THE MILTON FRIEDMAN INSTITUTE FOR RESEARCH IN ECONOMICS

MFI Working Paper Series
No. 2011-008

Micro-Savings and Informal Insurance in Villages: How Financial Deepening Affects Safety Nets of the Poor, A Natural Field Experiment

Jeffrey Flory
University of Chicago, Becker Center on Chicago Price Theory

October 2011

1126 East 59th Street
Chicago, Illinois 60637

T: 773.702.7587
F: 773.795.6891
mfi@uchicago.edu

Micro-Savings and Informal Insurance in Villages: How Financial Deepening Affects Safety Nets of the Poor, A Natural Field Experiment

Jeffrey A. Flory
Becker Center on Chicago Price Theory
University of Chicago
jflory@uchicago.edu

Draft October 26, 2011

Selected Paper prepared for presentation at the Advances with Field Experiments conference, Becker-Friedman Institute for Research in Economics, University of Chicago, October 28-29, 2011

Copyright 2011 by Jeffrey A. Flory. All rights reserved. Readers may make verbatim copies of this document for non-commercial purposes by any means, provided that this copyright notice appears on all such copies.

Micro-Savings & Informal Insurance in Villages: How Financial Deepening Affects Safety Nets of the Poor, A Natural Field Experiment

Jeffrey A. Flory[*]

October 26, 2011

Abstract

This paper uses a large natural field experiment to identify the effects of formal savings on inter-household transfers in villages, and the spillover impacts of service-expansion on de facto ineligibles residing in the same community. Despite widespread interest in microfinance, spillover effects on the very poor of expanding formal financial services remain largely unexplored. This study helps fill this gap by examining evidence from an experiment which uses an information intervention that mimics naturally-occurring institutions to increase formal service adoption. It also contributes to an emerging literature on the indirect impacts of policy interventions in developing countries, often evaluated solely on the basis of how they impact direct participants and beneficiaries. In developing regions, households vulnerable to extreme poverty often rely on local safety nets based on transfers from relatives and friends, which help them smooth consumption across food-deficits and household shocks. To date, little is known about how these pre-existing practices are affected as community members begin adopting newly available formal financial services. Using a panel dataset of over 2,000 households collected during a rapid expansion of formal savings services in Central Malawi, this paper shows that experimentally boosting use of formal savings in rural areas sharply increases inter-household transfers during peak periods of hunger. The impact on transfer receipts is strongest among the poorest households, a *de facto* financial services-ineligible group, among whom the effects are also linked to significant changes in welfare. The strong impacts of formal savings expansion on *non* service-users suggests that formal finance can have much greater immediate-term effects than would be suggested by focusing exclusively on impacts experienced by service-users. The findings also highlight the sensitivity of traditional safety nets and welfare outcomes among the highly vulnerable in villages to expansion of formal financial markets.

Keywords: Microfinance, formal savings, indirect effects, safety nets, poverty, food security

JEL Codes: O17, O16, O12, I30, I38, I10

[*] University of Chicago, Becker Center on Chicago Price Theory, jflory@uchicago.edu.

I. Introduction

The promise of financial services for the poor in developing economies has been embraced by the development sector, and is quickly broadening to encompass non-credit services. In addition to determining the best ways to get loans to poor households, economists and policy-makers are now eagerly exploring how crop-insurance may benefit smallholder farmers, and some large aid organizations have made it their mission to spread access to formal savings across low-income countries.[1] However, as the extension of capital markets broadens in scope and penetrates deeper into isolated areas, there remain crucial gaps in our understanding of the effects. In particular, there is little research on how formal services interact with pre-existing local practices which may be key to household welfare.

In his analysis of risk-bearing capacities among villages in northern Thailand, Townsend (1995a) observes that the community most integrated into outside markets also had notably weaker informal credit and insurance institutions, as well as sharper consumption shocks among its worst-off households. The suggestion that penetration of formal financial markets may harm village social safety nets, and increase vulnerability to adverse income and consumption shocks, is echoed elsewhere (e.g. Besley (1995), Morduch (1999)). Yet there remains little rigorous causal evidence, surprising given the intense interest in microfinance and its growing reach.

This paper takes advantage of a rapid expansion of access to formal savings in rural Malawi to examine this question more closely. Starting in late 2007, a local microfinance organization quite literally brought the bank closer to villages through a fully-equipped bank on wheels, driving the bank to more remote areas. The bank's expansion of formal savings access, along with a community-level information intervention that boosted local savings use, enables

[1] The Bill and Melinda Gates Foundation, for example, has made the promotion of formal savings in rural areas of the developing world a major objective of its development-related initiatives (Guth, 2008).

identification of causal impacts of formal savings technologies on informal insurance practices. The findings, which show that safety nets can *strengthen* after an exogenous increase in local savings rates, shed new light on the interaction of capital markets with social safety nets in villages. They also suggest a more careful appraisal of empirical studies in microfinance that do not explicitly account for indirect effects, as an inappropriate comparison group may strongly bias estimates.

Most research on microfinance has tended to focus on financial service-users themselves. Few studies consider the broader institutional contexts in which new product take-up decisions are being made, or explicitly consider spillover effects on the *non-using* population. Yet the practical reality of wealth-constrained access to financial services during the early stages of financial deepening means the non-using population can initially be large. Introducing new services may affect institutions which evolved to fulfill important economic roles, with unintended consequences for non-users, and there remains scant evidence to serve as a guide.

Ligon, Thomas, and Worrall (2000), and Foster and Rosenzweig (2000) represent two exceptions to the absence of studies examining the interaction of formal and informal institutions. Both model the introduction of formal services in the presence of informal mutual insurance contracts – the former restricting their focus to a new savings technology, the latter modeling the simultaneous introduction of both formal savings and credit. Both studies are consistent with the notion that the introduction of formal services tends to reduce the size and scope of informal insurance which is based on inter-household wealth flows.

This paper helps build our understanding of the interaction of formal and informal institutions by furnishing strong causal evidence through a clean and direct estimation strategy. First, the analysis here empirically disentangles the effects of formal savings from that of credit.

This is necessary in order to develop a deeper understanding of the interaction of formal and informal systems. Moreover, the reality that access to formal savings can precede access to formal credit, sometimes by long periods, highlights the policy relevance of distinguishing the empirical effects of one from the other.

Second, identification of causal effects in the present study rests on a more solid foundation. By relying on a randomly assigned instrument in the form of an information intervention, the analysis avoids many of the endogeneity concerns which hinder the limited existing evidence regarding impacts of formal capital markets on informal insurance.

The findings of this paper also helps advance our understanding of heterogeneous effects in microfinance. By looking at households of differing vulnerability, the analysis shows clear variation in impacts of formal savings expansion on inter-household transfers, across subpopulations of key policy relevance. In particular, the results show a strong impact of formal savings expansion on safety nets and outcomes of the poorest households – those least in a position to use formal services, and one of the most crucial populations for anti-poverty policy.

Finally, by identifying a strong spillover effect on non-users, this paper expands on an important emerging literature which explores indirect effects of policy interventions in developing countries. A seminal study in this new thread is that by Angelucci and DeGiorgi (2009), who find strong effects of the Mexican welfare program, Progresa, on households that are *not* eligible to participate. Their findings underscore the importance of accounting for the higher degree of interdependence common in many village settings, which can amplify the propagation of effects beyond those directly affected. Most project and policy evaluations focus on how participants and beneficiaries are affected, which can lead to important over- or under-assessment of effects, and inaccurate impact estimates. By identifying a group of *de facto*

ineligibles and the effects they experience, this paper finds broader local effects of an additional type of intervention now commonplace in the developing world – that of microfinance. That non-users can also be strongly affected also has important implications for a range of empirical studies of microfinance, suggesting qualifications may be necessary in cases where comparison groups are susceptible to indirect effects. In such cases, estimated effects may be biased.

Contrary to the limited existing evidence suggesting formal finance weakens local safety nets, the introduction of formal savings technologies in rural Malawi has a clear and significant *positive* effect on inter-household wealth flows over the short-term. In particular, in communities where savings use was experimentally boosted, the proportion of those receiving cash-gifts from other households during the hungry season is nearly 50% higher. When restricting to the most vulnerable, whose poverty makes them essentially ineligible for services-use, and for whom the impact is thus via an indirect channel, the difference grows to 180%. Instrumental variables estimates indicate that, for every one percentage-point increase in the proportion of local households using formal savings, the worst-off households experience a three percentage-point increase in the probability of receiving a cash gift.

In addition, the most vulnerable households in savings-encouraged communities experience an uptick in loan receipts very similar in scale to the increase observed for cash gifts. Savings-encouraged villages exhibit increases in the proportion of highly vulnerable households receiving loans from friends and relatives by 14.4 to 22.4 percentage points.

These increases in assistance-receipts are also associated with significant welfare impacts. Living in communities that received the saving encouragement caused two-year improvements in at least three key welfare indicators among the worst-off. Households are 11.8 to 16.3 percent more likely to exit the worst food-security category to enter one of the three other

less severe categories. They also experience a 1.3 to 1.4 reduction in a continuous food-insecurity score, representing a 10-12% improvement over baseline values for this food-security indicator. In addition, the worst-off households living in savings-encouraged communities were 12 to 17.4 percent less likely to report any members of the household as recently unwell.

The rest of the paper is organized as follows. The next section explains the centrality of risk and uncertainty in village life, indigenous responses to try to prevent its often severe consequences, and possible effects of microfinance on these practices. Section 3 describes the empirical setting, data, and identification strategy used to test for the effects of formal savings services on local inter-household assistance practices. Section 4 analyzes the effects of the information intervention on local rates of financial services use. Section 5 examines the relationship between the information-encouragement and receipts of cash and in-kind gifts among the most vulnerable households (i.e. the reduced form effect of the instrument on cash-gifts). Section 6 uses an instrumental-variables analysis to estimate the Indirect Treatment Effect (ITE) of increasing the proportion of local households using formal savings by one percentage point on transfers received by highly vulnerable non-users. Section 7 analyzes the reduced-form effect of the instrument on transfers in a panel-environment, confirming the cross-sectional results on cash-gifts with evidence of a similar difference in the two-year change in loan-receipts. Section 8 estimates the impact on the two-year change in welfare outcomes among the highly vulnerable. Section 9 concludes and indicates directions for future investigation.

2. The Impact of Formal Finance on Indigenous Institutions for Addressing Risk

A rich literature documents the centrality of risk and uncertainty in poor, rural communities of the developing world. Highly variable incomes and unexpected expenses can

lead to negative consumption shocks, often with dire welfare outcomes, often with long-lasting or permanent effects, such as serious illness and lower education levels (Alderman et. al., 2006; Jacoby and Skoufias, 1997; Dercon and Hoddinott, 2005), physical stunting (Foster, 1995; Alderman et. al., 2006; Dercon and Hoddinott, 2005), and higher infant mortality (Rose, 1999).[2] Negative impacts are often sharpest among the poorest, already at very low consumption levels, highlighting the importance of understanding how the process of financial deepening affects consumption insurance among the worst-off.

In the face of adverse income and consumption shocks, a variety of informal methods often help protect individuals in villages from dangerously low consumption. Some methods may be pursued in isolation. Many studies show, for example, that households use savings through a variety of durables and other non-financial assets to address future uncertainty (Paxson, 1992; Deaton, 1992; Rosenzweig and Wolpin, 1993; Fafchamps et. al., 1998), while other studies document adjustments of household production and labor decisions to avert income-volatility (Morduch, 1990, 1995; Antle 1987; Bliss and Stern, 1982; Walker and Ryan, 1990; Bliss and Stern, 1982; Binswanger and Rosenzweig, 1993; Giles, 2006). Yet precautionary savings has important limitations (Besley, 1995; Fafchamps et. al., 1998; Giles and Yoo, 2007) and the efficiency losses from income-smoothing are well-known.

Many local responses to risk are instead based on interdependence among households. Variously referred to as "hunger insurance", local "social security", "non-market institutions", and "informal insurance arrangements", these practices can fulfill a crucial function for individuals in poor, rural communities. Many studies, across a wide variety of settings, show that households frequently address short-falls in income through informal loans from friends and

[2]For more on long-term effects of negative shocks, permanent impacts of low-consumption, and links between health outcomes and risk, see also Dercon 2005, Dercon and Hoddinott 2005, Hoddinott and Kinsey 2001, Jalan and Ravallion 2004, Beegle at. al. 2006, Karlan and Morduch (2009) p.57.

relatives (Platteau and Abraham, 1987; Townsend, 1995a, 1995b; Fafchamps and Lund, 2003; Udry, 1994). Assistance from other households also commonly takes the form of gifts (Cox and Jimenez (1998), Fafchamps and Lund (2003), Dercon et. al. (2008)). These inter-household wealth flows are typically interpreted as informal contractual arrangements between parties who provide each other assistance in times of need (Coate & Ravallion, 1993; and Kletzer and Wright, 1992; Fafchamps, 1992). More recent work suggests motivations other than mutual insurance can also play an important role (Hoff and Sen, 2006; Baland et. al., 2007; Comola and Fafchamps 2010).

