Spillovers of Community-Based Health Interventions on Consumption Smoothing

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Abstract

Community-based interventions, particularly group-based ones, are considered to be a cost-effective way of delivering interventions in low-income settings. However, design features of these programs could also influence dimensions of household and community behaviour beyond those targeted by the intervention. This paper studies spillover effects of a participatory community health intervention in rural Malawi, implemented through a cluster randomised control trial, on an outcome not directly targeted by the intervention: household consumption smoothing after crop losses. We find that while crop losses reduce consumption growth in the absence of the intervention, households in treated areas are able to compensate for this loss and perfectly insure their consumption. Asset decumulation also falls in treated areas. We provide suggestive evidence that these effects are driven by increased social interactions, which could have alleviated contracting frictions; and rule out that they are driven by improved health or reductions in the incidence of crop losses.

Keywords: participatory community interventions, spillovers, consumption smoothing, Sub-Saharan Africa

JEL Classification: E21, G22, O12, O13

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1 Introduction

Community-based interventions, particularly group-based interventions, have become very wide-spread in developing countries (Mansuri and Rao [2004]). They are considered to be an effective, and relatively less costly way of delivering interventions, triggering and sustaining behaviour change, as well as shifting social norms in low-income settings. Goverments and NGOs often use group-based interventions to provide financial services (e.g. microfinance groups, savings groups, etc); promote collective action (e.g. Community-Led Total Sanitation; participatory women's groups); deliver early childhood parenting interventions; deliver aid and infrastructure in post-conflict as well as non-conflict settings among others.

Design features of these community-based interventions could influence dimensions of household or community behaviour beyond those directly targeted. For example, many interventions incorporate regular group meetings, which could encourage information sharing, thereby alleviating contracting frictions. It is important to account for such spillover effects when assessing the welfare impacts of interventions. However, empirically disentangling these is challenging since interventions might also provide resources which may also influence outcomes affected by the contracting frictions.

In this paper, we study the spillover effects of a participatory community based health intervention in rural Malawi on household consumption smoothing of idiosyncratic crop losses. The chief goal of the intervention was to improve maternal and child health. Consumption smoothing was not regarded as an issue of even mild importance by the implementation agency; and no direct efforts or resources were provided to influence this. Instead, the mere design of the program could have influenced consumption smoothing outcomes. Focusing on an outcome not directly targeted by the intervention allows us to shed light on this type of spillover effect. Our contributions are two-fold: First, we study whether participatory group-based community health interventions affect consumption smoothing. Second, drawing on the theoretical literature on informal risk sharing, we consider ways in which the intervention may have altered consumption smoothing patterns, paying special attention to features of its design.

Though households frequently experience shocks in the context we study, formal credit and insurance markets are severely lacking. Instead, informal tools such as transfers and informal loans between relatives, friends and neighbours, as well as assets such as livestock are the key tools used to deal with the consequences of risk. The intervention encouraged women to form groups which met on a fortnightly or monthly basis to engage in a participatory action cycle to improve reproductive and child health. The group meetings did not follow a top-bottom approach. A trained facilitator organised the discussion, but it was up to the villagers to come up with ideas, and take decisions. Initial meetings were discussion-based, while later meetings planned and implemented (along with the wider community) strategies identified by the groups to improve maternal and child health (such as for instance, lobbying a range of donors to provide bycicle ambulances to get to the health care facilities). By facilitating the formation of groups that met regularly, and that aimed to involve a broad swathe of the community, the intervention could have increased social interactions and alleviated contracting frictions thereby altering consumption smoothing. It provided no monetary resources whatsoever, and as such, is extremely cost effective, and thus increasingly used in developing countries to improve infant mortality and female reproductive health (see Prost et al. [2013] for a systematic review). If beneficial effects on outcomes even beyond those targeted directly by the program are achieved, the case for such already successful programs is strengthened even further.

The analysis begins by considering how consumption smoothing – measured as the response of changes in log household (non-durable) consumption to idiosyncratic crop loss events, net of village level aggregate shocks – varies in villages receiving the intervention relative to villages not receiving it. To identify causal impacts, we exploit the random allocation of the intervention to groups of villages (or clusters). Household crop losses are an important source of risk in these communities: around 28% of households in our sample experience a crop loss over a 2-year period. Our results indicate that the intervention yields improvements in consumption smoothing: while experiencing a crop loss leads to a reduction in household consumption growth by around 9% for households in control areas, those in intervention areas are able to compensate for this loss, and subsequently perfectly insure their consumption.

With very limited formal markets, self-insurance through accumulation and decumulation of assets such as livestock; and informal risk sharing through informal loans, gifts and transfers from friends, family and neighbours form the key ways through which households smooth consumption in Mchinji, rural Malawi. Examining data on savings and livestock holdings, we find that following a crop loss, households in intervention areas are less likely to draw down their savings, and to hold more large animals than households in control areas. Thus, households in intervention areas do not draw down as many assets to cope with crop losses, which also supports long run poverty alleviation efforts. We hypothesize instead that the improvements in consumption smoothing need to come from better informal risk sharing.

The next step of the analysis examines how the intervention might have improved informal risk sharing arrangements. These are likely to be constrained by contracting frictions such as information asymmetries (e.g. Ligon, 1998; Kinnan, 2014) and imperfect enforcement (Kocherlakota, 1996; Ligon et al., 2003; Ali and Miller, 2013). Through forming groups that met regularly, the intervention could have increased social interactions in the intervention areas. These could, in turn, improve information sharing, thereby alleviating the contracting barriers that come with information asymmetries; as well as provide more opportunities to punish renegers of informal arrangements and reduce limited commitment problems. Indeed, we document an increase in the probability that households chat with their friends and family on a one-to-one basis outside the intervention group, providing suggestive evidence that the intervention increased social interactions also outside of the groups. This is in line with Feigenberg et al. [2013], who find that increasing the frequency of meetings among microfinance clients increases risk sharing amongst themselves. Our study, however, considers consumption smoothing and risk sharing outcomes for households across the village regardless of their decision to participate in the groups.

Several aspects of the intervention likely contributed to the large observed impacts: first, the intervention had already been running for three years at the time we started data collection. Second, the group meetings did not follow a top-bottom approach but - on the contrary - a participatory one in which the role of the facilitator was to simply encourage discussion within a 4-cycle structure which also involved non-group villagers. This participatory approach is more likely to have resulted in increased communication (and ultimately information sharing) among group members. Moreover, the third and fourth parts of the participatory cycle – where groups implemented chosen strategies – also provided opportunities to participants to learn about the traits (e.g. trustworthiness, ability, etc) of their fellow group members.

Crucially, we rule out other channels through which the intervention might have contributed to risk sharing other than increasing social interaction. This is helped by the narrowly defined remit of the intervention: improving reproductive health. We find that the intervention did not lead to improvements in general health, nor reduce the severity of crop losses.

Our findings are important for the design of development programs. In particular, they speak to the debate on whether interventions should be delivered to beneficiaries on a one-to-one basis or within a group. One-to-one delivery of interventions such as health education allow for the provision of tailored services, and hence may be more effective (though costly); while group-based interventions might be less costly but may entail a dilution in services. Our findings provide one additional benefit of group-based interventions – improved consumption smoothing as a result of increased social interactions

– which not only influences household welfare, but may also affect households' ability to adhere to the services provided by the intervention.

