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HEALTH SHOCKS AND CONSUMPTION SMOOTHING IN RURAL HOUSEHOLDS: DOES MICROCREDIT HAVE A ROLE TO PLAY?

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Health Shocks and Consumption Smoothing in Rural Households: Does Microcredit have a Role to Play?*

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Abstract

This paper estimates, using a large panel data set from rural Bangladesh, the effects of health shocks on household consumption and how access to microcredit affects households' response to such shocks. Our results suggest that even though in general consumption remains stable in many cases when households are exposed to health shocks, households that have access to microcredit appear to cope (slightly) better. The most important instrument used by households appear be sales of productive assets (livestock) and there is a significant mitigating effect of microcredit: households that have access to microcredit do not need to sell livestock to the extent households that do not have access to microcredit need to, in order to insure consumption against health shocks. The results suggest that microcredit organizations and microcredit per se have an insurance role to play, an aspect that has not been analyzed previously.

JEL Classification: O12, I10, C23.

Keywords: Health Shocks, Microcredit, Consumption Insurance, Bangladesh.

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1 Introduction

One of the biggest shocks to economic opportunities faced by households is major illness to members of the households. While health shocks can have adverse consequences for households in both developed and developing countries, they are likely to have a particularly severe effect on households in the latter, because these households are typically unable to access formal insurance markets to help insure consumption against such shocks.

The literature on the effects of health shocks on household outcomes in developing countries is quite large and the results are (surprisingly) mixed. For example Townsend (1994), Kochar (1995) and Skoufias and Quisumbing (2005) find that illness shocks are fairly well insured. Others (for example Cochrane (1991), Gertler and Gruber (2002), Dercon and Krishnan (2000), Asfaw and Braun (2004), Wagstaff (2007), Lindelow and Wagstaff (2007) and Beegle, Weerdt, and Dercon (2008) however find that illness shocks have a negative and statistically significant effect on consumption or income. One general conclusion that could be drawn from the existing literature is that the impact of health shocks is crucially dependent on the ability of the households to insure against such shocks. In particular the literature focuses on the role of credit, financial savings and other assets. For example Gertler and Gruber (2002), Jalan and Ravallion (1999), Besley (1995), Udry (1990), Rosenzweig and Wolpin (1993) and Fafchamps, Udry, and Czukas (1998) who all reach essentially the same conclusion: wealthier households are better able to insure against income shocks in general and health/illness shocks in particular.

This implies that financial institutions could have an important role to play in insuring consumption against income shocks. Unfortunately commercial financial institutions in developing countries are, more often than not, weak and do not adequately service the poor. These institutions are typically not conveniently located, have substantial collateral requirements and impose large costs on savings (Morduch, 1999). In contrast microfinance institutions hold substantial promise. The microfinance programs are typically targeted to the poor (and the near-poor), do not impose significant physical collateral requirements and actively promote savings.[1]

[1]We use the terms microfinance and microcredit interchangeably, though it needs to be remembered

The primary aim of this paper is to examine, using data from Bangladesh, the potential role of microcredit in enabling households to insure consumption against health shocks. Microcredit can help smooth consumption in a number of ways. It can help households diversify income and free up other sources of financing that can be used to directly smooth consumption. No collateral requirement for microcredit loans means that poor households can get loans more easily compared to the formal sector alternative. Credit from microfinance organizations and informal sources play a pivotal role in the daily life of households in rural Bangladesh. The impact of microcredit on income and consumption has been investigated in the literature. Pitt and Khandker (1998) find that access to microfinance significantly increases consumption and reduces poverty. Amin, Rai, and Topa (2003) find that poor households that join in a microcredit program tend to have better access to insurance and smoothing devices compared to those who do not. Pitt and Khandker (2002) find that microcredit can help smooth seasonal consumption. Their results indicate that households participation in microcredit program is also motivated by smoothing seasonal pattern of consumption and male labour supply, and that the effect of microcredit on consumption smoothing is greatest in the lean season. However the ability of these households and the role of microcredit in enabling households to insure against income shocks in general and health shocks in particular has not been examined previously. In this paper we use data from one of the largest ever panel data sets consisting of households in both treatment and control groups to examine the role of microcredit in enabling households insure against health shocks.

2 The Data and Descriptive Statistics

The paper uses three rounds of a household level panel data set from Bangladesh. This data is a part of a survey of treatment and control households aimed at examining the effect of microcredit on household outcomes. While four rounds of the survey were conducted (in 1997-1998, 1998-1999, 1999-2000 and 2004-2005), for purposes of this paper we use data from the first, third and fourth round of the surveys. The primary reason for ignoring

that microfinance is wider in scope compared to microcredit.

the second round, is that this survey round did not collect comprehensive information on consumption.[2] All the surveys were conducted during the period December - March, which implies that seasonal effects can be ignored. The 2004-05 survey contains data on participation status, including the amount of microcredit borrowing for each year after year 2000. Many of the participants dropped out of the program for one year or more and some of the non-participants became participants later.

The survey sampled around 3000 households in 91 villages spread evenly throughout the country, which were selected to reflect the overall spread of microcredit operations in Bangladesh. The attrition rate was low $-$ less than 10 percent from first round to fourth round. The final round of survey consists of 2729 households in 91 villages. Because of missing data on some key variables for 35 households, our final estimating sample consists of a balanced panel of 2694 households. The survey collected detailed information on a number of socio-economic variables including household demographics, consumption, assets and income, health and education and participation in microcredit programs.

Previous studies indicate that the measurement of the illness shock variables is important to detect the impact of illness on growth of consumption. For example, Cochrane (1991) finds no effect on the growth of consumption when illness is measured as dummy variable but finds substantial effect (consumption growth decreases by $11 - 14\%$) when days of illness > 100 in the last one year (major illness) is entered as dummy variable for the sickness. Respondents in our survey were asked about new or ongoing and past illness of all members in the household. We use this information to compute a number of different measures of household level health shocks. The first measure that we use is *whether any member of the household was sick during the last 15 days prior to the survey.* This measure, while being simple to understand and compute can suffer from measurement error in the form of self-reporting bias with the more educated and richer people typically reporting more episodes of days sick. Second, we use *the number days sick in the last 15 days for all working age members of household.* This measure reduces some of the problems associated with the first measure of illness (see for example Schultz and Tansel (1997) and Dercon and

[2]The data was collected by the Bangladesh Institute for Development Studies (BIDS) for Bangladesh Rural Employment Support Foundation with the help of financial assistance from World Bank. The first author was involved in the fourth round of data collection, monitoring and writing the final report.

