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EVALUATING HEALTH-SEEKING BEHAVIOR, UTILIZATION OF CARE, AND HEALTH RISK: EVIDENCE FROM A COMMUNITY BASED INSURANCE MODEL IN INDIA

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EVALUATING HEALTH-SEEKING BEHAVIOR, UTILIZATION OF CARE, AND HEALTH RISK: EVIDENCE FROM A COMMUNITY BASED INSURANCE MODEL IN INDIA

KETKI SHETH[1]

ABSTRACT

This study evaluates a community based health insurance (CBHI) contract by randomizing the insurance offer to women in microfinance Self Help Groups in rural India. It finds no support for increased use of health care, and instead find limited suggestive evidence of reduction in health shocks and health care utilization. It also finds suggestive evidence that the insurance offer reduces health expenditure and health related debt. This suggests scope for additional indirect benefits of increased health to insured members and assisting in the financial sustainability of CHBI contracts.

1. INTRODUCTION

In recent years, there have been increased efforts to reduce vulnerability where formal insurance markets are missing. Though health care has been documented as a significant expenditure in poorer households (Banerjee et al. 2009, Dupas and Robinson 2009), and informal risk pooling shown to be incomplete (Townsend 1994, Morduch 1999, Jalan and Ravallion 1998), health insurance in most developing countries is virtually non-existent, with private prepaid plans being a small fraction of private expenditure on health[2]. Researchers and practitioners have long discussed the lack of insurance markets in poorer rural parts of the world, and the provision of community based health insurance, CBHI, also referred to as micro health insurance (MHI), has been gaining attention as a method to fill this gap.

Similar to the microfinance revolution providing missing credit markets to the poor, CBHI are arguably able to overcome the high loading costs and asymmetric information that have prevented formal insurance markets from serving the poor. Though community based health insurances differ in design subtleties, they also share a variety of common characteristics, such as lowering the price of health care, creating a network of facilities, and having a relatively low upper limit of coverage (Jakab and Krishnan 2003, Morduch 2003, Ekman 2004). CBHI differs from larger insurance companies in that they are often organized in closer connection to the local population, and in recent years, many have attempted to reach remote poorer populations by building upon pre-existing organizations, the most common of which are microfinance institutions.

A primary purpose of most of these programs is to both lower health expenditures and improve health care access of those who become insured. However, the extent to which CBHIs successfully achieve these goals is critically dependent on how insured members change their demand for health care in response to the insurance contract. When faced with lower health care costs, the direction and amount of change for health care consumption is ambiguous. To the extent that the insurer cannot observe the required treatment for the illness and lowers the cost of care, the quantity of health care demanded will increase. Such an increase may be seen as welfare enhancing by increasing access to health care for a population typically seen as underserved, such as the rural poor in a country like India. In theory, an increase in health care demanded could even lead to an increase in out of pocket health care expenditure by members, though this would imply a price elasticity beyond what has been commonly estimated. Members may even respond to being insured by increasing their health care consumption by such a large amount that the insurance contract becomes financially unsustainable and unravels as the cost of insurance becomes higher and higher.

But unlike other goods, the demand for health care is dependent on both cost and health status, which is a function of previously consumed health care. For example, if greater health care is initially purchased due to lower prices, this may lead to a long run increase in health status and reduced amount of needed health care. This dynamic relationship between health and health care could lead to a decrease in the overall health care sought even if the price of health care has decreased (Dupas 2011).

The effect of the insurance contract on long run health care consumption depends on which of these opposing effects dominates the change in health care usage.

This paper assesses the causal impact on health incidents and health care utilization when lowering the costs of health care through community based health

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2 According to WHO Core Health Indicators: http://apps.who.int/whosis/database/core/core_select_process.cfm

insurance contracts. I fail to find that households increased their health care utilization and find limited suggestive evidence of a decrease in health incident, care, and expenditure. I also find limited suggestive evidence of lower levels of debt used towards health, suggesting that these households found it less necessary to rely on credit as a coping mechanism for health shocks.

This paper is one of the few studies to find that health care use does not increase after being offered and enrolled in a CBHI program (Jutting 2004, Chankova et al. 2008, Jakab and Krishnan 2004, Wagstaff and Lindelow 2008). Previous studies evaluating the impact of health insurance on changes in health seeking behaviours have primarily used case studies, and identification of a causal link has been problematic. Because most evaluations compare the insured versus uninsured, it is unclear to what extent these results stem from the effect of being insured versus pre-existing differences between those who choose to enrol in the insurance and those who do not.

This paper adds to the literature by being the first of my knowledge in providing a causal link between CBHI and health incidence, health care utilization, and financial expenditure through a randomized controlled trial design. As described above, many studies have been stymied by identification and compare users and non-users of an insurance program; the randomized controlled trial methodology employed in this paper attempts to overcome this barrier and provide causal estimates without relying on differences between insured status. I review a CBHI scheme in India that shares many of the common features typical of the widespread growth of CBHI in developing countries. Unlike the majority of the studies, I reject the null hypothesis of an increase in the use of health care and find limited financial protection against health expenditures.

The remainder of this paper is organized as follows: Section 2 describes the insurance contract of the CBHI, Section 3 outlines a theoretical model of the effect of insurance on health care incidence and utilization, Section 4 describes the methodology, Section 5 reviews the datasets, Section 6 discusses results, robustness analysis and alternative interpretations, and Section 7 concludes.

2. OVERVIEW OF THE CBHI CONTRACT

In January 2011, Chaitanya, a non-profit microfinance institution (MFI) working on women's empowerment and microfinance in Junnar sub-district of rural Maharashtra, expanded its community based health insurance program, DAN. DAN capitalizes on Chaitanya's pre-existing microfinance Self-Help Groups3 (SHGs) structure, and the option to purchase the insurance is limited to SHGs in which at least 80 per cent of members are willing to purchase DAN (though women can decide who in their family will be included in the coverage). The cost of membership to DAN is Rs. 200 (USD 4) per person per year if the household insures 1 or 2 persons, or Rs. 150 (USD 3) per person per year if the household insures 3 or more persons. DAN does not involve a third party insurer, and health claims and operational costs are borne by the premiums collected4.

The main provisions of the health insurance contract are discounted prices (ranging from 5 to 20 per cent) negotiated at private network medical facilities, which include hospitals, medical laboratories, and pharmacies. Additionally, for in-patient treatment, the member will receive 60 per cent reimbursement of their medical fees at network private hospitals, and 100 per cent reimbursement at government medical facilities, up to a limit of Rs. 15,000 (USD 300)5. The product also includes a 24-7 medical help-line, health camps, and monthly visits by a doctor to villages to offer referrals and basic medicines. However, village visits by a doctor were intermittent and only one health camp was implemented during the timeframe of the research study.

Though CBHIs differ in design, DAN shares many of the common characteristics of CBHIs, including implementation through an existing MFI infrastructure, reducing the price of health care, establishing a network of medical facilities, and implementing an upper limit in coverage.

Chaitanya began enrolments into DAN in one area (Block 1: semi-urban) of Junnar sub-district in February 2011 and the remaining two areas (Block 2: more rural, Block 3: tribal and rural) in May 2011. Though enrolments were initially gradual, 61 per cent of the 1,311 members6 offered the contract were enrolled for at least some part of the study. In October 2012, the month in which a majority of the data used in this paper was collected, 47 per cent of members were enrolled, and 57 per cent were enrolled during the year recall period. Health claims were disbursed to 10 per cent of enrolled members, with an average pay-out of Rs. 3,610 (USD 73) (See Table 1).

3 SHG are groups of 15 - 20 women who voluntarily come together to save and access micro credit from Chaitanya.

4 A team of medical doctors, who are able to judge the technical validity of the claim, reviews the reimbursement claims. Afterwards, the claims are sent to a committee composed of local women from the Self Help Groups who decide the final outcome (e.g., should more or less be given).

5 Specific illnesses may have lower upper limits based on predefined categories of illness types.

6 The households included in all analysis were those that were present at the start of the research study. Households were considered to be present if at least one SHG meeting was held in the 3 to 4 months preceding the start of enrollments in the area.

Table 1: Enrollment and claims summary statistics

Panel A: Enrollment	
Never Enrolled	512
Enrolled	799
Percent Ever Enrolled	60.9%
Percent Enrolled at Endline Survey	47.0%
Percent Enrolled in Year Recall of Endline Survey	57.2%
Panel B: Claims (Conditional upon Enrollment)	
Percent Filed Claim	9.8%
Average Claim Disbursement	Rs. 253
Conditional upon Receiving Claim	
Average Claim Disbursement	Rs. 3610

3. A SIMPLE MODEL ON CHANGES IN HEALTH CARE AND HEALTH INCIDENT

When reducing the price of health care, we often assume that the overall quantity of health care consumed will increase. A common concern of insurance is that because it effectively lowers the price of health care for the insured, individuals consume more health care than if they were uninsured. In a developing country context such as India, this may be considered welfare enhancing by increasing access to healthcare. Nevertheless, because households also decide when and what type of health care to access, it may be the case that health improves and overall health care consumption decreases. The dynamics between these two factors, decreasing the costs of assessing health care and the timing and quality of the health care purchased, leads to a theoretically ambiguous response in the change of health care utilization when members become insured.

