Profit is Not Welfare:
Evidence from an Experiment on Bundling Credit and Insurance

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Abstract

This paper investigates a puzzle posed by the recent literature on the impact of microcredit loans: even in settings where there appear to be large marginal returns for business profits from microcredit loans, loan take-up is low, microfinance businesses do not expand, and these businesses often shut down. 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 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 economically substantial and statistically significant negative impacts on client businesses. 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 microfinance business operation. This result potentially reconciles the seemingly large returns to capital for micro enterprises with the lack of growth and frequent business closure, even in contexts where microfinance is available.

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Microfinance has spread around the world and, for many years, the rhetoric of microfinance organizations has been 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 microfinance 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. Several of these papers, though not all, find statistically significant and very 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. In addition, even when businesses expand following the introduction of microfinance, these businesses shut down frequently.

Further, a striking finding from all of these studies is that the take-up of microfinance tends to be low. Loan take-up is low even when the population is restricted to eligible households in demographics targeted by microfinance organizations, and even in settings where there appears to be essentially no alternative source of credit.[1] The early rhetoric from microfinance organizations has been seemingly turned on its head: the demand for microfinance is low even in settings where there appear to be large returns from their small business loans.

To shed some light on this puzzle, this paper uses an experiment in which microfinance clients in some areas of rural India became obliged to purchase a health insurance policy

[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 the village was only 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 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.

upon renewal of their microfinance loan. In rural Karnataka and Andhra Pradesh, 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 randomly leave out 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, to SKS's surprise and despite active information campaigns. There were immediate anecdotal accounts of client complaints, and we estimate using administrative data that loan renewal rates declined by 22 percentage points (31 percent) in treatment villages compared to control villages. Clients' self-reported data suggests that few of those who left SKS obtained microfinance loans from other organizations. The insurance scheme was never properly implemented and, as the relationship soured between SKS and the third party insurer (ICICI-Lombard), purchase of the insurance policy was made voluntary and then discontinued.

Clients' observed willingness to give up microfinance, however, allows us to bound their valuation of access to microfinance. Even if clients (rightly) assigned zero value to the insurance product, the policy was inexpensive (Rs. 525) 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. Many clients were unwilling to pay this small fee to receive both microfinance and insurance. Further, most clients did not even rejoin SKS after the health insurance requirement was eliminated, consistent with little valuation of microfinance and moderate fixed costs of re-joining 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 people becoming increasingly aware of unaddressed health problems.

The dropout of SKS clients did substantially harm their businesses, however, consistent with a real loss in access to microfinance and 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 a business, but SKS client businesses in treatment villages experienced declines in: amount spent on assets, amount spent on workers, total sales, and total profits. Pooling across these outcomes to create an index of business outcomes,

the health insurance requirement led to a substantial and statistically significant decline in client businesses. Decreased access to microfinance had few other impacts, such as impacts on household consumption or income. These results are 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 avoid paying a small fee amortized over the loan period. The actual welfare gains to microfinance borrowers must be lower than the small fee, after including all unmeasured costs (e.g., labor costs associated with higher entrepreneurial effort). For those clients that remain, the gains from microfinance are presumably higher.

While measurement of profits, income, and consumption are notoriously difficult, our results provide a bound on the welfare gains from microfinance as revealed by clients' decision-making. This conclusion depends on the assumption that microfinance clients make borrowing decisions in their best interests. It is possible that the requirement to purchase health insurance created an irrational distaste for purchasing the bundle of services, though we present estimates from after the health insurance requirement was lifted.

Our results may help resolve the puzzle of why microfinance is associated with a large proportional increase in small business profit in some settings, and yet, even in those settings, loan takeup is often low and the businesses do not expand further. Our results are consistent with the simplest explanation, in which financial gains are largely counterbalanced by unmeasured costs, such as the opportunity cost of clients' increased labor. This explanation is consistent with the absence of detectable impacts of microfinance on total income or consumption, the frequent closure of small businesses, and the lack of sustained economic progress from the spread of microfinance.

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 insurance enrollment and the initial processing of claims.