Our paper contributes to a number of strands of the literature. First, it adds to a body of work investigating the impacts and workings of community-driven (CDD) interventions, which have become an immensely popular way of delivering development aid (Mansuri and Rao, 2004). Such interventions aim to improve coordination between community members, as well as broaden participation in interventions by under-represented groups within a community. Existing studies, though, find mixed evidence on their efficacy. Fearon et al. [2009] find that a community driven reconstruction intervention in post-conflict Liberia improved social cohesion and co-operation; while Casey et al. [2012] document short-run improvements in local public goods in post-conflict Sierra Leone, but no long run changes on involvement of marginalised groups, collective action or decision making. Humphreys et al. [2015] also fail to find effects of a CDD program in the Democratic Republic of Congo on social outcomes. Labonne and Chase [2011] find that a CDD program in the Philippines has mixed impacts on social capital: the intervention increased participation in village assemblies and generalised trust, but lowered collective action and group membership. Banerjee et al. [2010] find no impacts of a program encouraging participation of parents on village education committees in rural India. However, an extensive literature in public health documents positive effects of participatory women's groups on health (see Prost et al., 2013 for a review); as well as community-led total sanitation on increasing uptake of sanitation (e.g. Pickering et al., 2016; Cameron et al., 2015). Our paper contributes to this literature by providing evidence of impacts on an as yet unconsidered outcome - consumption smoothing - in a poor, but not post-conflict setting.

Second, it also contributes to the literature on the strength of social ties and risk sharing or closely related outcomes. Guiso et al. [2004] show that individuals living in regions with higher social capital also engage in more complex financial transactions, Angelucci et al. [2015] show that individuals connected to a family network in the village attain better consumption smoothing. Fafchamps and Ferrara [2012] provide evidence supporting the hypothesis that self-help groups in urban Kenya play a mutual assistance role. Chandrasekhar et al. [2015] find, within lab-in-the-field experimental games, that socially connected ties are able to overcome enforcement constraints and cooperate in the absence of outside enforcement; while Breza and Chandrasekhar [2015] document that informing socially close, and more central villagers about an individual's savings increases the amount saved.

Finally, it contributes to a body of work on the impacts of groups on economic behaviour.

The microfinance literature has contributed to either justify theoretically or establish empirically a correlation between group formation and positive loan outcomes. Closest to our paper is the contribution by Feigenberg et al. [2013] who also randomly increase individuals' social interactions (in their case microfinance clients) by experimentally varying the frequency of microfinance meetings. Our paper complements theirs by looking at different outcomes (consumption smoothing rather than default probability), in a different setting (rural rather than urban), and studying a different type of group meeting - open community-wide meetings rather than microfinance group meetings. In another related study in post-conflict Uganda, Blattman et al. [2016] document that cash grants and training provided to women encouraged to form groups engaged more in informal finance as well as labour sharing, and enjoyed higher earnings, but did not increase their consumption in response.

The rest of the paper is organised as follows. Section 2 provides background on the context, and introduces the intervention and the experimental design. We outline our empirical strategy in section 3. Section 4 describes the data, including our measures of crop losses; Section 5 displays the main results, investigates the underlying mechanisms and rules out some alternative explanations. Section 6 concludes.

2 Background and Intervention

2.1 Context

Our study takes place in Mchinji, a rural district in the central part of Malawi. Socio-economic conditions in Mchinji are similar to or worse than the average for Malawi (in parentheses in what follows), with literacy rates of just over 60% (64%), poor quality flooring materials used by 85% (78%) of households, piped water access for 10% (20%) of households, and electricity access for just 2% (7%) of households. Subsistence agriculture is the most important economic activity in Mchinji, and provides the main income source for a large majority of households. Key crops include maize, tobacco and ground nuts. Agricultural production is mainly rain-fed, and the use of modern inputs and implements such as fertilizer is very rare; thereby leaving household incomes vulnerable to fluctuations from unpredictable weather as well as pests and crop diseases.

Access to formal financial and insurance products is limited: the 2008 Finscope Malawi Survey documented that only 3% of Malawians held an insurance product, and fewer than 20% had a formal bank account. Social connections such as family, friends and other community members are very important in helping households cope with risk. For example, Trinitapoli et al. [2014] document that older siblings play a role in protecting educational investments of younger siblings, while Munthali [2002] and Peters et al. [2008] document that extended family members foster and care for children orphaned by HIV/AIDS.

A number of contracting frictions have been documented to be present in the context of rural Malawi: Gine et al. [2012] document that difficulties in verifying an individual's identity introduce frictions in credit markets. Jack [2013] shows that private information hampers effective targeting of subsidies for an environmental program; while Guiteras and Jack [2014] document the implications of asymmetric information on labour markets in rural Malawi. These not only hamper the availability of formal credit and insurance markets, but may also influence the terms of informal arrangements, and may prevent them from providing full insurance.

2.2 Intervention

We consider a participatory women's group intervention aimed at improving reproductive health, set up in 2005 by the MaiMwana ("Mother and Child") Project. The intervention was implemented as a cluster randomised control trial in order to assess its effectiveness in improving maternal and neonatal health outcomes.

It aimed to impart information and mobilise the local community (village) through a participatory action cycle to act on issues relating to pregnancy, childbirth and newborn health, with the overarching goal of improving maternal and child health outcomes [Lewycka et al., 2010, Lewycka, 2011]. Trained local women (facilitators), covering a population of around 3,000 individuals each, visited study villages and encouraged women to form groups. Each was charged with forming and guiding between 6 and 12 groups.[1] Groups were guided through a four-stage community mobilization action cycle of fortnightly (in principle; every 4-6 weeks in practice) meetings under the guidance of the facilitator, where in the first stage (comprising 8 meetings) they identified and prioritised problems relating to pregnancy, childbirth and newborn health.[2] In the second stage (4

[1] The facilitators, who were aged 20-49 years, literate and mothers, received intial training over 11 days, followed by refresher training every 4 months. They were paid a salary, and provided with a bicycle, T-shirt, umbrella and field bag. They were also given a manual to implement the participatory action cycle, as well as picture cards to help guide discussions. They also received support and feedback from a supervisor.

[2] Rosato et al. [2006] summarize the problems identified by the groups in this setting relating to maternal health (anaemia, obstructed labour, malaria, retained placenta, and haemorrhaging, amongst others), while Rosato et al. [2009] summarise those relating to neonatal and infant health (diarrhoea, pre-term births, tetanus, asphyxia, infection and malaria).

meetings), the groups shared the results of their discussions with the wider community and devised strategies to overcome identified problems. To take an example, the problem identified might be a lack of transport to health centers in cases of emergency and the strategy to help overcome this was to lobby various donors to provide bicycle ambulances.[3] In the third stage (4 meetings), the groups involved the wider community in implementing the strategies, before evaluating their activities and making future plans in the final stage (4 meetings).

