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. Households appear to be fairly well insured against health shocks. Our results suggest that households sell livestock in response to health shocks and short term insurance is therefore attained at a significant long term cost. However microcredit has a significant mitigating effect. Households that have access to microcredit do not need to sell livestock in order to insure consumption. Microcredit organizations and microcredit therefore have an insurance role to play, an aspect that has not been analyzed previously.

JEL Classification: O12, I10, C23.

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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. Townsend (1994), Kochar (1995) and Skoufias and Quisumbing (2005) find that illness shocks are fairly well insured. Others (Cochrane, 1991; Dercon and Krishnan, 2000; Gertler and Gruber, 2002; Asfaw and Braun, 2004; Wagstaff, 2007; Lindelow and Wagstaff, 2007; Beegle et al., 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, which in turn is related to health and access to financial markets. Udry (1990), Rosenzweig and Wolpin (1993), Besley (1995), Fafchamps et al. (1998), Jalan and Ravallion (1999) and Gertler and Gruber (2002) 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 (see Morduch, 1999). In contrast microfinance institutions hold significant promise. Microfinance programs are typically targeted at the poor (and the near-poor), do not impose significant physical collateral requirements and actively promote savings.[1]

[1] Even though microfinance is wider in scope compared to microcredit, we will, for the purposes of this paper, use the two terms interchangeably.

The primary aim of this paper is to examine 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. In several cases microfinance institutions (MFIs) have an explicit insurance component associated with loans. Additionally, no collateral requirement for microcredit loans means that poor households can get loans more easily. Credit from microfinance organizations play a pivotal role in the daily life of households in rural Bangladesh. Pitt and Khandker (1998) find that access to microfinance significantly increases consumption and reduces poverty. Amin et al. (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. Morduch (1998) and Pitt and Khandker (2002) both find that microcredit can help smooth seasonal consumption. Their results indicate that household participation in microcredit programs is partially motivated by the need to smooth the seasonal pattern of consumption and male labour supply.

There is very little prior research on the role of microcredit in enabling households to insure against income shocks in general and health shocks in particular. Gertler et al. (2009) is one of the few examples in this respect. They use data from Indonesia and show that microfinance institutions play an important role in helping households self-insure against health shocks. Our paper builds on this line of research. We use in this paper a newly-available large and unique household-level panel dataset from Bangladesh, spanning about 8 years, to examine the role of microcredit in enabling households insure against health shocks. We find that in general health shocks do not have a significant effect on household consumption: households appear to be fairly well insured. But that leads us to the possibly even more important question − what institutions/arrangements enable households to insure against health shocks? We focus on institutions that enable *ex post* consumption smoothing, since health shocks are shown to be unanticipated (see Section 4.1 below) rendering *ex ante* consumption smoothing difficult.[2] While there are a large number of potential institutions, in this paper we focus on access to credit, measured by the amount of credit from "other" sources including relatives, friends or informal money lenders; and purchase and sale of assets and livestock. Our results show that in general households do not increase their borrowing from "other" sources in response to

[2]See Morduch (1995) for more on *ex ante* and *ex post* consumption smoothing.

long term health shocks. The most common way households insure is by selling productive assets (livestock) when faced with adverse health shocks. It is here that microcredit has a significant role to play: we find that households having access to microcredit are less likely or not likely to sell productive assets (livestock) in response to idiosyncratic health shocks.

There now exists a significant volume of literature from developing countries that finds that households use livestock (or productive assets in general) to smooth consumption against income shocks. Rosenzweig and Wolpin (1993) find that in rural India, bullocks, while also used as a sources of mechanical power in agricultural production, are sold to smooth consumption in the face of income shocks. Consumption is therefore smoothed at the cost of crop production efficiency. The authors find that borrowing-constrained households keep on average half of the optimal level of bullocks. Jodha (1978), again using data from India, argues that sales of productive assets when faced with shocks (a drought in this case) is so common that to outsiders it gives the false impression of "a low cost and smooth process of evening out consumption levels operated by farmers over a period of the famine cycle" (Jodha, 1978, Page A40). However the long term implications of such actions is quite severe, particularly in terms of production in the post-drought period. He also argues that the loss of productive assets during the drought reduces the capacity of farmers to re-initiate farm activity on their own.[3] Even though the specific shock that we consider in this paper is different, the basic story remains. While it can have a positive effect on consumption insurance in the short run, the sale of livestock and other productive assets can have significant welfare impacts in the long run. Using data from Bangladesh we find that access to microfinance reduces and often removes the requirement to sell livestock in response to health shocks. This is an aspect of microfinance that has not been addressed adequately in the literature.

[3]The evidence on, and the impact of, the use of livestock and other productive assets from other parts of the world is mixed. Fafchamps et al. (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 $15 - 30\%$ 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 sell off their livestock and other assets when they experience a shock, i.e., sell livestock in order to smooth consumption in the absence of alternative social network based mechanisms to smooth consumption.

2 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 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 the second round, is that this survey round did not collect comprehensive information on consumption.[4] All the surveys were conducted during the period December - March, which implies that it is unlikely that any of the results are driven by the timing of the survey. Many of the participants dropped out of the program for one year or more and some of the initial non-participants became participants later.

The survey sampled around 3000 households in 91 villages spread evenly throughout the country, selected to reflect the overall spread of microcredit operations in Bangladesh. The attrition rate was low − less than 10 percent from the first to the 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, income, health, education and participation in microcredit programs.