In June 2007, in 101 pilot villages randomly selected out of 201 candidates villages with SKS presence in two districts of Northern Karnataka,[2] SKS began requiring loan clients to

[2]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.

purchase health insurance at the time of renewing their loan.[3] 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 comparison, the average loan amount was Rs. 8300. 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. Amidst clients' rebellion, it was decided that purchasing insurance would only be mandatory for new clients or at the time of existing clients' loan renewal. Still, discontent with the policy and resulting client drop-out led SKS to make the insurance voluntary 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 discontinued in March 2009. Thus, by the time of our endline survey, clients had become free to re-join SKS without purchasing the insurance policy.

And as it turned out, SKS clients were correct in not wanting to purchase this particular health insurance policy. The insurance 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. Implementation of the insurance was mismanaged by the partnership of SKS and ICICI-Lombard, however. Reimbursements were difficult for clients to file, and often went unprocessed, such that their program efforts shifted to focus on upfront cashless treatment. Few hospitals were networked for cashless treatment, and many SKS clients did not receive insurance cards for cashless treatment, such that this 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 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

[3]Around this time, the health insurance program was rolled out in a non-experimental fashion to most of SKS's areas of operation.

would receive the same treatment/control status.

In December 2006, using SKS's list of villages, J-PAL randomly selected 101 villages for SKS to pilot the health insurance product. The remaining 100 villages form 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.[4] 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 village 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 on 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.[5] 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.[6] 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 such health event, the survey records basic information the type of event, how 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. Pooling

[4]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.

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

[6]Surveyors visted households multiple times to interview each adult (over the age of 14), though in some cases all adults were not found who were reported to be in the household.

across these four main business characteristics, we follow Kling, Liebman and Katz (2007) in analyzing an index of outcomes.[7]

Second, we collected similar survey data at endline, which came after insurance enrollment had been discontinued and clients had the opportunity to re-join 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 in treatment villages.

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. This includes when clients took out past loans and the loan amounts received. This gives us detailed information on clients' previous loan activities, as well as the ability to calculate the effect on loan renewal from the requirement to purchase health insurance. By merging this loan renewal data with our baseline sample, we previously showed there was no adverse selection in client sign-up, whereby healthier households might have disproportionately not renewed their loan in treatment areas compared to control villages (Banerjee, Duflo and Hornbeck, 2014). Second, SKS maintained a database of who was enrolled in insurance and all requested and processed insurance claims. This database provides information on who used a cashless facility and who received reimbursement for health expenses at other facilities.

Fourth, 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.[8] 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 parts.

In the first part, a survey monitor accompanied a SKS loan officer to multiple meetings and asked the clients about any major health events in their household. At the center

[7]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.

[8]We experimented with several definitions, but found this working definition to be most successful in the field at identifying the major health events that we were most interested in and might be under-represented in the baseline and endline surveys.

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.[9]

In the second part, the full survey was conducted with the SKS client and, generally, in the presence of the person affected by the health event. The full survey begins with verification of the information collected at the microfinance center meeting, and includes a brief description of the event, when it began, and the timing of treatment received. The person categorizes the seriousness of the event, along with how long it caused an inability to perform normal daily activities. The person also provides a list of symptoms, which allows us to further characterize the seriousness of the problem.[10] The surveyor then collects information on all health providers the person visited, along with basic information about the provider, what treatment was received and at what cost, and lost income due to the inability to work of this person and family caregivers. For expenses incurred, the person is asked through what sources these funds were provided, such as savings, borrowing, or sale of assets. Information is collected on whether and how this person used insurance to pay for the treatment expenses and 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 from relative impacts on SKS client loan renewal in treatment villages after June 2007.

Among the possible outcomes of interest, we begin by considering impacts on clients' SKS

[9] We began asking about all major health events since January 2008, and in July we switched to asking about all major health events in the last 30 days to improve recall ability of clients and to allow us to visit villages more frequently.

[10] As the person often does not know what caused the particular problem, this information could potentially be used to create a best-guess diagnosis.

loan takeup decisions using administrative data from SKS. Given the troubled implementation of the health insurance program, we then verify the absence of impacts on health care utilization and expenses using both the major health event survey and the endline survey.

We then focus on clients' business outcomes, which are potentially impacted by the loss of microfinance and are a particular focus of most of the microfinance literature. 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 measure 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 there is a great deal of rotation in business operation that appears to be unrelated to the treatment.