The primary targets of this intervention were women aged 15-49, and particularly pregnant women. However, older women who had completed their fertility were also encouraged to attend so as to share their experiences, and were considered to be very influential in shaping reproductive health outcomes within the community. From the third part of the cycle, where groups began implementing the strategies devised, men joined the groups and attended meetings. Around 31% of women aged 15-49 in our data report having attended at least one women's group meeting. Conditional on attending at least one meeting, women report having attended on average 12 women's group meetings over a 4-year period, indicating active engagement in the intervention among this group. Moreover, the groups were successful in reaching less affluent women and households: attendees were poorer than non-attendees (as indicated by an asset index and the quality of housing materials) older on average, and were less likely to have completed primary schooling. Attendees were also more likely to have been married at least once; while those from households with more than one adult woman were less likely to attend.

Potential Intervention Impacts on Consumption Smoothing The intervention could have influenced informal risk sharing arrangements in a number of ways, thereby affecting overall consumption smoothing. First, it provided women with a forum within which to interact with other women regularly, which could alleviate various contracting frictions. For example, the regular interaction might facilitate sharing of information on not just health, but also on other topics and local issues, reducing asymmetries in information about shocks experienced (or not) by other community members. The meetings also provide a forum for women to learn more about each other (e.g. about each other's 'types') thereby reducing adverse selection problems, and may also reduce the costs of monitoring effort, thereby reducing moral hazard frictions. Finally, interactions within the groups might also ease enforcement of risk sharing arrangements with other group

[3]Common strategies implemented by groups included vegetable garden cultivation, health education sessions, insecticide treated net distribution, health programme radio listening clubs and cleaning the surroundings around one's house.

members, reducing another barrier to efficient risk sharing (e.g. Feigenberg et al. [2013]). In reducing contracting frictions, the intervention might have allowed women (and their households) to either form new social connections that could support informal risk sharing; and/or strengthen existing arrangements to allow for more risk sharing.[4]

Second, the intervention could also have reduced households' exposure to health shocks, thereby improving their capacity to cope with crop loss shocks. We consider, and rule out this mechanism in Section 5.4.1.

2.3 Experimental Design

The evaluation is based on a cluster randomized controlled trial designed as follows (see Lewycka et al. [2010], Lewycka [2011]). Mchinji District was divided into 48 clusters by combining enumeration areas of the 1998 Malawi Population and Housing Census in a systematic way so that each cluster contained approximately 8,000 individuals.[5] Within each cluster, the 3,000 individuals (equating to 14 villages on average) living closest to the geographical centre of the cluster were chosen to be included in the study.[6] This leaves a buffer area between adjacent clusters created purposefully to limit contamination. 12 clusters were randomly selected to receive the intervention. A further 12 serve as controls.[7]

The intervention began in 2005, and was still in place when the data used in this study were collected (2008-09, and 2009-10).[8]

[4]However, the increased interactions with group members could crowd out other or weaken relationships with connections who were not group members, potentially crowding out existing risk sharing; and making the effects on overall consumption smoothing ambiguous.

[5]The District Administrative Centre was excluded because it is relatively more urbanized and hence less comparable to the rest of the District.

[6]The geographic centre was chosen to be the most central village in the cluster as shown on a cartographic map from the National Statistical Office, Malawi, and whose existence was corroborated with the District Commissioner's records. See Lewycka [2011], pp. 122 for more details.

[7]A further 12 were assigned an Infant Feeding intervention, and the final 12 were assigned both the Infant Feeding Intervention and the Women's Group one. We do not exploit these here; see instead Lewycka et al. [2013] and Fitzsimons et al. [2016]. MaiMwana Project also improved health facilities across the District, which benefitted both intervention and control clusters equally.

[8]Though groups began in 2005/06, the meetings cycle was not completed within 2 years as a result of delays arising primarily from groups trying to involve men in their activities (at the start of the second stage of the cycle). Most groups were in either the third or fourth stage of the cycle at the time of the first follow-up survey. Many groups were in the fourth stage at the time of the second follow-up survey.

3 Empirical Model

To identify whether the intervention improves consumption smoothing, we exploit its cluster randomised allocation and assess how household consumption smoothing varies with whether or not the household lived in a cluster that received the intervention. Our empirical specification is derived from a standard model of inter-temporal consumption smoothing in which households have a utility function of the constant relative risk aversion (CRRA) form (as is conventional - see for instance Cochrane, 1991, and Townsend, 1994), and face idiosyncratic and aggregate risk. The specification is as follows:

$$\Delta log(c_{hvt}) = \alpha \Delta crop_{hvt} + \gamma \Delta crop_{hvt} * D_v + \Delta X_{hvt} + \mu_{vt} + \Delta \varepsilon_{hvt} \tag{1}$$

where $\Delta log(c_{hvt})$ is the consumption growth rate for household h living in village v, between periods $t-1$ and t, $\Delta crop_{hvt}$ refers to changes between periods $t-1$ and t in household-level crop loss indicators (incidence and severity, separately), D_v is an indicator variable that equals 1 if, in 2004 (before the start of the intervention), household h lived in a cluster that was subsequently randomly selected to receive the women's group intervention, X_{hvt} is a vector of household level time-varying characteristics such as household size and demographics, and μ_{vt} is a set of village-specific time dummies which will absorb village-specific aggregate shocks.[9]

Under perfect consumption smoothing (or full insurance), the household consumption growth rate should move one-to-one with the aggregate consumption growth rate (Cochrane, 1991, Townsend, 1994), and be uncorrelated with household-level crop losses.[10] In our case, we take the village to be the unit within which risk sharing takes place, and so aggregate consumption is captured by the village-time dummies. We consider village-level risk sharing since the participatory groups were generally formed within a village, and so any impacts on consumption smoothing, including on social interactions would be concentrated within the village. Doing so also allows us to capture the net effects of the groups on village-level consumption smoothing.[11]

[9]In our application, because we have only two time periods, the term μ_{vt} is essentially a village fixed-effect.

[10]Shocks could also affect long term outcomes, such as education and health outcomes, see for instance Maccini and Yang [2009]. More generally, note that poor households who are close to subsistence could be smoothing consumption at the expense of investments, which would affect their long-term wellbeing (Chetty and Looney, 2006).

[11]It is possible that households that are part of a group may reduce risk sharing with those outside the group leading to some households being left worse off by the intervention. Unfortunately, we are unable to study such heterogeneity in effects since we do not have any exogenous variation in within-village group participation decisions.

If households are able to perfectly smooth consumption through existing mechanisms, and particularly in the absence of the participatory intervention, then we would expect the coefficient α to equal zero. If, however, households are unable to smooth consumption following a crop loss, α would be negative. γ identifies the additional consumption smoothing available to households in intervention areas experiencing a crop loss as a result of the intervention. The sum of the coefficients $\alpha + \gamma$ reveals whether households in intervention areas are perfectly able to smooth their consumption following an idiosyncratic crop loss. Our test for whether the intervention improves risk sharing therefore entails testing whether γ is positive and statistically significant from 0.

In terms of the dependent variable, we use two measures of household consumption: non-durable household consumption and food consumption. This choice is motivated by the fact that different types of consumption might be more or less sensitive to the effects of shocks, and also might be differentially sensitive to measurement error. Moreover, food consumption accounts for a large share of households' budgets in this context.