Krishnan (2000)) in that this is a more objective measure and is subject to less reporting bias. The third measure used is *the number of days a member had to refrain from work or income earning activities if any member in the household was sick in the last 15 days.* The fourth measure is *whether the household incurred any big expenditure or loss of income due to sickness in the past one year.* The last measure is *whether the main income earner died in the last one year.* The first three measures capture short-term health shocks while the last two measures capture long-term health shocks.

The descriptive statistics presented in Table 1, Panel A show some interesting and significant variations across the three rounds of data that we use for purposes of estimation. First, 49% of households in the 1997-1998 survey report that some member was sick in the past 15 days, this goes down to 44% in the 1999-2000 survey and further down to 21% in 2004-2005. 82% of households in the 1997-1998 survey report some sickness in the past one year, 95% do so in the 1999-2000 survey and 47% in the 2004-2005 survey. Average number of days lost in the past 15 days due to illness varies from 3.1 in the 1997-1998 survey down to 1.36 in the 2004-2005 survey. The percentage of households experiencing a large shock in expenditure in the last one year ranges from 15.7% in 1997-1998 to 22.6% in 2004-2005. Up to 1.5% of households report death of the main earner in the family in the past one year.

Table 1, Panel B presents descriptive statistics on other socio-economic and demographic characteristics of the household. The average size of the household varies from 5.63 members in 1997-98 to 7.23 members in 2004-05. The years of education attained by the most education member of the household has increased from 5.48 years in 1997-98 to 7.27 years in 2004-05. The majority of households are male headed, though it is worth noting that the proportion of female headed households have doubled over the period 1997-1998 − 2004-2005.

The impact of illness shocks on consumption and the ability of households and other risks sharing institutions to smooth consumption can vary from one item to another. For example, Skoufias and Quisumbing (2005) find that adjustments in non-food consumption can act as a mechanism for partially insuring food consumption from the effects of income changes. So we use change in food and the change in non-food consumption expenditure as the two main outcome variables in our analysis. Non-food consumption is measured yearly since some of the items are purchased occasionally. Data on non-food expenditure includes items such as kerosene, batteries, soap, housing repairs, clothing, but excludes expenditure on items that are lumpy (e.g., dowry, wedding, costs of legal and court cases, etc.). We also exclude expenditure on health and medical care. For each food item, households were asked about the amount they had consumed out of purchases, out of own production and from other sources in the reference period. The reference period for the food item differ depending on the type of food consumed by rural households. Some food items (e.g., beef, chicken) are consumed occasionally (once or twice in a month), while others more frequently (e.g., rice, lentil). We aggregate all consumption, which is valued using the price quoted by the household (unit value) since commodities differ in terms of quality.[3] This way we obtain information on expenditure on food in the last month prior to the survey.

Table 1, Panel C reports the mean and standard deviation of food and non-food consumption at the household level. Average household consumption varies from 2433 Taka in 1997-1998 to 3214 Taka in 2004-2005.[4] There are significant fluctuations across the different rounds with a big increase in food consumption between 1997-1998 and 1999-2000. The share of non-food consumption (including health and medical expenditure) in total household expenditure is 21.1% in 1997-1998, which declined to 13.5% in 1999-2000 and then went back to 21.1% in 2004-2005. This change is in non-food consumption expenditure in 1999-2000 can be attributed partly to floods at the end of 1998, which affected most of the country.[5]

Table 2 presents selected descriptive statistics on credit demand and supply. As many as 30% of households had taken some loan from relatives, friends, or others in the past one year and surprisingly this number has decreased to 18% by 2004-2005. The average amount of loan taken from other sources (in the past one year) has however increased consistently

[3]These values are verified using prices collected from the local shopkeepers. These values are then deflated using the rural household agricultural index (1997-1998 = 100).

[4]Taka is the currency of Bangladesh: 1USD = 40 Taka in 1998

[5]Although 1999-2000 survey took place more than one year after the flood, a shock of that magnitude is likely to, and indeed did, have a fairly long run effect on household behaviour and outcomes.

from 4657 Taka in 1997-1998 to 9646 Taka in 2004-2005. The percentage of households who borrowed for consumption purposes has fallen, as has the percentage of households who borrowed to pay for medical expenses. Average loans taken by microcredit borrowers has however increased over time, with the percentage increase being highest between 1997-98 and 1999-2000.

3 Estimation Methodology

Complete risk sharing within the community will result in each household belonging to that community being protected from idiosyncratic risk.[6] Consumption will still vary but only because of the community's exposure to risk. The test for full consumption insurance is therefore a test of the validity of Pareto Optimality for the economy under consideration. Since the Pareto optimal consumption allocations are derived from the social planner problem, it turns out that the planner needs to solve the following maximization problem (Cochrane, 1991; Townsend, 1994):

$$\text{Max} \sum_i \sum_t \sum_s \mu_{is} \pi_s \rho^t u(c_{its}; \theta_{its}) \tag{1}$$

subject to

$$\sum_i c_{its} = \sum_i y_{its} \forall t, s \tag{2}$$

where π_s is the probability of state $s; s = 1, \ldots, S$, c_{its} household consumption, y_{its} is household income, μ_{is} is the time invariant Pareto weight associated with household $i; i = 1, \ldots, I$ in state s; ρ is the rate of time preference assumed to be the same for all households, θ_{its} incorporates factors that change tastes. Finally I is the number of households in the village. Assuming an exponential utility function

$$u(c_{its}; \theta_{its}) = -\frac{1}{\alpha} \exp\{-\alpha(c_{its} - \theta_{its})\} \tag{3}$$

and manipulating the first order conditions (and ignoring the notation for the state) we get

$$\Delta c_{it} = \Delta c_t^a + (\Delta\theta_{it} - \Delta\theta_t^a) \tag{4}$$

[6]The degree of consumption insurance is defined as the extent to which the growth rate of household consumption co-varies with the growth rate of household income.

where

$$\Delta c_t^a = \frac{1}{I}\sum_i c_{it} \text{ and } \Delta\theta_t^a = \frac{1}{I}\sum_i \theta_{it}$$

Equation (4) implies that under the assumption of full consumption insurance individual consumption c_{it} depends only on the community/village level average consumption c_t^a.[7]

An empirical specification follows immediately. Regress the change in the consumption of the i^{th} household on the change in the village level average consumption and other explanatory variables (for example socio-economic characteristics and health status of household members). Formally the empirical specification can be written as:

$$\Delta C_{ivt} = \alpha_0 + \alpha_1 H_{ivt} + \alpha_2 X_{ivt} + \beta \Delta C_{vt}^a + \varepsilon_{ivt} \tag{5}$$

where ΔC_{ivt} is the change in (real) consumption of household i in village v at time t, H_{ivt} is the health shock faced by household i in village v and time t and the error term ε_{ivt} includes both preference shocks and measurement error and is distributed identically and independently. The risk sharing model predicts that $\beta = 1$ and $\alpha_1 = 0$, i.e., health shocks should have no role in explaining household consumption growth.[8] This way we can identify whether rural households are vulnerable to transitory shocks such as illness shocks.