Households' lost access to microfinance might, in priciple, affect a range of other household outcomes. Indeed, we observe a decrease in microfinance participation that is larger than the increase in microfinance in most randomized evaluations of microfinance. Drawing on the previous studies of microfinance, we consider the same outcomes as those previous studies when the data are available from our survey. To avoid the potential for specification searching, we simply follow the template for analysis that all randomized evaluations of microfinance have adopted in the forthcoming issue of the *American Economic Journal: Applied Economics*. We consider particular families of outcomes related to income, consumption, and social effects.[11] To avoid misleading inference due to multiple inference, we again follow these papers in computing an index of outcomes for each family and regress the 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 the income index, total consumption, and the social effect index (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.[13] The

[11] Unfortunately, we do not have data on labor supply.

[12] Effectively, this correction multiplies the business outcome index p-value by a factor of 4, given that its p-value is the lowest among the four outcome indices.

[13] 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 6-week period for clients to renew their loan.

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.[14]

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 a SKS loan. Based on administrative data, clients in treatment village were 16 percentage points (29%) 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 a SKS loan when these do not appear in the administrative data.[15] 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 by borrowing from another MFI. 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%).

We have previously discussed the characteristics of those clients who drop out (Banerjee, Duflo and Hornbeck, 2014). In general, we found clients dropping out to be similar to those who remain. In particular, we found no evidence of adverse selection based on health characterisics and even those 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.

[14]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 to not face the health insurance requirement.

[15]Both our survey and the administrative data may contain data 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.

IV.B Impacts on Health Status and Health Expenditures

For the surveyed "major health events," Table 3 reports indications of insurance usage. At the time of the health event, people in treatment villages were 51 percentage points more likely to report having health insurance (Panel A, column 1). In terms of the probability of receiving insurance benefits, however, this is only greater by 0.3 percentage points in treatment villages (column 2). This number includes both use of a cashless facility or receiving reimbursement, each of which show very small increases (columns 3 and 4). The major health event survey was conducted relatively close to the event, however, and an additional 4.5 percent of responders indicate they expect to receive reimbursement (column 5). While these estimates are statistically significant, due to near-zero insurance usage in control villages, the usage of insurance is also near zero in treatment villages.

Grouping the health event survey data by client (panel B), people in treatment villages were 69 percentage points more likely to ever report having insurance for a major health event. Clients were more likely to ever receive insurance benefits, or ever report expecting reimbursement, though the magnitudes remain very small.

For this sample of clients surveyed on a major health event, we can use administrative claims data to see whether they are ever appear to have used insurance. Column 1 reports that 85% appear in administrative data as being enrolled in the insurance program at some time.[16] In terms of these clients receiving insurance benefits at any time: 7.3% receive any benefit, 2.6% used a cashless facility, and 5.1% received some reimbursement.

Given that insurance usage was so rare, it is unsurprising that we see no meaningful difference in how households responded to a major health event (Table 4). Following one of these events, there is no substantial impact on whether the person stayed overnight in a hospital, the total cost of health care (including lost income), and the financing of associated costs.

In the endline survey (Table 5), there is also no meaningful impact on clients health and health care usage in the previous year. There is little 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). There is also no impact on the ability of individuals to perform basic activities in daily life (column 6).[17] Curiously,

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

[17] We ask each adult about their difficulty in performing 15 daily activities, 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 indices.

households report 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.[18] Overall life satisfaction, however, is not substantially affected (column 7).

The health insurance product had no direct impact on the benchmarks 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 useless, and anecdotal evidence suggests that clients understood this fairly quickly. The requirement to purchase insurance did inadvertently lead to a significant decline in the availability of microfinance loans, however, and the following sections explore how this impacted households.

IV.C Impacts on Client Businesses

Table 6 reports the impact on clients' businesses 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 31.8% 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. 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. 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.[19] This estimate would not be statistically significant, however, under the Hochberg correction for multiple hypothesis testing across all four families of household outcomes (business, income, consumption, and social outcomes).[20] Business outcomes are the primary channel impacted across all microfinance studies, however, so it is overly conservative to treat the inference as

[18] 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.

[19] 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.

[20] Following the microcredit papers in the special issue of the *American Economic Journal: Applied Economics*, we correct p-values of the indices for multiple inference using the Hochberg correction, treating our problem as one of multiple inference across the indices in each of the summary tables (business, income, consumption, and social outcomes).

if business outcomes were simply one of many potential sets of outcomes impacted.