Regarding crop losses, we exploit both their incidence – whether or not a household experienced a crop loss, and severity - the estimated loss in income associated with the crop loss, normalised by predicted pre-intervention household consumption.[12,13] The normalisation adjusts for the fact that more wealthy households might experience larger crop losses since they have more land. Our empirical strategy relies on crop losses representing idiosyncratic (household-level) rather than aggregate (village-level) adverse events. We provide evidence on this, along with more details on the measurement and specification of the crop loss variables in Section 4.

Important consideration needs to be given to inference. The cluster-correlated Huber-White estimator for clustered standard errors have been shown to provide standard error estimates which are too small (and thereby over-reject hypothesis tests of significance) when the number of clusters is less than 30 (Donald and Lang, 2007, Wooldridge, 2004, Bertrand et al., 2004, Cameron et al., 2008). This is important here since the number of clusters is 24. To ensure correct inference, we implement a wild cluster bootstrap-t procedure recommended by Cameron et al. [2008] to compute the correct p-values for hypothesis tests of significance. Their Monte Carlo simulations suggest that this method performs relatively well with as few as 5 clusters.[14] In all estimation tables, we report the clustered

[12] This variable is predicted from a linear regression of household consumption on pre-intervention education of the survey respondent. We also experimented with including other baseline assets in the specification. The qualitative results reported in this paper do not change with the alternative specification.

[13] Our use of $\Delta crop_{hvt}$ follows Gertler and Gruber [2002]. Note also that changes in crop loss intensity closely relates to changes in income used by among others, Townsend [1994].

[14] Monte Carlo simulations reported in Fitzsimons et al. [2016] also indicate that the wild bootstrap

standard error, clustered standard error p-value along with the wild cluster bootstrapped p-value.

4 Data

4.1 Sample Description and Balance

The analysis in the paper is based on two years of survey data collected in 2008-09 and 2009-10, respectively 3.5 and 4.5 years after the intervention started in May 2005, combined with a more limited set of variables collected in a pre-intervention baseline census of all women of childbearing age by Mai Mwana in 2004 (Mai Mwana census hereon). A large majority of the groups were still in operation at the time of our follow-up surveys (Rosato et al., 2012), partly due to late completion of the participatory action meeting cycle (the median group completed the action cycle in October 2008), as well as continuation of meetings beyond the initial participatory action cycle.

This census contains basic socio-economic and demographic characteristics, displayed in Table 1, for households in our analysis sample. Households contain an average of 6 members. Housing conditions are poor, as demonstrated by the roofing and flooring materials. Less than 7% of households have access to piped water and less than 1% have access to electricity. Among assets, most households own a paraffin lamp, 64% own a radio, and 52% own a bicycle. Ownership of other assets such as cars, motorbikes and oxcarts is rare. Almost all households are involved in agriculture.

The sample for the follow-up surveys was drawn from the census: we selected a random sample of 104 women per cluster to survey. The left panel of Table 2 compares the means of available pre-intervention characteristics between the intervention and the control clusters for the sample drawn, and confirms that the randomization worked well. In the 2008-09 ("first follow-up", hereon) survey, we were able to interview around two thirds of the sample drawn of women of child bearing age (aged 17-43 at the time of survey) and their households. In addition to the time-lapse between the baseline and the first follow-up, two additional factors contributed to the attrition. First, collecting longitudinal data in Mchinji is prticularly challenging since respondents are known to report 'ghost' – or fictitious – household members in an effort to increase future official aid/transfers

method performs well in this setting.

Table 1: Baseline Descriptive Statistics

Variable	Mean	Std. Dev.
Main flooring material: dirt, sand or dung	0.916	0.278
Main roofing material: natural	0.858	0.349
Agricultural Household (yes=1)	0.995	0.069
Traditional pit toilet (yes = 1)	0.783	0.413
Piped water (yes=1)	0.063	0.242
# of hh members	5.781	2.376
# of sleeping rooms	2.157	1.083
Household has electricity (yes=1)	0.002	0.049
Household has radio (yes=1)	0.647	0.478
Household has bicycle (yes=1)	0.528	0.499
Household has motorcycle (yes=1)	0.007	0.085
Household has car (yes=1)	0.004	0.063
Household has paraffin lamp (yes=1)	0.952	0.214
Household has oxcart (yes=1)	0.055	0.229
N	1249	

Notes to Table: Sample contains women whose households were interviewed in our follow-up surveys, and whose households were resident in the same village for both follow-up surveys.

(that may increase with household size). Hence, it is possible that some women listed in the baseline census did not actually exist, and hence could not be found by the field team in 2008. Second, an unexpected sharp drop in the British Pound against the Malawi Kwacha in 2008 resulted in fewer resources to track women who had moved.

Reassuringly, this attrition rate is very similar across intervention and control clusters, at 67% and 63% respectively. The balance on a range of observed baseline characteristics is mostly maintained, as shown in the middle panel of Table 2, with small statistically significant differences appearing only on 1 variable indicating that attrition was not significantly different between intervention and control zones. The 2009-10 ("second follow-up") survey followed those women (and their households) who had been successfully interviewed in the first survey. 91% of these were reached: 92% and 90% in intervention and control areas respectively.

As is common practice in the consumption smoothing literature, we restrict the sample to households that were resident in the same village over both survey rounds. This is in order to control accurately for aggregate (village-level) adverse events through the term μ_{vt} in (1). This results in a smaller analysis sample: 1636 vs. 1249. The balance for this sample, again based on observed baseline characteristics displayed in the last three columns of Table 2, is maintained for all variables.

Table 2: Sample Balance

	Sample Drawn			Sample Interviewed			Analysis Sample		
	Control	Diff: T - C	p-val	Control	Diff: T - C	p-val	Control	Diff: T - C	p-val
Main Floor Material: Dirt, sand or dung	0.914	-0.015	0.498	0.916	-0.007	0.765	0.922	-0.016	0.492
Main Roof Material: Natural	0.853	-0.002	0.937	0.857	-0.008	0.819	0.868	-0.021	0.482
Agricultural HH (yes=1)	0.995	-0.009	0.492	0.995	0.000	0.921	0.994	0.000	0.991
Traditional pit toilet (yes = 1)	0.772	0.017	0.717	0.791	-0.009	0.855	0.787	-0.011	0.797
Piped water (yes=1)	0.011	0.110	0.132	0.009	0.108	0.171	0.011	0.103	0.248
# of hh members	5.772	-0.129	0.446	5.848	-0.144	0.293	5.911	-0.262	0.124
# of sleeping rooms	2.116	-0.003	0.963	2.152	-0.007	0.931	2.177	-0.041	0.757
HH has electricity (yes=1)	0.002	0.002	0.278	0.002	-0.001	0.603	0.003	-0.002	0.561
HH has radio (yes=1)	0.629	-0.015	0.643	0.641	-0.027	0.509	0.672	-0.052	0.116
HH has bicycle (yes=1)	0.508	-0.001	0.965	0.512	0.003	0.927	0.531	-0.007	0.901
HH has motorcycle (yes=1)	0.008	-0.002	0.723	0.007	-0.001	0.841	0.006	0.002	0.827
HH has car (yes=1)	0.006	-0.002	0.400	0.007	-0.006	0.030**	0.006	-0.005	0.144
HH has paraffin lamp (yes=1)	0.926	0.026	0.427	0.926	0.025	0.354	0.952	-0.001	0.981
HH has oxcart (yes=1)	0.058	-0.008	0.577	0.059	-0.006	0.713	0.062	-0.014	0.411
N	1249	1247		846	790		629	620	

Notes: * significant at the 10% level, ** at the 5% level. p-values computed using the wild cluster bootstrap-t method recommended by Cameron et al. [2008].