A growing body of literature explores whether formal financial markets can improve on existing informal insurance and consumption-smoothing options to help the poor better address their acute vulnerability. Leaving this question aside, this paper explores how the expansion of financial services, and formal savings in particular, affects informal institutions, and the welfare impacts this can have on households unable to use formal services.

The impact of expanded formal savings services on informal mutual insurance among the poor has been theoretically explored in Ligon et. al. (2000) and Foster and Rosenzweig (2000). Predictions stemming from this work are either ambiguous, or suggest a reduction in size and scope of transfer-based social safety nets. While several studies have observed the strong correlation between formal capital markets and weaker inter-household assistance practices (Townsend 1195a, 1995b; Besley, 1995, Morduch, 1999), there exist almost no causal analyses testing the effects of formal finance on informal systems in villages. The only study we are aware of is Foster and Rosenzweig (2000), who find evidence suggesting the simultaneous presence of formal credit and savings in India and Pakistan leads to fewer transfers, using distance from banks as a source of variation.

This paper isolates the effects of formal savings on transfers using a randomly assigned instrument, with an emphasis on assistance received during the pre-harvest "hungry" period. This is the time of year of greatest hardship for most households, when negative consumption shocks are most likely to manifest, and when transfers are likely to have the highest positive impacts. The analysis shows assistance-receipts are strongly affected across multiple wealth levels, with sensitivity to the changes increasing with vulnerability.

Particular focus is placed on assistance-receipts by the worst-off households, in order to test for the presence of an *indirect* effect. The poorest in village communities, due to their wealth constraints, comprise a group which is *de facto* ineligible to use formal savings.[3] The costs of access are too high relative to their cash income and savings ability. Fees for opening and maintaining accounts, along with other explicit and implicit costs (purchasing identification documents, traveling to and from the bank, purchase of mandatory ATM cards, etc.) can be substantial relative to cash incomes of the poorest.[4] Combined with minimum balances, these costs create a barrier to access for those with limited income and low levels of savings. [5] Showing that those who do *not* start using formal savings are affected by changes in local practices caused by the entrance of this financial service means that analyzing the effects on service-users provides an incomplete picture of the immediate-term effects of introducing this financial service to village settings.

[3] The classification of this group as ineligible lies in the spirit of Angelucci et. al. (2009), who analyze the indirect impact of Mexico's welfare program, Progresa, on households not eligible to participate in the program.
[4] The average explicit cost of opening a formal savings account, among all formal accounts reported in 2008, is MK 1,462 (about US $9.50). The minimum balance for an account with the bank which expanded savings-access in this area over 2008-2010 is MK 500 (about US $3.25).
[5] Even in cases where a formal account may be technically affordable for a very poor household, the fixed costs associated with opening the account will often be high enough to cause net returns to drop below traditional savings alternatives (livestock, grains, durables, etc.) for deposit amounts such households can afford. Marginally eligible households such as these remain ineligible in the sense that they do not have access to a superior storage technology – i.e. savings with a higher rate of return than pre-existing options – in contrast to the wealthier households in the community.

3. The Data and Empirical Approach

To test the empirical effects of introducing formal savings vehicles to rural areas of the developing world, we draw on household survey data from Malawi. Malawi is among the poorest countries, has low participation in formal financial markets in rural areas, and significant incidence of inter-household assistance, gifts, and loans.[6] In late 2007, a local microfinance bank began expanding formal savings and credit access to rural areas of the three largest districts of central Malawi – Lilongwe, Mchinji, and Dedza. Expansion occurred through a mobile van-bank, which traveled along paved roads, and had six different stops at local trading centers – three stops along the highway running 110 km west from the capital city of Lilongwe (located in the center of Lilongwe district), and three stops along the highway running 90 km south.

This expansion of formal services into the thin financial environment of rural Malawi provides an ideal setting to examine the interaction between formal savings markets and indigenous safety-net systems. The data consist of a two-year household panel which spans the initial phases of access expansion. The baseline data was collected over February-April of 2008, during the pre-harvest "hungry" season, when household resources are often stretched thin and food-stocks for some are running low.[7] This was prior to any measurable use of the bank's services in these areas.[8] The second round was collected over the same period in 2010, after an extended information campaign designed to encourage use of the bank's services.

[6] In 2008, 6.0% of the sampled households had at least one current formal loan, while 11.6% of the households had one or more formal savings accounts. Only 2.8% of the sampled households reported both formal savings and formal credit, so about 14.7% of the sample reported using formal savings accounts, formal credit, or both. On the other hand, 23.6% of the sample reported having at least one current informal loan from a friend or relative.
[7] Malawi has a single growing season. Most farming households receive the majority of their annual income during one single period of the year – the harvest period, which in Central Malawi usually lasts from late April into June.
[8] Though the mobile bank began operations in late 2007, information collected in focus-group discussions in February and March of 2008 confirms awareness of it was still extremely low, and almost no households in the baseline data report using the bank's services.

Community sampling followed a matched-pair design. Each pair consisted of two village-clusters, a cluster being defined by enumeration areas (EAs) – sampling units defined by Malawi's National Statistics Office that typically include 2-4 villages[9]. Clusters of villages were first categorized based on distance from the mobile van-bank stop: (i) within 5km; (ii) 5-10 km; (iii) more than 10 km. They were then further split into two population categories: high versus low. Two clusters were then randomly sampled from each population-distance group to form a pair. A total of 60 pairs were sampled (120 clusters total). Finally, within each pair, one of the clusters was randomly selected to receive an information intervention.

From each cluster, 20-23 households were sampled. Due to minor sampling problems, some data loss, and complications with the information intervention in one location, four pairs had to be dropped. The final panel contains 112 clusters (about 325 villages), with a total of 2,006 households. Villages are located at radial distances from the mobile bank call-point ranging between 0 and 14 kilometers.

The Information Intervention

It was not feasible to directly randomize access to the bank's services. Even if it were, it would have been hard to make randomizing access seem natural, and it might have caused perceptions of discrimination and led to unpredictable changes in inter-household interactions unrelated to use of financial services. Instead, drawing from focus group discussions on how people obtain trustworthy information from sources outside the village, we designed a marketing campaign that would mirror these other methods of information dissemination, to serve as an instrument. The backbone of the campaign consisted of periodic visits (via foot and bicycle) to

[9] For very large villages, the EA may consist of only one village; in a few cases, the EA might include as many as 5 villages. Both of these cases are rare in the data.

each marketing-village from a paid Field-Based Promotional Assistant (FBPA). The FBPA brought informational materials on the bank's services, talked with community members, and left posters and other promotional materials in each village assigned to the marketing treatment.

The exclusion restriction relies on the assumption that periodic informational visits by these bank representatives did not influence inter-household transfers through a channel other than the local uptake of financial services. This would be violated, for example, if the information intervention affected other behaviors in the community besides service-adoption, in ways that affected transfers. This could happen through one of two ways. The form the intervention took – periodic visits by the FBPAs – would have had to introduce elements to the marketing clusters not also present in the non-marketing clusters, elements which affect household transfers. Or, the information-content itself would have had to affect choices other than financial services adoption, in ways that affect wealth transfers.

With regard to the first possibility, it is unlikely that visits by the FBPAs would introduce anything not already present in both the marketing and non-marketing clusters, other than information. Their job was restricted to providing information on the bank's products and recruiting new clients, and they were incentivized to do so as broadly and rapidly as possible. Each FBPA was responsible for as many as 20-30 villages, and as much as a month might pass between visits which lasted a few hours.[10]

It is plausible that tangential elements might be incidentally introduced by these types of visits to villages by urban outsiders, but it is unlikely this would have caused systematic differences between the encouraged and non-encouraged clusters. The majority are located within 10 km of a highway, and the periodic presence of non-locals whose job it is to bring

[10] The FBPAs typically walked or bicycled to the communities where they worked. Travel times could be as long as a few hours in many cases, which often left only a few hours during the day to interact with community members.

outside information to the community is not unusual. It is quite common for agricultural extension officers, or nutrition and health extension officers, to make informational visits to these villages to inform people about new techniques, practices, and available services.[11] Insofar as the form it took, the marketing campaign therefore represents nothing new to these areas.[12]

The second way that the exclusion restriction could be violated is that the information-content itself could have somehow affected inter-household transfer behaviors through a channel besides financial services use. However, there is no clear reason to expect that more information about formal financial products would, absent using them, lead to changes in inter-household assistance behavior. In particular, there is no reason to expect that simply knowing the details about formal savings and credit products should cause someone who does not use such products to start giving more assistance to others.

To the extent that marketing might contain non-informational components intended to persuade (e.g. framing), this would still likely affect only the adoption decision and not have lasting impacts on other behaviors. While the presence of any emotive or subjective aspects of the marketing might influence a decision of whether to adopt, it is unlikely a short interaction with an FBPA would have lasting influences on long-standing personal habits or responses to engrained social norms. Even if non-informational components of the marketing did somehow have lasting direct effects on behavior, they should be in the opposite direction of the effects found. It is perhaps possible, for example, that the bank's emphasis on building one's own personal wealth as an avenue to financial independence and future prosperity might be passed on

[11]This was, in fact, the primary inspiration for how we designed the encouragement. After learning this is the standard way villages receive information from outside, we intentionally fashioned the information intervention to mimic these pre-existing methods. A virtue of fashioning the encouragement in the way that we did is that it fits right in with other commonly experienced "interventions" in these communities, minimizing the risk that it did anything new to the marketing-areas, besides the provision of information on formal financial services.

[12]It is perhaps more likely that the survey interview itself would have some type of tangential effects of the sort that could be caused by the form of the information intervention (as the interview involves even longer and closer contact with a village outsider, the interviewer). Yet it was of course administered both in treated and control areas.

by the FBPAs and operate as an ideological influence on behavior, encouraging people to share less and focus more on the accumulation of personal or household cash resources and other assets.[13] However, such an effect would bias estimated impacts of formal savings uptake towards less assistance to other households. This would make it even harder to detect the patterns found in the data, and would suggest the findings discussed below are a lower bound of the true effects.

Descriptive Statistics and Balance-Check

Table 1 reports descriptive statistics on several important household dimensions of the baseline sample. As the statistics are from the baseline, it includes the 341 households that attrited and which are not part of the final full panel. The table presents overall figures, then split by marketing and non-marketing. The HFIAP-Score is a 4-point food-security indicator that forms the basis for vulnerability-categories. The HFIAS-score is a 21-point food-security indicator. (For both indicators, higher values imply less security.) Category A through Category G are household vulnerability indicators, defined in the next section, such that these take a value of 1 if the household belongs to the category. Unless otherwise indicated, the reported values are percentages of households in the sample for which the indicator variable is true. The column of differences indicates statistically significant differences based on two-sided t-tests (Mann-Whitney U-tests for household size and HFIAP), with standard levels of significance indicated.

[13] Such an affect would be at the level of altering preferences themselves. While perhaps not impossible, this type of effect would most likely require much more frequent and extended exposure than a handful of visits to the community over several months in order for new ideologies and ways of thinking to counter long-standing social practices and individual habits.

Table 1*:* Descriptive Statistics on HHs in Baseline, Overall and by Treated & Control Clusters

	Overall	Non-Mktg	Mktg	Difference
Number of HHs (qty)	2,352	1,178	1,174	4
Head is Male	0.851	0.838	0.864	.026*
HH Size (People)	5.13	5.03	5.23	0.20**
Head's Age (Years)	41.0	41.1	40.9	-0.15
Bank-Stop Distance (km)	7.92	7.87	7.98	0.11
HFIAP Score (1-4)	3.22	3.21	3.23	0.02
HFIAS Score (1-21)	7.78	7.81	7.75	-0.07
Has Cell phone	0.132	0.120	0.145	0.025*
Has Literate Members	0.858	0.860	0.856	-0.004
Has Salaried Member	0.155	0.144	0.166	0.022
Has Business	0.265	0.259	0.270	0.011
Physical Assets (Kwacha)	27,440	25,286	29,592	4,306
Amount of Land (Acres)	2.620	2.607	2.632	0.025
Has Formal Savings	0.117	0.100	0.134	0.033**
Has Formal Loan	0.061	0.061	0.061	-0.000
Category A	0.083	0.080	0.086	0.006
Category B	0.063	0.069	0.056	-0.013
Category C	0.407	0.414	0.400	-0.014
Category D	0.448	0.437	0.458	0.021
Category E	0.421	0.417	0.426	0.009
Category F	0.409	0.403	0.414	0.011
Category G	0.134	0.136	0.133	-0.004
Attrition	0.140	0.140	0.139	
Number of HHs (qty)	2335	1161	1174	

Except where indicated in parentheses, units are proportions.

