Chapter 2: Assessing adverse selection and health care demand in micro health insurance: Evidence from a community based insurance model in India

Abstract: Providing health insurance to the poor, commonly referred to as micro health insurance, has been gaining recent attention as a method to reduce vulnerability and increase access to health care. This study evaluates an imbalanced randomized controlled trial in which health insurance was offered to groups of women belonging to a microfinance institution in rural Maharashtra, India. I find evidence that group eligibility requirements successfully reduced concerns of adverse selection into the program, though there remained a positive correlation between health need and demand for insurance. I fail to find robust evidence of the health insurance increasing demand for health care, though I do find limited suggestive evidence of reductions in large health shocks and expenditure. This suggests scope for additional indirect benefits of increased health to insured members and improved financial sustainability of micro health insurance contracts.

I. 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.[1] The lack of insurance markets in poorer rural parts of the world has led to a growing movement among micro finance institutions to provide health insurance as a method to fill the gap. These programs are often referred to as micro health insurance (MHI) or community based health insurance.

This paper assesses both selection and program effects of one such micro health insurance program in rural India. I find that despite efforts to protect against adverse selection, members who enroll in the MHI have worse health status and higher health expenditure. However, this correlation is lower among microfinance members than non-microfinance household members enrolled in the program, suggesting that group eligibility requirements (which were binding only for the microfinance members) did successfully reduce adverse selection. I find mixed evidence on the effects of the program itself, with some suggestive evidence of a decrease in large health incidents, resulting in lower health expenditure and health related debt.

Similar to the microfinance revolution providing missing credit markets to the poor, MHI is arguably able to overcome the high loading costs and asymmetric information that have prevented formal insurance markets from serving the poor. Though MHI differ in design

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

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). MHI differs from larger insurance companies in that they are often organized in closer connection to the local population and are able to build upon the preexisting organizational structure.

Though insurance is theoretically am important tool to assist with the financial risk of poor health, the empirical evidence of enrollment into micro insurance programs have generally been very low, with take up rates rarely exceeding 30 percent (Matul et al. 2013). One promising proposal for improving uptake is bundling micro health insurance with micro finance. However, empirical evidence suggests that compulsory health insurance may be unsustainable as clients prefer to leave the institution rather than purchase the insurance (Banerjee et al. 2014). This paper assesses an alternative method for protecting against adverse selection and improving uptake using the MFI structure. I find that making the insurance voluntary at the MFI's Self Help Group[2] level, but compulsory conditional upon other group member's purchasing the product, resulted in high uptake of the product. I find that 60% of households insure at least one member, and 20% of all eligible individuals (MFI members + household members) enroll in the MHI. Unlike Banerjee et al. (2014), I do find that the demand for MHI is correlated with previous health needs. Despite the group level enrollment, I find a correlation between enrollment and baseline health use. However, enrolled members were free to enroll additional family members as they desired. The correlation between poor health and enrollment is higher among non-MFI family members, suggesting that the group level enrollment most likely reduced adverse selection, though it failed to eliminate it completely.

[2] A Self-Help Group is 15 – 20 women who come together to borrow and save under the MFI. Many MFIs operate through group lending, though the size of the group may vary.

Conditional upon enrollment, a primary purpose of MHI is to both lower health expenditures and improve health care access. However, the extent to which MHIs 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 may be 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 provides suggestive causal impact on health incidents and health care utilization when lowering the costs of health care through micro health insurance contracts. I find limited suggestive evidence of a decrease in large health incidents, resulting in lower levels of health related debt, health expenditure and health care utilization for large illnesses. However, I fail to find evidence for changes in overall health care utilization.

This paper is one of the few studies to find that health care use does not increase after being offered and enrolled in a MHI 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 behaviors 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 preexisting differences between those who choose to enroll in the insurance and those who do not.

This paper adds to the literature by being the first of my knowledge to provide suggestive evidence for adverse selection in MHI demand and a causal link between MHI and health incidence, health care utilization, and financial expenditure that does not rely on comparing unenrolled and enrolled members. Rather, the paper's identification strategy exploits an imbalanced 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 an MHI scheme in India that shares many of the common features typical of the widespread growth of MHI 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 MHI, Section 3 describes the methodology, Section 4 outlines a theoretical model of the effect of insurance on health care incidence and utilization, Section 5 reviews the datasets, Section 6 discusses results, robustness analysis and alternative interpretations, and Section 7 concludes.

II. Overview of the CBHI contract

Overview of the Micro Health Insurance 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 MHI program, Dipthi Arrogya Nidhi (DAN). Though micro health insurance contracts differ in design, DAN shares many of the characteristics common to MHI. These include distributing through an existing MFI infrastructure (the most common provider of micro health insurance), reducing the cost of health care, implementing a coverage cap and co-pay, and charging a single premium price. The reduction in the cost of health care includes both price reductions and mechanisms such as improved signals of health care quality (e.g., empanelling facilities), easier access to health care, and increased saliency of health.

The cost of membership to DAN is INR 200 (USD 4) per person per year if the household insures 1 or 2 persons, or INR 150 (USD 3) per person per year if the household insures 3 or more persons. The main provisions of the health insurance contract are discounted prices (5 to 20%) negotiated at private network medical facilities, which include hospitals, medical laboratories, and pharmacies. Additionally, for in-patient treatment, the member receives 60 percent reimbursement of their medical fees at network private hospitals, and 100 percent reimbursement at government medical facilities, up to a limit of INR 15,000 (USD 300) per

event.[3] The product also includes a 24-7 medical help-line, health camps, and monthly village visits by a doctor 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.

DAN capitalizes on Chaitanya's preexisting microfinance Self Help Groups[4] (SHGs) structure. The option to purchase the contract is limited to SHGs in which at least 80 percent of members purchase the MHI, though women can decide the number of family members to enroll. This eligibility requirement reduces concerns of adverse selection by reducing the likelihood of household characteristics being correlated with enrollment into the program. In addition to improving financial sustainability, this feature reduces concerns of endogeneity from the enrollment decision when estimating effects of MHI. If all population heterogeneity was within SHGs, then the eligibility requirement would be the most effective in ensuring enrollment is uncorrelated with household characteristics. The eligibility requirement falls short of preventing household characteristics from being correlated with enrollment by three factors: heterogeneity across SHGs, members being free to choose additional household members to enroll, and requiring 80 percent compliance (as opposed to 100).