4.2 Survey Data

The follow-up surveys contain detailed socio-economic information such as non-durable consumption, education, labour supply, and self-reported health and anthropometric measurements for young children, as well as information on adverse events experienced by the household and social interactions as measured by the extent of one-to-one chats taking place.

The extensive consumption module in the survey required respondents to report, at the household level, the quantity consumed and purchased, and the amount spent on the purchase of 25 different food items in the week prior to the interview. It also elicited expenditures on other important household items including clothing, health, education, and housing improvements, among others. The latter items were collected for recall periods of 1 month (for items such as fuel, utilities and transport), and 12 months or since the last survey (9-11 months) (for items such as house improvements, clothing, health, and education expenditures). In the 2009-10 survey, information was also collected on conversion units, allowing us to convert non-standard units of measurements (such as a cup of beans) to standard units (such as kilograms and grams). Total household food consumption is computed by summing expenditures on food and imputed values of non-purchased food.[15] The average monthly food consumption for households in our sample is about 9874 MK (~US$70) , while average total monthly non-durable consumption is 11,808MK (~US$84).

4.3 Measuring Adverse Events

Crop losses, described here, are a particularly important adverse event in our setting, where practically all households are involved in agriculture. These could arise for a number of reasons including poor weather conditions (which could be idiosyncratic within a village as documented by among others Udry, 1995), localised crop diseases, pests, fires, and so on. Information on crop losses was collected from two questions: first from a

[15] To value food that was not purchased, we first use the conversion units collected in 2009-10 to convert foods measured in non-standard units into standard units. We then use median unit-values (computed by dividing expenditure on a certain good by the quantity purchased, and taking the median at the zone and district levels) to value this non-purchased consumption. An alternative method is to use market prices, which were also collected from local markets and trading centres most regularly visited by sampled households. This is not our preferred method, since most households rarely purchase the foods they commonly consume from the markets, and we may over-value their consumption this way. Reassuringly though, valuing consumption by either method yields the same food consumption share of total non-durable consumption of 0.86. Total non-durable consumption is computed by converting all consumption and expenditure values into monthly terms and summing them up.

Table 3: Incidence and Severity of Crop Loss Events

Variable	N	Mean	Std. Dev.
Pooling Both Waves			
Crop loss in past 12 months (or since last survey); yes=1	2498	0.284	0.451
Intensity of crop loss (as share of predicted consumption)	2473	0.288	0.848
Wave 1			
Crop loss in past 12 months (yes=1)	1249	0.353	0.478
Intensity of crop loss (as share of predicted consumption)	1237	0.401	1.067
Wave 2			
Crop loss since last survey (yes=1)	1249	0.215	0.411
Intensity of crop loss (as share of predicted consumption)	1236	0.175	0.524

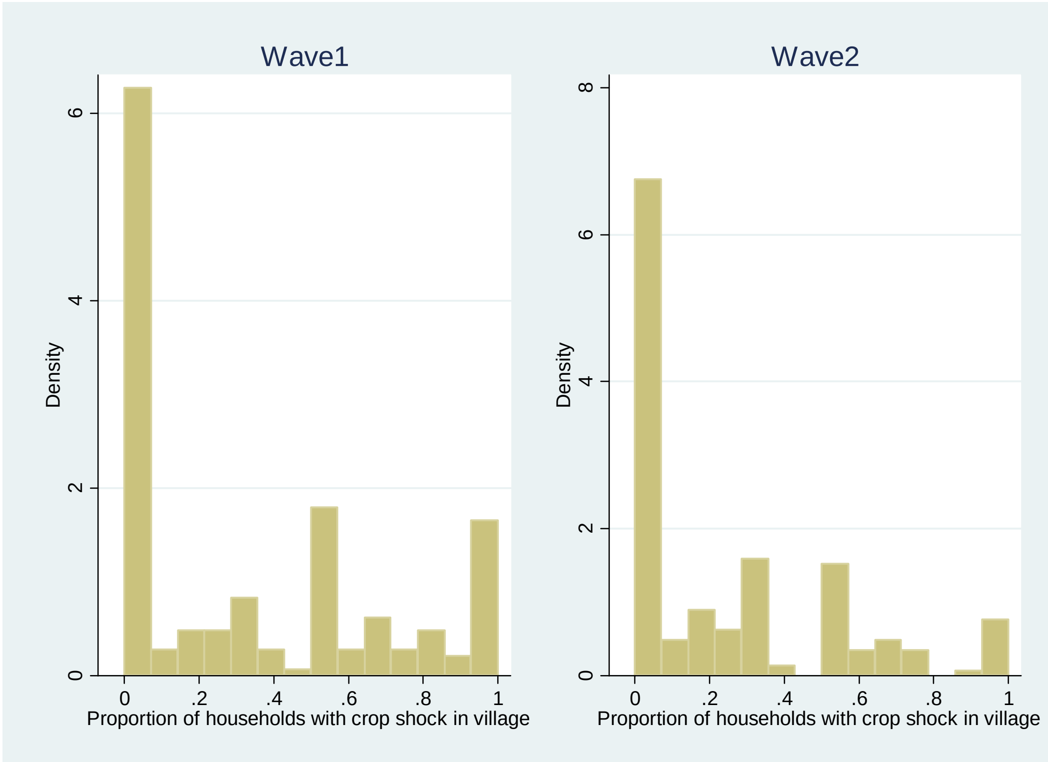
Notes to Table: This table pools households in both control and treated clusters, who were living in the same village during both follow-up surveys. Predicted consumption is calculated from a regression of monthly household non-durable consumption over baseline (2004) education of the main respondent.

question asking households were asked whether they had experienced a crop loss in the year (9-11 months) prior to the first (second) follow-up survey.[16] If yes, households were asked to report the size (severity) of the crop loss, in terms of the estimated income loss associated with it. Table 3 displays the prevalence and severity of crop losses in our sample, with the latter variable normalised by predicted pre-intervention consumption. The uppermost panel shows the incidence and size (as a proportion of predicted consumption) of crop loss events when data from both rounds are pooled, while the lower two panels show these for each round of data collection.

On average across both rounds, 28% of households experienced a crop loss. The size of the average loss was around 29% of predicted monthly pre-intervention consumption. Among households that experienced a crop loss, the average size of the loss is about 93% of predicted monthly pre-intervention consumption. Finally, disaggregating by round highlights differences in the incidence of crop loss events and the size of the crop loss between rounds, with a substantially higher incidence and severity of crop loss in the first wave compared to the second. This is most likely the case for two reasons. First, the incidence of crop losses in the second wave relates to a shorter period (9 to 11 months as opposed to 12 months). Second, data collection in the second (first) wave took place during (after) the main growing season, and so not all crop losses would have been re-

[16] In the second follow-up survey households were asked about crop losses since the first follow-up survey, between 9 and 11 months for most households.