Previous research indicates that the measurement of the illness shock variables is important in analysing the impact of illness on growth of consumption. Indeed the results can vary significantly depending on how the health shock is measured. For example, Cochrane (1991) using data from the US shows that *short spells of illness are well insured, . . ., but that very long spells are not fully insured* (Cochrane, 1991, Page 969).[5] In this paper we use self-reported health shocks: 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

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

[5]To be specific, the regression results presented in Cochrane (1991), Table 2, show that the regression of consumption growth on days > 0 work loss dummy shows almost no effect of illness on consumption growth. However the regression of consumption growth on the dummy days of illness ≥ 100 gives a large and significant coefficient estimate.

measures of household level health shocks. The first measure that we consider 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 is not particularly informative because of its binary nature. The problem is that an individual's self-reported health status is subjectively affected by an individual's social and cultural background, given the individual's subjective health. Schultz and Tansel (1997) argue that this is because of "cultural conditioning": the threshold of what is considered good health varies systematically across a society, controlling for their objective health status.[6] They go on to argue that self-reported functional activity limitations are better indicators of health status.[7]

Fortunately the survey also asks the respondents additional questions on their health status: *number of days sick in the last 15 days* and *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 duration of sickness in the last 15 days is likely to contain more information on health status compared to the simple binary indicator (*whether any member of the household was sick during the last 15 days prior to the survey*); but this is still not complete; again, the definition of sick could vary systematically across the society. The third measure (time off work due to health shock in the household) is possibly the best measure because here illness is considered severe enough to affect income earning activities of the individual who is sick or of another member of the household who had to refrain from work to take care of a member of the household who is sick, and is less likely to suffer from the cultural conditioning problem.

The three measures of health shocks that we have discussed so far could be viewed as measures of short-term health shocks. We also consider two longer-term measures of health shocks: *whether the household incurred any big expenditure or loss of income due to sickness in the past one year* and *whether the main income earner died in the last one year.* These two longer-term measures of health shock are less likely to suffer from the cultural conditioning problem that we have discussed above, as they are based on objective criterion.

[6] Individuals who are more educated, are more wealthy and are from socially advantaged groups are typically more aware of the limitations imposed on them by their health status and are more likely to report themselves (and their family) as being of poor health.

[7] Gertler et al. (2009) use measures of individuals' physical abilities to perform activities of daily living (ADLs) such as bending and walking 5 km. ADLs are regarded as reliable and valid measures of physical functioning ability in both developed and developing countries, and they distinguish the type of serious exogenous health problems that are likely to be correlated with changes in labour market and consumption opportunities. Unfortunately we do not have information on such variables.

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 our analysis. 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.[8] 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 1997-1998 to 7.23 members in 2004-2005. The years of education attained by the most educated member of the household has increased from 5.48 years in 1997-1998 to 7.27 years in 2004-2005. While the proportion of female headed households have doubled over the period 1997-1998 − 2004-2005, the majority of households continue to be male-headed.

The impact of illness shocks on consumption and the ability of households (and other risk sharing institutions) to smooth consumption can vary from one item to another. 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. 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 items differ depending on the type of food: some food items (e.g., beef, chicken) are consumed occasionally (once or twice in a month), while others (e.g., rice, lentil) are consumed much more frequently. Computing non-food consumption expenditure is much more problematic. Non-food consumption is measured yearly since some of the items are purchased occasionally. Our measure of non-food consumption 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.).

[8] This large drop in short run sickness is quite surprising and unfortunately we do not have a very good explanation for this. It could be that 2004-2005 was a particularly good year. We also cannot rule out reporting error or bias associated with answering the same question repeatedly.

We also exclude expenditure on health and medical care. We aggregate all expenditure in these two broad categories, and value it using the price quoted by the household (unit value) since commodities differ in terms of quality.[9] 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 expenditure. Average household consumption varies from 2433 Taka ($\approx$ 61 USD) in 1997-1998 to 3214 Taka ($\approx$ 55 USD) in 2004-2005.[10] There is considerable variation across the different rounds with a big increase in expenditure on food between 1997-1998 and 1999-2000. The share of non-food consumption (excluding 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 reduction in non-food consumption expenditure in 1999-2000 could partly be attributed to major floods at the end of 1998, which affected most of the country.[11]

Table 2 presents selected descriptive statistics on credit demand and supply. As of 1997-1998, 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 from 4657 Taka ($\approx$ 116 USD) in 1997-1998 to 9646 Taka ($\approx$ 166 USD) in 2004-2005. Average amount of borrowing from microcredit organizations has also increased over the relevant time period: from 7427 Taka ($\approx$ 186 USD) in 1997-1998 to 11682 Taka ($\approx$ 201 USD) 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.

Table 3 presents selected descriptive statistics on ownership of livestock and non-land non-livestock assets, separately for the treatment (microfinance recipient) and comparison (non-

[9] Price variation is at the item-household-year level. Households buy different quality of food (e.g., coarse rice, fine rice, etc.) and it is difficult to monitor the price of each quality (actually impossible using the data we have at our disposal). We use data reported by households. However, where we find some inconsistencies (for example a very high or a very low value) we use the village level median price to convert the reported quantity into monetary value. The village level price information was collected by a survey of village shopkeepers and this information was only used in special cases. These values are then deflated using the rural household agricultural index (1997-1998 = 100).

[10] Taka is the currency of Bangladesh: 1USD = 40 Taka in 1998, 1USD = 44 Taka in 2000 and 1USD = 58 Taka in 2005.

[11] 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.

recipient) households. The percentage of households who own livestock is higher for microfinance recipient households in each of the three survey years, as is the median value of livestock. This is not surprising because MFIs encourage borrowers (microfinance recipients) to invest in livestock. However the mean and median value of non-land non-livestock assets is higher for the comparison households, compared to the treatment households. The average savings (computed as income minus expenditure) of the two sets of households do not show any particular pattern. Part of this is possibly due to the fact that agricultural income is typically measured with error. Even expenditure is likely to be measured with error; after all this was a yearly survey and unlike Lim and Townsend (1998) we do not have detailed data on expenditure over the year.