DAN does not involve a third party insurer, and health claims and operational costs are financed by the premiums collected. A team of medical doctors, who are able to judge the technical validity of the claim, reviews the reimbursement claims. Afterward, the claims are sent to a committee composed of local women from the Self Help Groups to determine the final disbursement amount.

Chaitanya began enrollments 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 enrollments were initially gradual, 61 percent of the 1,311

[3] Specific illnesses may have lower coverage caps based on predefined categories of illness type. Relative to other micro health insurance plans, this limit is relatively generous. For example, VimoSEWA, a large micro insurer in India, has a limit of INR 2,000 – 6,000 (USD 40 – 120) and RSBY (government insurance for BPL households) has a limit of INR 30,000 (USD 600) for the entire household (SEWA 2013, RSBY 2013a).

[4] SHGs are groups of 15 – 20 women who voluntarily come together to save and access micro credit from Chaitanya.

members[5] offered the contract were enrolled for at least some part of the study (see Table 1: Treatment Village Enrollments). In October 2012, the month in which a majority of the data used in this paper was collected, 47 percent of members were enrolled, and 57 percent were enrolled during the year recall period. Though SHG member enrollment is high, remaining household members' enrollment is significantly lower. Of the entire target population (i.e., SHG members and their household members), enrollments reaches only 20 percent. As Table 2 shows, health claims were disbursed to 10 percent of enrolled members' households, with an average payout of Rs. 253 (USD 5) per enrolled household and Rs. 3,610 (USD 73) per claim. Among enrolled individuals, the claim rate drops to 5%. Table 3 provides descriptive information on claims, illustrating that the majority of claimable illnesses are incidents such as malaria and typhoid, and a significant number of claims are being approved.

III. Methodology

Finding a valid comparison group for estimating the effect of CBHI has been elusive due to endogeneity of placement of programs and voluntary enrollment. To overcome this issue, the MHI 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 sub-district of Maharashtra.[6] The randomization was stratified upon three distinct areas (referred to as Block *1*, *2*, *3*), which become increasingly rural.

Demand for Insurance:

[5] 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.
[6] 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 (see Appendix Table 1).

To estimate the demand for insurance, I observe the enrollment decisions of those villages that were offered the insurance after the completion of the randomized phase-in (i.e., after November 2012). For these villages, I observe health data prior to the insurance offer and their enrollment decisions for seven months after the MHI is made available. To estimate whether the MHI was able to protect against higher demand among individuals with worse health, I compare individuals who enrolled in the program relative to those who did not in control villages:

$$(1)\ \ Enrolled_{igvt} = \delta HealthStatus_{igvt-1} + \vartheta_{igvt}$$

where *Enrolled* is an indicator for whether the individual enrolled in DAN; *HealthStatus* is a proxy for the individual's health in the time prior to the introduction of MHI, and subscript *i* indicates the individual, subscript *g* indicates the SHG to which the household belongs, subscript *v* indicates the village, and subscript *t* indicates the time period where the MHI was offered to the household.

I additionally estimate Eqn (1) restricting observations to only SHG members to test whether δ reduces in magnitude because of the mandatory element of group enrollment of MHI. To test whether households selectively choose additional family members to enroll, I expand Eqn (1) to include household fixed effects. A comparison between δ estimated when restricted to only SHG members versus how households enroll additional family members provides an understanding of whether the added group eligibility requirement provides greater protection against adverse selection. Eqn (1) is also estimated at the household level to provide suggestive evidence of expanding on requirement rules where members were not able to choose which members to enroll. An additional measure of testing for enrollment selection is to observe whether households who enroll multiple members differ by health status:

$$(2)\ NumberEnrolled_{hgvt} = \gamma HealthStatus_{hgvt-1} + \vartheta_{hgvt}$$

where *NumberEnrolled* is the number of household members enrolled by the SHG member, subscript *h* 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 time period where the MHI was offered to the household.

MHI Effects:

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.

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

where *y* is the outcome of interest, *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.

Time Variation

A series of robustness tests are conducted to estimate whether the above ITT effects are consistent. This includes trimming and controlling for potential pre-existing differences (both directly controlling for observable differences and using propensity score matching) between the treatment and control arms for the estimated β from Eqn (3).

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

$$(4)\ y_{hgvt} = \theta Enrolled_{hgvt} + \alpha_h + \varepsilon_{hgvt}$$

where α_h 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 θ to be consistently estimated is the timing of the enrollment may not be exogenous. For example, we may expect that households choose to become enrolled into the health insurance contract when they foresee health consumption in the near future – biasing θ upwards. Table 4 shows the average duration between enrollment and the submission of the first claim. On average, households submitted a claim 7 months after being enrolled in DAN, suggesting that such endogenous timing of enrollment is not a concern.

Difference-in-Difference Observing Future Enrollment:

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.

$$(5)\ y_{igvt} = \theta_1 TreamentVillage_v + \theta_2 Enrolled_{igvt} + \theta_3 TreatmentVillage * Enrolled_{igvt} + BlockFixedEffects_v + \varepsilon_{igvt}$$

where *Enrolled* is now an indicator of the individuals who became in the treatment ***and*** control villages. Though there may be differences between the type of person who enrolled in the treatment villages almost two years earlier, it is likely that these individuals are similar. θ_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), θ_2 is a measure of the

type of individuals who choose to enroll in the program, and θ_3 is the parameter of interest – the effect of the insurance on those who enrolled (as opposed to the insurance offer). Eqn (5) is estimated using only households who enrolled in the first seven months of the insurance offer in treatment villages to be more comparable to the length of time observed in control villages. Because the non-enrolled treatment village individuals will now include some that were actually treated at the time of the data collection, θ_3 can be considered a lower bound of the ATET.