Consider a household that has the choice of seeking health care immediately or waiting to seek health care in the future depending on the course of the illness. If the household chooses to wait, with a certain probability they will recover on their own and will not have incurred any health cost. Alternatively, the illness may advance over time and require an increased amount of health care. Below I outline a simple two period model in which a household can either 1) seek care immediately when illness is still uncertain and face lower health expenditure with certainty, or 2) wait until the second period where the illness shock will become known, but conditional upon receiving a health shock the health expenditure will be higher.

I assume the household derives utility from two parameters, consumption and health. If the household chooses to purchase health care in period 1, then the household is not in risk of a health shock in period 2, and has the following expected utility (with certainty):

$$(1)\ EU = (1+\beta)u(Y - P * H_1, \bar{H} + H_1),$$
$$s.t.\ P * H_1 < Y$$

However, if the household chooses not to purchase health care in period 1, they risk a negative health shock in period 2 and have the following the expected utility:

$$(2)\ EU = (1-\pi_s)(1+\beta)u(Y, \bar{H}) + \pi_s[u(Y, \bar{H}) + \beta u(Y - P * H_2, \bar{H} - \theta + H_2)],$$
$$s.t.\ P * H_2 < Y$$

where π_s is the expected probability of the health shock in the second period, β is the discount rate for the second period, Y is the household's income endowment, $\bar{H}$ is the household's health endowment, P is the price of health care, and H is the amount of health care required to be purchased, assuming $H_1 < H_2$.

Depending on which equation yields a higher expected utility, the household will either purchase or wait to purchase health care in the first period. Depending on the curvature of the utility and the above parameters, such as the discount factor and the difference in health care required between the periods, one may choose to take the risk of increased health care in the future on the chance of not having to pay any health expenses. Assuming a homogenous society, if the expected utility of Eq (1) is higher, we would expect the population's average health care utilization to be H_1, with an average cost of $P * H_1$. If expected utility of Eq (2) is higher, then average health care utilization would be $\pi_s * H_2$, with average costs being $\pi_s * P * H_2$. It is not obvious whether the lower expected health care consumption will be optimal due to the discount rate. For example, as we imagine households to have higher and higher discount rates, they will be more likely to forego health care in the first period since the potential cost in the second period is valued less in the present period, even if poor health and health care utilization would be lower had they chosen to seek health care earlier.

A health insurance program effectively lowers the price of health care, P. While this is often done through directly lowering the monetary price of health care, it could also include other measures that lower the cost of seeking health care, such as creating a network of health facilities with increased quality or doctor visits which reduce the costs associated with travel. Using the model described above, a decrease in the price of health care could either cause people to seek care earlier (now that the foregone income is lower) or cause people to seek care later (now that

the risk to income from waiting has also reduced). Depending on which effect dominates, we could see a rise or fall of health status and health care utilization.

In the above model I assumed a fixed requirement of health care. However, the amount of health care purchased is also a factor in the household's decision making process. Though the potential health burden increases in period 2 if health care if not sought earlier, the household still chooses how much health care to purchase in both periods (i.e., H_1 and H_2 are usually not fixed amounts as depicted in the model above). Thus for any of the given periods, assuming increasing returns to health care, a drop in the price will lead to an increase in the consumption of health care.

Thus, the combination of reducing the price of health care with the dynamic element of when and what type of health care to purchase leads to ambiguity when predicting how health care utilization will change under a health insurance program that lowers the cost of health care.

4. METHODOLOGY

Finding a valid comparison group for estimating the effect of CBHI has been elusive due to endogeneity of placement of programs and voluntary enrolment. To overcome this issue, the CBHI program evaluated in this paper randomized the offer of the health insurance. Half of the 43 villages in which Chaitanya was operational were randomly offered the health insurance DAN in the Junnar subdistrict of Maharashtra[7]. The randomization was stratified upon three distinct areas (referred to as Block *1, 2, 3*), which become increasingly rural.

The randomization of the insurance offer assists in estimating the casual effect of the health insurance offer in the community. Using the following equation I estimate the effect of the insurance offer on illness, health care utilization, and health expenditures in the past week, month, and year.

$$(3)\ y_{igvt} = \beta_1 TreamentVillage_v + BlockFixedEffects_v + \varepsilon_{igvt}$$

where *y* is the variable of interest (health and financial variables), *TreatmentVillage* is an indicator of whether the household lives in a village that was offered DAN, *BlockFixedEffects* are indicators for whether the household lives in the area upon which the randomization was stratified; subscript *i* indicates the household or individual, subscript *g* indicates the SHG to which the household belongs, subscript *v* indicates the village, and subscript *t* indicates the month of the survey for estimations that use panel data. The randomization of DAN suggests *TreatmentVillage* is less likely to be correlated with the error term, ε_{igvt}, a necessary requirement for the consistent estimate of β_1.

4.1. ROBUSTNESS ANALYSIS

Upon estimating β_1 from Eq (3), I test for evidence of pre-existing differences between treatment and control villages. Pre-existing differences between village types would yield inconsistent estimates of β_1. Additionally, bounding exercises and comparison of responses across surveys are used to test for whether non-response rates raise doubt on the differential representativeness of the sample yielding a spurious estimate of β_1.

In addition to reviewing balance and attrition across treatment status, I run through a series of robustness analysis. One would expect that the treatment effect increases with greater exposure to the insurance contract, as more members become aware and enrolled in the program. Thus, I expand Eq (3) with a time trend interacted with the village's treatment status to test for increasing effects with exposure to the insurance.

$$(4)\ y_{igvt} = \beta_1 TreamentVillage_v + \beta_2 t + \beta_3 TreatmentVillage * t_{vt} + BlockFixedEffects_v + \varepsilon_{igvt}$$

where *t* the number of months since the start of the insurance offer. Since more and more members were enrolled over the course of the study, I hypothesize $\beta_3 > 0$.

The varied timing of enrolments provide additional opportunities for robustness analysis by including household fixed effects and estimating the effect of the treatment by comparing households before and after enrolment into the insurance program.

$$(5)\ y_{igvt} = \delta_1 Enrolled_{igvt} + alpha_i + \varepsilon_{igvt}$$

where α_i are household fixed effects, and *Enrolled* is an indicator for whether any member in the household is enrolled in the given month. One concern for δ_1 to be consistently estimated is the timing of the enrolment may not be exogenous. For example, we may expect

7 The randomization was originally done for 61 villages. However, in the early stages of the study it was realized that 18 of these villages were not operational and so were dropped from the study. These villages were equally assigned to treatment and control villages.

that households choose to become enrolled into the health insurance contract when they foresee health consumption in the near future – biasing δ_1 upwards. Alternatively, it could be the case that households have a health incident and suddenly value the health insurance more because of the increased saliency of health, biasing δ_1 in the opposite direction. In this context, when entire SHGs are enrolling at the same time and enrolment is dependent on a qualified staff from the MFI to enrol the members, such endogeneity of the timing of enrolment is likely to be limited. As an additional robustness check, I also estimate Eq (5) using the timing of the first enrolment in the village (in which case *Enrolled* varies with village and month). Though this is also subject to the same type of endogeneity concerns, it is arguably even less likely that the timing of the village's first enrolment is based on sorting by household into the contract.

At the start of the study, rudimentary pilot surveys were conducted in the SHGs on health parameters. Though these are not at the household level, I employ a difference-in-difference technique with the SHG as the unit of observation.

$$(6)\ y_{gvt} = \gamma_1 TreamentVillage_v + \gamma_2 PostBaseline_{avt} + \gamma_3 TreatmentVillage * PostBaseline_{gvt} + BlockFixedEffects_v + \varepsilon_{gvt}$$

where *PostBaseline* is an indicator for whether the survey source was conducted after the intervention. This estimation is limited to very few indicators for which data was collected in the pilot surveys. Nonetheless, it provides a method to control for pre-existing differences between treatment and control villages. γ_1 is a measure of differences in the two types of villages not related to the insurance, and γ_3 is the consistent estimate of the offer of the insurance contract. One concern is that the pilot surveys were technically implemented after the start of the study, though before most enrolments. Thus, γ_3 may not be the entire effect of the insurance offer, but perhaps isolating effects of actual enrolment into the contract. Likewise, γ_1, can either be interpreted as pre-existing differences between village types or an effect of the insurance offer that is not dependent on actually being insured.

Upon completion of the study, the SHGs in the control villages were also offered the insurance. This provided a natural test for the Treatment Effect on the Treated using a difference-in-difference technique with the enrolled households in the control and treatment villages.

$$(7)\ y_{igvt} = \gamma_1 TreamentVillage_v + \gamma_2 Enrolled_{igvt} + \gamma_3 TreatmentVillage * Enrolled_{igvt} + BlockFixedEffects_v + \varepsilon_{igvt}$$

where *Enrolled* is now an indicator of the households who became in the treatment and control villages. Though there may be differences between the type of households who enrolled in the treatment villages almost two years earlier, it is likely that these households are similar. γ_1 is the average difference between unenrolled members in treatment and control villages (which may be pre-existing differences and/or externalities from the insurance program), γ_2 is a measure of the type of households who choose to enrol in the program, and γ_3 is the parameter of interest – the effect of the insurance on those who enrolled (as opposed to the insurance offer).

