance (in any direction): we estimate at endline that clients in treatment villages are more likely to have been worried for a prolonged period in the previous 12 months (coefficient of 0.057, standard error of 0.019, control mean of 0.402), using the same survey question as Karlan and Zinman (2010), who find that clients become more worried when they receive access to microfinance. Transitions, in either direction, may be associated with this short-term stress and might as well be avoided if clients are largely indifferent in the end.

VI 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.

One reason for this misperception might be the fact, suggested by casual observation and supported by more rigorous research, that 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 large increases in business profits, 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 business outcomes. The second part is wrong: many households that gain in terms of profits receive very little in terms of welfare.

These results are discouraging to the view of microfinanced businesses as an engine for the average person 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. Of course, there are other indicators that point in the same direction as our results: the absence of impacts on household outcomes generally from microfinance, the failure of microfinanced businesses to grow, and the frequent closure of these businesses. If we take seriously the decisions of these borrowers, then we must look elsewhere to find growth opportunities they themselves consider worthwhile.

References

Angelucci, Manuela, Dean Karlan, and Jonathan Zinman. forthcoming. "Microcredit Impacts: Evidence from a Randomized Microcredit Program Placement Experiment by Compartamos Banco." *American Economic Journal: Applied Economics*.

Ashenfelter, Orley. 2006. "Measuring the Value of a Statistical Life: Problems and Prospects." *The Economic Journal*, 116: C10–C23.

Attanasia, Orazio, Britta Augsburg, Ralph De Haas, Emla Fitzsimons, and Keike Harmgart. forthcoming. "Group Lending or Individual Lending? Evidence from a Randomized Field Experiment in Rural Mongolia." *American Economic Journal: Applied Economics*.

Augsburg, Britta, Ralph De Haas, Heike Harmgart, and Costas Meghir. forthcoming. "Microfinance and Poverty Alleviation." *American Economic Journal: Applied Economics*.

Banerjee, Abhijit, Esther Duflo, and Richard Hornbeck. 2014. "Bundling Health Insurance and Microfinance in India: There Cannot be Adverse Selection if There Is No Demand." *American Economic Review: Papers and Proceedings*, 104(5): 291–297.

Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan. forthcoming. "The miracle of microfinance? Evidence from a randomized evaluation." *American Economic Journal: Applied Economics*.

Crepon, Bruno, Florencia Devoto, Esther Duflo, and William Pariente. forthcoming. "Estimating the impact of microcredit on those who take it up: Evidence from a randomized experiment in Morocco." *American Economic Journal: Applied Economics*.

Dow, William H., Paul Gertler, Robert F. Schoeni, John Strauss, and Duncan Thomas. 1997. "Health care prices, health and labor outcomes: Experimental evidence." *RAND Working Paper Series 97-01.*

Hochberg, Y. 1988. "A sharper Bonferroni procedure for multiple tests of significance." *Biometrika*, 75(4): 800–802.

ILO. 2013. "MicroInsurance Innovation Facility."

Karlan, Dean, and Jonathan Zinman. 2010. "Expanding Microenterprise Credit Access: Using Randomized Supply Decisions to Estimate the Impacts in Manila." *mimeo.*

Karlan, Dean, and Jonathan Zinman. 2013. "Long-Run Price Elasticities of Demand for Credit: Evidence from a Countrywide Field Experiment in Mexico." *Yale Economics Department Working Paper No. 115.*

Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. "Experimental Analysis of Neighborhood Effects." *Econometrica*, 75(1): 83–119.

National Sample Survey Office. 2010. "Employment and Unemployment Situation in India 2007-08." *NSS 64th Round (July 2007 – July 2008)*, page 162.

Tarozzi, Alessandro, Jaikishan Desai, and Kristin Johnson. forthcoming. "On the Impact of Microcredit: Evidence from a Randomized Intervention in Rural Ethiopia." *American Economic Journal: Applied Economics.*

Thornton, Rebecca L., Laurel E. Hatt, Erica M. Field, Mursaleena Islam, Freddy Solis Diaz, and Martha Azucena Gonzalez. 2010. "Social Security Health Insurance for the Informal Sector in Nicaragua: A Randomized Evaluation." *Health Economics*, 19: 181–206.

Townsend, Robert M. 1994. "Risk and Insurance in Village India." *Econometrica*, 62(3): 539–591.

Zwane, Alix Peterson, Jonathan Zinman, Eric Van Dusen, William Pariente, Clair Null, Edward Miguel, Michael Kremer, Dean S. Karlan, Richard Hornbeck, Xavier Gine, Esther Duflo, Florencia Devoto, Bruno Crepon, and Abhijit Banerjee. 2011. "Being surveyed can change later behavior and related parameter estimates." *Proceedings of the National Academy of Arts and Sciences*, 108(5): 1821–1826.