The randomization appears to have been successful at achieving a balance across the information-treated and non-treated clusters along most household dimensions, with a few exceptions. There is a small, but statistically significant, difference in terms of household size. Clusters that received the information intervention also have a slightly lower percentage of female-headed households. In addition, there appears to be a greater prevalence of formal savings in the information-treated clusters, even prior to the intervention. All three differences are driven by Lilongwe district (20% of the baseline sample), where the randomization appears

to have been less successful at achieving a balance. These differences disappear when restricting to the 80 % of the baseline sample comprised by Dedza and Mchinji districts.[14] In the cross-sectional regressions reported in sections 6 and 7, results on the full sample are therefore always accompanied by results for a restricted sample excluding Lilongwe, as a robustness check to confirm that pre-intervention differences do not drive any of the data patterns observed.

Defining The Vulnerable

Households are classified by level of vulnerability to hunger and low welfare outcomes using baseline (2008) variables on food-security status, assets, education, distance from major roadways and trading centers, and gender of household head. The primary indicator is the household's 2008 food-security status. The survey included a slightly modified version of the USAID Household Food Insecurity Access Scale for Measurement of Food Access (Coates, Swindale, and Bilinsky, 2007). Food insecurity scores are generated by examining the frequency with which each of 7 possible food-insecurity conditions occurred in the 30 days preceding the interview. Recall that, as the survey was conducted during the pre-harvest "hungry" season, these scores reflect conditions during the most intense period of vulnerability to low food-intake.

The Household Food Insecurity Access Prevalence (HFIAP) score, largely reflecting caloric intake, groups households into 4 categories – food secure, mildly food insecure, moderately food insecure, and severely food insecure. Mildly food insecure households usually have enough food, but may have poor food quality at times. Moderately and severely food

[14] The p-values for the corresponding tests of significance across treated and control communities when restricting to these two districts are: for formal savings (p=0.41), female-headed households (p=0.56), household size (p=0.14).

insecure households have problems with adequate food intake (or serious lack of access to quality food). The baseline HFIAP score helps identify vulnerable households.

This measure by itself, however, is too broad to identify those households of highest vulnerability – nearly 40% of the sample falls into the highest food-insufficiency category (HFIAP=4). In addition, measurement error in the food security questions and random variability in consumption introduces noise into this as a measure of vulnerability (some households may simply have had a bad year during the baseline and are not as vulnerable on average as this simple measure would predict). To more more narrowly zero-in on those of higher vulnerability, the following are added: distance from the van-bank stop (a proxy for distance from major roads), possessing a cell-phone (a proxy for wealth-level), literacy, and whether the household head is female. Literacy is defined as whether the household has any members that can read. Table 2 shows the definition for each classification, and indicates the number of households in the final panel in each category. Categories A-D are mutually exclusive; after category D, the following categories are successive subsets of each other.

The pivotal group for the analyses which follow is households in category G. As argued in section 3 (and empirically confirmed in section 4), these households comprise a group which is *de facto* ineligible to use formal savings, as the fixed costs are simply too high. They also represent those who are most susceptible to very low welfare outcomes and among the most sensitive to changes in local safety nets.

Table 2*:* Definition of Vulnerability Categories

Vulnerability Category	Definition	HHs Control	HHs Treated
Category A	2008 HFIAP = 1 Household classified as "food-secure" in 2008.	77	80
Category B	2008 HFIAP = 2 Classified as "mildly food-insecure" in 2008.	61	55
Category C	2008 HFIAP = 3 Classified as "moderately food-insecure" in 2008.	417	413
Category D	2008 HFIAP = 4 Classified as "severely food-insecure" in 2008.	443	463
Category E	2008 HFIAP = 4, 3+km Classified as "severely food-insecure" in 2008, located 3 or more kilometers from the bus-bank stop.	429	434
Category F	2008 HFIAP = 4, 3+km, no cell phone Classified as "severely food-insecure" in 2008, located 3 or more kilometers from the bus-bank stop, does not have cell-phone	415	427
Category G	2008 HFIAP = 4, 3+km, no cell phone, illiterate Classified as "severely food-insecure" in 2008, located 3 or more kilometers from the bus-bank stop, does not have cell-phone, and either: (i) no HH member is literate in Chichewa; or (ii) household head is female.	141	131

Note that A,B,C, and D are are mutually exclusive. But E is a subset of D, F is a subset of E, and G is a subset of F

4. Assessing the Instrument: Information Intervention Effects on Local Financial Services Use

Since access to formal credit through the organization's "bank on wheels" was not available to most communities, the information intervention was essentially an encouragement to open a formal savings account with the microfinance bank. [15] Inasmuch as it raised awareness and literacy with respect to financial services, however, t is plausible the campaign could have induced individuals to start using services of other financial organizations near the area. Moreover, as other organizations might be able to offer loans, it

[15] Access to formal credit from the van-bank is expanded quite slowly in the areas it serves. It is done on a village-by-village basis, and involves significant resources – several visits to a village by a bank officer, meetings with prospective borrowers and village leaders, risk assessments, etc. The banking officers explain that this process, which takes 1-2 months, targets areas closest to the bank's stop, and those with the greatest economic activity.

is possible the information campaign could have induced higher formal credit use in the area. The analyses therefore test for changes in use formal savings at any financial organization, as well as for any changes in formal credit use.

Effect of Instrument on Adoption of Formal Services

Table 3 shows the effect of the information intervention on changes in household financial service-use. It reports results from a simple OLS regression of the decision to adopt (or quit) use of formal savings (or credit) on a dummy indicating assignment of the community to the marketing intervention, with fixed effects at the cluster-pair level, and standard errors clustered at the village-cluster level.[16] The dependent variable is a 0-1 indicator for whether the household has at least one formal savings account (columns 1-4) or a formal loan (columns 5-8) in 2010. This is equivalent to regressing the percentage of households in the cluster with formal savings (credit) on the dummy for information intervention, accounting for pair-level fixed effects, and explicitly correcting for heteroskedasticity across clusters due to the variation in number of households (FGLS).

Columns 1 and 2 show results when the sample is restricted to those households which did not have formal savings accounts in 2008 (i.e. the baseline non formal-savers). The coefficient for the marketing dummy in these regressions therefore represents the amount by which the information intervention increased the proportion of baseline non formal-savers that chose to start using formal savings over the two year period . The effect of the information campaign should be smaller in areas close to the bank's stop, as those living nearest the bank's weekly location are likely to already have a high degree of

[16]The fixed effects account for the possibility that pairs experience the van-bank's expansion of formal services access differently. For example, villages in pairs closer to the bank-stop may be more responsive to access expansion than those in pairs further away, regardless of whether they are encouraged or non-encouraged.

information about its services. Thus, while the first specification (column 1) includes all village-clusters, the second (column 2) restricts the sample to those clusters for which both members of the cluster-pair are located three or more kilometers from the closest stop.

The results show the intervention had a significant effect on the proportion of previous non-savers that adopted formal savings, significant at the .05-level. In addition, both the magnitude and significance of its estimated effect increases with distance from the bank-stop, confirming that information on services is increasingly effective in more remote locations.[17] Among all clusters, the marketing increased the percentage of previous non-saving households that adopted formal savings by about 3.1 percentage-points (p=.03), while among clusters three or more kilometers away, the effect is an increase of 3.7 percentage points (p=.01). To put these figures in context, 9.3% of the baseline non-savers in all the control clusters adopted formal savings, which drops to 8.6% when restricting to clusters beyond the 3 km threshold. The boost to adoption rates thus represent a 33% increase and 43% increase, respectively.

Columns 3 and 4 show results from analogous regressions, but for which the sample is restricted to those households which did have at least one formal savings account in 2008. As before, the dependent variable indicates whether the household had a formal account in 2010. A value of zero means the previously formal-saving household stopped use of formal savings sometime over the two-year period. Here, the coefficient on the dummy represents any effect of the information intervention on the proportion of previous formal-saving households that stopped formal savings-use. The results in columns 3 and 4 reveal that

[17]The increasing effect of the information campaign with distance is even more pronounced when including a 1 km threshold: It raises local adoption rates by 3.1 percentage points (from 9.3% to 12.4%) across the whole sample, 3.5 percentage points (from 9.3% to 12.8%) across clusters one or more km from the bank's stop (results not shown), and 3.7 percentage points (from 8.7% to 12.4%) across clusters three or more km from the bank's stop.

marketing encouragement had no significant effect on the proportion of previously saving households that ceased use of formal savings accounts over the two-year period.

Columns 5-8 report results from regressions analogous to those reported in columns 1-4, but for changes in formal credit use. The dependent variable is a 0-1 indicator for whether the household had a current formal loan in 2010. For columns 5 and 6, the sample is restricted to households with no formal loan in 2008, and for columns 7 and 8, the sample is restricted to those with a formal loan in 2008. The results indicate the information intervention had no effect on the proportion of households that went from having no loan in 2008 to having a loan in 2010, or vice versa.

Effect of Instrument on Local Prevalence of Formal Services

Table 4 reports results on the effect of the information intervention from a different perspective – its impact on overall local prevalence of formal services-use, rather than its effect on the adoption decision. The response variable takes a value of -1 if a household moves from formal savings- (credit-) use to no formal savings (credit), 0 if it experienced no change, and 1 if it went from no formal savings (credit) to formal savings- (credit-) use. The interpretation for the coefficient on the dummy for marketing is now the effect of marketing on the change in proportion of the *entire* community that has a formal savings account or a formal loan (e.g. not the proportion of previous non-savers that started using formal savings). All regressions include cluster-pair fixed effects and clustered standard errors.

Column 1 shows the results for the full sample, column 2 shows them when restricting to cluster-pairs three or more kilometers from the bank-stop, and columns 3 and 4 show results from the same regressions when excluding Lilongwe district (which surrounds the capital and is

arguably the most connected to its markets and institutions). As columns 1 and 2 show, the information intervention does not have a significant effect on the overall proportion of households with formal savings when combining all three districts, whether the sample is restricted to the three kilometer threshold or not. However, as columns 3 and 4 show, when restricting to the 76% of the sample living in districts further from the capital city, the effect of the intervention is significant, resulting in a 2.8 percentage-point increase overall (p=.07), and a 3.2 percentage-point increase when restricting to the more remote village clusters (p=.05). As the average prevalence among non-marketing clusters in the endline was 12.2% overall and 10.4% in the more remote clusters, this represents a boost in the increased use of formal saving by 23% and 31%, respectively. The results reported in columns 5-8 confirm that the information campaign had no effect on use of formal credit.

There are two potential explanations for the intervention's lack of significance in Lilongwe for formal savings usage. The first is that, due to their closer proximity to the capital city and its financial centers, the 24% of the sample located in Lilongwe district have better access to information and are therefore less sensitive to the information campaign. The second is that expansion of financial services-use may be partly a function of initial penetration rates. As previously noted, pre-intervention formal savings usage was already higher in the information-treated communities of Lilongwe district. Communities which already have high usage rates should have less potential for service-use to grow. This would dampen the instrument's strength in these areas. Results from a community-level regression reported in Appendix 1 show that controlling for initial savings prevalence in each community increases the impact of marketing on savings penetration and causes it to be significant across all three districts, and that initial usage levels can have a substantial impact on the 2-year change.

The regressions reported in Table 4, however, are the simplest specification one might imagine, and represent the most conservative estimates of the instrument's effect. It may simply be the that communities in Lilongwe are less responsive to the marketing due to being closer to the capital and its financial organizations. Since the most conservative approach would suggest that the information treatment may have not had a significant impact in Lilongwe, all regressions which follow are accompanied by results when restricting to just Dedza and Mchinji, to serve as a robustness check.

Effect of Instrument by Vulnerability Level

Section 3 argues that the most vulnerable households are essentially ineligible to take advantage of increased formal savings access. A set of regressions which restricts the sample to the 272 households classified as highly vulnerable (group G) confirms the information intervention did not induce formal savings use among this group (see Appendix 2). A separate set of regressions which restricts to the non-vulnerable category (all those not in category G) shows higher magnitudes and significance of the instrument's effect on the change in percentage of households using formal savings (see Appendix 3). In Dedza and Mchinji, the marketing campaign increased the change in proportion of non-vulnerable households with formal savings by 3.3 percentage points (p=0.6) across all clusters, and by 4.0 percentage-points (p=.03) among clusters beyond the 3 km threshold. This represents a 24% and 34% increase, respectively, over the matched control clusters (for whom the average prevalence is 14.9% and 11.9%, respectively, among the non-vulnerable population).