5. DATA

The primary data source is an Endline Survey conducted in October 2012 on a randomly selected subsample of the population, approximately 18 to 21 months after the insurance was introduced. This survey was a detailed questionnaire on household demographics and illnesses in the past week and year. This survey provides a cross section of detailed information at the individual and household level.

In addition to the Endline Survey, short health surveys were conducted during monthly SHG meetings from October 2011 to July 2012. These SHG Monthly Surveys asked basic questions on household's rate of illness and health care utilization since the previous SHG meeting (i.e., a one month recall period). Unlike the Endline Survey, these surveys provide a panel on health status and health care use. However, the survey is limited to the household level (as opposed to the individual) and is dependent on whether the SHG meeting was held in the given month. Additionally, two pilot SHG Surveys were conducted in February and July 2011. These surveys reported illness in the household as a proportion of SHG Members for a one and three month recall period.

Financial activity with the MFI was sourced from Chaitanya's records. Households have two sources for loans – within their own SHGs and across SHGs through the MFI. SHG records were collected from September 2011 to August 2012. These records include all financial activities of the members, both within and across SHGs. However, the quality of data is low and highly dependent on whether the SHG chose to retain and complete the records. MFI records were collected from August 2011 to September 2012. These financial records are of much higher

quality, but only include financial activities across SHGs through the MFI (i.e., larger loans).

Enrolment, claims, and the insurance's doctor village visits are accessed from Chaitanya's internal records.

For all estimations, I only assess data collected from those households who were members of the MFI at the start of the insurance offer. This prevents the estimates from being driven by the entry and exit of members, which may be an effect of the insurance offer itself.

5.1.SUMMARY STATISTICS

Table 2 describes the demographics of the households in the research study collected in the Endline Survey. A significant number of households in this area are below the poverty line, belong to castes recognized as disadvantaged by the government, and have at least some household participation as agricultural labourers for employment. The population is approximately 50 percent female, has an average education level of 6th grade, and an average age of 31.

Table 2 also tests for balance in these characteristics between control and treatment villages. Treatment households have slightly higher socioeconomic status, which are statistically significant for some variables. Additionally, treatment villages have slightly more females (approximately 2%), though the magnitude of this difference is very small.

Table 2: Summary statistics and balance on demographic characteristics

	Control		Treatment		Treatment - Control	Treatment - Control
	No. SHG Women	Mean	No. SHG Women	Mean		
Household Characteristics:						
Household Size	938	5.801	748	5.698	-0.0643	-0.103
Forward Caste	887	0.297	707	0.633	0.226**	0.336***
Hindu	923	0.963	739	0.922	-0.0391	-0.0406
Above Poverty Line	905	0.012	710	0.029	0.0160**	0.0177**
Subsidized Ration Card	905	0.526	710	0.612	-0.0226	0.0859
Below Poverty Line	905	0.435	710	0.327	-0.00375	-0.108
Stamped Below Poverty Line	905	0.027	710	0.031	0.0104	0.00391
Agricultural Laborer	938	0.701	749	0.635	-0.0189	-0.0659
House Type	931	2.284	744	2.443	0.0683	0.160*
Individual Characteristics:						
Female	5333	0.496	4249	0.513	0.0179*	0.0171*
Age	5357	31.319	4276	31.347	-0.478	0.0288
Education	5176	6.131	4265	6.257	-0.0529	0.126

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

6. EMPIRICAL ESTIMATIONS

Incidence of illness, health care utilization, health expenditure

Table 3 estimates Eq (3) using data collected in the Endline Survey. When asked about the previous week's health incidents, I find that treatment villages are more likely to have been sick (statistically significant) and have higher health expenditure. When asked about the previous year, however, I find that treatment areas are 7 to 9 percentage points less likely to have had a major health shock, and have substantially less health expenditure (approximately $USD 100) and debt (approximately USD 50). This is true even if I control for initial health status by include the SHG health variables asked in the pilot SHG surveys as independent variables (even columns)[8].

Table 4 echoes the annual recall results in Table 3 using the SHG Monthly Surveys panel data. Treatment areas report being 6 percentage points less ill, less likely to seek health care, less likely to have extreme illnesses (as indicated by lower levels of being admitted or prolonged bed rest), and lower health expenditure. However, I do not find any statistically significant difference in prolonged bed rest when recalling the previous month, raising some doubt on

8 In the remaining tables, I estimate all regressions with the health variables as controls from the SHG Surveys and note if there are differences in estimated parameters when excluding the variables.

the lower levels of illnesses of households in villages offered insurance. Table 3 and 4 also estimates bounds for the estimates (discussed in the following section). Appendix Table 1 and 2 describe summary statistics of the variables used in Table 3 and 4.

Table 3: Endline survey - Health incidence and expense

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A: In the past week, the household experienced									
	no. of sick hh members		health expenses							
Treatment Village	0.167**	0.173**	388.5	327.1						
	(0.0757)	(0.0719)	(439.0)	(326.6)						
Bounds by quintile:										
0%	-2.878***	-3.011***	-39198.3***	-40789.6***						
20%	-1.738***	-1.872***	-23689.5***	-25289.4***						
40%	-0.625	-0.736*	-8180.7	-8763.5						
60%	0.488	0.483	6948.3	7372.8						
80%	1.602***	1.672***	22457.2***	23913.6***						
100%	2.676***	2.797***	37139.8***	39471.8***						
Obs (SHG Members)	1643	1643	1591	1591						
	Panel B: In the past year, the household experienced									
	Rs. 1000 ($US 20) health shock		admitted		prolonged bedrest		health expenses		health loan	
Treatment Village	-0.0720*	-0.0910***	-0.0783**	-0.0930***	-0.0527	-0.0729*	-5095.6**	-6709.4***	-2367.5***	-2195.1***
	(0.0364)	(0.0335)	(0.0362)	(0.0321)	(0.0400)	(0.0365)	(2273.1)	(2242.5)	(619.2)	(586.5)
Bounds by quintile:										
0%	-0.285***	-0.297***	-0.293***	-0.303***	-0.281***	-0.293***	-128529.8***	-133595.5***	-70546.3***	-72876.9***
20%	-0.199***	-0.212***	-0.204***	-0.214***	-0.192***	-0.204***	-79106.7***	-83755.1***	-43043.3***	-45621.4***
40%	-0.114***	-0.123***	-0.118***	-0.125***	-0.106***	-0.115***	-31656.2	-34256.4	-16730.2	-17819.8
60%	-0.0288	-0.0308	-0.0338	-0.0338	-0.0211	-0.0229	15971.4	17418.8	9576.1	10457.8
80%	0.0564	0.0612	0.0524	0.0602	0.0652*	0.0711*	63421.8***	67876.7***	35889.1***	38949.0***
100%	0.139***	0.149***	0.137***	0.150***	0.149***	0.161***	110259.3***	117761.1***	61436.8***	65737.9***
Obs (SHG Members)	1690	1690	1685	1685	1684	1684	1677	1677	1670	1670
Health Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Regressions includes block fixed effects. Standard errors are clustered at the village level. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 4: SHG monthly surveys - Illness, care, and expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	In the past month,							
	illness	visiting facility/ dan doctor	visiting health facility	doctor (not village visit)	doctor (including village visit)	admitted	prolonged bedrest	health expenses
Treatment Village	-0.0578**	-0.0368	-0.0749***	-0.0721***	-0.0319	-0.0125**	-0.00630	-737.8**
	(0.0228)	(0.0226)	(0.0175)	(0.0173)	(0.0237)	(0.00537)	(0.00739)	(286.6)
Upper Bound	0.00719	0.0330	-0.0123	-0.0125	0.0362	-0.00736	-0.000133	-625.3*
Lower Bound	-0.204***	-0.188***	-0.222***	-0.218***	-0.185***	-0.0239***	-0.0222***	-762.6**
N (SHG Member Month)	12765	12797	12766	12777	12796	12755	12742	10852
	Conditional upon being ill							
Treatment Village		-0.0562*	-0.0668**	-0.0690**	-0.0531*	-0.0190	0.00209	-1339.9**
		(0.0285)	(0.0324)	(0.0320)	(0.0296)	(0.0148)	(0.0203)	(607.6)
Upper Bound		-0.0398*	-0.0495*	-0.0407	-0.0253	-0.00480	0.0189	-1361.9**
Lower Bound		-0.152**	-0.226**	-0.357***	-0.242***	-0.0609***	-0.0556***	-1389.4**

N (SHG Member Month)	4429	4429	4429	4429	4424	4415	4281

All regressions include health status variables from the February 2011 SHG Pilot Survey as independent control variables. Households are weighted to be equal per month. Regressions include block area fixed effects. Standard error are clustered at the village level. Upper and Lower bounds refer to Lee (2009) bounds, stratified by block areas. Source: SHG Monthly Surveys. Statistical significance levels are as follows: *10%, **5%, ***1%.

One possible explanation for the differences in the week versus the year and month recall may be the type of illnesses that are being recalled. It is likely that smaller illnesses and minor health care consumption are more likely to be recalled in a week period, but not in longer timeframes (Das et al. 2011). The data collected on the previous year is limited to larger illnesses, which I proxy by asking households whether they suffered a health incident in which a household member was on prolonged bed rest for 5 or more days, admitted to a health care facility, or incurred health expenses which totaled over Rs. 1,000 (USD 20). Though the surveys recalling the previous month do not limit themselves to larger health illnesses, it may be the case that a month's recall period is too long of a timeframe for households to recall smaller and more regular illnesses. This is consistent with the possibility that households in treatment villages are seeking more health care, though for smaller health incidences. As a result, these households have a decrease in health incidents and expenditure for larger illnesses (captured in the month and year recall).