3 Estimation Methodology

Complete risk sharing within the community will result in each household belonging to that community being protected from idiosyncratic risk.[12] 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's problem, it turns out that the planner needs to solve the following maximization problem:

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

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.

[12]The degree of consumption insurance is defined as the extent to which change in household consumption co-varies with change in household income.

Assuming an exponential utility function[13]

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

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

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

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, specially since tastes/preferences are not expected to change frequently.[14]

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. 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 change in household consumption.[15] This way we can identify whether rural households are vulnerable to transitory shocks such as illness shocks.

[13] The assumption of an exponential (CARA) utility function is not crucial to our analysis. Assuming a CRRA utility function of the form $u(c_{its};\theta_{its}) = \frac{c_{its}^{1-\alpha}}{1-\alpha}\exp(\theta_{its})$ gives an estimating equation in log format as opposed to the specification that we present in equation (4). In this case the first order condition can be written as: $\Delta\log(c_{it}) = \Delta c_t^a + (\Delta\theta_{it} - \Delta\theta_t^a)$; $\Delta c_t^a = \frac{1}{I}\sum_i \log(c_{it})$; $\Delta\theta_t^a = \frac{1}{I}\sum_i \theta_{it}$.

[14] 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 the social planner's problem.

[15] 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 across the different villages.

However Ravallion and Chaudhuri (1997) argue that this test ($\beta = 0$ and $\alpha_1 = 0$ in equation (5)) 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:

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

where δ_v represents village fixed effects; μ_t represents the time effects; $\delta_v \times \mu_t$ captures village-time interaction 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 (or for that matter any shock) are typically characterized by substantial cross-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 accounted for 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. They also enable us to control for village level shocks. In the regression results that we report below (see Section 4), all standard errors are clustered at the village level.

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

Now we turn to the potential effects of microcredit. To examine whether microcredit plays a role in enabling households to insure against idiosyncratic shocks, we estimate an extended version of equation (6) as follows:

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

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 less than zero, and the coefficient of interaction of the treatment variable and the health shock (β_3) then represents the effect of microcredit on changes in consumption.

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: some households might *choose* not to participate in the microcredit program. Additionally microcredit programs are generally placed in selected villages. Fortunately, the availability of panel data at the household level allows us to consistently estimate the average treatment effect without assuming ignorability of treatment because in this case, first differencing the dependent variable eliminates the bias caused by the time invariant unobservables. However one must be careful because this procedure does not eliminate the potential bias caused by the possibility that a household's decision to participate depends on time varying unobservables, which in turn also affects the change in consumption. Our use of village fixed effects in the first-differenced model allows us to account for any further village-specific growth/shocks/unobservables. The impact of microcredit in mitigating health shocks is identified by the difference between the treatment and the comparison 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.[16] With fixed effects estimation, measurement error is likely to exacerbate the bias. So, we estimate the effects of microcredit on consumption smoothing using the instrumental variable (IV) strategy to take into account of the possible measurement error. Note that the IV method is also suitable if treatment status is correlated with the time-varying unobservables. The microcredit organizations we study here typically offer credit to eligible households in the program village, defined as those households that own less than half-acre land.[17] 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

[16]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 and therefore the net effect cannot be signed *a priori*.

[17]Credit is not available or offered to a household not living in a treatment (program) village.

instrument is $P \times E$, which takes the value of 1 if the household is eligible and resides in a program village. $P \times E$ is then a broad measure of eligibility.[18] There is an important issue to note here: the official eligibility criterion varies slightly across the different microcredit organizations and over time. Discussion with microcredit borrowers and local officials of microcredit organizations indicate that there are no significant differences among the different microcredit organizations as far as the eligibility status is concerned. However given that land quality differs widely among the different regions, a number of microfinance institutions have in the recent years relaxed the land-based eligibility criterion slightly (i.e., households owning more than half acre of land are also eligible for microcredit). We account for this and our instrument is time varying: for the first survey round (1997-1998), 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.

How well does eligibility predict microcredit receipt? To examine this question we present in Table 4 the results obtained from regressing microcredit received by the household (D_{ivt}) on eligibility ($P \times E$) and a set of other household characteristics. We present the results corresponding to 3 different specifications: in specification 1 the only variable included in the set of explanatory variables is eligibility ($P \times E$); in specification 2, we include a set of household characteristics in addition to eligibility; and finally in specification 3 we include a full set of village fixed effects, time fixed effects and village-time interaction fixed effects. Irrespective of the specification, the eligibility ($P \times E$) variable is positive and statistically significant, implying that eligibility is a good predictor for loan receipts.

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:

- Short-term measures of health shock:

[18] 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 the credit variable. Moreover, we control for the amount of arable land the household owns in our regressions so any effect of ownership of land on consumption or other outcome is directly accounted for. 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.

  - 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

- Long term measures of health shock:
  - 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);

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.

To examine the issue of whether health shocks are persistent or not, we estimate the following regression (see for example Beegle et al., 2006):

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

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. The coefficient estimates (using 3 different shock variables) are presented in Table 5. None of the coefficient estimates are statistically significant at the conventional level (the t-ratio is always less than 1). Additionally, summary statistics not presented here show that only about $10-12\%$ of the household that report some kind of illness in one survey round also report a health shock in the following survey round. 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.[19]

4.2 Basic Results

Table 6 presents the baseline regression results (corresponding to equation (6)). The set of control variables X_{ivt} includes demographic characteristics of the household head, household size and composition, educational attainment of the most educated member of the household and the amount of arable land owned by the household. We present the results corresponding to a number of different specifications. The coefficient estimates presented in columns (4) and (8) correspond to the complete specification, where we include the village fixed effects, the time effects and also the village-time fixed effects.