IV. 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, insured individuals will 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):

$$(6)\ EU = (1+\beta)u(Y - P * H_1, \overline{H} + H_1),$$

$$s.t. P * H_1 < Y$$

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:

$$(7)\ EU = (1-\pi_s)(1+\beta)u(Y, \overline{H}) + \pi_s[u(Y, \overline{H}) + \beta u(Y - P * H_2, \overline{H} - \theta + H_2)],$$

$$s.t. P * H_2 < Y$$

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, $\overline{H}$ 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.

V. Data

The primary data source is an Endline Household Health Survey conducted in October 2012 on a randomly selected subsample of the population, approximately 18 to 21 months after the insurance was introduced in treatment villages. 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 Household Health 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 Household Health 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 collecting data on 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. MFI records for loans across SHGs (i.e., larger loans that go through the MFI) were collected from August 2011 to September 2012.

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

The data collected should be considered Midline and Endline data for treatment villages, but are baseline information for control villages – those villages not yet offered the MHI.

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.

Summary Statistics

Table 5 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 laborers for employment. The population is approximately 50 percent female, has an average education level of 6^{th} grade, and an average age of 31.

Table 5 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 6 reviews the pilot SHG surveys to further explore the concern of pre-existing differences between treatment and control villages. The health data for Table 6 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 enrollments 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 potentially lower levels of illness even prior to the insurance program.

Due to these concerns of imbalance between treatment and control arms, the main tables in the remainder of the paper will control for the characteristics in Table 5 and 6. Due to non-response rates in these control characteristics, primary results without these controls are provided in the appendix.[7]

Survey Non-Response

Survey non-response is also a primary concern for the consistent estimates of the insurance program. The response rate of the October 2012 Endline Survey was 80% in both treatment villages and control villages (see Table 7). 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. There is some additional non-response for the individual questions on the survey. 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 expenditure questions.

[7] Though I attempt to control for these initial health variables by including them as independent variables in estimations, 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.

Unlike the Endline survey, the SHG Monthly Surveys suffered from even higher non-response rates and differential response rates by treatment status. As a result, the primary ITT estimates of the paper include Lee (2009) bounds[8].

VI. Empirical Estimations

MHI Demand:

Table 8 reports differences among enrolled and non-enrolled individuals in control villages. As Column (1) indicates, individuals enrolled in MHI generally tend to be older, are more likely to have experienced illness and had higher health expenses in the week prior to the survey. Column (2) restricts the sample to only SHG Members and continues to find correlations between poor health and demand for MHI. This suggests that the group eligibility requirement was not successful in eliminating adverse selection in the total population or among just SHG members. Column (3) includes household fixed effects and illustrates that households are consciously choosing to enroll family members that are more prone to illness. Interestingly, these relationships are stronger in the week recall than the annual recall period, particularly for the sample restricted to SHG members. It may be the case that the week recall period are better proxies for the individual's current health status, or it may be that the product was effective in controlling for adverse selection, at least among its members, on large health incidents (which is more likely to be dominant in the year recall period). Table 9 confirms the selection by documenting that households with more illness are more likely to enroll additional family members.

Incident of illness, health care utilization, health expenditure

[8] Bounding by the other method was also done at the quintile levels, but created bounds too large to have any meaningful contribution (not shown).

Table 10 estimates the intent to treat on household data from Eqn (1) using data collected in the Endline Household Health Survey. When asked about the previous week's health incidents, treatment villages report .2 more household members being sick in the past week, but have slightly lower health expenditure (approximately USD 4/week). When asked about the previous year, however, I find that treatment areas are 7 percentage points less likely to have had a major health shock, and have substantially less health expenditure (approximately $USD 135). These findings are robust to Lee (2009) bounds, and propensity score matching estimates suggest even slightly larger ITT effects.

Table 11 estimates ITT looking at individual level data and finds a similar effects for year recall, though with lower magnitudes, as those found in Table 10. For the week recall, the effects are reduced and are no longer statistically significant. The propensity score matching techniques find similar results on the year recall, but now even switch signs on the week recall. The estimations from Table 10 and 11 are similar even without the additional controls (Appendix Table 2, Appendix Table 3).

One possible explanation for the differences in the week versus the year 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). 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 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 10 and 11 suggests that even when ill, treatment villages have lower health expenditure. This is consistent with illnesses in treatment village households being less severe and the receipt of discounts from the insurance itself.

TET: Difference-in-Difference Using Enrolled Members in Control Villages:

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 12 estimates a difference-in-difference of household demographics among households and individuals enrolled in the first seven months of the MHI's initial offer in control versus treatment villages. These enrolled households/individuals do not have statistically significant differences by treatment status, except for slight differences in education levels. This suggests that the enrollment process is similar in treatment and control villages and that the difference-in-difference strategy outlined in Eqn (5) will provide consistent estimates of ATET despite pre-existing differences/imbalance between treatment and control villages. Table 13 estimate the TET and at the household level appears too underpowered to estimate the TET consistently – the point estimates depict a similar story as the ITT, with lower magnitudes, on the health expenditure, but have switched signs on illness levels. Household enrollment is defined as a household having at least one member

enrolled, but does not differentiate between households who enroll did number of family members. Furthermore, Appendix 4 now suggests that controlling for additional characteristic does change the sign of the estimates. Propensity score estimates of the average TET continues to find reductions in health expenditure, but also find a reduction in week and year illness, though the magnitude of the latter has dropped dramatically. Because, all results are statistically insignificant, it is not possible to differentiate whether there were no effects or if the statistical power is too low to detect them.

At the individual level, the TET finds statistically significant opposite effects of week illness than those estimated in the ITT, and similar estimates for year recall. The estimates also suggest increases in health care expenditure, particularly when conditional upon illness. The propensity score estimates suggest the same sign of those found in the ITT estimated in Table 11, but the magnitudes of all estimates have fallen dramatically.

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.

Additional Robustness: Time Variation

Table 15 uses the timing of the household enrollment, along with member fixed effects, to compare households before and after enrollment. The estimates suggest that even when comparing within households, households are less likely to be ill and have lower 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 does assume parallel time

trends between those who insure relative to those who do not, and does not differentiate between how many members were enrolled in the household.