Table 3. Effects of Marketing on Adoption and Dis-Adoption of Formal Savings and Formal Credit

	Formal Savings				Formal Credit			
	Start Formal Savings		Stop Formal Savings		Start Formal Credit		Stop Formal Credit	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Distances	3+km	All Distances	3+km	All Distances	3+km	All Distances	3+km
VARIABLES	Has Svgs	Has Svgs	Has Svgs	Has Svgs	Has Loan	Has Loan	Has Loan	Has Loan
Mktg Dummy	0.0306**	0.0371**	0.0441	0.0298	-0.00708	-0.00693	0.00782	-0.0752
	(0.0288)	(0.0129)	(0.490)	(0.655)	(0.416)	(0.430)	(0.948)	(0.619)
FSAV in 2008	N	N	Y	Y				
FCRED in 2008					N	N	Y	Y
Observations	1,784	1,593	217	169	1,860	1,651	120	93
R-squared	0.064	0.066	0.270	0.308	0.038	0.035	0.396	0.419

Cluster-Robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include cluster-pair fixed effects. Columns 1 & 2 restrict sample to households without formal savings in 2008, columns 3 & 4 restrict to those with formal savings. Columns 5 & 6 restrict to households without current formal loans in 2008, columns 7 & 8 to those with formal loans.

Table 4. Effects of Marketing on *Local Proportion* of HHs with Formal Savings and HHs with Formal Credit

	Change in Proportion of Households with Formal Savings				Change in Proportion of Households with Formal Credit			
	All Districts		Excluding Lilongwe		All Districts		Excluding Lilongwe	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Distances	3+ km	All Distances	3+ km	All Distances	3+ km	All Distances	3+ km
VARIABLES	Chg in FSAV	Chg in FSAV	Chg in FSAV	Chg in FSAV	Chg in FCRED	Chg in FCRED	Chg in FCRED	Chg in FCRED
Mktg Dummy	0.0143	0.0185	0.0283*	0.0323*	-0.00250	-0.00414	0.00357	-0.000404
	(0.291)	(0.211)	(0.0695)	(0.0522)	(0.820)	(0.689)	(0.793)	(0.974)
Observations	2,001	1,762	1,523	1,334	1,978	1,877	1,506	1,439
R-squared	0.036	0.034	0.038	0.038	0.041	0.050	0.044	0.054

Cluster-Robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include cluster-pair fixed effects.

5. Reduced-Form Effect of the Savings Encouragement on Receipts of Assistance

The data used to test the effects of local formal savings adoption contain information on transfers made during the pre-harvest hungry season, the time of year when household resources are most restricted. This is the period during which transfers received are likely to have the highest positive marginal impacts. It is also the time during which requests for assistance are arguably most abundant.

The data also benefit from a group which experienced a strong exogenous boost to the formal savings adoption decision, and a comparable control group. The randomly assigned information treatment serves as an instrument for the adoption decision to enable unbiased inferences about the impact of formal savings on transfers. In addition, the data also provide a well-identified group of households among whom changes in transfer receipts are a clean signal of the response of *others* to improved savings options. Any change in receipts by the *de facto* ineligibles (i.e. the highly vulnerable) clearly cannot be driven by their own adoption of formal savings. They are instead a sign of changes in the provision of assistance induced by formal savings.

The empirical analysis focuses on the effects of local formal savings rates on *receipts* of assistance, particularly by the most vulnerable households. This is due mainly to the study's focus on empirically testing for the presence of *indirect* effects, and the indirect effects on the worst-off households in particular. As the data do not identify recipients of transfers-out, or whether recipients have formal accounts, examining transfers-out provides less information about indirect effects. Moreover, as the worst-off households do not use

formal savings, analyzing impacts on formal savers is less directly relevant to the outcomes of the focal group for this paper.

The discussion of the impacts of formal savings expansion begins with a brief look at simple percentage changes across the encouraged and non-encouraged clusters. It then proceeds to an analysis of a variety of sharp differences in vulnerable-household transfer receipts, between marketing and non-marketing village clusters. In the context of assessing direct impacts, this would be analogous to intention to treat (ITT) effects. However, since analyzing *indirect* effects, we call this the "indirect intention to treat", or "IIT".[19] The indirect intention to treat effects are interpreted as preliminary evidence suggesting a causal effect from savings accounts.

While the baseline includes data on a broad range of financial services and transactions, the detailed questions on inter-household transfers were not added to the questionnaire until the endline survey. Discussion of the impacts of formal savings adoption on inter-household transfers therefore begins with a cross-sectional analysis of the endline data.

Since the savings-encouragement is randomly assigned, it is valid to interpret relationships between the encouragement and household outcomes as causal. However, the full panel does contain limited information for both years on certain types of wealth

[19] This estimand is similar in spirit to the "ITE" estimand defined by Angelucci et. al. (2009), as the indirect treatment effect from policy interventions on non-participants in the program. Though used in the present study only as an intermediate step, it bares mentioning that this is one of the first studies I am aware of to use an empirical approach which includes examining the indirect impacts, on non-eligibles, of the intention to treat eligibles. Though they introduce the ITE as a novel estimand for impact evaluations, Angelucci et. al. did not include an analysis of indirect *intention* to treat, as there was almost 100% compliance among eligibles in their sample, since the program they were discussing was welfare payments from the government, and almost all those who were eligible chose to be treated. In the present context of wealth-constrained access to formal savings, however, the non-eligibles easiest to identify are the poorest households (group G). Those defined as "eligible" (potentially anyone not in group G) had a compliance rate far less than 100%, creating the need to distinguish between an indirect treatment effect (ITE, as in Angelucci et. al.) and an indirect *intention to treat* effect (IIT). In the present context, however, the ultimate object of analysis is not the IIT, but rather it is used as an intermediate step to get to the ITE.

transfers that are similar to the gifts-information captured only in the endline. Section 8 uses a difference-in-differences estimation of these related types of transfers to explore panel results which also serve as a robustness check for the results presented in this section.

We gathered data on cash gifts of 50 kwacha (about $.30) or more, received over a 90-day recall period preceding the interview.[20] The vast majority are from within the local community. While we did not gather data on the actual distances between giving and receiving households, nor on whether the households were located in the same village, the data do include total round-trip travel times required to obtain each gift. About 80% of the reported round-trip travel times are below 30 minutes (implying one-way trips of a maximum 5-15 minutes)[21]. This proportion remains about 80% whether looking at the sample overall, or just the highly vulnerable category. Given that the standard mode of transport in these areas is usually walking, and sometimes bicycling, this suggests that most of these transfers are between households within the same village, or at furthest from neighboring villages.

Table 5 shows simple comparisons of the percentage of households receiving cash gifts in the non-marketing and marketing clusters – overall and by household vulnerability type. Appendix 5, Table 5.A.1 is analogous, but compares percentages of households receiving *multiple* cash gifts. Before analyzing separately by vulnerability level, we already see a large difference in receipts of cash gifts from other households across marketing and non-marketing areas. While 20.8% of all households in the non-marketing areas received a

[20] Interviewers were intensively trained on the difference between a "gift" and a loan, the latter carrying with it an expectation of repayment of some type of wealth in the future. In addition, the module I added to the survey with questions on gifts came after a section in which detailed information was already gathered on loans. Interviewers were trained to distinguish between the two and collect information on each only in their respective parts of the questionnaire.

[21] The question was asked so as to include time spent at the location of where they were requesting or receiving the gift. That is, it is a total time-cost figure, inclusive of time spent communicating with anyone providing assistance.

cash gift in the last 90 days, 30.6% of those in the marketing areas received one. (Significant with a t-test at the .00 level.) This change in the proportion of households represents a difference of almost 50%. In addition, while 7.4% of all households in the non-marketing areas received more than one cash gift, 12.0% of all those in the marketing areas received multiple cash gifts – a difference of 62%. This difference is also highly significant ($p<.001$; see Table 5.A.1 in Appendix 5).

Result 1: Receipt of cash gifts during the hungry season is significantly more prevalent in the marketing villages than the non-marketing villages. Both the likelihood of (i) ever receiving a cash gift; and (ii) receiving multiple gifts is higher in marketing than non-marketing villages.

Note that this difference is not necessarily evidence of an *indirect* effect of formal financial services uptake, since these figures include households that did adopt formal services. It is therefore possible these differences could be driven by some direct effect that formal services use might have on a household's probability of receiving a cash gift from others. However, digging deeper and looking at differences by vulnerability level shows very strong differences among households of high vulnerability, among whom adoption rates are virtually non-existent.

The figures in Table 5 in fact show that the relationship between the marketing instrument and incidence of cash-gift receipts depends quite heavily on household vulnerability level. When we restrict our focus to the least vulnerable groups, for example, the difference between marketing and non-marketing areas in cash gift receipts attenuates substantially. Among those households that were food-secure (category A) or mildly food insecure (category B) in

Table 5. Percentage of Households that Received at Least One Cash Gift

HH Type (Based on 2008 Characteristics)	Non-Mktg Clusters (#HHs)	Mktg Clusters (#HHs)	Difference
All HHs	20.8% (995)	30.6% (997)	9.8% *** (p=.000)
A (Food-Secure)	28.6% (77)	32.9% (79)	4.3% (p=0.560)
B (Mildly Food-Insecure)	27.9% (61)	36.4% (55)	8.5% (p=0.331)
AB (Secure & Mildly Insecure)	**28.2% (138)**	**34.3% (134)**	**6.1% (p=0.282)**
C (Moderately Insecure)	22.6% (416)	33.0% (406)	10.4%*** (p=.0008)
D (Severely Insecure)	16.8% (441)	27.4% (457)	10.6% *** (p=.0001)
E (D + No Cell)	16.2% (427)	27.1% (428)	10.9% *** (p=.0001)
F (E + 3 or more km)	16.0% (413)	27.5% (412)	11.5% *** (p=.0001)
G (F + Non-literate or Female-head)	9.9% (141)	27.7% (130)	17.8% *** (p=.0001)

The number of households in each category above is slightly smaller than the actual total number of households overall and total number in each category, as there are a few randomly missing responses for the cash gift receipt question.

2008, the percentage of households receiving at least one cash gift is not significantly higher in the marketing villages than in the non-marketing villages (p=.294).

There is a remarkably consistent pattern of an increasingly high marketing/non-marketing difference as we move towards indicators of increasing vulnerability. The amount by which the percentage of households receiving gifts is higher in marketing than non-marketing areas is only 4.3% among the category A households (not significant), and 8.5% among category B (not significant). The difference grows to 10.4% among category C households, 10.6% among category D, 10.9% among category E, 11.5% among category F, and 17.8% among Category G –

all of which are highly significant (at the .01 level or higher). Looking at the *percentage* changes in the proportion of households receiving gifts in moving from non-marketing to marketing (rather than just the change in the proportions), the pattern of increases is even more striking. Gift receipts in marketing areas are only 14% more common among the A-category, and 33% more common among the B-category (neither significant). The percentage difference grows to 48% among the C-category, 62% among the D-category, 69% E-category, 71% among the F-category, and 180% among the G-category. This leads to the next main result.

Result 2: The difference in prevalence of cash-gift receipts between marketing and non-marketing villages increases as vulnerability increases. The marketing/non-marketing difference is negligible among the best-off households, but highly significant among the worst-off households. This is true for both receiving any gift, and receiving multiple gifts. The positive impact on the likelihood of (i) ever receiving a cash gift; and of (ii) receiving multiple cash gifts increases as vulnerability goes up.

A Deeper Look: Impact on the De Facto Ineligibles

We now proceed to a deeper analysis of the Indirect Intention to Treat Effect, which I call "IIT". This is the first stage of analysis, before moving in the following section to an initial look at the Indirect Treatment Effect, or "ITE" (as defined by Angelucci et. al., 2009).

Since this study is primarily concerned with the *indirect* effects of formal savings on consumption-smoothing of non-users, and the impact on assistance receipts by the most vulnerable households in particular, I focus on the experience of the highest vulnerability category. This is group G, which includes households that were highly food insecure in 2008

according to the HFIAP scale, live in communities more than 3 kilometers from the bank-stop, and do not possess a mobile phone. In addition, they either have no literate household members, or are female-headed (57 of the 272 households in this group are both female-headed and have no literate members).

Restricting attention to the highest vulnerability group simplifies the interpretation of any causal effects as deriving from *indirect* effects of local formal savings usage, rather than direct effects from own use of formal savings. As discussed above in the theoretical framework, it is assumed that the minimum balance and fixed costs of opening a formal savings account are too high to be affordable by the poorest households.[22] This group is therefore assumed to not have access to formal savings, making them a *de facto* "ineligible" group. The classification of this group as ineligible lies in the spirit of Angelucci et. al. (2009), who analyze the indirect impact of Mexico's welfare program, Progresa, on ineligible households. (In their case, there is no IIT-analysis, since nearly everyone offered participation enrolls, making indirect intention to treat almost identical to the indirect treatment effect.)