The baseline results presented in Table 6 indicate that the short-term health shocks experienced by the households do not have a statistically significant effect on changes in food expenditure. The effects of long term health shocks on food expenditure are mixed: the effects vary by the shock variable under consideration and while the coefficient estimates do not always have the right sign, in general they indicate that if the household incurs a big expenditure or income loss due to sickness, it reduces its food expenditure from one period to the next.

The results for the long-term health shocks on non-food consumption expenditure are however puzzling. Some of the coefficient estimates are statistically significant but have the opposite

[19] It should be noted that equation (8) is essentially a dynamic panel data regression model and the presence of the lagged dependent variable (H_{it-1}) might result in an endogeneity problem. In unreported regressions we consider an IV regression where we use a set of exogenous variables to construct valid instruments for the lagged dependent variable. The results were not affected.

(positive) sign.[20] One possible explanation is that non-food expenditure is measured yearly, and we might not observe significant variability in that expenditure if households had previously reduced expenditure in anticipation of such shocks. This could be true if household reduced non-food consumption to pay, for example, for health and related expenditure (e.g., transport or funeral expenditure).

Next we investigate the role of aggregate shocks in consumption smoothing. When we estimate equation (6), all aggregate shocks are absorbed in the village-time fixed effects ($\delta_v \times \mu_t$), making the test agnostic on the households' ability to cope with aggregate shocks. To examine the exposure to aggregate risk we exclude the village-time fixed effects, which summarize the co-variate shocks, from equation (6), and compare the coefficient estimates presented in columns (3) and (4) and those in columns (7) and (8) for changes in food expenditure and non-food expenditure respectively. Recall that the coefficient estimates presented in columns (3) and (7) are those corresponding to the following specification:

$$\Delta C_{ivt} = \widetilde{\alpha}_0 + \widetilde{\alpha}_1 H_{ivt} + \widetilde{\alpha}_2 X_{ivt} + \delta_v + \mu_t + \varepsilon_{ivt} \tag{9}$$

The coefficient $\widetilde{\alpha}_1$ provides an estimate of consumption variability inclusive of both idiosyncratic and aggregate shocks. Kazianga and Udry (2006) argue that if aggregate shocks are important and there is substantial risk sharing, then $\widetilde{\alpha}_1 > \alpha_1$ and the difference $\delta = \widetilde{\alpha}_1 - \alpha_1$ captures the extent of risk sharing in response to aggregate shocks. The results for changes in food consumption are mixed (i.e., whether or not households are able to self-insure) depends on the particular shock under consideration. The results for the changes in non-food consumption tend to suggest that aggregate shocks are important and the results overwhelmingly reject the null hypothesis of full risk-sharing within the village.

Table 7 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

[20]Note that since data on non-food consumption expenditure is available only at the year level, we do not consider the effect of short-term health shocks on changes in food consumption expenditure.

(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. This result is true for both food and non-food expenditure and for both short-term and long-term health shocks.[21]

The IV/2SLS estimate of the effects of microcredit on consumption smoothing are presented in Table 8. The corresponding first stage results are presented in Table A-1.[22] While it is true that we are correcting for the potential endogeneity of the Treatment (access to microfinance) variable, this variable does not enter the estimating equation directly; rather it enters as an interaction term, Health Shock $\times$ Treatment. It is this variable that is the relevant endogenous variable. We therefore present the first stage results corresponding to the five different measures of health shocks that we consider in this paper. The control variables in the first stage regressions include the full set of exogenous variables, village and time fixed effects and their interactions and the relevant instrument ($P \times E$). The coefficient estimate of β_1 is always negative and the coefficient estimate of β_3 is always positive. Thus, health shocks adversely affects households' consumption and access to microcredit reduces the problem. However, neither the non-interacted term, β_1, nor the interaction term, β_3, is ever statistically significant. So, we cannot draw any conclusion as to whether health shocks really matter or not. It is however to be noted that the IV estimates of β_3 are always larger than the corresponding OLS fixed effects estimates presented in Table 7. This indicates the measurement error in the 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. The additional controls however do not have a consistent and meaningful interpretation.

4.3 How do Households Insure?

It appears (see Tables 6 $-$ 8 and discussion in Section 4.2) that health shocks do not have a statistically significant effect on household consumption. However, this is not the end

[21]It is worth noting that the health shock variable is not always negative and statistically significant $-$ in fact the coefficient estimates associated with the long-term health shocks are more often than not positive and statistically significant.

[22]The regressions were run using the xtivreg2 command in STATA 10.

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. There are a large number of possibilities: migration/re-organization of the household, remittances, adjusting labour supply including child labour, reducing educational expenditures, sale of non-land non-productive assets like gold and jewelery, increasing borrowing and setting of non-land assets and productive assets like livestock. For the purpose of this paper 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 mechanisms that enable *ex post* consumption smoothing by households. We have seen (in Section 4.1) that health shocks are unpredictable and random, which means that households are unlikely to change their behaviour in anticipation of health shocks. Accordingly we can focus on *ex post* mechanisms.

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 more from any "other" source (relatives, friends or informal money lenders). The estimated equation takes the following form:

$$\Delta L_{ivt} = \alpha_0 + \alpha_1 H_{ivt} + \alpha_2 X_{ivt} + \delta_v + \mu_t + (\delta_v \times \mu_t) + \varepsilon_{ivt} \qquad (10)$$

where ΔL is the change in loans from "other" sources. 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 two alternative measures of loans from other sources:

1. additional amount of loan taken in the last one month; and

2. additional amount of loan taken in the last one year

The random effects tobit regression results presented in Table 9 show that in general health shocks are not associated with an increase in the amount of borrowing from other sources (such as relatives, friends or informal money lenders). The only exception is the case of the death of the main earner in the household, which is associated with increased borrowing in the last one year. It appears therefore households in general do not (or possibly cannot, though it is difficult to make the distinction using the data at our disposal) use borrowing from other sources to insure against income shocks.