In order to better understand why the insurance led to lower health care utilization, I estimate health care consumption differences in only those members who reported an illness in their household. Because incidence of illness is correlated with the insurance offer, these estimations provide suggestive evidence but should not be viewed as consistent estimates of the insurance product. Table 4 suggests that even when ill, treatment villages are less likely to report going to visit a health facility and have lower health expenditure. This is consistent with illnesses in treatment village households being less severe. Table 5 observes individual level responses conditional upon being ill and exhibits a similar pattern even conditional upon being ill, individuals in treatment villages are ill for fewer days and miss less days of school/work. For larger illnesses, they are admitted for shorter durations, have lower health expenditures, and less likely to have borrowed for health. These results continue to be more robust for the year recall as opposed to the week recall. Relative to the year recall, the week recall suggests slightly higher expenditure and visits to health facilities for treatment households, though these results are not statistically different from zero.

Table 5: Individual health behavior conditional on illness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Week Recall						
	Conditional upon being sick in the past week,						
	was sick	Poor health today	days ill	days missed	health facility	total health expenses	
Treatment Village	0.0277**	-0.0611	-0.388*	-0.0720	0.0119	183.7	
	(0.0136)	(0.0491)	(0.203)	(0.197)	(0.0108)	(280.0)	
Obs (Individuals)	9651	1212	1213	1174	1221	1228	
	Panel B: Year Recall						
	Conditional upon having experienced a health shock in the previous year,						
	experienced a health shock	days missed	doctor	total days admitted	health expenses	loan for health	borrowed
Treatment Village	0.00162	-5.757***	0.00850	-1.116*	-7186.6	-0.0243	-1970.1*
	(0.0100)	(1.548)	(0.00518)	(0.605)	(4292.1)	(0.0337)	(1009.2)
Obs (Individuals)	9664	878	901	904	889	795	904

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. All regressions include health status variables from the February 2011 SHG Pilot Survey as independent control variables. A health shock is defined as having had a health incident with high expenditure, being admitted, or being on prolonged bed rest. Regressions include block fixed effects. Standard errors are clustered at the village level. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

Financial vulnerability

Being insured against health shocks and lowing the cost of health care would theoretically lead to a decrease in financial vulnerability. One would expect to see a decrease in debt and selling of assets used to finance health. However, this may result in capital and credit being more available for investments leading to a potentially ambiguous result in the overall debt burden. Table 6 finds no statistically significant effect on the selling or purchasing of assets or levels of debt (though point estimates on credit use are in the expected direction). Table 7 reviews the financial records of the SHG and MFI and finds a lack of statistically significant results. Though treatment villages have a lower level of outstanding debt in the SHGs (Panel A), Column (1) and Column (2) suggest that treatment areas are both saving and borrowing at lower amounts. Furthermore, the lower debt burden is limited only to the smaller loans in SHGs, and I find no impact on the larger loans offered by the MFI. Due to the lack of consistent and statistically significant results in the financial patterns of the insured village, the remainder of the paper focuses only on health outcomes[9]

6.1. ROBUSTNESS ANALYSIS FOR HEALTH INCIDENCES AND HEALTH CARE

Pre-existing differences

A potential concern is that pre-existing differences, instead of the offer of the insurance, account for the difference between households in treatment and control villages. Though Table 2 suggested that differences along household demographics were minimal, Table 8 raises the concern of the two areas differing in health status from the start of the intervention. The health data for Table 8 is from the pilot SHG Surveys conducted in February and July of 2011. These surveys recorded the proportion of the SHG that had experienced household illness in the past month, and prolonged bed rest or high health expenditure in the past three months. Unfortunately, this data has relatively low response-rate, a slight imbalance in the response rate by treatment status, and identification only at the SHG level (not at the household level). Also, these surveys were technically conducted after the start of the intervention – though insurance coverage only began in February 2011, and enrolments had only minimally begun in Block 2 and 3 by July 2011. Nonetheless, the results of these initial surveys are disconcerting as they report that SHGs in treatment villages had a lower proportion of illnesses in their households at magnitudes similar to the ITT estimates found in Table 3 and 4. This holds true in every Block which the randomization was stratified. However, this same pattern does not hold for other the indicators of health, prolonged bed rest and high expenditure, where treatment and control villages are relatively balanced.

Using the Pilot SHG Monthly Surveys in February and July 2012, I am able to construct a panel for estimating a difference-in-difference of the treatment effect with the SHG as the unit of observation (Eq. 6)[10]. As expected, a difference-in-difference estimation suggests no difference (or even an increase) of illness in the treatment villages after the insurance is offered (see Table 9). The coefficient on prolonged bed rest continues to suggest a decrease of five percentage points. Neither coefficient is statistically significant, most likely due to a reduction in statistical power from the decrease in the number of observations. These results do give rise to the concern that the Intent to Treat Effect of the insurance found in Table 3 and 4 are not due to the insurance itself, but perhaps to pre-existing differences or treatment effects on survey response. Though I attempt to control for these initial health variables by including them as independent variables in estimations[11], the low correlation between the measures (especially once treatment status is included), does not result in large differences in the estimated coefficient of the treatment effect.

Survey non-response

Survey non-response is also a primary concern in the robustness of the effect of the insurance offer estimated in Table 3 and 4. The response rate of the October 2012 Endline Survey was 80% in treatment villages and 79% in control villages. As Table 10a confirms, there was no statistically significant difference in the response rate by treatment status. Table 10b provides the reasons for nonresponse rates – very few households refused consent and the majority of households not surveyed were due to relocation, which seems unlikely to be a result of the insurance offer. One primary reason for the low-response stemmed from three villages which were experiencing difficulties with the MFI due to high defaults. This made it difficult for surveyors to contact households in these villages, and thus accounts for over 50% of the unknown non-response. In a small number of households, a shorter survey was implemented which asked basic health and

9 The estimations outlined in the robustness analysis for financial outcomes also find varying and statistically insignificant results. These tables are available upon request.

10 In the SHG Monthly Surveys, members are directly asked about illness and prolonged bed rest in their household in the past month. The prolonged bed rest in both surveys use a different number of days as the minimum (3 versus 5), and in the latter survey is reconstructed by the past 3 months' surveys to match the recall period. Averages of the SHG are then taken to match the unit of observation in the Pilot SHG Surveys.

11 In order to not lose observations by including health controls from the February 2011 Pilot Survey, village averages are assigned to those SHGs where the Pilot Survey was not conducted. If no SHG in the village was surveyed, then the respective treatment status average was assigned.

expenditure questions, increasing the response rate to 80% in both treatment and control villages.

Table 6: Assets and loans

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	In the past year,			*Of current outstanding loans,*			
	mortgaged assets	sold assets	purchased assets	number of loans	has health loan	has home/ business improvement loan	total loan outstanding
Treatment Village	-0.00579	-0.0244	-0.0270	-0.0802	-0.0218	0.0261	-6027.3
	(0.0245)	(0.0257)	(0.0244)	(0.0746)	(0.0187)	(0.0394)	(8542.4)
Obs (SHG Members)	1657	1650	1654	1529	1516	1516	1520

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. All regressions include health status variables from the February 2011 SHG Pilot Survey as independent control variables. Regressions include block area fixed effects. Standard errors are clustered at the village level. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 7: Financial vulnerability

	(1)	(2)	(3)	(4)
Panel A: Internal SHG Financial Records -- Internal and External Finances				
	input into fund	output from fund	loan disburse-ment	outstanding amount
Treatment Village	-124.3	-4880.1	28.01	-3790.7***
	(135.3)	(4017.2)	(84.76)	(1103.3)
N (SHG Members Month)	20988	20988	20970	4352
Panel B: MFI Financial Records -- External Finances				
	outstanding loan amount	overdue principle	overdue interest	amount paid
Treatment Village	363.6	136.3	2.815**	27.42
	(770.8)	(137.1)	(1.137)	(66.28)
N (SHG Members Month)	36792	36792	36792	34164

All regressions include health status variables from the February 2011 SHG Pilot Survey as independent control variables. Regressions include block area fixed effects. Standard errors are clustered at the village level. Source: Panel A: SHG Financial Records, Panel B: MFI Financial Records. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 8: Pseudo-baseline health status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	February, 2011				July, 2011			
	Ill	Prolonged Bedrest	High Expenditure	Collected Data	Ill	Prolonged Bedrest	High Expenditure	Collected Data
	Panel A							
Treatment Village	-0.0836	-0.00239	-0.00127	-0.0349	-0.129	0.0128	0.0455	0.0337
	(0.0665)	(0.0368)	(0.0385)	(0.102)	(0.0639)	(0.0481)	(0.0420)	(0.0788)
Obs (SHGs)	99	98	98	160	64	64	61	85
	Panel B: Block 1 (Most Urban)							
Treatment Village	0.374	0.151	0.159	0.867				

Obs (SHGs)	37	37	37	45				
Control Village	0.446	0.152	0.185	0.867				
Obs (SHGs)	26	26	26	30				
	Panel C: Block 2 (Semi-Urban)							
Treatment Village	0.430	0.088	0.227	0.316	0.407	0.108	0.110	0.789
Obs (SHGs)	6	6	6	19	14	14	14	19
Control Village	0.598	0.218	0.224	0.350	0.504	0.126	0.066	0.800
Obs (SHGs)	7	6	6	20	16	16	13	20
	Panel D: Block 3 (Rural/Tribal)							
Treatment Village	0.438	0.154	0.167	0.438	0.273	0.108	0.108	0.875
Obs (SHGs)	7	7	7	16	14	14	14	16
Control Village	0.505	0.083	0.094	0.533	0.432	0.067	0.061	0.800
Obs (SHGs)	16	16	16	30	20	20	20	30

Observations include all SHGs for which at least one member was included in the baseline. Illness is a recall period of one month. Prolonged bedrest and high expenditure has a recall period of 3 months. July 2011 excludes Block 1 where a significant amount of enrollments had already occurred to date. Panel A: Regressions include block area fixed effects. Standard errors are clustered at the village level.