Panel B tests for whether households initially experience an increase in health events and a subsequent decrease, which would be consistent with the long run health improving due to initial increases in seeking health care. Though panel B suggests that the initial months of enrollment do not see the magnitude of the decrease in health incidents, even in those early months of being enrolled households experience a net decrease in health incidents relative to when they were not enrolled.

Potential Mechanisms and Indirect Effects:

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 16, Panel A, based on self-reported Endline survey data, finds no effect on selling or mortgaging of assets, or on the total amount of outstanding loans, though less debt appears to be directed towards health needs. Reviewing Chaitanya's own administrative data, Table 16 Panel B, suggests mixed results on loans – though treatment village households have no differences in outstanding loans, they are .4 percentage points less likely to undertake new loans.

Table 16, Panel C, estimates household behavior 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.

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.

VII. Conclusion

The success and effectiveness of insurance contracts are critically dependent on the demand for MHI and changes in the demand for health care conditional upon enrollment. MHIs are concerned with both improving the take up of products and making insurance available to those who need it most, but it is unclear whether such efforts will increase or deter adverse selection. To the extent that an increase in health care consumption is often an indirect goal of community based insurance providers, it raises concerns of the financial stability of the insurance contract. Contrary to the majority of studies evaluating MHI, I find relatively high demand for the insurance, at least among the SHG members themselves. Given the low enrollment of additional family members, it suggests that the group eligibility rule may have been beneficial in increasing the uptake of the insurance. Unlike Banerjee et al. (2014), I do not find that the members were so compelled to purchase the insurance that there was no relationship between health needs and enrollment. In contrast, I find that those individuals with poor health are more likely to demand insurance, both among SHG members and even within households. This

suggests that the group eligibility rule falls short of completely eliminating concerns of adverse selection, though it does successfully reduce it. Though adverse selection continues to be a threat, I fail to find convincing evidence of increases in health care utilization among enrolled members. Instead, I find limited suggestive evidence of the insurance possibly reducing the consumption and expenditure of health care. The evidence on the MHI reducing health care expenditure is also mixed.

I find some evidence that MHI improves the health status of those enrolled as proxied by reduced self-reported levels of illness. In general, the potential of MHI to increase health status and lower the amount of health care warrants further research. Numerous factors in the design of the MHI 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 MHI programs.

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Table 1: Enrollment Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	All Villages		Treatment Villages		Control Villages	
	Panel A: Households					
Households Enrolled as of:	Obs	Percent	Obs	Percent	Obs	Percent
October 2012	2,625	30%	1,311	61%	1,314	0%
(Household Health Survey)						
June 2013	2,625	47%	1,311	62%	1,314	33%
(7 months after Initial MHI Offer to Control HH)						
7 months after Initial MHI Offer	2,625	31%	1,311	30%	1,314	33%
	Panel B: Individuals					
Individuals Enrolled as of:	Obs	Percent	Obs	Percent	Obs	Percent
October 2012	9754	9%	4370	20%	5384	0%
(Household Health Survey)						
June 2013	9754	15%	4370	20%	5384	11%
(7 months after Initial MHI Offer to Control HH)						
7 months after Initial MHI Offer	9754	10%	4370	8%	5384	11%

Notes:
Enrolled Household: Indicator for whether the household had at least one member insured for at least one year.
Individual data represents 1703 of the 2625 households.

Table 2: Enrolled Households

	(1) Mean	(2) SD	(3) Min	(4) Max
HH Members Enrolled	1.932	1.40	1	8
HH Submitted Claim	0.0977	0.30	0	1
Claim Disbursement (*INR per household*)	253.3	1,087	0	9,985
	(USD 5)	(USD 22)	(USD 0)	(USD 200)
Individual Submitted Claim	0.052	0.221	0	1
Claim Disbursement (*INR per individual*)	82	520	0	5967
	(USD 2)	(USD 10)	(USD 0)	(USD 119)

Notes:
Summary statistics are given for the subset of households/individuals which were enrolled for at least one year (798 households, 872 individuals) as of October 2012.
Individual dataset represents 1703 of the 2625 households.

Table 3: Claim Summary Statistics

	(1) Claimable Expense	(2) Disbursed Amount
Mean Amount (INR)	5,533 (USD 111)	2,911 (USD 58)
25% (15 cases) Malaria Claims (INR)	5,161 (USD 103)	2,371 (USD 47)
42 % (25 cases) Enteric Fever/Typhoid Claims (INR)	4,812 (USD 96)	2,863 (USD 57)
Amount Disbursed/Claimable Expense > .60: 75%		
Amount Disbursed/Claimable Expense > .50: 92%		

Summary statistics are based on 62 claim cases; Claims for which the claimable expense exceeding INR 25,000 are excluded (3 claims: mean claimable expendable INR 45,135; mean disbursed amount INR 8,662).

Table 4: Duration Between Enrollment and First Claim (Conditional upon Claim Submission)

	(1)	(2)	(3)	(4)	(5)
	Panel A: Summary Statistics				
	Obs (HH)	Mean	SD	Min	Max
Duration Between Enrollment and	64	7.39	4.27	0	18
	Panel B: Duration by Poverty Status				
Dependent Variable:	Duration Between Enrollment and First Claim (Months)				
Below the Poverty Line (BPL)			0.459		
			(2.047)		
Constant			8.472***		
			(0.961)		
Obs (HH)			31		

Notes:

Observations are limited to treatment households which submitted a claim as of October 2012.

Observations in Panel B are limited to treatment households selected and surveyed in the Household Health Survey and are weighted to be representative of the target population.

Standard errors are in parentheses and are robust standard errors.

Statistical significance levels are as follows: *10%, **5%, ***1%.