Households can also insure consumption by selling productive (for example livestock) or non-productive assets (for example consumer durables).[23] Households that have access to micro-credit might have focused on asset building/accummulation 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 large fraction of the households in our sample save in the form of investment in livestock. Almost all the households own some livestock (e.g., cows, goats, chicken, ducks, etc.). As described in the introduction, there is also a significant volume of literature from developing countries that finds that households use livestock (or productive assets in general) to smooth consumption against income shocks.

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:

$$\Delta A_{ivt} = \beta_0 + \beta_1 H_{ivt} + \beta_2 X_{ivt} + \beta_3 (H_{ivt} \times D_{ivt}) + \delta_v + \mu_t + (\delta_v \times \mu_t) + \varepsilon_{ivt} \qquad (11)$$

Here ΔA_{ivt} measures the change in the value of non-land asset or livestock owned 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

[23]The value of consumer durables 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. Specifically the question was: *List if you have any of the following assets (give list). If yes, then please tell us how much did it cost to buy, and what would be the approximate value at present.* We used the "value at present". Households picked from the list of assets provided.

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 10.[24] 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-land assets, those for the change in ownership of livestock are much more systematic. The coefficient estimate associated with the health shock variable (β_1) is always negative and generally statistically significant in the change in value of livestock regressions. In addition, the interaction term (β_3) is generally positive and statistically significant. The effect of microcredit on the change in the value of livestock owned is given by $\hat{\beta}_3$. A positive and statistically significant $\hat{\beta}_3$ in implies that, for a household that receives a health shock, an increase in the amount of microcredit available increases the value of livestock owned by the household. The effect of health shock is given by $\hat{\beta}_1 + \hat{\beta}_3 \times$ Treatment, which in turn depends on the amount of microcredit received. $\hat{\beta}_1$ then gives us the direct effect of health shock, conditional on the household not receiving any microcredit. Now to interpret the results in column 4, Table 10. For households that do not receive any microcredit, the presence of a sick member in the household reduces ownership of livestock by 7.94 thousand Taka. For the household receiving the average amount of microcredit (approximately 10000 Taka), the change in the value of livestock owned is $-7.94 + 12.97 = 5.03$ thousand Taka (≈ 106.27 USD); i.e., actually increases ownership of livestock by 5.03 thousand Taka.[25] This total effect is however not always positive. To take another example, for households that do not receive any microcredit, the death of the main income earner reduces livestock ownership by 38.59 thousand Taka. For the households receiving the average amount of microcredit, the change in the value of livestock owned (following the death of the main income earner) is $-38.59 + 36.27 = -2.32$ thousand Taka (≈ -49 USD), and this total effect is negative but statistically significant (the joint test $\hat{\beta}_1 + \hat{\beta}_3 = 0$ is rejected). The remaining estimates can be interpreted in the same way.

There is therefore a significant mitigating effect of microcredit. In all cases the health shock variable ($\hat{\beta}_1$) is negative and generally statistically significant; the interaction term ($\hat{\beta}_3$) is

[24]The first stage results are again given by those presented in Table A-1.
[25]We have assumed an exchange rate of 1USD = 47.33 Taka.

always positive and generally statistically significant and in several cases the total effect ($\hat{\beta}_1 + \hat{\beta}_3$) is actually positive and statistically significant (this of course depends on the specific shock that we consider). Households having access to microcredit either do not have to sell livestock or have to sell less livestock in response to idiosyncratic health shocks. While households cannot explicitly borrow from MFIs for insurance purposes, it is clear that access to microfinance gives households some freedom to re-organize funds within the household leading to the observed outcome that these households do not need to sell the productive asset (livestock) either at all or to the extent that the non recipients need to. Unfortunately we do not have detailed data on expenditure over the year to answer the question where the trade-off happens. What is clear however is that over the long term, the microcredit recipient households benefit, relative to the non-recipient households.

We examine the robustness of these results using the propensity score matching method, where we match households on the basis of their socio-economic status and we restrict our analysis to the matched sample. This controls for heterogeneity in initial socio-economic conditions that may be correlated with subsequent health shocks and the path of consumption growth.[26] Regression conducted on the matched sample again show that the strongest effects are in terms of changes in livestock owned. The regression results (presented in Table A-2), show that the magnitude of the health shock coefficients are, in general, larger using the matched sample, compared to the full sample. For example the 2SLS results for change in ownership of livestock presented in column 4, Table A-2 imply that conditional on the household not receiving any microcredit, the household responds to any member being sick by reducing the value of livestock owned by 9.5 thousand Taka (compared to 7.94 thousand Taka for the full sample). For the household receiving the average amount of microcredit, the change in the amount of livestock owned is $-9.50 + 17.88 = 8.38$ thousand Taka ($\approx$ 177 USD), more than what we obtained for the full sample (Table 10, column 4).