Table 9: Difference-in-difference using pseudo-baseline

	(1)	(2)	(3)	(4)
	Proportion of households in the SHG who experienced:			
Dependent Variable:	illness (1 month recall)	prolonged bedrest (3 month recall)	illness (1 month recall)	prolonged bedrest (3 month recall)
Treatment Village * Post Intervention	0.00664	-0.0533	0.156	-0.0540
	(0.0660)	(0.0454)	(0.0942)	(0.0430)
Treatment Village	-0.117*	-0.00745	-0.107*	0.0204
	(0.0633)	(0.0328)	(0.0592)	(0.0401)
Post Intervention	-0.0891*	-0.0168	-0.133**	-0.0269
	(0.0461)	(0.0280)	(0.0565)	(0.0305)
N(SHG Month)	197	210	119	149
Month for Pre-Intervention	Feb-11	Feb-11	Jul-11	Jul-11
Includes Block 1	Yes	Yes	No	No

July 2011 excludes Block 1 where a significant amount of enrollments had already occurred to date. Regressions include block area fixed effects. Standard errors are clustered at the village level. Sample limited to SHGs for which Pilot SHG Surveys were conducted. Post Intervention refers to the SHG Monthly Survey conducted in the same month as the Pilot Survey the following year. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 10a: Endline survey non-response

	(1)
Dependent Variable : Surveyed	
Treatment Village	0.0302
	(0.0838)
Obs (SHG Member)	2068

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Regressions include block area fixed effects. Standard errors clustered at the village level. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 10b: Reasons for 2012 October endline attrition (by fraction)

	(1)	(2)	(3)
		Not Surveyed	
	Control	Treatment	Treatment - Control
Did not give consent	0.019	0.008	-0.00949
Could not find house	0.007	0.030	0.0234*
Passed Away	0.041	0.032	-0.00785
Pilgrimage/Trip	0.010	0.019	0.00986
Out of town for health	0.010	0.000	-0.0108**
Door locked/not home	0.043	0.016	-0.0281
Moved	0.372	0.502	0.154
Unknown	0.497	0.394	-0.131
No. SHG Women	219	169	388

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Treatment - Control are based on regressions which include block area fixed effects. Standard errors clustered at the village level. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 11 (Columns 1 and 2) and Table 12 provide suggestive evidence of the health status of non-respondents in the Endline Survey. Table 11 uses the February 2011 Pilot SHG Survey to test whether non-respondents in treatment villages differed from those in control villages. Here I find suggestive evidence that non-respondents in treatment villages were more likely to be sick than those in control villages. However, Table 12 compares responses in the SHG Monthly Survey by Endline Survey response status and suggests these differences are relatively small. These results suggests that non-respondents in treatment areas were sicker relative to respondents (compared to households in control households), biasing estimated ITT effects downward. However, the magnitude of these differences is small enough making it unlikely to be accounting for the entire estimated ITT in Table 3 and 4.

Table 3 estimates bounds by quintile. Though the attrition rate is too large to confidently provide a range for most variables, the incidence of large health shocks is robust to a 20 percentage point difference in treatment and control household non-respondents, the high end of what Table 11 and 12 suggest.

Table 11 (Column 3 and 4) similarly suggest that non-respondents from treatment villages in the SHG Monthly Surveys are also more likely to be ill in the previous month, though not more likely to have experienced a large health shock in the previous three months. Table 13, Panel A, is analogous to Table 12, where I compare responses on the 2012 Endline Survey based on respondent status in the SHG Monthly Survey. If anything, non-respondents in treatment areas are even less ill than those in the control areas (relative to respondents). Thus it is unclear whether the ITT coefficient estimated in Table 4 is biased upwards or downwards. Panel B tests for balance between non-respondents and respondents using the demographic data collected in the Endline Survey – though results are mostly statistically insignificant, it is suggestive of non-respondents being slightly better off socioeconomically.

Time variation

Due to the concerns of pre-existing differences and non-response rates in the surveys, I next turn to time variation as a method to test for the robustness of the initial results found in Table 3 and 4. Table 14 estimates whether the length of the exposure to the insurance offer results in increased treatment effects. I find no statistically significant evidence that the effects of the insurance offer is increasing over time. This is surprising since enrolments are varied over the first year of the program, with many enrolments not occurring until at least 6 months after the start of the intervention. This result echoes the concern raised by the Pilot SHG Surveys, suggesting differences existed even before most members are enrolled in the insurance program.

Figure 1 and 2 plot point estimates θ from a regression that includes a variable for each month enrolled in the program, using only the sample of households in treatment villages:

$$(8)\ y_{igvt} = \theta_1 Enrolled1Mon_{igvt} + \theta_2 Enrolled2Mon_{igvt} + \ldots + \theta_{18} Enrolled18Mon_{igvt} + MonFixedEffects_t + AreaFixedEffects_v + \varepsilon_{igvt}$$

The figure show steadily decreasing level of illness and utilization of health care facilities as the household is enrolled in the program for longer duration. However, an odd feature of the pattern is that these estimates are relative to before the household was enrolled – it is surprising that the month after enrolment, the household has lower rates of illness and health care consumption. If we believed that the health insurance encouraged seeking care earlier or better care, we should expect that the insurance first causes an increase in health care and then a subsequent drop – however, this initial rise is absent from the data.

Table 11: Attrition in 2012 endline survey by February 2011 SHG health status

	(1)	(2)	(3)	(4)
Dependent Variable: HH in	Endline Survey		SHG Monthly Survey	
Treatment Village	0.109	0.0376	0.331**	0.108
	(0.103)	(0.0627)	(0.139)	(0.115)
Ill	0.0606		0.388**	
	(0.144)		(0.156)	
Ill * Treatment	-0.230		-0.491**	
	(0.190)		(0.233)	
Health Shock		-0.0303		0.273
		(0.297)		(0.323)
Health Shock * Treatment		-0.142		0.0348
		(0.325)		(0.432)
Obs (SHG Member, SHG Member Month)	2068	2068	26250	26250

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Recall period for being ill is one month. Recall period for health shock (proxied by an expenditure over Rs. 1000 or 3 days of consecutive bed rest) is 3 months. Independent variables are proportion of SHG Members who had such an incident in their household. Regressions includes block fixed effects. Standard errors are clustered at the village level. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 12: SHG monthly survey - Health responses by endline response status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable:	surveyed in SHG Monthly Surveys	illness	visiting health facility/ dan doctor	visiting health facility	doctor (not village visit)	doctor (including village visit)	admitted	prolonged bed rest	health expenses
Endline Respondents:									
Treatment Village	0.0535	-0.0757***	-0.0715***	-0.0962***	-0.0852***	-0.0586**	-0.0207***	-0.0162**	-869.6***
	(0.0423)	(0.0245)	(0.0217)	(0.0209)	(0.0209)	(0.0217)	(0.00420)	(0.00670)	(295.4)
Obs (SHG Member Month)	16800	8611	8636	8612	8621	8636	8606	8597	7243
Endline Non-Respondents									
Treatment Village	0.368***	-0.0261	-0.00496	-0.0261	-0.0405	-0.0154	-0.0194*	-0.0273**	-910.7**
	(0.124)	(0.0410)	(0.0398)	(0.0387)	(0.0354)	(0.0367)	(0.0102)	(0.0135)	(419.4)
Obs (SHG Member Month)	3880	1178	1186	1180	1182	1185	1179	1177	1080

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Regressions includes block fixed effects. Standard errors are clustered at the village level. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 13: SHG survey response rates and demographics