However Ravallion and Chaudhuri (1997) argue that this test gives biased estimates of the excess sensitivity parameter against the alternative of risk-market failure whenever there is a common village level component in household income changes. They suggest (and this is the method that we use in this paper) the use of the following specification:

$$\Delta C_{ivt} = \alpha_0 + \alpha_1 H_{ivt} + \alpha_2 X_{ivt} + \delta_v + \mu_t + (\delta_v \times \mu_t) + \varepsilon_{ivt} \tag{6}$$

[7]To examine how the Pareto Optimal allocation is attained in a decentralised economy, we assume the existence of a complete set of Arrow-Debreu securities. The existence of such securities allows us to decentralise the economy and examine whether full insurance can be attained through market mechanisms in such an economy. It can be shown that if there exists a complete set of Arrow-Debreu securities, the equilibrium consumption allocation will be identical to that obtained under a social planner's problem.

[8]Notice that the empirical specification uses the change in consumption rather than the level of consumption as the dependent variable because in this way potential omitted variable biases caused by the unobserved household characteristics can be avoided. Our model can therefore be viewed as the first-difference of a random growth model where we allow consumption growth to be different in different villages.

where δ_v represents village fixed effects, and μ_t represents the time effects, ε_{ivt} is the household-specific error term capturing the unobservable components of household preferences. Since changes in consumption in response to health shocks are typically characterized by substantial inter-household heterogeneity, we include in the set of explanatory variables a set of time varying controls at the household level (X_{ivt}). Changes in village-level consumption values are approximated by including village fixed effects (δ_v). Without village fixed effects, the regression may yield biased estimates because of possible correlation between the omitted or unobserved village characteristics and the error term. It also allows us to control for any aggregate or co-variate risks faced by all households in the village. The time dummies control for prices, and the interaction of the time dummies with the village fixed effects allows us to control for price changes that are village-specific over time. All standard errors are clustered at the village level.

If there is perfect risk sharing within the village then change in household consumption should not be sensitive to the idiosyncratic health shock H_{ivt}, once aggregate resources are controlled for, i.e., $\alpha_1 = 0$. The alternative of interest is $\alpha_1 < 0$.

As already mentioned, the primary aim of the paper is to examine the role of microcredit in enabling households insure against idiosyncratic shocks. To examine this, we estimate an extended version of equation (6) as follows:

$$\Delta C_{ivt} = \beta_0 + \beta_1 H_{ivt} + \beta_2 X_{ivt} + \beta_3 (H_{ivt} \times D_{ivt}) + \delta_v + \mu_t + (\delta_v \times \mu_t) + \varepsilon_{ivt} \qquad (7)$$

Here D_{ivt} is the treatment status of the household in a microcredit program and is measured by the amount borrowed. If households are unable to fully share the risk then β_1 will be different from zero, and the coefficient of interaction of treatment variable and health shock (β_3) then represents the effect of microcredit on changes in consumption.[9]

A major concern in estimating equation (7) is that the estimated coefficient of β_3 might be biased. This could be because of two reasons. The first is self-selection: for example, some households might *choose* not to participate in the microcredit program. Additionally microcredit programs are generally placed in selected villages. Fortunately, the availability

[9] We also make the following standard assumptions: separability of consumption and leisure, common rates of time preference and additively separable preferences over time.

of panel data at the household level allows us to consistently estimate the average treatment effect without assuming ignorability of treatment and without using instrumental variable (IV) estimation. Since we are using first-difference of the consumption variable, we eliminate the bias caused by households selecting themselves into the program based on any unobserved characteristic. While the first-differencing also eliminates village level unobserved characteristics that may cause non-random program placement, our use of village fixed effects in the first-differenced model accounts for any further village-specific growth or shocks or unobservables. So, there is no need to look for village level co-variates that may affect the program availability in a village. The impact of microcredit in mitigating health shock is identified by the difference between the treatment and the control households over time, conditional on controls.

The second reason for this bias is measurement error, which arises largely from the usual reporting problems. Measurement error of this kind would tend to induce an attenuation bias that biases the coefficient towards zero. In this case, OLS estimates provide a lower bound for the true parameters.[10] With fixed effects estimation, measurement error is likely to exacerbate the bias. So, we estimate the effects of microcredit on consumption smoothing using instrumental variable (IV) strategy to take into account of the possible measurement error. Note that the IV method is also useful if treatment status is correlated with the time-varying unobservables. In the context of Bangladesh there is a natural instrument available. Microcredit is typically offered to households who are eligible in the program village[11], defined as those households that own less than half-acre land. We use a dummy variable indicating whether or not the household is eligible in a program village as the instrument. To be more specific define $E = 1$ if the household is eligible and 0 if not; $P = 1$ if the household resides in a program village, 0 if not. The relevant instrument is $P \times E$, which takes the value of 1 if the household is eligible and resides in a program village.[12] It is important to note that the official eligibility criterion varies slightly

[10] However, imputation errors in the construction of consumption variable and reporting error in credit variable may bias the credit coefficient upwards (Ravallion and Chaudhuri, 1997). For a positive coefficient, this bias is in the opposite direction of the standard downward attenuation bias due to measurement errors, so that the net effect cannot be signed *a priori*.

[11] Credit is not available or offered to a household not living in a treatment village.

[12] However, it is to be noted that the primary purpose of using IV estimation here is not to tackle the endogeneity of program participation. It is more to address the issue of possible measurement error in

across the different microcredit organizations and over time. Discussion with microcredit borrowers and local officials of microcredit organizations indicate no significant difference among different microcredit organizationsas far as the eligibility status is concerned. Since land quality and price differ widely among different regions, a number of microfinance institutions have in the recent years relaxed the land-based eligibility criterion slightly (i.e., households with more land ownership are also eligible for microcredit). Our instrument is therefore time varying: for the first survey round (1997-98), our instrument is whether household owns less than half-acre land or less. We change this eligibility criterion to 0.75 acre for the 1999-2000 survey and to 1 acre for the 2004-2005 survey.