Households with access to microcredit therefore do not either need to reduce their ownership of livestock or do not need to reduce it by *as much* in response to health shock (irrespective of how the shock is defined). Access to microcredit then helps in two different ways. First, in the short run, it helps insure consumption (see Table 7). This effect is however not particularly strong. Second, recipient households do not need to sell livestock, or do not need to sell

[26]See Islam and Maitra (2009) for more on the methodology used.

livestock to the extent non recipient households need to, in response to health shocks and therefore insurance does not come at the cost of production efficiency. There is therefore both a short run (direct) and a long-run (somewhat indirect) impact of microcredit. Despite a fairly large literature on the impact of microfinance (see Pitt and Khandker (1998); Morduch (1998); Pitt and Khandker (2002); Roodman and Morduch (2009) for quasi experimental research and Banerjee et al. (2009); Karlan and Zinman (2010) for experimental evidence), the existing literature has not focussed on role microcredit plays in terms of providing insurance in the manner we discuss here. An examination of the role of microcredit in terms of providing insurance against shocks is an important contribution of this paper.

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. Is there a role for microcredit in this respect? Our results show that households that have borrowed from microcredit organizations appear to be better able to cope with health shocks. The primary instrument through which households insure is by trading in livestock. Households that have access to microcredit do not need to sell livestock or do not have to, to the extent households that do not have access to microcredit need to, in order to insure consumption against health shocks.

On a broader and quite a positive note, credit markets (of which microcredit is one aspect) appears to be play a significant role in insuring households against income fluctuations. This is nothing new - there is evidence from a number of different developing countries around the world regarding the role of credit markets in providing insurance. Munshi and Rosenzweig (2009) show using a panel data set from India that nearly one-quarter of the households in the sample participated in the insurance arrangement in the year prior to each survey round, giving or receiving transfers (broadly classified into gifts and loans). Although loans account for just 20 percent of all within-caste transactions by value, they are more important than bank loans or moneylender loans in smoothing consumption and in particular for meeting contingencies such as illness and marriage. They go on to argue that (in the context of rural India) such within-caste loans are actually more important than microcredit. The institutional structure within which households in our sample operate are different - indeed microcredit is more common and it is not surprising that the insurance aspect of microcredit is more apparent from the data. Microcredit can help in two ways. In the short-run, it helps insure consumption. This effect is however not particularly strong. In the long-run the change in the value of livestock in response to health shocks is lower for households with access to microcredit, and thus insurance does not come at the cost of production efficiency. There is therefore both a short run (direct) and a long-run (somewhat indirect) impact of microcredit. The literature has not focussed on this indirect but as it turns out rather important role performed by microcredit. Indeed 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 continue to remain high.

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Table 1: Household Level Descriptive Statistics

	1997-1998		**1999-2000**		**2004-2005**	
	Mean	**Std. Dev**	**Mean**	**Std. Dev**	**Mean**	**Std. Dev**
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
Working age population	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
Arable land owner	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 elderly	0.25	0.49	0.39	0.60	0.31	0.54
Number of married members	2.38	1.10	2.70	1.37	3.16	1.98
Female headed household	0.05	0.23	0.05	0.23	0.11	0.31
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 expenditure (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. Dev**	**Mean**	**Std. Dev**	**Mean**	**Std. Dev**
Microcredit 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 neighbours 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 2004-2005

Table 3: Descriptive Statistics. Asset Ownership for Microcredit recipients and Others

	1997-1998		**1999-2000**		**2004-2005**	
	Treatment	Comparison	Treatment	Comparison	Treatment	Comparison
Percentage of Households owning livestock	0.92	0.87	0.89	0.84	0.77	0.74
Median value of livestock	3964.64	2105.29	1270.57	1060.30	968.18	731.03
Mean value of non-land non-livestock asset	5812.58	11486.35	14167.00	15025.18	10418.21	17796.65
Median value of non-land non-livestock asset	915.39	1077.72	12729.20	12837.01	4743.32	5104.27
Savings	3622.75	-4989.37	-469.60	-1909.29	2009.67	1459.27

Table 4: How Well Does Eligibility Predict Microcredit Receipt

	(1)	**(2)**	**(3)**
Eligibility ($P \times E$)	1,681***	1,935***	1,434***
	(395.3)	(505.8)	(413.0)
Age of household head		9.581	-0.380
		(14.52)	(16.17)
Working age population		183.4	446.1**
		(243.1)	(209.5)
Household size		203.2	-315.6*
		(194.4)	(174.4)
Maximum education attained by any		29.98	-3.348
member of household		(32.61)	(36.45)
Arable land owned		-2.336**	-2.051**
		(1.057)	(0.960)
Number of children		245.9	558.2***
		(205.3)	(172.4)
Number of women		-113.9	41.81
		(179.6)	(151.9)
Number of elderly		-1,421***	-667.3
		(416.3)	(439.3)
Married Members		694.9***	507.3**
		(235.2)	(251.1)
Female Headed Household		663.0	-755.5
		(776.9)	(722.3)
Observations	8,082	8,072	8,072
Village Fixed Effects	No	No	Yes
Time Fixed Effects	No	No	Yes
Village $\times$ Time Fixed Effects	No	No	Yes

Notes:
Clustered Standard errors in parentheses
$^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$

Table 5: Persistence of Health Shock. Coefficient Corresponding to the Lag Health Shock Variable

	Fixed effects
Whether any household member is sick in period $t-$	-0.193
	(6.78)
Whether incurred any big expenditure	0.002
or income loss due to sickness in period $t-1$	(0.003)
Death of the main family member in period $t-1$	-0.016
	(0.023)

Notes:
Clustered Standard errors are reported in parentheses

Table 6: Effect of Health Shocks on Changes in Consumption

Dependent Variable	Change in Food Consumption				Change in Non-food Consumption			
Shock variable (past 15 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	-2.712	-3.486				
	(2.144)	(2.351)	(2.334)	(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 Fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Village × Time Fixed effects	No	No	No	Yes	No	No	No	Yes