	(1)	(3)	(4)	(5)	(7)	(8)	(9)	(10)	(11)
	Panel A: Household Health Characteristics								
	In the past week,		In the past year,						
	no of sick hh members	health expenses	Rs. 1000 ($US 20) expenditure	admitted	prolonged bed rest	no. of health shocks	health expenses	health loan	
SHG HH per Month Surveyed									
Treatment Village	0.0224	31.59	-0.0827**	-0.0781**	-0.0686	-0.0790	-4075.9	-2290.8***	
	(0.0853)	(371.5)	(0.0375)	(0.0352)	(0.0428)	(0.0524)	(2478.7)	(724.0)	
N (SHG Members Month)	8629	8357	8843	8816	8809	7948	8808	8777	
SHG HH Per Month Not Surveyed									
Treatment Village	0.217**	141.0	-0.114***	-0.128***	-0.103***	-0.137**	-5586.8***	-1933.1***	
	(0.0852)	(171.3)	(0.0372)	(0.0374)	(0.0376)	(0.0570)	(1791.3)	(464.6)	
N (SHG Members Month)	6191	6123	6407	6384	6381	5822	6352	6323	
Test of Equality (P-value)	0.0084	0.7589	0.454	0.1763	0.4973	0.4026	0.5366	0.458	
	Panel B: Household Demographics								
	Forward Caste	Poverty Status: Above	Poverty Status Subsidized	Poverty Status Below	Agricultural Laborer	House Type	Mean Female	Mean Age	Mean Education
SHG HH per Month Surveyed									
Treatment Village	0.156	0.0114	-0.0265	0.0107	-0.0219	0.0258	0.00949	-0.137	-0.404
	(0.101)	(0.00921)	(0.0556)	(0.0507)	(0.0649)	(0.0646)	(0.0104)	(0.889)	(0.322)
N (SHG Members Month)	8470	8469	8469	8469	8821	8852	9823	9847	9833
SHG HH Per Month Not Surveyed									
Treatment Village	0.262***	0.0224***	-0.0515	0.0237	-0.0346	0.120*	0.00850	-0.428	-0.262
	(0.0887)	(0.00692)	(0.0465)	(0.0450)	(0.0686)	(0.0671)	(0.0136)	(1.027)	(0.270)
N (SHG Members Month)	6080	6041	6041	6041	6389	6408	6887	6893	(0.270)
Test of Equality (P-value)	0.2668	0.2487	0.6529	0.8286	0.7346	0.0297	0.9893	0.6098	(0.270)

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Regressions includes block fixed effects. Standard errors are clustered at the village level. Tests of equality are not weighted. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 14: Length of exposure to insurance offer

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	illness	visiting facility/ dan doctor	visiting health facility	doctor (not village visit)	doctor (including village visit)	admitted	bed rest	health expenses
Treatment Village	3.127	0.646	1.164	0.531	0.259	-0.548	-1.814	-30210.1
	(8.432)	(8.770)	(8.578)	(8.353)	(8.579)	(2.036)	(1.783)	(76944.6)
Month Number	-0.0336***	-0.0325***	-0.0323***	-0.0306***	-0.0307***	-0.00587**	-0.0121***	-53.00
	(0.00979)	(0.0101)	(0.0101)	(0.00975)	(0.00981)	(0.00272)	(0.00188)	(118.7)
Month * Treatment	-0.00510	-0.00110	-0.00199	-0.000975	-0.000475	0.000855	0.00289	47.18
	(0.0135)	(0.0140)	(0.0137)	(0.0133)	(0.0137)	(0.00325)	(0.00284)	(122.9)
N (SHG Member Month)	12765	12797	12766	12777	12796	12755	12742	10852

Regressions include block area fixed effects and health controls. Standard errors are clustered at the village level. Source: SHG Monthly Surveys. Statistical significance levels are as follows: *10%, **5%, ***1%.

Figure 1a: Illness by months enrolled

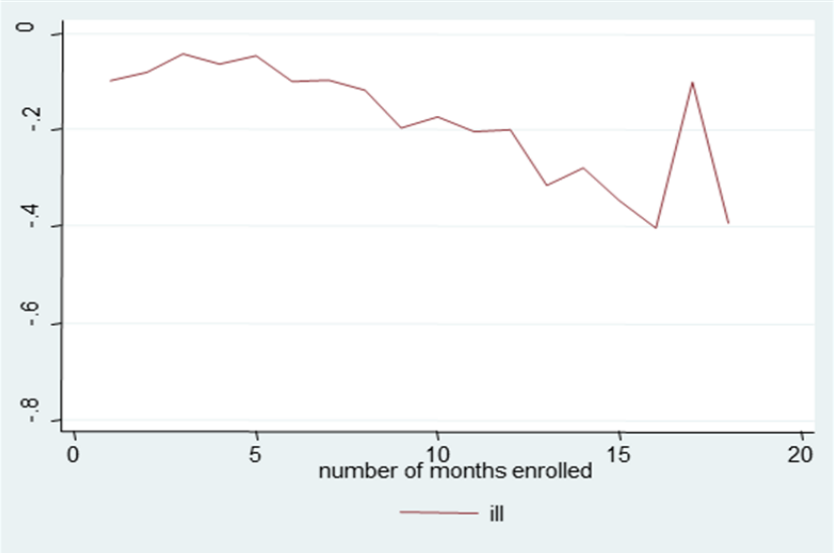

Figure 1b: Health care consumption by months enrolled

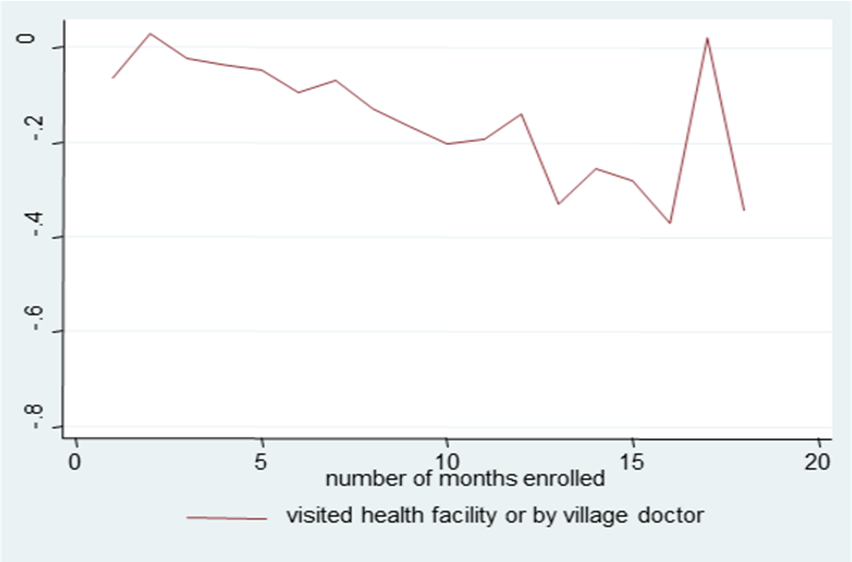

Table 15 uses the timing of the village and household enrolment, along with member fixed effects, to compare households before and afte enrolment. Panel A compares each household before and after the village had its first enrolment (a proxy for the initial exposure to the insurance offer). Village timing of enrolment is more likely determined by constraints of the MFI's human resources than household health status at the time. Panel B compares the household to itself before and after becoming enrolled in the program. Both panels suggest that even when comparing within households, households are less likely to be ill, consume health care, and have high health expenditures after being enrolled in the program. This result is supportive of our initial findings in Table 3 and 4 and do not rely on methods balance in characteristics and non-response between treatment and control households. Nonetheless, the methodology is open to concerns of endogeneity if the timing of enrolment is a function of health status and decreasing time trends of poor health. For example, an alternative interpretation to Table 15 is households and villages become insured when health care needs are high and salient, and thus the estimated treatment effect is spurious.

Table 15: Variation on time of village enrollment on health variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	In the past month,							
	illness	visiting facility/ dan doctor	visiting health facility	doctor (not village visit)	doctor (including village visit)	admitted	prolonged bed rest	health expenses
	Panel A: Village Enrollment							
Indicator for Village Enrollment	-0.0615	-0.0461	-0.102**	-0.0938*	-0.0346	-0.0340	-0.0626**	-248.7
	(0.0519)	(0.0528)	(0.0516)	(0.0516)	(0.0529)	(0.0243)	(0.0307)	(264.6)
N (SHG Member per month)	12765	12797	12766	12777	12796	12755	12742	10852
	Panel B: Household Enrollment							
Enrolled	-0.0779*	-0.105*	-0.125**	-0.121**	-0.100*	-0.0421**	-0.0573**	-246.0
	(0.0403)	(0.0575)	(0.0509)	(0.0483)	(0.0548)	(0.0195)	(0.0235)	(230.7)
N (SHG Member Month)	12765	12797	12766	12777	12796	12755	12742	10852

Regressions include member fixed effects and a time trend. Standard errors are clustered at the village level. Indicator for Village Enrollment is an indicator as to whether at least one household in the village has been enrolled. Enrolled is an indicator for whether the household was enrolled during the given month.Source: SHG Monthly Surveys. Statistical significance levels are as follows: *10%, **5%, ***1%.