Before proceeding further it is worth re-iterating that we use two different outcome measures: change in food consumption and change in non-food consumption (excluding medical/health expenditure). Remember also that we use a number of different measures of health shock. They are:

- Whether any member of household was sick during the last 15 days prior to survey (binary variable)
- The number of days sick in the last 15 days for all working age members of household
- The number of days a member had to refrain from work or income earning activities if any member in the household was sick in the last 15 days
- Whether the household incurred any big expenditure or loss of income due to sickness in the past one year (binary variable)
- Whether the main income earner died in the last one year (binary variable)

the credit variable. Moreover, we control for household land ownership in our regression so any effect of land on consumption or other outcomes is adequately addressed. The exclusion restriction is the following: conditional on land-ownership and other socio-economic characteristics of the household, eligibility is independent of outcomes, given participation.

4 Estimation Results

4.1 Are Health Shocks Persistent?

The estimation methodology that we use in this paper (see Section 3) depends, crucially, on the assumption that health shocks are unpredictable and idiosyncratic in nature. Before we proceed to the results, we examine the validity of this assumption. In particular we examine whether households that experience health shocks in the current period are more likely to receive health shocks in the future i.e., whether health shocks are correlated over time. Morduch (1995) points out that if an income shock can be predicted beforehand, then households might side-step the problem by engaging in costly ex-ante smoothing strategies (e.g. diversifying crops, plots and activities). The data in such a situation would (incorrectly) reveal that income shocks do not matter. Although health shocks are less vulnerable to this critique than income shocks, the possibility exists.

To examine the issue of whether health shocks are persistent or not, we estimate the following regression:

$$H_{it} = \delta_i + \lambda H_{it-1} + \pi X_{it} + \varepsilon_{it} \tag{8}$$

Here H_{it} is some measure of health shock. The coefficient of interest is λ. If shocks are not persistent, i.e., households experiencing a shock in period $t-1$ are not significantly more likely to experience a shock in period t, then λ will not be statistically significant. Equation (8) is estimated as a fixed effect logit, with survey round dummies. Note that equation (8) is essentially a dynamic panel data regression model and the presence of the lagged dependent variable (H_{it-1}) results in an endogeneity problem. This implies that the fixed effects logit regression would give us biased and inconsistent estimates. To address this issue we use IV estimation, where the period $t-2$ (H_{it-2}) shock variable is used as an instrument for the lagged dependent (potentially endogenous) variable. This is specification IV1. In an alternative specification we use a set of exogenous variables to construct valid instruments for lagged dependent variable. This is specification IV2. Here we add household level characteristics with two period lag as instruments. We report results for the fixed effects and the two IV specifications in Table 3. None of the coefficient estimates are statistically significant at the conventional level (the t-ratio is always less than 1), irrespective of the shock variable that we use. These results imply that the health shocks as defined above are large, idiosyncratic and unpredictable and are relevant for studying the implications of the full insurance model.

4.2 Basic Results

Table 4 presents the results of the regression of equation (6) for the different specifications, with and without village and time fixed effects. The set of control variables X_{ivt} includes demographic characteristics of the household head, household size and composition, educational attainment and the amount of arable land owned by the household.

The baseline results presented in Table 4 indicate that health shocks experienced by the households do not have a statistically significant effect on changes in food or non-food expenditure.[13] Household consumption appears to be well insured against health shocks. It is worth noting that the estimated coefficients do not differ much with or without village-year fixed effects. This means that households rely almost exclusively on self-insurance to smooth consumption and that full insurance model at the village level may not be a correct specification for the sampled households. Similar results were obtained by Kazianga and Udry (2006) in case of rural Burkina Faso.

Table 5 presents the regression results for the extended baseline specification (equation (7)). Our interest is to examine whether participation in microcredit programs (measured by the amount of loans borrowed from a microcredit organization) help households better insure against health shocks of the kind discussed above. If microcredit does have a role to play in this respect the coefficient estimate of the interaction term ($\widehat{\beta_3}$) should be positive and statistically significant. It is interesting to note that this difference estimate is *always* positive (though not always statistically significant). There are therefore *some* mitigating effects of microcredit: the more credit the household has access to, the greater is the ability of the household to insure against health shocks.

[13]Note that the data on non-food consumption expenditure is available only at the year level. Accordingly we consider only the year level shock variables.

The IV/2SLS estimate of the effects of microcredit on consumption smoothing are presented in Table 6. Note that while neither the non-interacted term nor the interaction term is ever statistically significant, the signs are in the right direction: the coefficient estimate of β_1 is always negative and the coefficient estimate of β_3 is always positive. The IV estimates of β_3 are always larger than the corresponding OLS fixed effects estimates presented in Table 5. This indicates the measurement error in credit variable is indeed a possibility.

Given space constraints, we do not present the results for the additional controls, but they are available on request. However it is to be noted that the additional controls do not have a consistent and meaningful interpretation. In general we find that most of the household composition variables do not have a statistically significant effect on changes in household consumption.

4.3 How do Households Insure?

It appears (see Tables 5 and 6 and discussion in section 4.2) that health shocks do not have a statistically significant effect on household consumption. However, this need not be the end of the story. Indeed, it is important to examine what are the relevant institutions that enable households to insure against health shocks of this kind: after all markets in developing countries are incomplete. Our analysis thus far does not tell anything about how households insure. We next address this issue.

Potentially households could use a number of different means to insure consumption against income shocks. Morduch (1995) categorizes the different mechanisms into two broad categories: ex-ante income smoothing and ex-post consumption smoothing. The data set available to us enables us to examine the role of certain institutions in this context. In particular we focus on the role of credit, on the role of livestock and on the role of other assets. All of these can be categorized as being institutions that enable ex-post consumption smoothing by households.

Suppose, for example, households are able to borrow more in response to health shocks. In this case, we might not observe any changes in consumption as a result of health shocks faced by the households since they have engaged in ex-post consumption smoothing having already borrowed the amount of money to be either spent on health related expenditures and/or maintain the current level of consumption expenditure. For example access to microcredit might free up other sources of financing that can be used to directly smooth consumption. To explore this issue we examine whether the household responds to shocks by borrowing from any other source. The estimated equation takes the following form:

$$L_{ivt} = \alpha_0 + \alpha_1 H_{ivt} + \alpha_2 X_{ivt} + \delta_v + \mu_t + (\delta_v \times \mu_t) + \varepsilon_{ivt} \quad (9)$$

A positive and statistically significant estimate of α_1 implies that a household responds to a health shock by borrowing more from other sources.