Figure 1: Village level proportion of crop losses among all sampled households

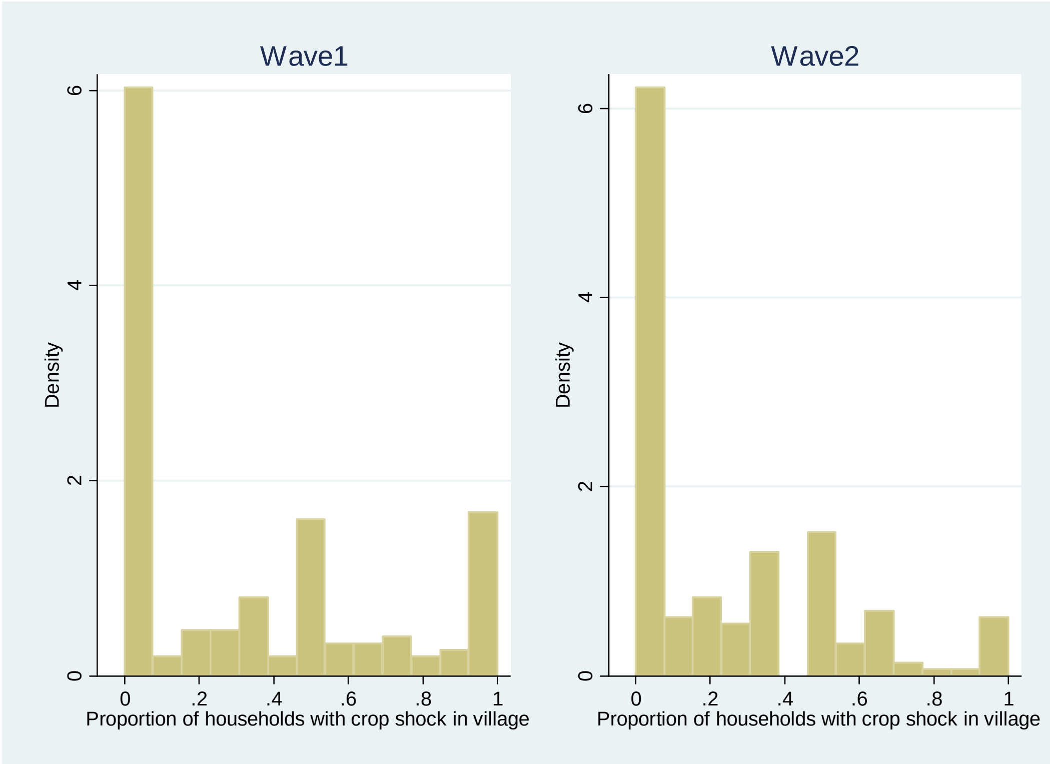


alised by the time of the second wave interview. Note that we control for the differing household-level recall periods in the empirical work.

Our empirical strategy relies on the fact that crop losses represent idiosyncratic (household-level) and not aggregate (village-level) adverse events. Crop losses could affect all households in a village if they are caused by common weather shocks or natural disasters affecting the whole village. To assess the idiosyncratic variation in crop losses, Figure 1 plots a histogram of the village level proportion of households experiencing a crop loss in our sample.

The left (right) panel of the figure shows the density of the village level proportion of crop losses in the first (second) wave. Both show substantial variation within the village in the incidence of crop losses, though there are some villages where no household experienced a crop loss and others where all households experienced a crop loss. Reassuringly, this variation does not simply reflect variation in the distribution of occupations across villages, as can be seen in Figure 2, which plots the same distribution as Figure 1 but is restricted to households where the head or spouse report agriculture as their main income generating activity.

Figure 2: Village Level proportion of crop losses among agricultural households



In any event, we note that we net out the effect of any aggregate (village-wide) events by controlling for village-time dummies in the estimation, so the crop loss picks up the effects conditional on these.[17]

5 Results

5.1 Consumption Smoothing

We now turn to the main findings, as estimated using the specification shown in section 3. Table 4 displays the main results, for log total non-durable consumption (Cols. 1 and 2) and log total food consumption (Cols 3 and 4). The Table indicates that crop losses result in a reduction in household consumption growth, as is evidenced by the negative coefficients on $\Delta crop$. A crop loss equivalent to a month's predicted pre-intervention consumption results in a reduction in consumption growth of around 6%. However, there

[17]Since we only have two time periods, the village-time dummies are indeed village fixed-effects.

Table 4: Consumption Smoothing Results

	[1]	[2]	[3]	[4]
	$\Delta\, log(C_{hvt})$ crop = 0 or 1	$\Delta\, log(C_{hvt})$ crop = loss/pre-shock cons	$\Delta\, log(F_{hvt})$ crop = 0 or 1	$\Delta(log\, F_{hvt})$ crop = loss/pre-shock cons
Δcrop	-0.0948	-0.0592**	-0.0985	-0.0485*
	[0.0567]	[0.0220]	[0.0602]	[0.0278]
Wild cluster bootstrap p-val	{0.158}	{0.032}	{0.151}	{0.074}
Δcrop*treat	0.140*	0.0411**	0.138*	0.0308
	[0.0792]	[0.0227]	[0.0778]	[0.0288]
Wild cluster bootstrap p-val	{0.068}	{0.011}	{0.078}	{0.266}
N	1245	1221	1243	1219
Intra-cluster correlation	0.111	0.111	0.095	0.095
R-squared	0.409	0.412	0.398	0.402

Notes to Table: Sample includes households resident in the same village in both follow-up surveys. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the cluster level. Wild cluster bootstrap p-values reported in curly braces. Non-durable and food consumption values in per-capita terms.

is also strong evidence of consumption smoothing in intervention villages: indeed, the coefficient on the interaction term suggests that households living in villages in intervention clusters are managing to completely protect their consumption against crop losses. This finding is particularly strong for the 'severity' measure of crop losses (crop loss as a proportion of predicted consumption), as can be seen from Column [2].

From this evidence, it appears that participatory community interventions can help households to completely smooth even significant crop losses. Our results also suggest that the crop loss incidence (most commonly used in the literature) is a rather blunt measure of the adverse event, and data on its severity also provides more useful information.

The Table also presents the results for total household food consumption. The results are qualitatively similar to those reported for household non-durable consumption. Households reduce food consumption in order to deal with more severe crop losses, and the intervention appears to aid households in protecting their food consumption, as evidenced by the positive coefficients on the interaction terms. All coefficients on the interaction terms have the expected sign and are statistically significant in most specifications.

5.2 Savings and Assets

Having considered how the intervention influences consumption smoothing in the event of a crop loss, we next study effects on cash savings and assets, specifically livestock. In addition to informal risk sharing with other households through transfers, gifts and loans, another way through which households could cope with crop losses may be by drawing down savings and assets (Paxson, 1992; Rosenzweig and Wolpin, 1993). This is a margin with important consequences for long run household well-being, since production and consumption decisions for farm households in developing countries are known to be interlinked. Households might use productive assets such as livestock to smooth their consumption, impacting future income generation and longer-run poverty status (Banerjee and Newman, 1993; Aghion and Bolton, 1997).