Enrolled members in control villages: difference-in-difference

Upon completion of the research timeframe, the insurance was offered to control villages. This provides potential identification of households who would have enrolled in the program had the insurance been offered to all villages initially. Table 16 estimates a difference-in-difference of household demographics and health status among the enrolled in the control versus treatment villages. Though differences are not statistically significant, enrolled households in treatment villages appear to be slightly better off socioeconomically and have lower levels of health incidence. Table 17 and 18 estimate the difference-in-difference outlined in Eq. 7 and shows no statistically significant Treat Effect of the Treated from the insurance on the enrolled. This may be due to low statistical power, but even the point estimates are of low magnitude and opposite signs of the results found in Table 3 and 4. I continue to see the coefficient on treatment village remain large and statistically significant, suggesting either very large externalities to households that chose not to enroll or that differences do not stem from the insurance itself. Of course, it may be the case that those enrolled in the control villages are not a comparable group to the enrolled in the treatment village, though it seems unlikely that the TET would not be more similar to estimates found in Table 15

Table 16: Enrolled members in control versus treatment villages

	Source: Endline Survey on Demographics						Source: February 2011 SHG Survey on Proportion of Illnesses	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Forward Caste	Poverty Status: Above	Poverty Status: Subsidized	Poverty Status: Below	Agricultural Laborer	House Type	Proportion Ill	Proportion with Health Shock
Treatment Village	0.268***	0.00618	-0.0677	0.0480	0.0466	-0.00803	-0.100	-0.00652
	(0.0981)	(0.00816)	(0.0625)	(0.0548)	(0.0694)	(0.0932)	(0.0758)	(0.0353)
Enrolled Household	-0.0417	-0.000932	-0.0128	-0.00129	0.0233	-0.0621	0.102**	0.0438
	(0.116)	(0.0112)	(0.0544)	(0.0475)	(0.0517)	(0.0674)	(0.0491)	(0.0393)
Treatment Village * Enrolled Household	-0.0509	0.0156	0.0743	-0.0804	-0.108	0.139	-0.0369	-0.0312
	(0.143)	(0.0142)	(0.0718)	(0.0618)	(0.0743)	(0.101)	(0.0824)	(0.0471)
N (SHG Member)	1594	1615	1615	1615	1687	1675	1243	1243

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Data from February 2011 Pilot SHG Survey limited to those SHGs for which data was collected. Health shock is defined as having been on prolonged bed rest or high health expenditure. Regressions include block area fixed effects. Standard errors are clustered at the village level. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 17: Endline survey - Treatment effect on the treated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	In the past week,		*In the past year,*					
	no. of sick hh members	health expenses	Rs. 1000 ($US 20) health shock	admitted	prolonged bed rest	no of health shocks	health expenses	health loan
Treatment Village	0.345*	1928.5	-0.0987**	-0.107**	-0.0958*	-0.0812	-5875.6**	-2532.4***
	(0.201)	(1539.4)	(0.0462)	(0.0482)	(0.0499)	(0.0807)	(2644.3)	(670.3)
Enrolled	0.112	173.7	0.0204	0.0254	0.00430	0.0566	-935.3	1068.8
	(0.0740)	(304.4)	(0.0352)	(0.0392)	(0.0446)	(0.0500)	(2247.1)	(1080.2)
Treatment Village * Enrolled	-0.270	-2479.0	0.0312	0.0332	0.0667	-0.0366	-15.13	-50.14
	(0.200)	(1735.2)	(0.0498)	(0.0538)	(0.0593)	(0.0883)	(3695.6)	(1352.4)
N (SHG Member)	1643	1591	1690	1685	1684	1526	1677	1670

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Enrolled is an indicator for whether the household was enrolled during the given month. Regressions include block fixed effects. Standard errors are clustered at the village level. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 18: SHG monthly survey - Treatment effect on the treated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	In the past month,							
	illness	visiting facility/ dan doctor	visiting health facility	doctor (not village visit)	doctor (including village visit)	admitted	prolonged bedrest	health expenses
Treatment Village	-0.0788**	-0.0857***	-0.0985***	-0.0920***	-0.0782***	-0.0229***	-0.0237**	-716.0***
	(0.0369)	(0.0296)	(0.0298)	(0.0274)	(0.0274)	(0.00527)	(0.0104)	(196.9)
Enrolled	0.0643**	0.0597**	0.0582**	0.0529**	0.0538**	0.00977	0.0121	436.5
	(0.0286)	(0.0265)	(0.0267)	(0.0248)	(0.0250)	(0.00617)	(0.00951)	(390.5)
Treatment Village * Enrolled	-0.00154	0.0195	0.00253	0.00662	0.0255	0.00797	0.0121	-194.2
	(0.0397)	(0.0343)	(0.0345)	(0.0317)	(0.0325)	(0.00807)	(0.0129)	(453.3)
N (SHG Month Member)	12765	12797	12766	12777	12796	12755	12742	10852

Enrolled is an indicator for whether the household was enrolled during the given month. Regressions include block fixed effects. Standard errors are clustered at the village level. Source: SHG Monthly Surveys. Statistical significance levels are as follows: *10%, **5%, ***1%.

Comparative methodologies: comparing enrolled members

As a comparison to the most common methodologies in previous papers, I analyse the data as if the insurance offer had not been randomized. I compare those who enrolled in the insurance contract with those who did not, limiting the sample to the villages that were offered the insurance contract. Table 19 presents drastically different estimates, suggesting that the type of people who choose to enrol in the insurance differ from those who do not enrol in the very variables in which we are interested in measuring an effect. While Panel A, variables at the household level, suggest that the insurance has no effect, Panel B and C illustrate that at the individual level the insurance contract is correlated with poorer health and higher health care consumption. This highlights the differences between those who choose to enrol and those who do not, and the necessity of a valid comparison group in evaluating CBHI programs when enrolment into the program is optional.

Table 19: Enrolled members versus unenrolled members

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Household Illnesses							
	In the past week,		*In the past year,*					
	no. of sick hh members	health expenses	Rs. 1000 ($US 20) health shock	admitted	prolonged bed rest	no of health shocks	health expenses	health loan
Enrolled HH Members	0.0202	-535.8	0.0100	0.0131	0.0160	0.00551	-461.3	-141.4
	(0.0367)	(392.3)	(0.00903)	(0.00906)	(0.00990)	(0.0196)	(547.4)	(118.5)
Obs (SHG Members)	736	702	746	742	741	684	743	740
	Panel B: Individual Illness, Week Recall							
	poor health (today)	was sick	days ill	days missed	health facility	doctor	was admited	total health expenses
Enrolled	0.0513***	0.0629***	0.333***	0.196**	0.0630***	0.0587***	0.0283	-148.7
	(0.0170)	(0.0195)	(0.0940)	(0.0744)	(0.0173)	(0.0157)	(0.0172)	(135.9)
Obs (Individuals)	4146	4285	4274	4254	4279	4276	4285	4285
	Panel C: Individual Illness, Year Recall							
	experienced a health shock	days missed	health facility	doctor	total days admitted	health expense	received health loan	amount borrowed
Enrolled	0.0457**	0.863**	0.0437**	0.0431**	0.285***	684.1	0.0142***	148.9
	(0.0177)	(0.411)	(0.0196)	(0.0194)	(0.0769)	(524.8)	(0.00489)	(93.39)
Obs (Individuals)	4289	4278	4287	4287	4290	4282	4237	4290

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Health Shock is defined as high expenditure, being admitted, or prolonged bed rest. Enrolled HH Members is the maximum number of household members on the policy during the time of the study. Enrollment is an indicator for whether or not the individual was ever enrolled. Sample is limited to only treatment villages. Regressions include block fixed effects and health controls from February 2011 Pilot SHG Surveys. Standard errors are clustered at the village level. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

6.2. POTENTIAL MECHANISMS

Indirect effects

Table 20, Panel A, estimates household behaviour when ill in villages offered the insurance. One would expect that treatment areas would seek care faster, forego treatment less, and be more likely to recover from illnesses. However, I find no statistically significant effect on any of these variables and even the sign of the point estimate is often opposite of the expected direction. Panel B estimates whether these behaviours are affected by the number of times the village was visited by the insurance's doctor[12]. Again, I find no evidence that there was a positive effect on such behaviours from the insurance or the insurance's doctor's visits.

Whether these estimates provide lower or upper bounds depends on our beliefs of the characteristics of the individuals who are not ill in the villages offered the insurance. For example, if we believe that those who did not suffer a health shock were the type of people who would have had milder shocks requiring less health care, then the estimates provided in Table 20 are lower bounds. The assumptions required for the estimates of foregoing treatment and days waited to seek medical care to be lower bounds is that those who did not become ill were the type of people who would be less likely to wait or forego medical care. The opposite assumptions would imply the estimates are upper bounds of the effect of the insurance offer conditional upon illness.

Table 21 and 22 estimate whether village visits by the insurance's doctor had any discernible effect on health incidents, health consumption, and health expenditure. Though I do not argue that the doctor's visits to villages are exogenous, the correlation can help to understand whether the results found in Table 3 and 4 are spurious or due to such mechanisms as the visits from the doctor. Though Table 21 finds no

12 Appendix Table 5 summarizes the frequency of village visits by the insurance doctor for out-patient care (OPD Doctor).

consistent effect of the doctor on health, and Table 22 suggests that the doctor did encourage consumption of health care. I also find a correlation between illness incidence and doctor visits, which may be a function of endogeneity of the doctor visits or that the diagnosis and recall of the illness was altered by the doctor visits.