We use three alternative measures of loans from other sources:

1. whether any loan was taken in the last one month (binary variable);
2. amount of loan taken in the last one month; and
3. amount of loan taken in the last one year

On the other hand we consider two health shock variables:

1. whether any member of the household has been sick in the last 15 days
2. whether the main household earner died in the last one year

The Fixed Effects Logit and the Random Effects Tobit regression results presented in Table 7 show that the death of the main earner in the household is associated with increased borrowing in the last one year. This result indicates that long term health shocks increase borrowing from other sources, such as relatives, friends or informal money lenders.

Households can also insure consumption by selling productive (for example livestock) or non-productive (for example consumer durable) assets.[14] Households that have access to

[14] The value of consumer durable is the aggregated current market value of items like radio, fans, boats and pots that are owned by the household. The information on the stock of assets is available only at the year level.

microcredit might have focused on its asset building and on the creation or expansion of one or more income generating activities compared to households that do not. Similarly, livestock is a very important asset in rural Bangladesh. A considerable portion of the households in our sample save in the form of investment in livestock. Almost all the households possess some livestock (e.g., cows, goats, chicken, ducks, etc.). There has been a considerable attention paid by previous studies on the role of livestock as a buffer stock.[15]

To examine the issue of how purchase and sale of assets and livestock is used to smooth consumption in response to health shocks, we estimate an equation similar to equation (7): the only difference being that here the dependent variable is the change in the values of assets owned by the household. The estimated equation is:

$$\Delta A_{ivt} = \beta_0 + \beta_1 H_{ivt} + \beta_2 X_{ivt} + \beta_3 (H_{ivt} \times D_{ivt}) + \delta_v + \mu_t + (\delta_v \times \mu_t) + \varepsilon_{ivt} \tag{10}$$

Here ΔA_{ivt} measures the change in ownership of non-land asset or livestock over two successive rounds of the survey. A negative and statistically significant β_1 implies that the household reduces its ownership of assets or livestock in response to a health shock. A positive and statistically significant β_3 implies that access to microcredit reduces the impact of the health shock and households do not need to take re-course to sale of assets to insure against health shocks. The 2SLS and OLS fixed effects estimates of the mitigating effects of microcredit on sale of assets and livestock are presented in Table 8. While the coefficient estimates of β_1 and β_3 do not have a systematic pattern in the case of change in ownership of non-productive assets, those for the change in ownership of livestock are much more systematic. The coefficient estimate associated with the health shock variable is always negative and generally statistically significant in the change in value of livestock regressions. In addition, the difference estimate is generally positive and statistically significant

[15] Fafchamps, Udry, and Czukas (1998) find limited evidence that livestock inventory serve as buffer stock against large variation in crop income induced by severe rainfall shock. They find that livestock sales compensate for 15-30 percent of income shortfalls due to village level shock. On the other hand in their study of consumption insurance and vulnerability in a set of developing and transitional countries Skoufias and Quisumbing (2005) find that loss of livestock do not have a significant negative effect on the growth rate of consumption per-capita. Kazianga and Udry (2006) also find little evidence of the use of livestock as buffer stocks for consumption smoothing. Instead they find households rely exclusively on self-insurance in the form of adjustments to grain stocks to smooth out consumption. Park (2006) finds that households who do not live very close to other households do sell off their livestock and other assets when experience a shock.

implying that households with access to microcredit are less likely to sell livestock in order to insure against health shocks. However the total effect is generally still negative (and statistically significant), implying that even these households (with access to microcredit) are not fully able to insure against health shocks and need to sell assets and livestock (in particular) to insure consumption.

4.4 Income Smoothing and Consumption Smoothing

Next we estimate the extent to which households are able to insure consumption. This magnitude is critical for assessing the importance of our findings for welfare and for considering their policy implications. Rather than directly examining the impact of microcredit, here we examine the role of transitory changes in income on consumption smoothing. If permanent income hypothesis model holds, then household would smooth consumption when facing temporary income fluctuations. We measure the extent to which households are not able to insure consumption against illness as the share of the costs of illness that are financed out of consumption. To do so, we estimate a model of the effect of changes in (net of medical spending) income on the growth of consumption. Specifically, we estimate the following regression:

$$\Delta C_{ivt} = \phi_0 + \gamma \Delta Y_{ivt} + \theta X_{ivt} + \delta_v + \mu_t + (\delta_v \times \mu_t) + \varepsilon_{ivt} \tag{11}$$

where Y_{ivt} is income minus medical care expenditure of household i in village v in year t.[16] If there is perfect income insurance within a village, then changes in household income will have no effect on consumption after controlling for common village and time effects, i.e., $\gamma = 0$. Income is however potentially endogenous because of the correlation of the error term with the growth in income and consumption. It is also likely to be measured with error. So we account both endogeneity and measurement error in income by instrumental variable estimation of equation (11). We use the health shock variable as the relevant instrument

[16] Income includes earnings from self-employment and business activities, net wages earned, net profits from crop and livestock production. It excludes net borrowing or savings and gifts received. It is to be noted that income is measured annually. So seasonal variation of income is not captured in our data. Some of the categories of income (such as income from household production and working in a household enterprise) are imputed.

for changes in income under the assumption that changes in consumption due to changes in income is due only to changes in income due to health shock. Table 9 presents the OLS and 2SLS results of the estimated coefficient γ. OLS estimates show that there is a significant but very small relationship between income changes and consumption changes. A 100 Taka increase in income is estimated to increase total (food and non-food) consumption by only 0.43 Taka. 2SLS coefficients are larger but are not statistically significant. This is possibly due to the lack of sufficient variation in income changes in response to health shocks.[17] The results essentially suggest that households are not fully able to smooth consumption in response to transitory income shocks and transitory income shocks induced by health shocks can have a long term effect on consumption. The coefficient estimates suggest that a 100 Taka increase in income is estimated to increase food expenditure by 12 Taka. The effects are much stronger for non-participants. A 100 Taka increase in income is estimated to increase consumption expenditure by 38 taka for the control group, while it does increase only by 1.3 Taka for the treatment group. Since health shocks reduces income, a positive coefficient, albeit indirectly, indicates that health shocks have negative influence on consumption smoothing and that the results are stronger for the control households.