The data is consistent with the assumption that this highly vulnerable group is essentially ineligible. Very few households in this group use formal savings services. Only 8 households (3.2%) of group G had formal accounts in 2010, 6 of which (2.4% of the entire group) started using formal savings between 2008 and 2010. (Of the 6 households across both groups that switched from no formal savings to formal savings, 4 of them were located in intensive-marketing areas.) Any effects on group G households stemming from local formal savings adoption rates are therefore highly unlikely to be caused by direct effects of having an account, but are instead attributable to indirect effects of others in the community having accounts.

[22] Even in cases where a formal account may be technically within the range of affordability for a very poor household, the fixed costs associated with opening the account should be high enough to cause total returns to drop below traditional alternatives for low deposit amounts.

Moreover, the response variables for these few households tend to run in the opposite direction as that for the other 97% of category-G households (for example *none* of the 6 savings-adopting households in group G received a cash gift from friends or relatives in the endline). So they are clearly not driving the results.

We have already seen above (Table 5) that there is a large and significant difference in the proportion of category-G households reporting cash gift receipts between the marketing and non-marketing village clusters. Due to the random assignment of the marketing instrument, these differences in simple averages are sufficient to infer causal effects. However, those were simple t-tests, and it would clarify the picture to account for pair-level effects, and probable intra-cluster correlation among households in the same village cluster. In addition, we have seen that it is unclear whether in Lilongwe the instrument actually boosted the increase in proportion of households using formal savings. We therefore run a set of OLS regressions on the 0-1 variable for whether a household received a cash gift over the last 90 days, with pair-level fixed effects, and clustered standard errors, both for the entire sample as well as the sample restricted to Dedza and Mchinji districts. We estimate the regressions including all household types, and then restricting to just the highly vulnerable category.

Table 6 reports the results. The conclusions are roughly the same as those based on the simpler t-test. Among all vulnerability categories, the marketing increases the percentage of households in the cluster receiving a cash gift by about 10 percentage points, while it increases the percentage of highly vulnerable households receiving a cash gift by about 15 or 16 percentage points, all of which are significant at the .01-level. Note that the results are nearly identical when the sample is restricted to Dedza and Mchinji districts. (The

significance level of the coefficient on the marketing dummy does drop, due to the fewer number of observations, but is still highly significant).

A linear regression may not be appropriate for a regression of percentages. Table 7 therefore shows estimates from a Probit regression, with errors clustered at the village-cluster level, but which omit the pair-level fixed effects.[23] Marginal effects are reported. As can be seen, the estimated effects are quite similar across the two specifications.

Appendix 5 discusses results from a set of regressions which repeats the same IIT analysis as that reported in Table 6, but which takes the village-cluster as the unit of observation (instead of the household), the cluster-mean as the response variable, and uses Huber-White heteroskedasticity-robust standard errors. This estimation approach, the results of which are reported in appendix Table 5.A.2, is clearly less efficient. However, the instrumental variables regressions used to estimate the full ITE in section 7 below are run at the cluster-level. Table 5.A.2 therefore reports the IIT estimates for cluster-level regressions for easier comparison with the ITE. Magnitudes of the coefficient estimates from this adjusted regression run at the cluster-level are essentially the same as those reported in Table 6. However, with many fewer observations, the significance for some estimates drop from the .01-level to the .05-level.

[23] The inclusion or omission of pair-level fixed effects do not alter the basic results of the linear regression. When fixed effects are omitted, estimated magnitudes of effects are almost identical, and they remain significant well beyond the .01-level within all subsamples, whether using all households together or just the highly vulnerable. For example, across all households regardless of type, the estimated magnitudes are slightly lower when fixed effects are omitted, but just barely. The largest difference in estimated magnitude is .006 (an estimated coefficient of .089 versus .095). Across just the highly vulnerable households, the largest difference in magnitude is .02 (an estimated coefficient of .178 versus .159).

Reduced-Form Effect of Information-Treatment on Percentage of Households in Cluster Receiving A Cash Gift

Table 6. Linear Regression: WLS Approach – Explicitly correcting for heteroskedasticity by running on the whole sample

	All Household Types				Vuln HHs (G)			
	All Districts		Dedza & Mchinji		All Districts		Dedza & Mchinji	
VARIABLES	(1) All Distance Rcv Csh Gft	(2) 3+km Rcv Csh Gft	(3) All Distance Rcv Csh Gft	(4) 3+km Rcv Csh Gft	(5) All Distance Rcv Csh Gft	(6) 3+km Rcv Csh Gft	(7) All Dist Rcv Csh Gft	(8) 3+km Rcv Csh Gft
Mktg Dummy	0.0989***	0.111***	0.0953***	0.0983***	0.159***	0.159***	0.153***	0.153***
	(1.17e-08)	(4.82e-09)	(2.31e-06)	(1.23e-05)	(0.000371)	(0.000346)	(0.00314)	(0.00295)
Constant	0.0409	0.0315	0.0438	0.0413	-0.106*	-0.106*	-0.102	-0.102
	(0.397)	(0.539)	(0.367)	(0.407)	(0.0989)	(0.0963)	(0.117)	(0.114)
Pair Fxd Effcts	YES	YES	YES	YES	YES	YES	YES	YES
EA-Clust SEs	YES	YES	YES	YES	YES	YES	YES	YES
No. Clusters	112	96	85	72	99	91	76	69
Observations	1,992	1,754	1,519	1,330	271	250	205	187
R-squared	0.046	0.043	0.047	0.038	0.225	0.230	0.220	0.221

Cluster-robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 7. Probit – Marginal Effects

	All Household Types				Vuln HHs (G)			
	All Districts		Dedza & Mchinji		All Districts		Dedza & Mchinji	
VARIABLES	(1) All Distance Rcv Csh Gft	(2) 3+km Rcv Csh Gft	(1) All Distance Rcv Csh Gft	(2) 3+km Rcv Csh Gft	(1) All Distance Rcv Csh Gft	(2) 3+km Rcv Csh Gft	(1) All Distance Rcv Csh Gft	(2) 3+km Rcv Csh Gft
Mktg Dummy	0.0979***	0.110***	0.0885***	0.0961***	0.178***	0.180***	0.166***	0.169***
	(5.57e-06)	(5.68e-07)	(0.000450)	(0.000201)	(6.44e-05)	(0.000171)	(0.00112)	(0.00247)
Pair Fxd Effcts	NO	NO	NO	NO	NO	NO	NO	NO
EA-Clust SEs	YES	YES	YES	YES	YES	YES	YES	YES
No. Clusters	112	96	85	72	99	91	76	69
Observations	1,992	1,754	1,519	1,330	271	250	205	187

Cluster-robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.

Table 8. Reduced-Form Effect of Information-Treatment: *Amount* of Cash-Gift Received (Conditional on Receiving a Cash Gift)

	All Household Types				Vuln HHs (G)			
	All Districts		Dedza & Mchinji		All Districts		Dedza & Mchinji	
VARIABLES	(1) All Distance Amt Csh Rcvd	(2) 3+km Amt Csh Rcvd	(3) All Distance Amt Csh Rcvd	(4) 3+km Amt Csh Rcvd	(5) All Distance Amt Csh Rcvd	(6) 3+km Amt Csh Rcvd	(7) All Dist Amt Csh Rcvd	(8) 3+km Amt Csh Rcvd
Mktg Dummy	-173.8	-118.8	-177.4	-100.0	113.0	113.0	130	130
	(0.188)	(0.361)	(0.297)	(0.542)	(0.214)	(0.201)	(0.208)	(0.198)
Constant	1,486***	1,449***	1,488***	1,437***	108.0	108.0	96.67	96.67
	(3.31e-07)	(2.89e-06)	(1.06e-06)	(1.25e-05)	(0.195)	(0.182)	(0.245)	(0.234)
Pair Fxd Effcts	YES	YES	YES	YES	YES	YES	YES	YES
EA-Clust SEs	YES	YES	YES	YES	YES	YES	YES	YES
Observations	512	435	384	324	50	47	40	38
R-squared	0.172	0.194	0.166	0.192	0.969	0.968	0.977	0.977

Cash Gift Amounts Received

Given the impressive impacts on the proportion of highly vulnerable households that receive any cash gifts, one might suppose cash gift *amounts* received by vulnerable households would also be affected by formal savings adoption rates in the local community. The data include amounts for the most recent gift received over the last 90 days. Across all households receiving gifts, the overall average amount reported is 620 kwacha (about $4). However, amounts vary substantially by vulnerability category. The average gift amount is 1,250 kwacha among A-category households, 890 kwacha among B-category households, 540 kwacha among category C, 520 among category D, and 290 kwacha among category G.

In contrast to the very strong association between the marketing instrument and numbers of households receiving gifts, simple tests on amounts show no significant differences across clusters exposed to the instrument and those not exposed to it. This is true overall as well as by vulnerability category. For example, whether looking at all households together, or just the highly vulnerable category, simple t-tests and Mann-Whitney U-tests on the differences in amounts received (conditional on receiving a gift) are nowhere near significant. The average amounts received are in fact mildly higher in the control villages.

Results for a linear regression on the amount of cash received, including pair-level fixed effects and standard errors clustered at the village cluster level are reported in Table 8 below. These results also show that, conditional on receiving a cash gift, amounts are no higher in the treated villages than the non-treated. When restricting the sample to those households that report receiving a cash gift and regress the amount of the gift on the marketing dummy, the dummy is not significant. In the full sample, across all households, a total of 512 households received one or more cash gifts. Among these, the gift amounts were actually slightly lower in the treated

areas, although not significantly so (estimated coefficient on marketing of 174 kwacha, with a p-value of 0.19). Among the highly vulnerable, a total of 50 households received at least one cash gift, and the savings encouragement similarly has no measurable effect on the amount (p=0.21).

These results suggest that the key decision of a benefactor household is not *how much* to give in a cash gift to a supplicant, but instead *whether* to give a cash gift at all. This may indicate that the gifts serve a similar purpose – for example, small amounts of emergency food-consumption maintenance. At the very least, it suggests that there are standard or commonly accepted gift amounts, which may vary by wealth-level of the recipient.

Result 3: The instrument for formal savings adoption is <u>not</u> associated with an increase in the value of cash-gifts received, but instead only the probability of receipt.

In-Kind Gifts and Payment of Fees

The data also include information on receipts of in-kind gifts, as well as occurrences of someone outside the household making payment to a third party on behalf of a household member. The latter might include, for example, paying for a household member's medical fees, school fees, etc. Just as for the cash-gift variable, the question is limited to a recall period of 90 days, and only includes values that are greater than or equal to 50 kwacha (approx. $0.30). Appendix 5, Tables 5.A.3 and 5.A.4 show the results from an IIT analysis of these transfer receipts which parallels that for cash-gifts.

Though the estimated effects are rarely significant, the signs are consistent with the results for cash gifts. In particular, the estimated impact of the information intervention on the

percentage of highly vulnerable households that report an in-kind ranges from 6.0 to 8.9 percentage points, depending on the sample, with borderline significance in two of the four regressions. The estimated impact on the percentage of highly vulnerable households receiving help paying fees to a third party is 4.4 percentage points higher when including all three districts, representing a 62% difference, though this is not significant (p=.206).

This analysis yields two important insights. On the one hand, it appears that *direct monetary transfers to households* are much more sensitive to the change introduced by formal savings uptake than either non-monetary wealth transfers, or monetary payments to a third party. More importantly, the fact that neither of these other two types of assistance decrease (and, if anything, appear to increase) suggests that the cash-gifts result is not simply the result of a substitution. It is not the case, for example, that vulnerable households are now receiving cash gifts in lieu of in-kind gifts, such as food. This is important, as it strongly suggests the change in transfer behaviors induced by the boost in formal savings leads to an improvement in welfares of the vulnerable households, an issue that will be taken up in greater detail in Section 8.

Result 4: The results on cash gifts do <u>not</u> appear to be driven by a substitution of cash-assistance in place of other types of assistance. This suggests that the increased cash assistance may lead to a positive welfare effect among recipients.

6. ITE: Quantifying the Effects of Formal Savings on Transfers to the Most Vulnerable

Up until this point, analysis of effects has been limited to the framework of intention to treat – or, in this case, the *indirect* effect (on *non-users*) of the intention to treat users (what we call "IIT"). The ultimate goal, however, is to determine the causal indirect effects of financial

service use expansion itself on inter-household transfer receipts by non-users. To do this, I regress transfer receipts on the percentage of households in the local community using formal savings, instrumenting for the latter with the randomly assigned information intervention.

Since the information on cash gifts was collected only in the endline, I am constrained to a cross-sectional analysis. That is, in the first stage, I regress local percentage of households with formal savings in 2010 on the marketing dummy, and in the second stage regress assistance receipts in 2010 on the instrumented local percentage of households with formal savings. However, as we have already seen in the panel analysis of the marketing instrument's effects, it appears as if the instrument may not have been successful in boosting the local formal savings use in Lilongwe district. In addition, the marketing-clusters in Lilongwe already had a higher average level of formal savings use than the non-marketing clusters (though it was shown to be driven by one or two outliers). I therefore also report results restricting the data to the other two districts, Dedza and Mchinji.