The estimates in panel A are dampened substantially by the presence of a substantial number of zeros, 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, no longer dampened by 68% of the observations having zeros, and are generally statistically significant. In Column 6, the treatment reduces the index of business outcomes and the estimate is statistically significant even after adjusting for multiple hypothesis testing across the four families of household outcomes.

In Panel C, we expand the sample from Panel B to include all businesses that exist at endline.[21] 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 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 four types of outcomes. These results are robust to controlling for the businesses' outcome at baseline (when observed), and to estimating impacts on the log of business outcomes at endline.[22]

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, income, and social effects. We analyze data for these other outcomes, following the same template as those papers, and find similar non-results from our "reverse" experiment.

Appendix Table 1 reports estimated impacts on household consumption at endline.[23] 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 is consistent with the generally opposite impacts of gaining access to microfinance.

Appendix Table 2 reports no detectable impact on wage income or self-employed business

[21] Note that 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.

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

[23] 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.

profits. Average self-employed business income is not impacted when pooling across all households, similar to all studies of microfinance, because only 19% of households own a business at endline and this figure is dominated by zeros.

Appendix Table 3 shows no systematic impacts on social outcomes, consistent with findings across other studies of microfinance. There is no impact on the share of younger children in school or the share of older children in school. There is some impact on adolescent girls' reported "progressive" attitudes,[24] showing more traditional responses with the loss of microfinance, though an index across all outcomes is statistically insignificant (particularly when adjusting for multiple hypothesis testing).

Across all three tables, the outcome indices show no substantial or statistically significant differences in treatment villages compared to control villages. Adjusting for multiple hypothesis testing across these families of outcomes, there is particularly no statistically significant impact on these other three types of outcomes (consumption, income, social outcomes).

V Interpretation

The striking comparison is how much business owners were impacted by the loss of microfinance, compared to the small insurance premium required to maintain their loan (Rs. 525). 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 5 then imply that losing access to microfinance reduces business owners' profits, on average, by Rs. 20,888 (5431/0.26) or 62% of the control group mean. Put differently, the ratio of the decline in business profit over the decrease in funds borrowed is 2.04.[25] These numbers may appear extremely large at first glance, though they are in the range of prior estimates of how much microfinance borrowing impacts business owners' profits.[26]

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 group

[24]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.

[25]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.

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

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 explanation, which we find most appealing, 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 labor is just one of the often unmeasured costs, such that measured gains in "profits" may well be illusory.[27] In the presence of substantial measurement challenges, we find it appealing to rely on clients' choices revealing the 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 or overall life satisfaction, consistent with the financial losses being counterbalanced elsewhere.[28] 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 SKS after the health insurance requirement was eliminated is consistent with clients' low valuation of microfinance and moderate fixed costs of re-joining SKS and changing business practices.[29]

VI Conclusion

Narrowly, 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 demise.

Generally, however, this failure illustrates a broader phenomenon that microfinance clients receive little surplus from their borrowing relationship. SKS certainly did not anticipate the resistance from clients. While SKS did not presume that clients would value health insurance, at least initially while clients became more educated about insurance and experienced its benefits, they thought this learning period could be cross-subsidized by the

[27]Note that we asked businesses owners about profits directly, rather than calculating the difference between reported revenues and reported costs, so "unmeasured costs" in our context refers to "unconsidered costs" from the business owner's perspective. The financial impact of microfinance was also apparent across the range of business outcomes.

[28]Indeed, for business owners only, there is no impact on consumption or overall life satisfaction.

[29]There is some indication that clients become stressed by changes in access to microfinance: 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.

benefits clients derive from microfinance. Indeed, previous research and our estimates show that business owners' profits are impacted substantially by the availability of microfinance.

An underlying confusion between profits and welfare underlies SKS's mistaken expectations, and indeed the 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, that SKS influenced business profits along with other microfinance institutions that have been evaluated. The second part is wrong, however, in that households received little gain in welfare. Households were content to accept business losses to avoid paying the modest premium, and they did not come back to microfinance after they had dropped out. This must imply that the utility gain from running these businesses at a large scale was sufficiently low that it was not worth it for them.