Table 5: Demographics Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All Households		Treatment Households		Control Households		Treatment-Control
	Obs	Mean	Obs	Mean	Obs	Mean	
				Households			
Below the Poverty Line (BPL)	*0.411*	*1599*	*0.358*	*705*	*0.463*	*894*	0.00667
Above the Poverty Line (Ration Card)	*0.0206*	*1615*	*0.0295*	*710*	*0.0118*	*905*	0.0160**
Subsidized (Ration Card)	*0.569*	*1615*	*0.612*	*710*	*0.526*	*905*	-0.0226
Below the Poverty Line (Ration Card)	*0.381*	*1615*	*0.327*	*710*	*0.435*	*905*	-0.00375
AAY Stamp	0.0292	1615	0.0311	710	0.0272	905	0.0104
House Type	1.635	1675	1.555	744	1.715	931	-0.0683**
Agricultural Laborer (in past year)	0.670	1687	0.639	749	0.701	938	-0.0189
Disadvantaged Caste	0.534	1594	0.367	707	0.703	887	-0.224***
Agricultural Cultivator	0.768	1682	0.770	748	0.766	934	0.0161
Hindu	0.943	1662	0.924	739	0.963	923	-0.0358***
Household Size	5.723	1686	5.678	748	5.768	938	-0.0599
				Individuals			
Female	0.502	9530	0.509	4239	0.496	5291	0.0181
Age	31.30	9581	31.30	4266	31.30	5315	-0.473
Education	6.097	9393	6.018	4255	6.166	5138	-0.0527

Notes:

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

All variables are indicators except for House Type (1 to 3, increasing with worse infrastructure) and Education (0 to 15, increasing in higher education with 0 - 12 representing each additional promotion in school and each additional increment representing some college, bachelors degree, and post graduate degree respectively.)

Column 7 includes block fixed effects with robust standard errors.

Table 6: Pilot SHG Surveys by Treatment Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)	(8)	(10)
	Collected Data		Ill		Admitted		Bedridden		Large Health Expense	
	Feb	Jul	Feb	Jul	Feb	Jul	Feb	Jul	Feb	Jul
					Block 1					
Treatment Village	0.0235	0.0684	-0.100*	0.102*	-0.00291	-0.00425	-0.0113	0.0527**	-0.0426	0.0173
	(0.0735)	(0.0865)	(0.0576)	(0.0609)	(0.0274)	(0.0227)	(0.0345)	(0.0216)	(0.0387)	(0.0217)
Constant	0.861***	0.778***	0.443***	0.394***	0.0865***	0.0551***	0.156***	0.0634***	0.189***	0.0837***
	(0.0583)	(0.0701)	(0.0417)	(0.0491)	(0.0195)	(0.0191)	(0.0256)	(0.0120)	(0.0280)	(0.0149)
Obs (SHG)	88	88	77	72	77	72	77	72	77	72
					Block 2					
Treatment Village	-0.0342	-0.0105	-0.168	-0.109	-0.119**	0.00270	-0.119***	-0.0221	0.0117	0.0282
	(0.155)	(0.133)	(0.139)	(0.0812)	(0.0469)	(0.0333)	(0.0384)	(0.0500)	(0.0644)	(0.0258)
Constant	0.350***	0.800***	0.598***	0.504***	0.187***	0.0706***	0.207***	0.126**	0.216***	0.0808***
	(0.109)	(0.0918)	(0.0844)	(0.0585)	(0.0399)	(0.0210)	(0.0238)	(0.0466)	(0.0342)	(0.0189)
Obs (SHG)	39	39	13	31	13	31	13	31	13	31
					Block 3					
Treatment Village	-0.0958	0.0750	-0.0661	-0.148*	-0.00561	-0.0780***	0.0710	0.0376	0.0733	0.0376
	(0.157)	(0.113)	(0.0840)	(0.0736)	(0.0242)	(0.0219)	(0.0534)	(0.0722)	(0.0544)	(0.0722)
Constant	0.533***	0.800***	0.505***	0.421***	0.0329*	0.0971***	0.0827***	0.0702***	0.0938***	0.0702***
	(0.0931)	(0.0747)	(0.0475)	(0.0368)	(0.0171)	(0.0177)	(0.0231)	(0.0149)	(0.0209)	(0.0149)
Obs (SHG)	46	46	23	38	23	38	23	38	23	38

Notes:

Dependent variables represent the proportion of members in which their household experienced the given incident in the past month (ill, admitted) or past three months (bed ridden, large expense).

Bedridden is an indicator of a household member being bedridden for a minimum of 3 or 5 days and large expenses are an indicator of health expenditures with a minimum of USD 20 or USD 1000, for Feb and July respectively.

Standard errors are robust.

Table 7: Survey Response Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Control Villages			Treatment Villages		
	Resonse Rate	Obs	Obs Unit	Response Rate	Obs	Obs Unit
Endline Household Health Survey	80%	1152	HH	80%	916	HH
SHG Monthly Survey	49%	13140	HH Month	62%	13110	HH Month

Notes:

Observations for Endline Household Health Survey are weighted to be representative of the target population.

Table 8: MHI Demand

	(1)	(2)	(3)	(4)
	Dependent Variable: Enrolled			
Age	0.00124***	0.000958	0.00132***	-0.0000760
	(0.000348)	(0.00169)	(0.000190)	(0.000138)
Obs (Individual)	4497	840	4497	3657
Education	-0.00229	-0.00281	-0.00439***	0.000316
	(0.00156)	(0.00581)	(0.000942)	(0.000547)
Obs (Individual)	4352	808	4352	3544
Below the Poverty Line	-0.00600	-0.0859		
	(0.0240)	(0.0650)		
Obs (Individual)	4303	802	4303	
Ill (Week Recall)	0.124***	0.0898**	0.0936***	0.0256**
	(0.0255)	(0.0389)	(0.0154)	(0.0119)
Obs (Individual)	4502	840	4502	3662
Health Expenditure (Week Recall)	0.0000329***	0.0000473**	0.0000191*	0.00000181
	(0.0000109)	(0.0000225)	(0.00000974)	(0.00000750)
Obs (Individual)	4502	840	4502	3662
Health Shock (Year Recall)	0.0963***	0.0208	0.0632***	0.0195
	(0.0222)	(0.0458)	(0.0158)	(0.0119)
Obs (Individual)	4489	837	4489	3652
Health Expenditure (Year Recall)	0.000000316	-0.00000254***	0.000000502	0.000000324
	(0.000000398)	(0.000000843)	(0.000000352)	(0.000000313)
Obs (Individual)	4502	840	4502	3662
Sample	All Individuals	SHG Members	All Individuals	Non-SHG Members
Household Fixed Effects	No	No	Yes	Yes

Notes:
Observations limited to individuals in control villages.
Each row represents a separate regression.
Standard errors are clustered at the SHG level for Col 1 - 2 and are robust for Col 3 - 4.
Week recall variables: Self-reported illness and subsequent health expenditure.
Year recall variables: Ill is an indicator for whether the individual experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total expenditure for all such events.
Health expenditure variables have been windsored at the 99% level.