4.5 Using Alternative Estimation Techniques

Our identification strategy is based on the implicit assumption of separability between consumption and health status. Otherwise, health status would change the marginal utility of consumption (see for example Gertler and Gruber (2002)). Therefore, α_1 in equation (6) might not be an unbiased estimator of the effect of idiosyncratic shock on changes in consumption because health shock may be correlated with omitted preferences (error term), biasing the estimated value of α_1 in equation (6). There are some additional estimation issues that need to be considered here. For example the perception of being sick or being healthy can vary considerably across households. This could lead to a significant measurement error problem. If measurement error is random, then we do not need to worry

[17]We use number of days sick in previous year as instrument for change in income. We also experiment with other health shock variables using these as instruments for changes in income, but none of them capture enough variation of changes in income − a result consistent with our earlier findings in regard to changes in consumption

about this. However, it is possible that likelihood of reporting illness is closely related to the socio-economic status of the household (for example the income of the household or the education level of the most educated member of the family). Additionally, our sample consists of households who have been exposed to the treatment and those who have not been. If households in the treatment group have a better knowledge about how to prevent sickness, or have better coping strategies because of training provided by the microcredit providers then we could expect that either households in treatment group are systematically less exposed to shock or even when they experience such a shock we would not observe significant changes in consumption because of the specific design of the microcredit program.

We can indeed adopt an IV strategy here to control for time-varying unobserved heterogeneity affecting the changes in consumption and health shocks.[18] For this, we need to search for a variable that is correlated with health shock but does not directly affect the changes in consumption expenditure and the variable is not correlated with idiosyncratic error term. Remember that past health does not have any persistent or permanent effects on current health. We cannot therefore use lagged health shock as instrument for current health shock. We experimented with past family income/consumption/household characteristics as the relevant instrument, but none of these appeared to be satisfactory. Lacking an identifying instrument, we choose to adopt the propensity score matching (PSM) strategy of Rosenbaum and Rubin (1983) that is now widely used in the program evaluation literature.[19] Typically we would expect that the likelihood of reporting illness is closely related to individual/household characteristics. We therefore match households based on their socio-economic status. We include a number of household characteristics and restrict our analysis to the matched sample. This controls for heterogeneity in initial socioeconomic conditions that may be correlated with subsequent health shocks and the path of consump-

[18]Unobserved heterogeneity that is time invariant in this context is automatically captured by our regression specification. The vector X in the regression controls for observed heterogeneity.

[19]In our case, PSM compares households who reported illness to those that did not, with the same (or similar) values of those variables thought to influence both illness and consumption. We can think households reporting illness in our sample as treatment group and the households that did not as the control group, following the program evaluation literature. Under the matching assumption, the only *remaining* difference between the two groups is reported sickness. Any difference in outcome between these two groups can be entirely attributed to the sickness effect provided we are able to have made sufficient arguments to guarantee that there are no further systematic differences between these two groups.

tion growth. To estimate the propensity score we estimate a conditional fixed-effects logit model with binary dependent variable whether a member of household was reported to be sick (=1) or not (= 0) using the panel data. So, unlike a cross-sectional propensity score estimate, we control for unobservables that might influence households reporting of sickness. We then discard observations that do not have any common support, and observations with households having very low or very high probability of sickness. We consider a caliper matching method, which uses all of the comparison units within a predefined propensity score radius. Therefore, we use only as many comparison units as are available within the calipers, allowing for the use of extra (fewer) units when good matches are (not) available (Dehejia and Wahba, 2002). We set the radius less than or equal to 0.00005, and discard about one-third of the observations from the sample (these do not have common support within this propensity score range). We combine matching with IV approach (to account for measurement error) to estimate the effects of health shocks and the role of microcredit in mitigating the consequences of health shocks. The results are reported in Table 10. The sign of the estimated coefficients are similar to that of 2SLS estimates using the full sample. The magnitude of the health shocks coefficients are, in general, larger using matched sample. The interaction terms of loan and health shock variables also indicate a larger coefficient estimates and most of them are statistically significant. Our results are again indicative of the role of microcredit in insuring households against idiosyncratic health shocks.

5 Conclusion

This paper examines, using a large panel data set from Bangladesh, the ability or otherwise of poor households to insure against idiosyncratic and unanticipated health shocks. Also, we assess the role of microcredit. Our results suggest that even though consumption remains stable in many cases when households are exposed to health shocks, households that have access to microcredit appear to cope (slightly) better. The most important instrument used by households appear be sales of productive assets (livestock). There is a significant mitigating effect of microcredit: households that have access to microcredit do not need to sell livestock to the extent households that do not have access to microcredit need to, in order to insure consumption against health shocks. The results therefore suggest that microcredit organizations and microcredit per se have an insurance role to play, an aspect that has not been analyzed previously. The welfare implications of microcredit therefore remain high.

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Table 1: Household Level Descriptive Statistics

	1997-1998		1999-2000		2004-2005	
	Mean	**Std. Deviation**	**Mean**	**Std. Deviation**	**Mean**	**Std. Deviation**
Panel A: Health Shock Variables						
Whether any member was sick in last 15 days	0.492	0.500	0.438	0.496	0.211	0.408
Number of days sick in last 15 days due to sickness	2.445	3.187	2.056	2.930	1.306	3.065
Number of days work lost due to sickness	3.119	3.631	3.017	3.095	1.349	3.057
Whether household incurred any big expenditure	0.157	0.402	0.144	0.352	0.226	0.419
Death of the main earner in the family	0.010	0.012	0.010	0.101	0.015	0.121
Panel B: Demographic Variables						
Age of the Household Head	44.52	13.36	46.81	13.34	47.75	12.20
Number of working people in the household	2.81	1.38	3.02	1.53	3.59	2.12
Household size	5.63	2.29	6.06	2.48	7.23	3.85
Maximum education attained by any household member	5.48	4.13	6.23	4.07	7.27	6.53
Area of arable land	68.47	146.66	80.79	159.03	73.68	225.92
Number of children	2.83	1.66	2.22	1.46	3.01	2.39
Number of women	2.66	1.40	2.94	1.52	3.26	2.00
Number of old people of age 60 above	0.25	0.49	0.39	0.60	0.31	0.54
Number of married people	2.38	1.10	2.70	1.37	3.16	1.98
Whether women is the head of the household	0.05	0.23	0.05	0.23	0.11	0.31