As the focus here is the *indirect* effects of local formal savings use, I restrict analysis to just the highly-vulnerable category. The unit of observation in these regressions is the village-cluster, and the variables are therefore cluster-level aggregates. The dependent variable is the cluster mean of the 0-1 variable for receiving a cash gift among just the category-G households. That is, it is the proportion of the given village cluster's category-G households that receive a cash gift.

The chief regressor of interest is the cluster mean of the 0-1 variable indicating whether a household has a formal savings account. That is, the key regressor is the proportion of households (among *all* households in the cluster) that report having one or more formal savings accounts. This variable may be endogenous for several possible reasons. For example,

integration into the modern economy may weaken norms for assisting other households (e.g. dilute traditional safety nets and informal aid networks), and may also simultaneously increase the probability of having formal savings. Communities whose residents are more integrated into modern life may therefore have higher formal savings usage rates, and lower gifts among non-users, but not due to an effect of formal savings on assistance.

The first stage is a simple linear estimation that regresses the percentage of households (among everyone) in the village cluster with formal savings on the dummy for marketing. The unit of observation is the village-cluster, so the dependent variable here is the cluster mean of the 0-1 indicator variable for a household having formal savings in 2010. The regression includes pair-level fixed effects.

It should be noted that, since the analysis is restricted to category-G households, I am forced to drop from the regression any clusters that do not have households in this category. This results in dropping 13 village clusters (11%) from the sample. The analysis in Section 4 of the information intervention's effect on use of formal financial services included these 13 clusters. In order to give a more accurate picture of the first stage in the actual IV regressions below, I therefore repeat the analysis of the instrument's effect on local formal savings and loans prevalence, leaving out these 13 clusters. The results are reported in Appendix 5, Table 5.A.5 (formal savings) and Table 5.A.6 (formal credit), and are directly comparable to the results reported in Table 4. As seen in Tables 5.A.5 and 5.A.6, there is very little difference between the results from the full sample, and the results when omitting the 13 clusters without category-G households. The estimated effect of the information intervention on the local percentage of formal savers in the endline is still significant at the .01 to .05 level, depending on the subsample

and specification. In addition, as before, the information intervention has no measurable effect on prevalence of formal credit.

The regressions in Table 4, as well as those in Table 5.A.5, are at the household-level, and therefore yield the most efficient estimates of the effect of the information intervention on the local percentage of formal savers. The first stage in the IV estimations below, however, is a regression of the (calculated) percentage of households in each cluster with formal savings in 2010 on the marketing dummy. That is, the first stage is a regression of the cluster mean for the 0-1 household indicator for formal savings on the dummy for information intervention. We then correct for heteroskedasticity using heteroskedastic-robust standard errors. To the extent that this less efficient estimation of the instrument's effect on formal savings prevalence in the first stage results in a weaker instrument for endline local savings prevalence, this would be evident in the second stage, where the estimate for the instrumented variable would be non-significant. In linear IV regressions with a just-identified first stage, the standard errors in the second stage can still be trusted even if the instrument is marginally "weak".[24]

The second stage is a simple cross-sectional Linear-IV with the endline data, where I regress the percentage of category-G households that receive a cash gift on the predicted percentage of households in the cluster with formal savings. I include pair-level fixed effects. There is of course no reason to cluster at the EA-level, since here the EA is the unit of observation. I also use Huber-White sandwich errors to account for heteroskedasticity caused by use of variables that are percentages with variation in the number of observations used to construct each percentage. I report results from the non-instrumented OLS, as well as the IV, for all distance levels, as well as those pairs beyond the three kilometer threshold, for all three districts and for just Dedza and Mchinji.

[24] Angrist and Pischke, *Mostly Harmless Econometrics*, as well as their follow-up note on this, published online.

The results are reported in Table 9. Both variables have been scaled up so that they are in terms of percentage points (i.e. they are multiplied by 100). The OLS estimates suggest a positive relationship between local formal savings prevalence and cash gift receipts among the most vulnerable. A one point increase in the percentage of local formal savings users is accompanied by an increase in the percentage of vulnerable households that receive a cash gift ranging from between 0.5 to 0.9 percentage points. This is only significant, however, when including all districts and restricting to the three kilometer threshold (though it is quite close to significance at the .10-level in the other samples – especially when restricting to Dedza and Mchinji and looking across all distances).

Instrumenting for local formal savings prevalence to remove the endogeneity sharply increases both the sign and magnitude, suggesting a negative bias in the OLS estimates. As shown in Table 9, a one point increase in the percentage of households in the cluster using formal savings leads to a 2.4 point increase in the percentage of vulnerable households that receive a cash gift. When restricting to Dedza and Mchinji districts, the magnitude of the effect grows to 3.2 percentage points. The effect is highly significant in all four subsamples.

Tables 10 and 11 show results for the same regressions, but instead where the response variables are (i) percentage of vulnerable households that received help paying fees or expenses to a third party; and (ii) percentage of vulnerable households that received an in-kind gift. The results for help paying fees tend to mirror those for cash gifts, though the effects are not nearly as strong, and not significant in all subsamples. The results for receipt of in-kind gifts are more mixed, the effect having a positive sign in some cases and negative in others, but never significant in any of the subsamples. That is, there is essentially no effect on receipt of in-kind gifts by the vulnerable group. This may suggest the effect is stronger for *monetary* wealth

transfers than non-monetary transfers. Regardless, these instrumental-variables estimates of the indirect treatment effect of local formal savings rates on transfer receipts by the highly vulnerable confirm the result articulated in "Result 4" above (and supported by the evidence in the IIT analyses reported in Tables 5.A.3 and 5.A.4). That is, the indirect effect on transfer receipts appears strongest for *direct monetary transfers to households*, and that the effect on receipts of in-kind gifts and help paying fees to a third party are either positive or not significantly different from zero. This suggests the influx of transfer receipts by the highly vulnerable is not driven by a substitution away from other types of transfers.

7. Changes in Informal Loans to the Most Vulnerable

In addition to the information on receipts of cash gifts in 2010, the survey data also contain information on receipts of informal *loans*. These are cash loans received from friends or relatives anytime in the past two years. Unlike that for cash gifts, the data on loans was collected in both years, enabling a panel analysis.

Among the control villages, the percentage of highly vulnerable households reporting informal loans over the previous 2 years was the same in 2008 as it was in 2010, 42.6%. Among information-treated villages, however, there was nearly a ten point increase in the percentage of highly vulnerable receiving informal loans, from 39.7% in 2008 to 49.2% in 2010. When restricting to clusters 3 km out, this trend grows stronger. In control communities, there was a slight reduction in the proportion of highly vulnerable who received loans from friends and relatives, from 45.0% in 2008 to 43.4% in 2010. In treated areas, there was a twelve point increase, from 38.5% to 50.4%.

Table 9. ITE: Effect of Increase in Pctg of HHs Using Formal Savings on Pctg of Vulnerable HHs in Cluster Receiving a Cash Gift

	All Districts				Dedza & Mchinji			
	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Pctg Vuln Rcv Cash Gift	3+km Pctg Vuln Rcv Cash Gift	Pctg Vuln Rcv Cash Gift	3+km Pctg Vuln Rcv Cash Gift	Pctg Vuln Rcv Cash Gift	Pctg Vuln Rcv Cash Gift	Pctg Vuln Rcv Cash Gift	Pctg Vuln Rcv Cash Gift
Pctg HHs w FSAV	0.547	0.547*	2.382**	2.382**	0.927	0.927	3.191**	3.191**
	(0.112)	(0.0978)	(0.0111)	(0.0111)	(0.125)	(0.108)	(0.0109)	(0.0109)
Constant	-4.101	-4.101	-17.87	-17.87	-6.954	-6.954	-23.93	-23.93
	(0.428)	(0.408)	(0.217)	(0.217)	(0.434)	(0.412)	(0.216)	(0.216)
Pair Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Hetsk-Robust SEs	Y	Y	Y	Y	Y	Y	Y	Y
Observations	99	91	99	91	76	69	76	69
R-squared	0.540	0.526	0.272	0.250	0.572	0.555	0.310	0.282

Robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 10. ITE: Effect of Increase in Pctg of HHs With Formal Savings on Pctg of Vulnerable HHs Receiving HELP PAYING FEES

	All Districts				Dedza & Mchinji			
	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Pctg Vuln Rcv Help w Fees	Pctg Vuln Rcv Help w Fees	Pctg Vuln Rcv Help w Fees	Pctg Vuln Rcv Help w Fees	Pctg Vuln Rcv Help w Fees	Pctg Vuln Rcv Help w Fees	Pctg Vuln Rcv Help w Fees	Pctg Vuln Rcv Help w Fees
Pctg HHs w FSAV	0.514	0.514	1.011*	1.011*	0.392	0.392	0.713	0.713
	(0.164)	(0.147)	(0.0558)	(0.0558)	(0.276)	(0.253)	(0.264)	(0.264)
Constant	-3.857	-3.857	-7.582	-7.582	-2.940	-2.940	-5.351	-5.351
	(0.444)	(0.425)	(0.255)	(0.255)	(0.485)	(0.463)	(0.381)	(0.381)
Observations	99	91	99	91	76	69	76	69
R-squared	0.623	0.615	0.589	0.579	0.647	0.636	0.636	0.625

Robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 11. ITE: Effect of Increase in Pctg of HHs With Formal Savings on Pctg of Vulnerable HHs Receiving An IN-KIND Gift

	All Districts				Dedza & Mchinji			
	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Pctg Vuln Rcv In-Kind Gft	Pctg Vuln Rcv In-Kind Gft	Pctg Vuln Rcv In-Kind Gft	Pctg Vuln Rcv In-Kind Gft	Pctg Vuln Rcv In-Kind Gft	Pctg Vuln Rcv In-Kind Gft	Pctg Vuln Rcv In-Kind Gft	Pctg Vuln Rcv In-Kind Gft
Pctg HHs w FSAV	0.451	0.451	0.314	0.314	1.006	1.006	-0.398	-0.398
	(0.338)	(0.318)	(0.674)	(0.674)	(0.273)	(0.250)	(0.709)	(0.709)
Constant	-3.383	-3.383	-2.358	-2.358	-7.547	-7.547	2.986	2.986
	(0.508)	(0.490)	(0.687)	(0.687)	(0.484)	(0.462)	(0.718)	(0.718)
Observations	99	91	99	91	76	69	76	69
R-squared	0.660	0.612	0.659	0.611	0.706	0.655	0.632	0.568

Robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 12. Reduced-Form Effect of Information-Treatment: Change in Percentage of Households Receiving an Informal Loan

	Among Highly Vulnerable Households				Among All Households			
	All Districts		Excluding Lilongwe		All Districts		Excluding Lilongwe	
Variables	(1) All Distances Δ Loan	(2) 3+km Δ Loan	(3) All Distances Δ Loan	(4) 3+ km Δ Loan	(5) All Distances Δ Loan	(6) 3+km Δ Loan	(7) All Distances Δ Loan	(8) 3+ km Δ Loan
Marketing Dummy	0.158**	0.158**	0.222***	0.224***	0.0372	0.0478	0.0547	0.0611
	(0.0291)	(0.0281)	(0.00965)	(0.00845)	(0.220)	(0.143)	(0.161)	(0.149)
Change in Date	-0.00912**	-0.00913**	-0.00628	-0.00689	-0.00194	-0.000550	0.00298	0.00363
	(0.0268)	(0.0325)	(0.253)	(0.209)	(0.219)	(0.789)	(0.212)	(0.133)
Obsv.	271	250	205	187	1,988	1,750	1,516	1,328

Cluster-Robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1. All regressions include cluster-pair fixed effects. Columns 1-4 restrict sample to the highly vulnerable households, while columns 5-8 include all households. Columns 2, 4, 6, and 8 restrict to cluster-pairs located three or more kilometers from the bank-stop.

Analyzing the changes, adding controls, and controlling for intra-cluster correlation sharpens the picture. Table 12 reports results from a set of simple first-differenced regressions of informal loan receipt on a dummy for the treatment, with clustered standard errors. The regressions are run at the household-level and include cluster-pair fixed effects.[25] The dependent variable is the change in whether a given vulnerable household received an informal loan over the two years preceding the interview date, taking values of {-1,0,1}. The coefficient on treatment represents the effect of the information intervention on the change in percentage of vulnerable households that received an informal loan. Columns 1-4 report results when the sample is restricted to the highly vulnerable, with column 1 including all category-G households, column 2 restricting to those communities more than 3 km away, and columns 3 and 4 showing results from the same regressions but which exclude Lilongwe.