Table 9: MHI Demand for Additional Household Members

Dependent Variables:	(1) Age Mean	(2) Education Mean	(3) BPL	(4) Number of HH Members Ill (Week Recall)	(5) Health Expenditure (Week Recall)	(6) Illness (Year Recall)	(7) Health Expenditure (Year Recall)
1[Enrolled Only Self]	0.00940	-0.0972	-0.157**	-0.0404	87.59	-0.0280	-1705.1
	(0.965)	(0.214)	(0.0712)	(0.0623)	(137.7)	(0.0433)	(2122.8)
1[Enrolled Additional HH Members]	-0.579	0.308	-0.0666	0.357***	631.1***	0.108*	5765.0
	(1.301)	(0.407)	(0.0840)	(0.0847)	(221.9)	(0.0622)	(3550.6)
Constant	33.01***	6.181***	0.516***	0.712***	473.2***	0.555***	17845.6***
	(0.649)	(0.122)	(0.0418)	(0.0431)	(60.84)	(0.0266)	(1501.8)
Obs (HH)	927	925	894	907	889	933	925

Notes:

Enrollment is an indicator of at least one household member enrolling into the insurance program.

Observations limited to individuals in control villages and weighted to be representative of the target population.

Standard errors are clustered at the SHG level.

Week recall variables: Self-reported illness and subsequent health expenditure for the household.

Year recall variables: Ill is an indicator for whether any individual in the household experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total health expenditures reported by the household for the past year.

Health expenditure variables have been windsored at the 99% level.

Table 10: ITT (Household)

	(1)	(2)	(3)	(4)
	HH Members Ill (Week Recall)	Health Expenditure (Week Recall)	Illness (Year Recall)	Health Expenditure (Year Recall)
Treatment Village	0.194**	-227.0**	-0.0672*	-6284.7***
	(0.0785)	(89.14)	(0.0336)	(1935.2)
Obs (HH)	1450	1418	1486	1477
Upper Lee Bound	0.196**	-225.6**	-0.0646*	-6150.5***
	(0.0790)	(89.32)	(0.0339)	(1956.0)
Lower Lee Bound	0.180**	-236.6**	-0.0679*	-6725.3***
	(0.0783)	(89.63)	(0.0337)	(1916.9)
Obs (HH) when Bounded	1448	1417	1484	1475
	Propensity Score Matching			
Treatment Village	0.361	-4.422	0.015	-2072.838
	Conditional on illness in given recall period			
Treatment Village		-442.8***		-7646.8**
		(128.2)		(3441.5)
Obs (HH)		834		790

Notes:

Observations are weighted to be representative of the target population.

All regressions include controls listed in Table 5 and Table 6. These are the controls used to estimate the propensity score.

Regressions include block fixed effects and standard errors are clustered at the village level.

Week recall variables: Self-reported illness and subsequent health expenditure for the household.

Year recall variables: Ill is an indicator for whether any individual in the household experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total health expenditures reported by the household for the past year.

Health expenditure variables have been windsored at the 99% level.

Table 11: ITT (Individual)

	(1) Illness (Week Recall)	(2) Health Expenditure	(3) Illness (Year Recall)	(4) Health Expenditure
Treatment Village	0.0133	-39.02**	-0.0118	-791.0***
	(0.0112)	(15.95)	(0.00805)	(210.8)
Obs (Individual)	8360	8360	8342	8342
	Propensity Score Matching			
Treatment Village	-0.070	-27.817	0.004	-1165.457
	Conditional on illness in given recall period			
Treatment Village		-406.7***		-6772.5***
		(120.9)		(2080.4)
Obs (Individual)		1069		775

Notes:

All regressions include controls listed in Table 5 and Table 6. These are the controls used to estimate the propensity score.

Regressions include block fixed effects and standard errors are clustered at the village level.

Week recall variables: Self-reported illness and subsequent health expenditure for the individual.

Year recall variables: Ill is an indicator for whether the individual experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total expenditure for all such events.

Health expenditure variables have been windsored at the 99% level.

Table 12: Differential MHI Demand by Treatment Status

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Early Enroller					
BPL	-0.00692			0.0276		
	(0.0563)			(0.0226)		
Age		-0.00185			0.00116***	
		(0.00188)			(0.000348)	
Education			-0.00484			-0.00280*
			(0.00976)			(0.00147)
Treatment Village	-0.0863	-0.0510	-0.314***	-0.0176	-0.0230	-0.0588*
	(0.0838)	(0.115)	(0.104)	(0.0310)	(0.0299)	(0.0301)
Treatment Village * Covariate	**-0.0681**	**-0.00195**	**0.0326****	**-0.0554**	**-0.000580**	**0.00330***
	(0.0927)	**(0.00285)**	**(0.0147)**	**(0.0338)**	**(0.000408)**	**(0.00190)**
Obs (HH)	1599	1670	1667	7909	8293	8138

Notes:

All regressions include controls listed in Table 5 and 6. These are the controls used to estimate the propensity score.

Regressions include block fixed effects and standard errors are clustered at the village level.

Columns 1 - 3 are household level regressions, Column 4 - 6 are individual level regressions.

after the introduction of MHI.

Columns 1 - 3 are weighted to be representative of the target population.

Column 2 and 3 use age and education means of the household for the independent covariate.