Table 1 (continued): Household Level Descriptive Statistics

Panel C: Outcome Variable (in Taka)						
Food Consumption (Monthly)	2432.8	1832.2	2949.5	2721.1	3214.4	3296.1
Non-Food consumption expenditure (yearly)	5628.2	6877.2	3499.4	7022.8	6024.0	9563.7
Non-land Asset (excluding livestock)	13128.1	27327.5	18529.7	14554.0	17661.2	44394.1
Value of livestock	5956.2	7664.7	4027.5	6242.8	4296.7	7432.9
Income	32975.1	33572.6	35733.6	50804.0	45252.5	50515.5
Self-employment income	6009.8	104059.0	5377.4	28842.5	6788.1	63987.5
Medical Expenditure	2191.5	10254.5	2015.7	8799.6	4295.1	12406.1
Total non-food including medical exp (monthly)	651.6	1427.6	459.6	1318.5	859.9	1830.8
Total expenditure	3084.5	3259.9	3409.0	4039.7	4074.3	5126.9
Percentage of non-food in total expenditure	21.1		13.5		21.1	
Number of observations	2694		2694		2694	

Table 2: Descriptive Statistics. Microcredit and Other Loans (in Taka)

	1997-1998		1999-2000		2004-2005	
	Mean	**Std. Deviation**	**Mean**	**Std. Deviation**	**Mean**	**Std. Deviation**
Microredit borrowing						
Amount of loan taken from Microcredit organization	7427.3	7165.0	10616.8	11332.4	11682.5	17378.7
Number of microcredit borrowers	1592		1532		1280	
Borrowing from other sources						
Percentage of households taken loan in last month	5.18		4.3		NA	
Amount of loan taken in last month	167	2284.1	468.07	15573.2		
Percentage of households taken loan in last year	29.4		26.6		18	
Amount of loan taken in last year	4657.2	12712.1	7350.7	28640.3	9464.0	18045.8
Percentage of households who took loan from neighbour and relatives	53		35.1		NA	
Percentage of households who took loan for consumption	23.4		11.1		9.1	
Percentage of households who took loan for medical purpose	3.5		0.5		0.6	

Note: Monthly loan data is not available for the last round of survey 2004-05

Table 3: Persistence of Health Shock. Coefficient Corresponding to the Lag Health Shock Variable

	Fixed effects	**IV1**	**IV2**
Whether any household member is sick in period t-1	-0.193	0.1	0.005
	(6.78)	(0.266)	(0.127)
Whether incurred any big expenditure or income loss due to sickness in period t-1	0.002	3.543	-0.169
	(0.003)	(14.081)	(0.453)
Death of the main family member in period t-1	-0.016	1.106	-0.12
	(0.023)	(2.145)	(0.162)

Notes:
Clustered Standard errors are reported in parentheses
IV1 includes only two period lagged value of the dependent variable as instrument, IV2 adds household level characteristics of two period-lag as instruments

Table 4: Effect of Health Shocks on Changes in Consumption

Dependent Variable	**Change in Food Consumption**				**Change in Non-food Consumption**			
Shock variable (past 30 days)								
Whether any household member is sick[1]	2.475	2.458	1.728	1.932				
	(1.785)	(1.848)	(1.770)	(1.21)				
Number of days sick	3.364	2.759	3.012	0.272				
	(2.059)	(2.018)	(2.057)	(2.202)				
Number of Working days lost	-1.61	-2.688	-2.688	-3.486				
	(2.144)	(2.351)	(2.351)	(2.158)				
Shock variable (past one year)								
Whether household incurred any big expenditure or income loss due to sickness[1]	-3.417	-0.367	-0.335	0.0188	2.69	2.54	1.99	1.05
	(1.957)+	(0.209)+	(0.197)+	(0.121)	'(0.635)*	(0.694)*	(0.658)*	(0.649)
Death of the main family earner[1]	-1.61	-0.55	-0.409	0.312	2.54	2.96	2.73	1.64
	(2.144)	(0.431)	(0.425)	(0.252)	'(1.259)**	(1.306)**	(1.236)**	(1.316)
Village Fixed effects	No	yes	yes	yes	No	yes	yes	yes
Time Effects	No	No	yes	yes	No	No	yes	yes
Village*time FE	No	No	No	yes	No	No	No	yes

Notes:
Clustered Standard errors in parentheses;
+ significant at 10%; ** significant at 5%; * significant at 1%.
[1]: coefficients are divided by 100 for changes in food consumption, divided by 1000 for changes in non-food consumption

Table 5: Effect of Health Shocks on Changes in Consumption and the Mitigating Effects of Microcredit

Dependent Variable	Change in Food Consumption				Change in Non-food Consumption			
Whether any household member is sick[1]	1.758	1.786	0.875	0.108				
	(1.884)	(1.955)	(1.952)	(1.511)				
Shock * Treatment	1.3	1.211	1.378	1.392				
	(0.05)**	(0.5322)**	(0.528)**	(0.9853)				
Number of days sick	2.4	1.663	1.842	-0.868				
	(2.340)	(2.238)	(2.281)	(2.526)				
Shock * Treatment	0.0058	0.0066	0.0070	0.0069				
	(0.0039)	(0.0038)+	(0.0041)+	(0.0065)				
Number of Working days lost	-2.489	-3.411	-3.682	-4.775				
	(2.175)	(2.371)	(2.339)	(2.145)**				
Shock * Treatment	0.0213	0.0176	0.0236	0.0316				
	(0.0229)	(0.0245)	(0.0234)	(0.0214)				
Whether household incurred any big expenditure or income loss due to sickness[1]	-0.355	-0.368	-0.344	-0.065	2.489	2.36	2.36	1.18
	(0.202)+	(0.212)+	(0.202)+	(0.127)	(0.675)*	(0.736)*	(0.736)*	(0.676)+
Shock * Treatment	0.159	0.097	0.106	0.869	2.27	2.1	.9474	-1.411
	(0.356)	(0.403)	(0.405)	(0.783)	(1.554)	(1.70)	(2.446)	(2.377)
Death of the main family earner[1]	-1.815	-1.7264	-1.628	0.778	2.605	2.62	2.46	1.49
	(4.213)	(4.423)	(4.423)	(2.869)	(1.342)+	(1.364)+	(1.364)+	(1.429)
Shock * Treatment	1.46	1.15	1.195	2.32	-0.0659	3.3	2.595	1.4
	(9.030)	(18.75)	(18.57)	(11.79)	(3.914)	(2.80)	(2.632)	(2.70)
Village Fixed effects	No	yes	yes	yes	No	yes	yes	yes
Time Effects	No	No	yes	yes	No	No	yes	yes
Village*time FE	No	No	No	yes	No	No	No	yes