Table 20: Indirect effects of insurance

	(1)	(2)	(3)	(4)	(5)
	Week Recall		*Year Recall*		
	days waited before seeking care	forewent treatment	days waited before seeking care	forewent treatment	recovered
	Panel A: Intent to Treat				
Treatment Village	-0.898	0.0414*	1.219	0.0369	0.00667
	(0.736)	(0.0215)	(3.325)	(0.0236)	(0.0288)
Obs (Individuals)	1145	1131	846	892	892
	Panel B: Intensity of Doctor Visits to Villages				
Treatment Village	-0.490	0.0443*	1.947	0.0161	0.0340
	(0.536)	(0.0254)	(3.431)	(0.0380)	(0.0482)
Number of months village visited by OPD	-0.193	-0.00137	-0.331	0.00939	-0.0124
	(0.169)	(0.00953)	(1.060)	(0.0128)	(0.0183)
Obs (Individuals)	1145	1131	846	892	892

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. All regressions include health status variables from the February 2011 SHG Pilot Survey as independent control variables. Regressions include block area fixed effects. Standard errors are clustered at the village level. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 21: Endline survey - Village doctor visits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	In the past week,		*In the past year,*					
	no. of sick hh members	health expenses	Rs. 1000 ($US 20) health shock	admitted	prolonged bed rest	no of health shocks	health expenses	health loan
Treatment Village	0.0950	312.8	-0.138***	-0.140***	-0.134***	-0.148**	-7680.2**	-2363.5***
	(0.111)	(327.1)	(0.0396)	(0.0440)	(0.0483)	(0.0675)	(2877.2)	(695.2)
Number of months village visited by OPD	0.0365	6.727	0.0218	0.0215	0.0279	0.0196	448.4	77.66
	(0.0494)	(150.9)	(0.0156)	(0.0162)	(0.0191)	(0.0280)	(968.0)	(252.5)
Obs (SHG Members)	1643	1591	1690	1685	1684	1526	1677	1670

Observations are limited to households selected and surveyed in the Endline Health Survey and are weighted to be representative of the target population. Regressions includes block fixed effects and health controls. Standard errors are clustered at the village level. Source: 2012 October Endline Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 22: SHG monthly survey - Village doctor visits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	In the past month,						
	illness	dan network doctor	visiting health facility	doctor (not village visit)	doctor (including village visit)	health shock	health expenses
Village Visited by OPD	0.0245	0.0202*	0.0126	0.0173	0.0391	0.0104	181.8*
	(0.0250)	(0.0103)	(0.0206)	(0.0210)	(0.0242)	(0.00697)	(96.25)
Number of villages visited by OPD	0.00486***	0.00164**	0.00465***	0.00429***	0.00421**	0.00177***	33.02**
	(0.00145)	(0.000679)	(0.00149)	(0.00144)	(0.00161)	(0.000502)	(13.22)
N (SHG Members per month)	12765	11502	12766	12777	12796	12757	10852

Each row is a separate regression. Health shock is defined as having either been admitted or on prolonged bed rest. Regressions include treatment indicator, time trend, health controls and block fixed effects. Standard errors are clustered at the village level. Source: SHG Monthly Survey. Statistical significance levels are as follows: *10%, **5%, ***1%.

7. CONCLUSION

The success and effectiveness of insurance contracts are critically dependent on the health care utilization of its member base. An increase in health care consumption is often an indirect goal of community based insurance providers, though it also raises concerns of the financial stability of the insurance contract. Contrary to the majority of studies evaluating CBHI, I find this insurance contract does not increase health care consumption. Instead, I find limited suggestive evidence of the insurance possibly reducing the consumption and expenditure of health care. Not only is this indicative of improving the health status of the insured, but additionally helps households with their health expenditure beyond the discounted health care received directly through the insurance. Furthermore, a decrease in health care utilization increases the probability of financial sustainability of the program. However, the insurance lowering the need and demand for health care is only one possible interpretation of the findings, as discussed in Section 6, and thus should be considered an upper limit.

In general, the potential of CBHI to increase health status and lower the amount of health care warrants further research. Numerous factors in the design of the CBHI may be responsible for decreasing the barriers of access to health care and potentially reduced health shocks: direct price reductions, network facilities with quality checks, and local doctors being monitored. Further research is required to decipher which of these factors led to a decrease in health shocks and health care utilization and how these can be promoted and integrated into the designs of CBHI programs.

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APPENDIX

Table 1: Health illness summary statistics (HH)

	Control Villages				Treatment Villages			
	Obs (SHG Members)	Mean	SD	Max	Obs (SHG Members)	Mean	SD	Max
In the past week,								
number of sick hh members	907	0.74	0.83	6	736	0.88	1.06	12
health expenses	889	618	1792	23000	702	1085	9734	152270
In the past year,								
Rs. 1000 ($US 20) health shock	944	0.56	0.50	1	746	0.47	0.50	1
admitted	943	0.53	0.50	1	742	0.44	0.50	1
bed rest	943	0.50	0.50	1	741	0.43	0.50	1
health expenses	934	19189	34859	500500	743	15669	39520	550000
health loan	930	3683	15537	300000	740	1330	10097	200000

Observations are limited to households selected and surveyed in the Household Health Survey and are weighted to be representative of the target population. Expenses given in Ind Rupees (1USD = 50 Rs). Source: 2012 October Endline Survey.

Table 2: SHG monthly summary statistics

	Control Villages					Treatment Villages				
	Obs (SHG Members)	Mean	SD	Min	Max	Obs (SHG Members)	Mean	SD	Min	Max
present at meeting	6425	0.881	0.324	0	1	8089	0.821	0.384	0	1
In the past month, the household has experienced										
illness	5819	0.383	0.486	0	1	6958	0.317	0.465	0	1
visiting health facility/dan doctor	5834	0.376	0.484	0	1	6975	0.322	0.467	0	1
visiting health facility	5829	0.374	0.484	0	1	6949	0.299	0.458	0	1
doctor (including village visit)	5833	0.354	0.478	0	1	6975	0.310	0.462	0	1
doctor (not village visit)	5832	0.353	0.478	0	1	6957	0.284	0.451	0	1
admitted	5825	0.041	0.199	0	1	6942	0.024	0.155	0	1
bed rest	5817	0.052	0.221	0	1	6937	0.033	0.180	0	1
health expenditure	4861	1067	11527	0	700000	5999	433	3240	0	150000

Source: SHG Monthly Surveys.

Table 3: Summary statistics on assets

	Control Villages					Treatment Villages				
	Obs (SHG Members)	Mean	SD	Min	Max	Obs (SHG Members)	Mean	SD	Min	Max
In the past year,										
Mortgaged Assets	919	0.209	0.406	0	1	738	0.198	0.398	0	1
Sold Assets	915	0.142	0.349	0	1	735	0.131	0.338	0	1
Purchased Assets	918	0.298	0.458	0	1	736	0.273	0.446	0	1
Conditional upon selling or mortgaging assets, reasons given for doing so:										
Health	315	0.182	0.378	0	1	217	0.129	0.332	0	1
Business Requirements	315	0.203	0.389	0	1	217	0.284	0.440	0	1
Current Outstanding Loans										
Number of Loans	835	0.903	0.855	0	7	694	0.831	0.821	0	6
Has health loan	826	0.123	0.329	0	1	690	0.091	0.288	0	1
Has home/business improvement loan	826	0.390	0.488	0	1	690	0.418	0.494	0	1
Total Loan Outstanding	831	36135	107212	0	1200000	689	53076	453380	0	11500000

Observations are limited to households selected and surveyed in the Household Health Survey and are weighted to be representative of the target population. Source: 2012 Endline Survey.

Table 4: Summary statistics of SHG finances (internal and external)

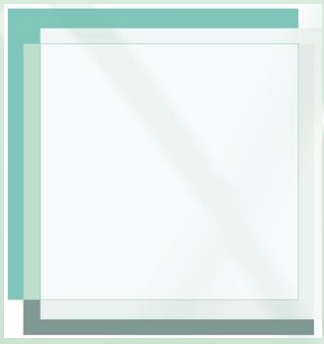
	Control Villages				Treatment Villages			
	Obs (SHG Members)	Mean	SD	Max	Obs (SHG Members)	Mean	SD	Max
SHG Financial Records								
input into funds	10139	882	2023	37840	10849	791	2154	109740
output into funds	10139	5390	490469	49400000	10849	569	3605	72000
loan disbursement	10132	477	2814	50000	10838	501	3020	50000
outstanding amount	2195	12614	19604	544000	2157	10994	11903	80000
MFI Financial Records								
outstanding loan amount	18438	2501	7759	50000	18354	2305	7432	50600
overdue principle	18438	137	1037	28533	18354	170	1058	26533
overdue interest	18438	3	53	3047	18354	4	65	2773
amount paid	17121	276	1253	31000	17043	239	1148	50000

Source: Panel A - SHG Financial Records, Panel B - MFI Financial Records.

Table 5: Distribution of visits by insurance doctor to villages

Number of months village visited by doctor	Number of villages	Percentage of villages	Number of households	Percentage of households
0	5	23.81	119	9.5
1	3	14.29	254	20.29
2	3	14.29	118	9.42
3	5	23.81	497	39.7
4	3	14.29	196	15.65
5	1	4.76	13	1.04
6	1	4.76	55	4.39
Number of months, household visited by doctor	**Number of villages**	**Percentage of villages**	**Number of households**	**Percentage of households**
0			1,100	87.86
1			107	8.55
2			23	1.84
3			20	1.6
4			2	0.16

Observations include only treatment villages.



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