Notes:
Clustered Standard errors in parentheses
$^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$
Coefficients and standard errors are expressed per 100 Taka for changes in food consumption, and per 1000 Taka for changes in non-food consumption

Table 7: 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			
Shock variable (past 15 days)								
Whether any household member is sick	1.76	1.79	0.88	1.08				
	(1.89)	(1.96)	(1.95)	(1.51)				
Shock × Treatment	1.3	1.21	1.38	1.392				
	(0.05)**	(0.53)**	(0.53)**	(0.99)				
Joint test F-statistic	0.87	0.84	0.2	0.51				
Number of days sick	0.02	0.02	0.02	-0.01				
	(0.02)	(0.02)	(0.02)	(0.03)				
Shock × Treatment	0.01	0.01	0.01	0.01				
	(0.00)	(0.00)*	(0.00)*	(0.01)				
Joint test F-statistic	1.05	0.55	0.65	0.12				
Number of Working days lost	-0.03	-0.03	-0.02	-0.05				
	(0.02)	(0.024)	(0.02)	(0.02)**				
Shock × Treatment	0.02	0.02	0.02	0.03				
	(0.02)	(0.02)	(0.02)	(0.02)				
Joint test F-statistic	1.31	2.07	2.48	4.96**				
Shock variable (past one year)								
Whether household incurred any big expenditure	-3.55	-3.68	-3.44	-0.65	2.49	2.36	1.95	1.18
or income loss due to sickness	(2.02)*	(2.12)*	(2.02)*	(1.27)	(0.68)***	(0.74)***	(0.70)***	(0.68)*
Shock × Treatment	0.16	0.10	0.11	0.87	0.23	0.21	0.10	-0.14
	(0.36)	(0.40)	(0.41)	(0.78)	(0.16)	(0.17)	(0.25)	(0.24)
Joint test F-statistic	3.09*	3.01*	2.9*	0.27	13.6***	10.3***	7.82***	3.05*
Death of the main family earner	-1.82	-1.73	-1.63	0.78	2.61	2.62	2.46	1.49
	(4.21)	(4.42)	(4.42)	(2.869)	(1.34)*	(1.36)*	(1.36)*	(1.43)
Shock × Treatment	1.46	1.15	1.20	2.32	-0.66	0.33	0.26	0.14
	(9.03)	(1.88)	(1.86)	(1.18)	(0.39)	(0.28)	(0.26)	(0.27)
Joint test F-statistic	0.19	0.15	1.14	0.07	3.77*	3.69*	3.86*	1.09
Village Fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Time Fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Village × Time Fixed effects	No	No	No	Yes	No	No	No	Yes

Notes:
Clustered Standard errors in parentheses
$^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$
Regressions include full set of additional controls
Coefficients and standard errors are expressed per 100 Taka for changes in food consumption, and per 1000 Taka for changes in non-food consumption

Table 8: 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**
Shock variable (past 15 days)		
Whether any household member is sick	-6.23	
	(9.31)	
Shock × Treatment	13.38	
	(15.17)	
Joint test F-statistic	0.44	
Number of days sick	-0.86	
	(1.32)	
Shock × Treatment	0.51	
	(0.79)	
Joint test F-statistic	0.43	
Number of Working days lost	-0.28	
	(0.28)	
Shock × Treatment	0.59	
	(0.69)	
Joint test F-statistic	0.93	
Shock variable (past one year)		
Whether household incurred any big expenditure	-14.57	-16.13
or income loss due to sickness	(18.3)	(19.29)
Shock × Treatment	18.78	21.81
	(23.5)	(24.44)
Joint test F-statistic	0.63	0.70
Death of the main earner in the family	-37.69	-41.93
	(45.55)	(34.17)
Shock × Treatment	39.56	43.11
	(44.9)	(38.62)
Joint test F-statistic	0.64	1.15

Notes:
Each regression also incorporates village fixed effects, time effects and their interactions
Regressions include full set of additional controls
Coefficients and standard errors are expressed per 100 Taka for changes in food consumption and per 1000 Taka for changes in non-food consumption

Table 9: Effect of Health Shocks on Loans from Other Sources

	Amount of loan taken in last one month ('00 Taka)[1]	**Amount of loan taken in last one year (in'000 Taka)**[2]
Shock variable (past 15 days)		
Whether any household member is sick	3.51 (3.842)	
Number of days sick	0.04 (0.0779)	
Number of Working days lost	-2.06 (5.531)	
Shock variable (past one year)		
Whether household incurred any big expenditure or income loss due to sickness		0.41 (1.013)
Death of the main earner in the family		2.40 (1.1374)**

Notes:
Clustered Standard errors are reported in parentheses
** : $p < 0.05$
[1]: using the first two rounds of survey data
Regressions include full set of additional controls
[2]: using all 3 rounds of survey data

Table 10: Effect of Health Shocks on Change in Ownership of Assets and Livestock

	Change in Assets		Change in Livestock	
	OLS	**2SLS**	**OLS**	**2SLS**
Shock variable (past 15 days)				
Whether any household member is sick			0.05	-7.94
			(0.22)	(4.66)*
Shock × Treatment			-0.01	12.97
			(0.14)	(7.54)*
Joint Test F-statistics			0.05	2.9*
Number of days sick			0.00	-0.79
			(0.01)	(0.90)
Shock × Treatment			0.00	0.48
			(0.00)***	(0.54)
Joint Test F-statistics			0.21	0.77
Number of Working days lost			-0.00	-0.22
			(0.00)	(0.13)*
Shock × Treatment			0.00	0.54
			(0.00)	(0.31)*
Joint Test F-statistics			0.1	3.02*
Shock variable (past one year)				
Whether household incurred any big expenditure	3.01	-11.58	-0.19	-15.20
or income loss due to sickness	(1.23)**	(22.56)	(0.23)	(12.24)
Shock × Treatment	0.54	20.85	0.06	19.11
	(0.67)	(30.32)	(0.07)	(15.52)
Joint Test F-statistics	5.97**	0.29	0.72	1.54
Death of the main family earner	-2.47	-46.80	-1.84	-38.59
	(2.68)	(52.66)	(0.58)**	(18.86)**
Shock × Treatment	-2.93	40.87	-0.09	36.27
	(1.20)**	(51.90)	(0.29)	(18.60)*
Joint Test F-statistics	0.85	0.84	10.15***	4.18**