Notes:
Clustered Standard errors in parentheses; + significant at 10%; ** significant at 5%; * significant at 1%.
Treatment coefficients are multiplied by 100.
[1]: coefficients are divided by 100 for changes in food consumption, divided by 1000 for changes in non-food consumption

Table 6: 2SLS Estimates of the Effect of Health Shocks on Changes in Consumption and the Mitigating Effects of Microcredit

Dependent Variable	Change in	
	Food Expenditure	Non-Food Expenditure
Whether any household member is sick[1]	-2.32	
	(2.65)	
Shock * Treatment	14.2	
	(16.1)	
Number of days sick[1]	-0.086	
	(0.132)	
Shock * Treatment	0.51	
	(0.79)	
Number of Working days lost[1]	-0.259	
	(25.97)	
Shock * Treatment	0.554	
	(0.638)	
Whether household incurred any big expenditure or income loss due to sickness[2]	-1.43	-15.49
	(1.77)	(18.26)
Shock * Treatment	18.4	209.4
	(22.7)	(231.3)
Death of the main family earner[2]	-3.67	-40.66
	(4.48)	(37.94)
Shock * Treatment	39.4	418.2
	(44.2)	(373.7)

Notes
Each set of coefficients is obtained from a separate regression of changes in outcome variable on health shock variables (left side of the table) and their interaction with instrumented loan variable. Each regression also incorporates village fixed effects, time effects and their interactions.
Shock * Treatment coefficients are multiplied by 100.
[1]: coefficients are divided by 100
[2]: coefficients are divided by 1000 for changes in non-food consumption

Table 7: Effect of Health Shocks on Loans from Other Sources

	Whether any household member was sick in the last 15 days	**Death of a main income earner**
Whether any loan was taken in last one month[1]	0.0814	
	(0.1772)	
Amount of loan taken in last one month ('00 Taka)[1]	3.509	
	(3.842)	
Amount of loan taken in last one year (in'000 Taka)[2]		2.40
		(1.1374)**

Notes:
Clustered Standard errors are reported in parentheses
+ significant at 10%; ** significant at 5%; * significant at 1%.
[1]: using the first two rounds of survey data
[2]: using all 3 rounds of survey data

Table 8: Effect of Health Shocks on Change in Ownership of Assets and Livestock

	Change in Assets		**Change in Livestock**	
	OLS	**2SLS**	**OLS**	**2SLS**
Whether any household member is sick in the last 15 days			0.0501	-7.94
			(0.2217)	(4.657)+
Shock * Treatment			-0.12	129.7
			(1.38)	(75.4)+
Number of days sick			0.0038	-0.7878
			(0.0082)	(0.8989)
Shock * Treatment			0.0317	4.79
			(0.01)*	(5.4)
Number of Working days lost			-0.0014	-2.207
			(0.0042)	‘(1.271)+
Shock * Treatment			0.029	5.4
			(0.03)	(3.1)+
Whether household incurred any big expenditure or income loss due to sickness	3.013	-11.58	-193.87	-15.20
	(1.233)**	(22.562)	(229.19)	(12.241)
Shock * Treatment	5.45	208.5	0.588	191.1
	(6.68)	(303.16)	(0.707)	‘(155.2)
Death of the main family earner	-2.472	-46.80	-1.837	-38.59
	(2.675)	(52.66)	(0.576)	(18.86)**
Shock * Treatment	-29.3	408.7	-0.91	362.7
	(11.95)**	(519.0)	(2.92)	(186.0)+

Notes:
Clustered Standard errors are reported in parentheses
+ significant at 10%; ** significant at 5%; * significant at 1%.
Regressions include Village Fixed Effects, Time Effects and Village * Time Fixed Effects
Shock * Treatment coefficients are multiplied by 100.

Table 9: OLS and 2SLS Estimates of Income Smoothing and Consumption Smoothing

	OLS	IV
All Households	0.0043	0.1217
	(0.0009)*	(0.1139)
Treatment Group: Microfinance	0.0033	0.0132
	(0.0010)*	(0.0426)
Control group: Microfinance	0.0053	0.3831
	(0.0014)*	(0.7315)

Notes
Clustered Standard errors are reported in parentheses
+ significant at 10%; ** significant at 5%; * significant at 1%.
The coefficients reported in column 3 are 2SLS estimates of the effects of income changes on total consumption changes, respectively.
Each regression includes set of controls.
The instrument for change in income is a number of days sick in last one year by income-earning household member.

Table 10: OLS Fixed Effects Estimate of the Effects of Health Shocks Using Matched Sample

	Food	Non-food	Asset	Livestock
Whether any household member is sick in the last 15 days[1]	-0.497			-9.50
	(0.990)			(5.099)+
Shock * Treatment	13.02			178.8
	(18.85)			(97.1)+
Number of days sick[1]	-0.0702			-0.9604
	(0.1235)			(1.081)
Shock * Treatment	0.364			5.04
	(0.639)			(5.60)
Number of Working days lost[1]	-0.02			-0.262
	(0.0277)			(0.139)+
Shock * Treatment	0.502			6.9
	(0.722)			(3.6)+
Whether household incurred any big expenditure or income loss due to sickness[1]	-1.11	-25.42	-8.87	-9.66
	(1.834)	(12.01)**	(8.78)	(4.49)**
Shock * Treatment	18.4	300.3	102.8	120.4
	(28.4)	(1.486)**	(1.087)	(55.7)**
Death of the main family earner[1]	-3.74	-145.00	-54.83	-38.59
	(6.34)	(92.06)	(58.23)	(18.86) **
Shock * Treatment	39.9	1362.7	463.9	362.72
	(58.2)	(8.456)	(5.349)	(186.04)+

Notes:
Clustered Standard errors are reported in parentheses
+ significant at 10%; ** significant at 5%; * significant at 1%.
Each set of coefficients is obtained from a separate regression of changes in outcome variable on health shock variables (left hand side of the table) and their interaction with instrumented loan variable.
Each regression also includes village fixed effects, time effects and their interactions.
The number of matched sample is determined by propensity score, where a household is considered in the regression if we find another household with estimated propensity score lies within a range of 0.00005.
Shock * Treatment coefficients are multiplied by 100.
[1]: coefficients are expressed in terms of per thousand Taka of the dependent variable