Table 13: TET (Household)

	(1)	(2)	(3)	(4)
	HH Members Ill (Week Recall)	Health Expenditure (Week Recall)	Illness (Year Recall)	Health Expenditure (Year Recall)
Treatment Village	0.209**	-99.42	-0.0675*	-5396.1**
	(0.0891)	(98.38)	(0.0381)	(2419.0)
Early Enroller HH	0.123	239.9	0.0406	-207.8
	(0.0846)	(160.5)	(0.0325)	(2286.2)
Treatment Village * Early Enroller HH	**-0.0193**	**-326.7**	**0.00888**	**-2662.9**
	(0.127)	(208.1)	(0.0422)	(2553.7)
Obs (HH)	1450	1418	1486	1477
	Propensity Score Matching			
Early Enroller HH	0.0775821	-712.77481	-0.06849243	-6041.9403
	Conditional on illness in given recall period			
Treatment Village		-276.2**		-6825.9
		(135.5)		(4204.9)
Early Enroller HH		174.3		-471.5
		(198.9)		(3761.4)
Treatment Village * Early Enroller HH		**-412.2**		**-2789.9**
		(250.8)		(5288.5)
Obs (HH)		834		790

Notes:

Observations are weighted to be representative of the target population.

All regressions include controls listed in Table 5 and Table 6. These are the controls used to estimate the propensity score.

Regressions include block fixed effects and standard errors are clustered at the village level.

Early Enroller HH is an indicator for whether the household had at least one member enroll in the insurance after 7 months of the initial MHI offer.

Week recall variables: Self-reported illness and subsequent health expenditure for the household.

Year recall variables: Ill is an indicator for whether any individual in the household experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total health expenditures reported by the household for the past year.

Health expenditure variables have been windsored at the 99% level.

Table 14: TET (Individual)

	(1)	(2)	(3)	(4)
	Illness (Week Recall)	Health Expenditure (Week Recall)	Illness (Year Recall)	Health Expenditure (Year Recall)
Treatment Village	0.0264**	-26.94*	-0.00485	-779.9***
	(0.0115)	(15.66)	(0.00715)	(232.9)
Early Enroller Ind	0.141***	128.9**	0.0659***	-67.57
	(0.0288)	(62.79)	(0.0134)	(305.4)
Treatment Village * Early Enroller Ind	**-0.0968*** **	**-90.69**	**-0.0595** **	**56.87**
	(0.0325)	(64.11)	(0.0243)	(474.1)
Obs (Individual)	8394	8394	8376	8376
	Propensity Score Matching			
Early Enroller Ind	0.046	-28.492	0.020	-242.926
	Conditional on illness in given recall period			
Treatment Village		-405.5***		-7839.8***
		(123.9)		(2383.1)
Early Enroller Ind		-30.46		-6085.1***
		(125.0)		(1931.3)
Treatment Village * Early Enroller Ind		**184.6**		**7121.3** **
		(149.2)		(3267.5)
Obs (Individual)		1075		780

Notes:

All regressions include controls listed in Table 5 and Table 6. These are the controls used to estimate the propensity score.

Regressions include block fixed effects and standard errors are clustered at the village level.

Early Enroller Ind is an indicator for whether the individual had at least one member enroll in the insurance after 7 months of the initial MHI offer.

Week recall variables: Self-reported illness and subsequent health expenditure for the individual

Year recall variables: Ill is an indicator for whether the individual experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total expenditure for all such events.

Health expenditure variables have been windsored at the 99% level.

Table 15: Enrollment Time Variation

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Illness	Health Shock	Health Expenditure	Illness	Health Shock	Health Expenditure
Enrolled HH	-0.0764**	-0.0636***	-304.9***	-0.0833**	-0.0653***	-315.6***
	(0.0329)	(0.0170)	(111.3)	(0.0336)	(0.0170)	(117.2)
First Three Months of Enrollment				0.0322	0.00784	23.17
				(0.0246)	(0.0126)	(71.83)
Obs (HH Month)	12765	12757	10852	12765	12757	10852

Notes:

Regressions include household fixed effects with standard errors clustered at the household level.

Dependent variables are self-reported monthly recall for whether any member in the household experienced the event (illness, health shock), and the total health expenditure for the household in the past month.

Health shock is an indicator for being admited to a health facility or being on prolonged bedrest.

Health Expenditure is windsored at the 99% level.

Source: SHG Monthly Surveys

Table 16: Indirect Effects

	(1)	(2)	(3)	(4)
	Panel A: Year Recall on Health Related Loans			
Dependent Variable:	Has a health related loan	Outstanding Loan Amount (Rs.)	Health Related Debt (Year Recall)	
Treatment Village	-0.00878	-9519.8	-2114.0***	
	(0.0208)	(9855.0)	(732.5)	
Obs (HH)	1345	1347	1480	
	Panel B: Year Recall on Financial Activity			
Dependent Variable:	Mortgaged Assets	Sold Assets		
Treatment Village	-0.00328	-0.0311		
	(0.0300)	(0.0232)		
Obs (HH)	1486	1480		
	Panel C: Week Recall on Health			
Dependent Variable:	Days Waited Before Seeking Care	Foregone Treatment		
Treatment Village	-0.0634	0.0260		
	(0.134)	(0.0228)		
Obs (Individual)	1002	993		
	Panel D: Administrative Financial Data			
Dependent Variable:	Outstanding Amount	Outstanding Amount	New Loan	New Loan
Treatment Village	-429.4		-0.00373**	
	(519.4)		(0.00177)	
Enrolled (HH)		250.6		0.00726***
		(236.0)		(0.00165)
Obs (HH Month)	25116	60830	25116	60830

Notes:

All regressions include controls listed in Table 5 and Table 6.

All regressions include block fixed effects and have standard errors clustered at the village level, except Panel D, Col 2 & 4 which have standard errors clustered at the household level.

Panel C: Dependent variables are conditional upon being ill in the week; Days Waited is given a maximum value of 15, Foregone Treatment is an indicator of whether the individual reported not undertaking treatment recommended by a health professional.

Panel D: Enrolled (HH) is an indicator for whether the household is enrolled in the given month; Col 2 and 4 use household fixed effects and include a monthly time trend.