These results are simply and strikingly discouraging about the focus on microfinance businesses as an engine to escape poverty. Indeed, these estimates call into question the welfare impacts from large financial gains among micro enterprises more generally. Of course, there have been indications for pessimism: the absence of impacts on household outcomes generally, the failure of businesses to grow, and the frequent closure of businesses. If we take seriously the decisions of these borrowers, then we must look elsewhere to support growth opportunities they consider worthwhile.

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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***	-0.161***	-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	5358	5358	5232	5358
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	0.717	0.025
Number of Clients	993	993	974	993
Panel C. Loan Amount, Full Sample				
Treatment	-2078***	-1990***		72
	(339)	(438)		(45)
Control Group Mean	7490	7202		63
Number of Clients	5358	5358		5358
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. 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	0.513***	0.0031***	0.0016***	0.0018***	0.045***
	(0.035)	(0.0006)	(0.0004)	(0.0005)	(0.012)
Control Group Mean	0.055	0.0003	0.0001	0.0002	0.012
Number of Events	24898	24898	24898	24898	24898
Panel B: For Each Client (Self-Reported)					
Treatment	0.686***	0.0107***	0.0055***	0.0063***	0.092***
	(0.040)	(0.0019)	(0.0014)	(0.0016)	(0.023)
Control Group Mean	0.086	0.0011	0.0003	0.0008	0.027
Number of Clients	6899	6899	6899	6899	6899
Panel C: For Each Client (Administrative Data)					
Treatment	0.849***	0.073***	0.026***	0.051***	
	(0.016)	(0.007)	(0.004)	(0.007)	
Control Group Mean	0.000	0	0	0	
Number of Clients	6899	6899	6899	6899	

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	-82	0.002	0.021	0.010
	(0.011)	(153)	(0.005)	(0.029)	(0.031)
Control Group Mean	0.067	1667	0.022	0.436	0.288
Number of Clients	24898	24898	24898	24898	24898

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	-0.2	0.009	-228	0.021	0.011	-0.010	-0.210***	-0.006
	(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	5341	5341	5341	5341	5341	5341	5341	5341

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, 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: Business owners at baseline						
Treatment	0.006	-308	-534*	-2529	-1449	-0.050*
	0.025	(192)	(311)	(2337)	(1271)	(0.028)
Control Group Mean	0.316	503	973	21681	11030	0
Number of Clients	2150	2150	2150	2150	2150	2150
Hochberg p-value						0.292
Panel B: Business owners at baseline and endline						
Treatment		-950	-1953*	-11318**	-6613*	-0.126**
		(600)	(1033)	(5708)	(3533)	(0.049)
Control Group Mean		1594	3080	68644	34922	0
Number of Clients		683	683	683	683	683
Hochberg p-value						0.068
Panel C: Business owners at endline						
Treatment		-1568	-1879**	-9492**	-5431**	-0.119***
		(953)	(782)	(4439)	(2692)	(0.041)
Control Group Mean		2408	2796	66897	33718	0
Number of Clients		993	993	993	993	993
Hochberg p-value						0.016

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). 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 four summary outcomes (business, consumption, income, social) in Table 6 and Appendix Tables 1 to 3.

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	-65	-57*	-9	115	-14	-24	23	-148
	(275)	(33)	(265)	(181)	(12)	(22)	(18)	(106)
Control Group Mean	11452	259	11193	6508	249	257	299	2102
Number of Clients	5341	5341	5341	5341	5341	5341	5341	5341
Hochberg p-value	>0.999							

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 four summary outcomes (business, consumption, income, social) in Table 6 and Appendix Tables 1 to 3.

Appendix Table 2. Estimated Impacts of Treatment on Household Income

	Self-Employment Profit	Wage Income, Daily and Salaried	Index of (1) and (2)
	(1)	(2)	(3)
Treatment	-792	1451	0.005
	(702)	(2132)	(0.039)
Control Group Mean	6136	28329	0
Number of Clients	5341	5341	5341
Hochberg p-value			0.905

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: total self-employed business profits over the previous year (Column 1); and total wage income over the previous year from daily labor and salaried labor, summing across all adults in the household (Column 2). In Column 3, the outcome variable is an index reflecting the equal-weighted average of the component variables in Columns 1 and 2 (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 four summary outcomes (business, consumption, income, social) in Table 6 and Appendix Tables 1 to 3.

Appendix Table 3. 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 four summary outcomes (business, consumption, income, social) in Table 6 and Appendix Tables 1 to 3.