To see how household savings and asset holdings following a crop loss vary by treatment status, we estimate Equation 1 using as the dependent variable changes in cash savings, changes in the number of small animals (e.g. poultry and rabbits), and changes in the number of large animals (e.g. cows and sheeps) rather than changes in log consumption. Table 5 displays the results. Though the estimates are noisy, the Table indicates that households in control areas hold fewer savings and livestock following a crop loss, indicating that they partly self-insure these shocks. This pattern is reversed in treated clusters: households experiencing a crop loss hold one more large animal than those in control areas, suggesting that these households no longer rely on livestock sales to smooth their consumption.

Thus, the intervention not only improves consumption smoothing, but also reduces asset decumulation following crop losses. This has important implications for future household well-being since higher savings has been shown to be linked with higher agricultural input use as well as higher agricultural yields in this context (Gine et al., 2012).

5.3 Possible Mechanisms

The findings thus far indicate that not only did consumption smoothing improve in intervention areas, but also that households in intervention areas are less likely to draw down assets, particularly large animals, to smooth their consumption compared to households in control clusters. Thus, improvements in consumption smoothing are likely to be driven by improvements in informal risk sharing. In particular, we hypothesize that the groups

Table 5: Effects of Crop Losses on Savings and Assets

	[1]	[2]	[3]	[4]	[5]	[6]
	Δ Savings (MK)		**Δ # Small animals**		**Δ # Large animals**	
	crop = 0 or 1	crop = loss/pre-shock cons	crop = 0 or 1	crop = loss/pre-shock cons	crop = 0 or 1	crop = loss/pre-shock cons
Δcrop	-2,786*	106.3	-0.642	-0.216	-0.695*	-0.308
	[1,450]	[1,397]	[0.545]	[0.219]	[0.362]	[0.298]
Wild cluster bootstrap p-val	{0.062}	{0.989}	{0.198}	{0.302}	{0.076}	{0.296}
Δcrop*treat	2,280	-951.6	1.525	0.23	1.117**	0.403
	[2,601]	[1,448]	[0.961]	[0.312]	[0.490]	[0.309]
Wild cluster bootstrap p-val	{0.372}	{0.573}	{0.090}	{0.410}	{0.016}	{0.162}
N	1,235	1,211	1,244	1,220	1,245	1,221
R-squared	0.329	0.332	0.355	0.355	0.348	0.351
Intracluster Correlation	0	0	0.0259	0.0259	0	0

Notes to Table: Sample includes households resident in the same village in both follow-up surveys. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the cluster level. Wild cluster bootstrap p-values reported in curly braces. 'Savings' capture cash savings in Malawi Kwacha; 'Small Animals' include livestock such as poultry and rabbits while 'Large Animals' include cattle, goats and sheep.

increased social interactions within communities, which in turn could have reduced contracting frictions including information asymmetries and imperfect enforcement.

We now test more directly whether the intervention did indeed improve social interactions. To do so, we focus on a measure of interactions outside the participatory women's groups – one-to-one chats with family or friends about a range of health and non-health related topics. By focusing on chats with family and friends, we capture interactions with 'strong' connections, who are likely to be informal risk sharing partners.[18] Note that while this measure provides an indication of changes in the frequency of interaction with relatives and friends as a whole, we are unable to disentangle whether the change comes from more frequent interactions with the same contacts; or from similarly frequent interactions with a broader group of social contacts; or a combination of the two.

To improve power, we combine indicators of whether a woman chatted with family or friends on a one-to-one basis on each of 6 topics into two indices following the method of Anderson [2008].[19] We construct indices for any chats about health related topics (pregnancy and birth delivery, breastfeeding and post-breastfeeding nutrition and family planning), and any chats about non-health topics (local governance, work opportunities and obtaining credit).

To study the intervention impacts on social interactions outside the group, we estimate regressions of the following form:

$$y_{hvt} = \beta_0 + \beta_1 D_v + X_{hvct}\pi + \eta_t + \nu_{hvt} \tag{2}$$

where y_{hvt} is the index of chats, D_v is the treatment indicator, X_{hvct} is a vector of observed characteristics including the age and education of the main respondent, and cluster-level baseline controls including average female education, marriage rates and average household size and η_t includes controls for month-year of interview. If the intervention changed social interactions with family and friends outside the groups, $\beta_1 > 0$.

Table 6 displays the results. They indicate that the intervention has positive effects on one-to-one chats on both health and non-health topics, though only the effect on the former is statistically significant. This might be because the intraclass correlation for non-health outcomes outcomes is more than double that for health chats (0.178 vs. 0.088), thereby re-

[18]Existing literature indicates that the extended family is an important source of risk sharing in this context (see, for example, Fitzsimons et al., 2015 among others).

[19]The survey main respondent was asked to give the number of relatives, friends, and health visitors/facilitators she chatted with in the week prior to the survey on a one-to-one basis about pregnancy or birth delivery, breastfeeding or post-breastfeeding nutrition, family planning, local governance, work opportunities and obtaining credit. The second follow-up survey also asked women to provide the number of acquaintances they spoke to about these topics on a one-to-one basis.

Table 6: Intervention Effects on Chats

	[1]	[2]
	Any health chats	**Any non-health chats**
Treat	0.278**	0.161
	[0.101]	[0.130]
Wild Cluster Bootstrap p-val	{0.024}	{0.292}
N	2,503	2,503
R-squared	0.126	0.076
Intracluster Correlation	0.088	0.178
Mean-Control	-0.093	-0.053

Notes to Table: Sample includes households resident in the same village in both follow-up surveys. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the cluster level. Wild cluster bootstrap p-values reported in curly braces. 'Health Chats' is an index aggregating binary variables indicating whether the main respondent chatted with a friend or family member on a one-to-one basis about pregnancy and birth, breastfeeding and post-breastfeeding nutrition, and family planning in the week prior to the survey. 'Non-health chats' constructs a similar index for chats related to credit, jobs and local politics. All indices are constructed according to the method of Anderson [2008]. All regressions include controls for age, age-squared and education for the main respondent, month of survey dummies, household demographics, and baseline cluster-level variables including proportion of women with secondary schooling, average female marriage rate and average household size.

duing our power to detect true effects. These increased interactions could have improved the sharing of information relevant to alleviating information related contracting frictions (for example, conversations originally about health related topics could also involve sharing of "gossip" which may spread information about specific households experiencing a shock -and those lying about them-, thereby reducing frictions associated with hidden income); or enforcement constraints (since more frequent interactions might make it more costly for a household to renege on an arrangement). Unfortunately, our research design does not allow us to disentangle the precise constraint(s) affected by the intervention. This is left to future work.

5.4 Ruling out alternative mechanisms

5.4.1 Improved Health

Though we consider intervention impacts on consumption smoothing in response to a shock not targeted by the intervention, it is possible that this response could also be influenced by health improvements due to the intervention. In particular, the intervention could have improved both reproductive health outcomes, as well as health more generally within the household, for instance by increasing awareness of health issues, or making these more salient. This may put households in a better position to cope with any adverse events that occur – being in better health may mean that individuals can work harder, for instance.

Lewycka et al. [2013] document a reduction in maternal mortality of 74% due to the intervention. This would have resulted in approximately 3 less maternal deaths among treated households in our sample, which is too small to explain the entire consumption smoothing effect we uncover. This makes it unlikely to be the mechanism through which the intervention improves consumption smoothing of crop losses.