Source: Panel A - C use the Endline Household Health Survey; Panel D use Chaitanya administrative financial data.

Appendix Table 1: Baseline Villages After Randomization

	(1)	(2)	(3)	(4)	(5)	(6)
	Control Villages			**Treatment Villages**		
	Villages Original	Villages Baseline	Members Baseline	Villages Original	Villages Baseline	Women Baseline
Total	31	22	1314	30	21	1311
Block 1	9	9	583	8	7	801
Block 2	8	5	291	9	7	295
Block 3	14	8	440	13	7	215

Notes:

Baseline Member defined as member being present for at least one meeting in the three months prior to the intervention start. Baseline Village defined as having at least one baseline member.

Ordinary least regression estimates no statistically significant difference of villages dropped by treatment status.

Appendix Table 2: ITT (Household)

	(1)	(2)	(3)	(4)
	HH Members Ill (Week Recall)	Health Expenditure (Week Recall)	Illness (Year Recall)	Health Expenditure (Year Recall)
Treatment Village	0.166**	-127.1	-0.0637*	-5031.9***
	(0.0757)	(87.31)	(0.0359)	(1697.3)
Obs (HH)	1643	1591	1674	1663
Upper Lee Bound	0.167**	-125.9	-0.0616	-4986.8***
	(0.0766)	(87.30)	(0.0366)	(1689.0)
Lower Lee Bound	0.146*	-142.4	-0.0646*	-5291.5***
	(0.0776)	(87.00)	(0.0360)	(1741.7)
Obs (HH) when Bounded	1625	1589	1672	1661
	Conditional on illness in given recall period			
Treatment Village		-245.9**		-7466.0**
		(120.4)		(3029.0)
Obs (HH)		931		878

Notes:

Observations are weighted to be representative of the target population.

Regressions include block fixed effects and standard errors are clustered at the village level.

Week recall variables: Self-reported illness and subsequent health expenditure for the household.

Year recall variables: Ill is an indicator for whether any individual in the household experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total health expenditures reported by the household for the past year.

Health expenditure variables have been windsored at the 99% level.

Appendix Table 3: ITT (Individual)

	(1)	(2)	(3)	(4)
	Illness (Week Recall)	Health Expenditure (Week Recall)	Illness (Year Recall)	Health Expenditure (Year Recall)
Treatment Village	0.00613	-26.81*	-0.0118	-651.2***
	(0.0112)	(15.42)	(0.00939)	(217.7)
Obs (Individual)	9620	9620	9602	9602
	Conditional on illness in given recall period			
Treatment Village		-209.8**		-5076.4***
		(101.4)		(1863.9)
Obs (Individual)		1230		884

Notes:

Regressions include block fixed effects and standard errors are clustered at the village level.

Week recall variables: Self-reported illness and subsequent health expenditure for the individual.

Year recall variables: Ill is an indicator for whether the individual experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total expenditure for all such events.

Health expenditure variables have been windsored at the 99% level.

Appendix Table 4: TET (Household)

	(1)	(2)	(3)	(4)
	HH Members Ill (Week Recall)	Health Expenditure (Week Recall)	Illness (Year Recall)	Health Expenditure (Year Recall)
Treatment Village	0.179*	2.264	-0.0672*	-5528.1**
	(0.0889)	(105.1)	(0.0390)	(2185.8)
Early Enroller HH	0.128*	219.8	0.0194	-1191.7
	(0.0694)	(153.8)	(0.0346)	(2265.9)
Treatment Village * Early Enroller HH	0.00461	-325.5*	0.0173	1127.1
	(0.100)	(189.0)	(0.0418)	(2941.2)
Obs (HH)	1643	1591	1674	1663
	Conditional on illness in given recall period			
Treatment Village		-45.08		-9025.3**
		(130.5)		(3532.4)
Early Enroller HH		207.8		-2510.9
		(203.4)		(3617.4)
Treatment Village * Early Enroller HH		-510.8**		3888.4
		(250.4)		(5196.7)
Obs (HH)		931		878

Notes:

Observations are weighted to be representative of the target population.

Regressions include block fixed effects and standard errors are clustered at the village level.

Early Enroller HH is an indicator for whether the household had at least one member enroll in the insurance after 7 months of the initial MHI offer.

Week recall variables: Self-reported illness and subsequent health expenditure for the household.

Year recall variables: Ill is an indicator for whether any individual in the household experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total health expenditures reported by the household for the past year.

Health expenditure variables have been windsored at the 99% level.

Appendix Table 5: TET (Individual)

	(1) Illness (Week Recall)	(2) Health Expenditure (Week Recall)	(3) Illness (Year Recall)	(4) Health Expenditure (Year Recall)
Treatment Village	0.0193*	-13.85	-0.00277	-649.3**
	(0.0108)	(14.26)	(0.00788)	(242.4)
Early Enroller Ind	0.150***	119.7**	0.0847***	169.8
	(0.0188)	(47.02)	(0.0131)	(306.0)
Treatment Village * Early Enroller Ind	-0.0802***	-89.03*	-0.0676***	111.8
	(0.0255)	(49.01)	(0.0225)	(476.3)
Obs (Individual)	9623	9623	9605	9605
	Conditional on illness in given recall period			
Treatment Village		-217.3**		-6648.7***
		(101.9)		(2263.0)
Early Enroller Ind		-80.20		-7163.3***
		(124.5)		(2294.6)
Treatment Village * Early Enroller Ind		9.647		8013.7**
		(143.0)		(3634.6)
Obs (Individual)		1231		884

Notes:

Regressions include block fixed effects and standard errors are clustered at the village level.

Early Enroller Ind is an indicator for whether the individual had at least one member enroll in the insurance after 7 months of the initial MHI offer.

Week recall variables: Self-reported illness and subsequent health expenditure for the individual.

Year recall variables: Ill is an indicator for whether the individual experienced a health event that met one of the following criteria: expenditure greater than USD 20, required being admitted to a facility overnight or commuting for 2 or more days, or was on bedrest for 5 or more days; Health Expenditure is the total expenditure for all such events.

Health expenditure variables have been windsored at the 99% level.