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

	All Villages (1)	Treatment Villages (2)	Control Villages (3)	Difference: (2) - (3) (4)	Number of Clients (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	-0.010	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]	[0.310]	(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

	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	-0.221***	-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	5366	5366	5232	5359
Panel B. Loan Renewal, Endline Business Owners				
Treatment	-0.261***	-0.214***	-0.102***	0.008
	(0.036)	(0.041)	(0.031)	(0.011)
Control Group Mean	0.795	0.644	0.813	0.025
Number of Clients	993	993	974	993
Panel C. Loan Amount, Full Sample				
Treatment	-2083***	-2003***		72
	(339)	(439)		(45)
Control Group Mean	7490	7202		63
Number of Clients	5365	5366		5359
Panel D. Loan Amount, Endline Business Owners				
Treatment	-2657***	-3109***		274*
	(465)	(674)		(164)
Control Group Mean	8706	9275		135
Number of Clients	993	993		993

Notes: Column 1 reports the estimated impact of treatment (imposing the insurance requirement) on whether clients took out a new SKS loan 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 endline, and Panels C and D report impacts on the amount of the loan (with non-renewed loans set equal to zero). 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 Insurance Usage

	Have Insurance	Received Insurance Benefits	Used Cashless Facility	Received Reimbursement	Expect To Receive Reimbursement
	(1)	(2)	(3)	(4)	(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***
	(0.040)	(0.0019)	(0.0014)	(0.0016)	(0.023)
Control Group Mean	0.057	0.0003	0.0001	0.0002	0.012
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 4. 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 5. 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	0.6	0.009	-228	0.022	0.011	-0.010	-0.209***	-0.005
	(278)	(0.018)	(237)	(0.034)	(0.015)	(0.013)	(0.065)	(0.020)
Control Group Mean	2727	0.337	1454	1.691	0.271	0.004	6.867	3.556
Number of Clients	5340	5340	5340	5340	5340	5340	5340	5340

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 6. Estimated Impacts of Treatment on Business Outcomes at Endline

		Business Outcomes, Sum Over the Previous Year (in Rs.):					
	Owns Business	Spent on Assets	Spent on Workers	Total Sales	Total Profits	Index of (2) to (5)	Other Labor Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Business owners at baseline							
Treatment	0.006	-308	-534*	-2529	-1449	-0.050*	-1068
	(0.025)	(192)	(311)	(2337)	(1271)	(0.028)	(901)
Control Group Mean	0.316	503	973	21681	11030	0	5401
Number of Clients	2150	2150	2150	2150	2150	2150	2149
Hochberg p-value						0.220	
Panel B: Business owners at baseline and endline							
Treatment		-950	-1953*	-11318**	-6613*	-0.126**	-3258
		(600)	(1033)	(5708)	(3533)	(0.049)	(2372)
Control Group Mean		1594	3080	68644	34922	0	17102
Number of Clients		683	683	683	683	683	682
Hochberg p-value						0.033	
Panel C: Business owners at endline							
Treatment		-1568	-1879**	-9492**	-5431**	-0.119***	-3416*
		(953)	(782)	(4439)	(2692)	(0.041)	(1806)
Control Group Mean		2408	2796	66897	33718	0	17381
Number of Clients		993	993	993	993	993	991
Hochberg p-value						0.012	

Notes: Each column reports the estimated impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the endline survey. Panel A limits the sample to business owners at baseline, Panel B limits the sample to business owners at baseline and endline, and Panel C limits the sample to business owners at endline only. 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 Panel A only), 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). Column 7 reports impacts on earnings from salaried jobs and daily labor, summed across adults in the household. 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 Table 6 and Appendix Tables 1 and 2.

Appendix Table 1. Estimated Impacts of Treatment on Annual Consumption (per capita, adult equivalent)

	Total	Durables	Non-Durables	Food	Health	Education	Temptation Goods	Festivals and Celebrations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-31	-59*	28	77	-11	-25	17	-123
	(225)	(31)	(216)	(157)	(10)	(21)	(16)	(91)
Control Group Mean	10406	237	10170	5967	228	240	276	1903
Number of Clients	5340	5340	5340	5340	5340	5340	5340	5340
Hochberg p-value	0.890							

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 reflect per capita household consumption over the previous year, where columns 4 to 7 are monthly measures multiplied by 12. Column 3 is calculated to be the difference between columns 1 and 2. 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 toddlers aged 1 to 3, and 0.05 for all infants. 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 Table 6 and Appendix Tables 1 and 2.

Appendix Table 2. 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 Progressive Attitudes	Index of (1) to (5)
	Girls	Boys	Girls	Boys		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.009	-0.028*	0.002	-0.020	-0.073**	-0.020
	(0.019)	(0.015)	(0.020)	(0.024)	(0.037)	(0.030)
Control Group Mean	0.680	0.732	0.166	0.279	0.034	0.042
Number of Clients	3006	3159	1678	1628	1446	4767
Hochberg p-value						>0.999

Notes: Each column reports the impact of treatment (imposing the insurance requirement) on the indicated outcome variable from the endline survey. 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 Table 6 and Appendix Tables 1 and 2.