Notes:
Clustered Standard errors are reported in parentheses
$^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$
Regressions include Village fixed effects, Time fixed effects and Village × Time fixed effects
Regressions include full set of additional controls
Coefficients and standard errors are expressed per 1000 Taka for changes in Assets and Livestock

Table A-1: First Stage Results corresponding to IV results presented in Table 8

	1	**2**	**3**	**4**	**5**
Eligibility ($P \times E$)	742.4**	20,114*	17,290**	506.8**	264.7*
	(315.6)	(12,302)	(8,508)	(211.7)	(143.3)
Health Shock	6,154***	16,637	-518.3	7,860***	10,111***
	(702.8)	(13,260)	(796.4)	(1,110)	(2,835)
Age of Household Head	-6.409	1,267	-378.9	-9.081	-3.276
	(12.63)	(1,193)	(497.1)	(12.11)	(3.650)
Working Age Population	-99.40	16,967	-4,867	301.3	142.0
	(297.6)	(16,022)	(9,246)	(220.1)	(87.89)
Household Size	225.8	-3,144	9,829	-182.3	-96.52*
	(214.4)	(4,605)	(7,807)	(158.0)	(57.12)
Maximum Education Attained by Any	-31.10***	408.2	-447.3	10.402	5.286
Member of Household	(15.76)	(732.3)	(744.4)	(22.68)	(4.889)
Arable Land Owned	-0.102	-2.190	-7.805	-0.459	0.771
	(1.002)	(8.831)	(19.60)	(0.984)	(0.931)
Number of Children	-160.3	3,360	-4,229	201.4	169.7
	(274.0)	(5,940)	(8,865)	(262.5)	(129.5)
Number of Working Age Females	-105.4	-15,230	-4,547	1.082	20.10
	(167.9)	(12,592)	(3,647)	(105.0)	(46.07)
Number of Elderly	-552.6***	-8,182	6,322	-8.671	-85.78
	(321.6)	(10,464)	(10,396)	(251.9)	(75.63)
Number Married	373.8***	-2,595	7,390	262.5	32.24
	(203.8)	(11,124)	(5,959)	(171.8)	(67.79)
Female Headed Household	-23.16	-13,239	2,321	-292.8	23.09
	(475.1)	(25,534)	(13,009)	(858.5)	(115.3)
Constant	-305.1	-70,039	21,343	-130.4	-527.9
	(1,017)	(71,396)	(27,566)	(812.8)	(521.5)
Observations	5378	5378	5378	5378	5378
R^2	0.122	0.153	0.045	0.158	0.161
$F(1,.)$	4.42	0.83	3.87	3.6481	5.44
$Prob > F$	0.0355	0.3629	0.0492	0.057	0.0197
Partial R^2	0.0009	0.0002	0.0007	0.000681	0.0011

Notes:
Regressions include Village fixed effects, Time fixed effects and Village $\times$ Time fixed effects
Standard errors in parentheses
$^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$
Dependent Variables:
1: Whether any household member is sick $\times$ Treatment
2: Number of days sick $\times$ Treatment
3: Number of working days lost $\times$ Treatment
4: Whether household incurred any big expenditure or income loss due to sickness $\times$ Treatment
5: Death of main earner in the family $\times$ Treatment

Table A-2: 2SLS Fixed Effects Estimate of the Effects of Health Shocks Using Matched Sample

	Food	**Non-food**	**Asset**	**Livestock**
Shock variable (past 15 days)				
Whether any household member is sick	-4.97			-9.50
	(9.90)			(5.10)*
Shock × Treatment	13.0			17.88
	(18.9)			(9.71)*
Joint Test F-statistic	0.25			3.47*
Number of days sick	-0.70			-0.960
	(1.24)			(1.08)
Shock × Treatment	0.36			0.50
	(0.64)			(0.56)
Joint Test F-statistic	0.32			0.79
Number of Working days lost	-0.20			-0.262
	(0.28)			(0.139)*
Shock × Treatment	0.50			0.69
	(0.72)			(0.36)*
Joint Test F-statistic	0.55			3.55*
Shock variable (past one year)				
Whether household incurred any big expenditure	-11.1	-38.96	-11.17	-16.92
or income loss due to sickness	(18.34)	(30.25)	(16.8)	(11.78)
Shock × Treatment	18.4	63.29	21.6	25.87
	(28.4)	(46.8)	(25.98)	(18.53)
Joint Test F-statistic	0.37	1.66	0.44	1.99
Death of the main family earner	-37.4	-145.0	-54.83	-60.62
	(63.4)	(92.1)	(58.23)	(35.51)*
Shock × Treatment	39.9	136.3	46.39	54.9
	(58.2)	(84.7)	(53.49)	(32.62)*
Joint Test F-statistic	0.35	2.48	0.89	2.91*

Notes:
Clustered Standard errors are reported in parentheses
$^{***}: p < 0.01, ^{**}: p < 0.05, ^{*}: p < 0.1$
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
Regressions include full set of additional controls
Coefficients and standard errors are expressed per 100 Taka for changes in food consumption and per 1000 Taka for changes in others