The effect of the treatment is highly significant across all four samples, increasing the change in percentage of vulnerable households that received an informal loan by an estimated 15.8 to 22.4 percentage points over the comparable control villages. Note that this effect is remarkably similar to the reduced form effect of the savings encouragement for cash-gift receipts (an increase of roughly 18 percentage points). Just as in the case for cash gifts, the effect grows stronger as remoteness of communities increases and the strength of the instrument exogenously boosting formal savings rates grows. Grouping the treated and

[25] Omitting fixed effects does not alter the main results. (See Flory, 2011 for a more detailed analysis.) The location-specific fixed effects are added to account for interactions of sample-wide changes over the two years with local variables, as well as changes which vary across communities. As an example of the former case, even when an adverse shock is experienced by all communities in the sample (e.g. a region-wide drought), villages with strong urban linkages are likely to have more households which can easily mitigate the effects (for example, through access to non-agricultural income sources). More households in such a community would be in a position to offer loans to less fortunate households than in a community with weaker links to modern economies. For the latter case, any region-specific or spatially covariate shocks which occur over the two-year period (positive or negative) may affect the ability of neighboring households to provide assistance differently in one area than another. Including spatial fixed effects accounts for both possibilities.

control communities, the overall two-year change in percentage of highly vulnerable receiving informal loans at the 3 km threshold is an increase of 4.8 percentage-points.[26] The estimated effect is therefore quite large also in relative terms.

Columns 5-8 report results from analogous regressions, but instead using the total sample of all households. Here, while the sign for the coefficient estimate for the information intervention is always positive, the magnitudes are much smaller, and never significant across any of the specifications. The reduced-form impact of the information intervention on the two-year change in proportion of households receiving a loan from friends or relatives is therefore limited to the highly vulnerable group.[27]

Result 5: The instrument for local rates of formal savings adoption is associated with a substantial increase in the two-year change in the proportion of highly vulnerable households receiving cash-loans from friends or relatives. However, it does not affect the proportion of non-vulnerable households receiving such loans.

The positive impact of the information campaign on the proportion of highly vulnerable households in the community receiving informal loans (14.4-22.4 percentage points) is remarkably similar in scale to its estimated effect on the proportion receiving cash gifts (15.3-18.1 percentage points). While this serves as confirmation that the cash gifts result is not driven by baseline differences, it is also an important finding in its own right. Similar to the evidence on in-kind gift receipts and cash-help with fees, it indicates the influx of cash gift assistance is not driven by a substitution effect. That is, it does not appear to be the case that cash gifts to the

[26] There was a drop of 1.6 percentage points in the information-control clusters, and an increase of 11.6 percentage points in the information-treated.

[27] When the above regressions are run on the sample restricted to the non-vulnerable (i.e. all household except for category G), the highest significance for the estimated coefficient of the information intervention is p=0.385. Results not shown.

highly vulnerable are being substituted in place of cash loans. Rather, both types of wealth-flows to the highly vulnerable are increasing. This suggests possible welfare improvements among the highly vulnerable.

Finally, the results on two other panel variables deserve brief mention. Both years of data include information on whether a household received cash help in response to a specific shock, and also whether a household received cash help specifically in order to buy food. Analyses paralleling that for informal loans shows that the effect of the marketing on these transfers is in the same direction. The estimated effects are consistently positive. However, they are significant at conventional levels in only a few specifications. Appendix 6 reports the results.

8. Welfare Changes Among the Highly Vulnerable

Upon observing the substantial magnitude of increased local savings rates on transfer receipts by the worst-off households, the natural follow-up question is whether this measurably improves welfares. It is not clear *a priori* that increased receipts of this type of assistance will improve household welfares. While all measurable indicators suggest the influx of assistance receipts in marketing clusters is not through a substitution away from other forms of assistance, the data may be failing to capture effects on other smoothing options. Townsend (1994) notes that studying informal insurance institutions one at a time may lead one to overlook important supplementary smoothing options. Focusing on final consumption and related outcomes, however, enables the researcher to evaluate all available institutions jointly. While the data suggests the increased receipts of assistance are at least not substituting for other types of observed inter-household assistance receipts, it is possible the increased assistance might have

general equilibrium effects on other types of consumption-smoothing devices. To determine whether the influx of cash assistance actually improves consumption smoothing and the well-being among the worst-off non service-users we can check simple welfare measures.

This section examines the evidence for improvements in three different welfare indicators: two food-security indicators and one simple health indicator. The first is the percentage of highly vulnerable households that move up the HFIAP scale, from the category "severely food insecure", to one of the three other categories ("moderately insecure", "mildly insecure", "secure"). Across the entire sample, 43.3% of the households in this category in 2008 moved up, to be classified in one of the three less severe food-security categories in 2010 (44.1% in the marketing clusters, 42.4% in the non-marketing clusters, not significantly different).[28] When restricting to the sample of highly vulnerable households, 40.3% of those in marketing-clusters exited the "severely insecure" category, while only 29.3% of those in non-marketing clusters did. This difference is significant at the .05-level (two-sided t-test).

A simple first-differenced regression examines the effect more closely, controlling for location fixed-effects and adjusting for intra-cluster correlation. Table 13 reports the results. The response variable is simply a dummy, which takes a value of one if the household is no longer in the "severely insecure" category in 2010. (Recall that all of the highly vulnerable households, by definition, were in the "severely food insecure" category in 2008.) The coefficient on the marketing dummy thus represents the effect of the information intervention on the proportion of highly vulnerable households that exit the severely-insecure category. As in the first-differenced regressions above, location fixed effects are included to account for the possibility that economy-wide changes are experienced differently in the different locations (due, for example, to market fragmentation or district-level economic changes), of spatially covariate shocks, or of

[28] 30.6% of the sample moved in the opposite direction, from one of the less-severe categories into the most severe.

differences in risk-bearing capacities of different locales. Errors are clustered at the village-cluster level. Change of date is included as an added control under the hypothesis that being interviewed later in the pre-harvest "lean" season might lower the measured food-security of a household and thereby diminish its likelihood of being measured as having exited the "severely insecure" category. The results are fully robust to omitting the change-of-date variable.

The effect is substantial in magnitude, and significant in all specifications with the cluster fixed effects, as well as some without. The estimated effect of the information intervention on the proportion of vulnerable households exiting the severely food-insecure category ranges from 7.1 percentage points to 16.3 percentage points. This represents a 23% - 55% difference over the average proportion of highly vulnerable exiting the severely insecure category in the non-marketing clusters.

The more continuous food-security indicator, HFIAS, indicates similar improvements in household food-consumption among the highly vulnerable in information-treated communities over the two-year period. Recall that the HFIAS indicator runs from 0 to 21, with higher numbers indicating worse food-security. While the average HFIAS score improved by 1.1 points among the highly vulnerable in non-marketing clusters (from 12.0 down to 10.9), it improved by 2.0 points among the highly vulnerable in marketing clusters (from 11.9 down to 9.9). This simple difference is not significant at conventional levels.[29]

However, after controlling for location fixed effects, any changes in the interview date, and accounting for intra-cluster correlation, the estimated effect is significant at the .05-level. The second half of Table 13 reports results from a first-differenced regression of a vulnerable household's HFIAS score on the dummy for the information intervention, the interview date, and

[29] It is significant under a one-sided t-test when the hypothesis that the average effect is larger in the treated areas. A two-sided t-test yields a p-value of 0.194, while a Mann-Whitney U-test yields a p-value of 0.186. A one-sided t-test yields a p-value of .097.

pair-level fixed effects interacted with the period dummy, with errors clustered at the village-cluster level. The response variable is thus the change in the household's HFIAS score, while the regressors are the marketing dummy, and any change in the interview date, with time-varying location fixed-effects. As before, the fixed effects are included in the model to account for any variation in relevant changes across locations (such as covariate shocks or any economic changes confined within certain segmented markets) or location-dependent variations in capacities to address any universally experienced fluctuations.

The coefficient for the marketing dummy represents the average effect of living in a community assigned to the information treatment, on the change in a highly vulnerable household's HFIAS score. It is statistically significant across all four samples, and its magnitude is substantial. Those living in a community exposed to the marketing experienced an estimated reduction of 1.25 to 1.40 points. The effect grows stronger when restricting to Mchinji and Dedza districts, and is also stronger at the more remote distance threshold. As the average baseline value for this variable among the highly vulnerable group in 2008 was 12.0 overall (11.9 in treated, 12.0 in control), this represents a 10-12% improvement in food-security as measured by this scale.

It is also possible that increased cash assistance might lead to improvements in health outcomes. This could occur through several different possible channels. On the one hand, health effects might be directly related to food-security outcomes. Receiving loans or cash and in-kind transfers may reduce the probability of needing to consume poorer quality food.

Cash assistance may also be helping to cover non-food consumption such as medical-related expenditures. Malaria, for example, is extremely prevalent in Malawi, with one of the highest rates in the world. Medical-related expenses may be high enough to inhibit timely

preventive treatment which might avert more serious illness and even death. Anecdotal evidence suggests it is not uncommon, for example, to wait and see how an illness develops to determine whether it is simply a cold or Malaria, because the transportation costs of going to a clinic may be high.[30] In this context cash assistance may help cover transportation costs to free clinics, or help cover admission to often less-overburdened and perhaps closer paying-clinics. This might hasten treatment, or even induce an individual to seek treatment at all (rather than try to wait the illness out). Dercon et. al. (2008) find that in Ethiopian villages a certain type of health insurance provided by informal household networks offers help cover observable components of health-related shocks, for example medical expenses. This may also be occurring in Malawi. Indeed, in qualitative interviews in rural areas of central Malawi, formal-savers report the top reasons people ask them for cash help are for medical expenses and sickness-related issues, to buy food, or to pay for funeral expenses.

One simple measure of health outcomes the data contain is whether any household member was injured or sick over the last 14 days preceding the interview. The overall change in the percentage of households that answered "yes" to this question was an increase of 6.1%, from 75.6% to 81.8% over the two-year period. The change was mildly lower in the marketing communities (+5.8%) than in the non-marketing communities (+6.5%), though the difference is not significant. When restricting to the highly vulnerable, however, the difference is striking. In non-marketing communities, the percentage of highly vulnerable households reporting at least one household member unwell enough to stop normal activities increased by 19.9 percentage

[30] One widow living with her two grandchildren explained that she waited until a very late stage of cerebral Malaria before asking to a borrow a bike so her second grandchild could cycle him to the closest clinic. The survey teams periodically encountered parents seeking urgent assistance to get their children to a clinic after realizing the child had Malaria. The teams would use their car to drive the child to the hospital. Some of the children lived, but others died. In discussions, parents seem to know a sickness may be Malaria, but they note the high cost of going to a clinic every time a household member is ill.

points (from 72.3% to 92.2%). In marketing clusters, the percentage increased only 6.1 percentage-points – the same as the overall change across the sample – from 77.1% to 83.2%. The difference, which amounts to a 13.8 percentage is significant with a two-sided t-test (p=.029).

Once again, a simple first-differenced regression examines the effect more closely, controlling for location fixed-effects and any possible changes in interview date, as well as adjusting for intra-cluster correlation. The results are reported in Table 14. The effect is significant in all four subsamples. The information intervention is associated with a reduction in the proportion of highly vulnerable households reporting an unwell member, ranging from 11.6 to 17.4 percentage points. Note that the scale of the effect, once again, is quite similar to the increase in the percentage of highly vulnerable households that received cash gifts and that received informal loans.

Table 13. Changes in Food-Security Outcomes: Exiting Most Severe Food-Deficiency Status, and Lowering Deficiency Scores

	Exit Severely Food-Insecure				Change in HFIAS Food-Insecurity Score			
	All Districts		Mchinji & Dedza Districts		All Districts		Mchinji & Dedza Districts	
	All Distances	3+ km	All Distances	3+ km	All Distances	3+ km	All Distances	3+ km
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Exit Severe	Exit Severe	Exit Severe	Exit Severe	Δ HFIAS	Δ HFIAS	Δ HFIAS	Δ HFIAS
Marketing	0.162***	0.163***	0.118*	0.120*	-1.251**	-1.252**	-1.393**	-1.402**
	(0.00272)	(0.00241)	(0.0780)	(0.0720)	(0.0147)	(0.0143)	(0.0308)	(0.0285)
Change Date	-0.00351	-0.00389	-0.00249	-0.00290	0.0468	0.0473	0.0801*	0.0827**
	(0.332)	(0.301)	(0.704)	(0.655)	(0.138)	(0.151)	(0.0552)	(0.0477)
Constant	-0.138*	-0.142*	-0.100	-0.105	7.240***	7.245***	7.623***	7.651***
	(0.0642)	(0.0582)	(0.274)	(0.250)	(1.12e-05)	(1.08e-05)	(1.67e-05)	(1.53e-05)
Pair-Lvl FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	272	251	206	188	269	248	203	185
R-squared	0.291	0.279	0.276	0.264	0.219	0.212	0.221	0.211

Cluster-robust Pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 14. Change in the Proportion Vulnerable Households Reporting a Member Unwell

	All Districts		Mchinji & Dedza Districts	
	All Distances	3+ km	All Distances	3+ km
VARIABLES	(1)	(2)	(3)	(4)
Marketing	-0.120*	-0.116*	-0.174**	-0.174**
	(0.0727)	(0.0775)	(0.0470)	(0.0456)
Change Date	-0.00502	-0.00618*	-0.00958	-0.00965
	(0.175)	(0.0938)	(0.171)	(0.167)
Constant	0.370	0.357	0.366	0.365
	(0.117)	(0.126)	(0.178)	(0.176)
Pair-Lvl FE	Y	Y	Y	Y
Observations	272	251	206	188
R-squared	0.278	0.280	0.291	0.294

Cluster-robust Pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

9. Conclusion

Households in the developing world face frequent, often severe, adverse income and consumption shocks. To help cope with hard times, communities with low access to formal financial markets typically have a broad array of informal financial tools and safety-nets which can be used to help smooth consumption and prevent poor outcomes. It is unclear *a priori* how these pre-existing assistance practices will be affected by the spread of market-based instruments.