Nonetheless, the intervention could also have improved adult health more generally, thereby leading to fewer health shocks. To investigate this possibility, Tables 7 and 8 report the effect of the intervention on several self-reported indicators of adult health, for males and females respectively. According to the results, there is no evidence that the intervention improved adult health, for both symptoms experienced in a short interval prior to the survey, as well as in indicators relating to activities of daily living, which are better at capturing longer-term health issues.[20] This provides further evidence indicating that the key findings are not driven by health improvements due to the intervention.

[20] Though not reported here, we also examine and rule out that the intervention affected children's health. These results are available on request.

Table 7: Effect of Intervention on Male Adult Health

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
	Health Index	No diarrhoea in last 30 days	No fever in last 30 days	No cough in last 30 days	No chills in last 30 days	No vomit in last 30 days	Able to carry out daily activities	Can easily carry 10kg load for 20m	Can easily walk 5km
Treat	0.022	-0.004	-0.069	0.016	0.001	0.015	-0.043	0.022	0.041
	[0.063]	[0.013]	[0.043]	[0.050]	[0.031]	[0.018]	[0.039]	[0.033]	[0.029]
Wild Cluster bootstrap p-val	{0.673}	{0.781}	{0.138}	{0.637}	{0.975}	{0.390}	{0.340}	{0.519}	{0.190}
Observations	3124	3,124	3,124	3,124	3,124	3,124	3,124	3,124	3,124
R-squared	0.051	0.003	0.014	0.002	0.005	0.004	0.009	0.091	0.119
IntraCluster Correlation	0.0868	0.0114	0.0557	0.0648	0.0604	0.0173	0.0358	0.0719	0.0629
Average in Control	-0.0107	0.947	0.77	0.724	0.912	0.901	0.719	0.911	0.89

Notes to Table: Sample includes male individuals aged 15-64 living in households that were resident in the same village in both follow-up surveys. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the cluster level. Wild cluster bootstrap p-values reported in curly braces. The Health Index aggregates the responses to questions in Columns 2 - 9 using the method of Anderson [2008].

Table 8: Effects of Intervention on Adult Female Health

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
	Health Index	No diarrhoea in last 30 days	No fever in last 30 days	No cough in last 30 days	No chills in last 30 days	No vomit in last 30 days	Able to carry out daily activities	Can easily carry a load of 10kg for 20 metres	Can easily walk 5km
Treat	0.006	-0.008	-0.045	-0.028	0.003	-0.002	0.007	0.04	0.027
	[0.062]	[0.018]	[0.041]	[0.050]	[0.042]	[0.029]	[0.043]	[0.035]	[0.036]
Wild Cluster Bootstrap p-val	{0.849}	{0.753}	{0.336}	{0.639}	{0.875}	{0.989}	{0.871}	{0.292}	{0.448}
Observations	3398	3,398	3,398	3,398	3,398	3,398	3,398	3,398	3,398
R-squared	0.069	0.003	0.019	0.003	0.006	0.008	0.011	0.144	0.153
IntraCluster Correlation	0.0855	0.021	0.0386	0.064	0.0832	0.0329	0.0358	0.0656	0.0623
Average in Control	-0.00183	0.924	0.668	0.719	0.87	0.854	0.588	0.87	0.855

Notes to Table: Sample includes female individuals aged 15-64 living in households that were resident in the same village in both follow-up surveys. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the cluster level. Wild cluster bootstrap p-values reported in curly braces. The Health Index aggregates the responses to questions in Columns 2 - 9 using the method of Anderson [2008].

Table 9: Intervention Effects on Crop Loss Incidence and Intensity

	Wave 1		Wave 2	
	[1] **Crop Loss=1**	[2] **Crop Loss Size/ Predicted Cons**	[3] **Crop Loss=1**	[4] **Crop Loss Size/ Predicted Cons**
Treat	0.0739	0.096	0.0688	0.0225
	[0.114]	[0.149]	[0.0733]	[0.0787]
Wild Cluster Bootstrap p-val	{0.390}	{0.525}	{0.517}	{0.553}
N	1,249	1,237	1,249	1,236
IntraCluster Correlation	0.313	0.0929	0.169	0.103

Notes to Table: Sample includes households resident in the same village in both follow-up surveys. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the cluster level. Wild cluster bootstrap p-values reported in curly braces.

5.4.2 Reduced Prevalence and/or Severity of Crop Loss Events

Another way in which the intervention could improve consumption smoothing is by reducing the incidence or severity of crop losses. For instance, the intervention could have induced increased co-operation within the village, increasing sharing of important inputs (such as farming tools and labour) and agricultural knowledge, and as a result, lowering exposure to crop losses. With less frequent crop losses, a household's within-village risk sharing partners would potentially face fewer requests for help, thereby enhancing risk sharing possibilities within the village.

We test whether the intervention altered exposure to crop losses using the following specification:

$$crop_{hvt} = \lambda_0 + \lambda_1 D_v + \psi_{hvt} \tag{3}$$

where $crop_{hvt}$ is the measure of crop loss incidence or intensity for household h in village v at time t, and D_v is as defined in Section 3. $\beta < 0$ would indicate that the intervention indeed reduced the incidence and intensity of crop losses.

Table 9 displays the coefficients from an OLS regression of Equation 3 separately for the two survey rounds. All coefficients are positive and statistically insignificant from 0, suggesting that the intervention did not decrease exposure to cross losses.

6 Conclusion

This paper studies the spillover effects of a participatory community based health intervention in rural Malawi on an outcome not directly targeted by the intervention: household consumption smoothing following idiosyncratic crop losses. Group-based community ones are considered to be a cost-effective way of delivering interventions in low-income settings. However, design features of these programs could also influence dimensions of household and community-behaviour beyond those targeted by the intervention, generating important spillovers.

To provide causal estimates, we exploit a cluster randomised trial in which a participatory women's group intervention targeting maternal and child health was randomly allocated to some groups of villages (clusters), while a similar number of clusters received no intervention. The intervention encouraged community members, particularly women, to meet on a regular basis to discuss maternal and child health related issues, before mobilising the community to combat the identified issues. By facilitating the formation of groups that met regularly, and that aimed to involve a broad swathe of the community, the intervention could have increased social interactions and alleviated contracting frictions thereby improving consumption smoothing.

We find that consumption smoothing – measured as the response of changes in log household (non-durable) consumption to idiosyncratic crop loss events, net of village level aggregate shocks – worsens following a crop loss for households in control clusters. Those in intervention clusters, however, are able to compensate for this loss and almost perfectly insure their consumption. We also find that households in intervention areas are less likely to draw down their savings, and hold more large animals following a crop loss than households in control areas. This has important implications for households' efforts to escape poverty in the long-run.

Examining mechanisms underlying this effect on consumption smoothing, we find suggestive evidence that the intervention increased social interactions. This, in turn, could have alleviated a number of contracting frictions impeding informal risk sharing in this context, thereby increasing overall risk sharing and consumption smoothing. Crucially, we also rule out that these effects were driven by improvements in general health, or a reduction in the incidence of crop losses.

Though we find suggestive evidence indicating that social interactions increased, and contracting frictions may have been alleviated, our research design doesn't allow us to pin down more clearly the exact frictions alleviated in this context. Testing this is left to future work.

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