This amplifies the uncertainty over the impacts that financial deepening will have in poor, rural areas of developing economies. Not only is it necessary to understand the effects of introducing new financial technologies on service-users themselves, but high intra-community interdependence means effects may propagate beyond new service-adopters. Expansion of formal financial markets is likely to interact with indigenous institutions which have evolved to fulfill important economic roles. This may result in unintended consequences for non-users, and there is scant evidence to serve as a guide.

The findings of this paper show that the increased use of formal savings that followed a rapid expansion of savings-access in rural Malawi led to large impacts on inter-household transfers, with particularly strong effects on assistance receipts and welfare outcomes by the worst-off non-using population. Using a community-level information treatment as a source of exogenous variation, instrumental-variables regressions show that a one percentage-point increase in the local proportion of formal-savers leads to an estimated 2.4 to 3.2 percentage-point increase in the proportion of highly vulnerable households that received one or more cash-gifts during the peak period of hunger for the year. In addition, in treated communities – where savings adoption rates were 33% to 40% higher than control communities – the proportion of

highly vulnerable households that received a loan from a friend or relative increased by an estimated 15.8 to 22.4 percentage points over the two-year period, relative to control villages.

These changes in assistance receipts are linked to strong welfare impacts, reflected in at least three indicators. Highly vulnerable households in treated villages are 11.8 to 16.3 percent more likely than comparable households in control villages to exit the worst food-security category in the HFIAP scale ("severely insecure") to enter one of the three less severe categories. They also experience a 1.3 to 1.4 point reduction in the continuous food-insecurity score, HFIAS, relative to the highly vulnerable in control villages. This represents a 10-12% improvement in food-security over baseline values. In addition, highly vulnerable households in treated villages were 12 to 17.4 percent less likely than those in control villages to report any members of the household as recently unwell.

These findings provide some important lessons regarding microfinance and financial deepening. First and foremost, introducing formal financial services can have substantial immediate-term impacts on the *non service-using* population living in these communities. This means that knowing the full effects of expanding financial services into new areas requires accounting for its effects on non-users. It may be possible in some cases that the spillover impacts on non-users are greater than the effects on the clients themselves. Failing to account for the short-term effects on non-clients can lead to a severe under-appreciation of impacts of microfinance.

Second, the intensity of impacts of financial services on non -clients can vary by household vulnerability level. This paper shows that, in the case of formal savings, the poorest households are the most sensitive to changes in inter-household transfer practices which can follow on the introduction of savings services. This is of critical importance if one of the

objectives of microfinance is to positively affect the lives of the poorest of the poor. While this study shows the poorest appear ineligible to take advantage of easier access to formal savings, they are nevertheless the *most* strongly affected by spillover effects of formal savings on inter-household assistance.

Third, the welfare consequences of these changes in inter-household assistance behavior are not trivial. Indeed, one of the most noteworthy findings of this study is the large magnitude of these spillover effects on transfer receipts, particularly among the worst-off households, and the substantial impacts on welfare outcomes. The positive indirect effects experienced in the treated villages of Central Malawi is certainly encouraging. However, the lesson here should perhaps be one of caution.

This paper demonstrates that the informal social safety nets relied on by highly vulnerable households in villages can be highly sensitive to changes induced by the entrance of formal financial services. As discussed in Flory (2011), current models lead to ambiguous predictions of the effects that introducing formal savings can have on inter-household transfer practices. This makes it *possible* for the introduction of formal savings to have a stimulating effect on inter-household wealth flows. It is not clear, however, that introducing formal savings will always have a positive effect. Differing local customs, cultural norms, and levels of social cohesion may cause strong negative effects in other settings. A deeper understanding of the underlying causal mechanisms and the drivers which may lead to positive or negative impacts on local social safety nets is an important avenue for further research.

References

Adelman, Sarah, and Geetha Nagarajan (2009). “Who Des Formal Finance Reach in Rural Malawi?” IRIS Center, University of Maryland, December 2009.

Alderman, H., J. Hoddinott, and B. Kinsey. 2006. “Long-term consequences of early childhood malnutrition.” *Oxford Economic Papers* 58: 450–474.

Angelucci, Manuela and Giacomo De Giorgi, 2009. “Indirect Effects of an Aid Program: How do Cash Transfers Affect Non-Eligibles Consumption?" American Economic Review, 99, 486-509.

Antle, John M., "Econometric Estimation of Producers' Risk Attitudes," American Journal of Agricultural Economics, August 1987, 69:3, 509-22.

Arimond, Mary, and Marie T. Ruel (2004). “Dietary diversity is associated with child nutritional status: evidence from 11 demographic and health surveys”. *Journal of Nutrition* 2004; 134(10): 2579–85.

Arnott, Richard, and Joseph E. Stiglitz,1991. "Moral Hazard and Nonmarket Institutions: Dysfunctional Crowding Out or Peer Monitoring," American Economic Review, 1990, 81:1,179-90.

Banerjee, A. (2005). "The two poverties". In: Dercon, S., (Ed.), Insurance Against Poverty. Oxford University Press, Oxford.

Beegle, Kathleen, and Rajeev H. Dehejia, Roberta Gatti, “Child labor and agricultural shocks,” *Journal of Development Economics*, Volume 81, Issue 1, October 2006, Pages 80-96,

Besley, T. (1995**a**). Nonmarket institutions for credit and risk sharing in low-income countries. Journal of Economic Perspectives, 9, 115–127.

Besley, Timothy. 1995**b** Chapter 36 Savings, credit and insurance, In: Jere Behrman and T.N. Srinivasan, Editor(s), Handbook of Development Economics, Elsevier, Volume 3, Part 1, Pages 2123-2207,

Besley & Coate (1995) “Group Lending, Repayment Incentive, and Socail Capital” Journal of Development Economics

Binswanger, Hans, and Mark Rosenzweig, "Wealth, Weather Risk and the Composition and Profitability of Agricultural Investments," Economic Journal, January 1993, 103, 56-78.

Bliss, Christopher, and Nicholas Stern, Palanpur: The Economy of an Indian Village. Oxford: Oxford University Press, 1982.

Coate, S., Ravallion, M., 1993. “Reciprocity without commitment: characterization and performance of informal insurance arrangements.” *Journal of Development Economics* 40, 1–24.

Coates, Swindale, and Coates J, Swindale A, Bilinsky P. (2007). *Household Food Insecurity Access Scale (HFIAS) for Measurement of Household Food Access: Indicator Guide (v.3).*Washington, DC: Food and Nutrition Technical Assistance Project, Academy for Educational Development; August 2007.

Cox, Donald (1987). “Motives for Private Income Transfers.” *Journal of Political Economy*, 95, 508-46.

Cox, Donald, and Emmanuel Jimenez , 1995 "Private Transfers and the Effectiveness of Public Income Redistribution in the Philippines." In Dominique van de Walle and Kimberly Nead, eds., Public Spending and the Poor: Theory and Evidence. Baltimore: Johns Hopkins University Press.

Cox, Donald, and Emmanuel Jimenez , 1998. "Risk-Sharing and Private Transfers: What about Urban Households?"Economic Development and Cultural Change 46(3):621-39.

Deaton, A. 1992. *Understanding consumption*. Oxford: Clarendon Press.

Deaton, Angus. 1997. *The Analysis of Household Surveys: A Microeconometric Approach to Development Policy*. Baltimore: Johns Hopkins University Press.

Dercon, S. (2005). Insurance Against Poverty. Oxford University Press, Oxford.

Dercon, S. (2005). "Risk, insurance, and poverty: a review". In: Dercon, S., (Ed.), Insurance Against Poverty. Oxford University Press, Oxford.

Dercon, S., Hoddinott, J. (2005). "Health, shocks, and poverty persistence". In: Dercon, S., (Ed.), Insurance Against Poverty. Oxford University Press, Oxford.

Dercon, Stefan, John Hoddinott, Pramila Krishnan and Tassew Woldehannna 2008. “Collective Action and Vulnerability: Burial Societies in Rural Ethiopia.” Collective Action and Property Rights working paper no 83. Washington D.C.: International Food Policy Research Institute.

Fafchamps, M.. 1992. Solidarity networks in rural Africa: rational peasants with a moral economy. Economic Development and Cultural Change 41:1, 147-174.

Fafchamps, Marcel (1999). “Risk-Sharing and Quasi-Credit”. *Journal of Interational Trade & Economic Development.* 8:3 257-278.

Fafchamps, M. and S. Lund (2003) “Risk Sharing Networks in Rural Philippines," *Journal of Development Economics* 71, 261-87.

Fafchamps, M., & Gabre-Madhin, E. (2001). “Agricultural markers in Benin and Malawi: The operation and performance of traders.”
Policy Research Working Paper 2734. Washington, DC: World Bank Development Research Group, Rural Development.

Fafchamps, Marcel, Christopher Udry, and Katherine Czukas, (1998) .” Drought and Saving in West Africa: Are Livestock a Bu¤er Stock?” Journal of Development Economics, Vol. 55, pp. 273-305.

Flory, Jeffrey and Geetha Nagarajan. “The Poor and their Management of Shocks”, IRIS Center Report for “Assessing the Impact of Innovation Grants in Financial Services”, University of Maryland. December, 2009. Available online at:
http://www.fsassessment.umd.edu/publications/pdfs/Malawi-Shock-Management.pdf

Foster, A. (1995). "Prices, credit markets and child growth in low-income rural areas". Economic Journal 105**,** 551-570.

Foster, A., Rosenzweig, M., 2000. “Financial intermediation, transfers, and commitment: do banks crowd out private insurance arrangements in low-income rural areas?” In: Mason, A., Tapinos, G. (Eds.), *Sharing the Wealth: Demographic Changes and Economic Transfers Between Generations.* Oxford Univ. Press, Oxford, pp. 207–228.

Giles, J. 2006. "Is life more risky in the open? Household risk-coping and the opening of China's labor markets." *Journal of Development Economics* 81 (1): 25-60.

Giles, John and Kyeongwon Yoo (2007) “Precautionary Behavior, Migrant Networks and Household Consumption Decisions: An Empirical Analysis Using Household Panel Data from Rural China” The Review of Economics and Statistics, 89(3): 534-551.

Guth, R. A. (2008). Giving a lot for saving a little (Gates foundation to invest in programs to help collect deposits for the world's poor), Wall Street Journal

Heckman, J., R. Lalonde, and J. Smith (1999): "The Economics and Econometrics of Active Labor Market Programs," in *Handbook of Labor Economics*, Vol. 3, ed. by O. Ashenfelter and D. Card. Amsterdam: North-Holland, 1865–2086.

Hoddinott, J. and Kinsey, B. (2001), Child Growth in the Time of Drought. Oxford Bulletin of Economics and Statistics, 63: 409–436.

Jakiela, Pamela and Owen Ozier (2011), "Does Africa need a rotten kin theorem? Experimental evidence from village economies," Washington University in St. Louis, *mimeo*.

Jalan, Jyotsna, and Martin Ravallion. (1999). "Are the Poor Less Well Insured? Evidence on Vulnerability to Income Risk in Rural China." Journal of Development Economics 58**,** 61-82.

Jalan, J., Ravallion, M. (2004). "Household income dynamics in rural China". In: Dercon, S., (Ed.), Insurance Against Poverty. Oxford University Press, Oxford.

Jensen, Robert T., 2004 "Do private transfers 'displace' the benefits of public transfers? Evidence from South Africa" *Journal of Public Economics*, 2004, 88, 1-2, 89-112

Jacoby, H. G., Skoufias, E. (1997). "Risk, Financial Markets, and Human Capital in a Developing Country". Review of Economic Studies 64**,** 311-335.

Karlan, Dean and Jonathan Morduch (2009). "Access to finance," Chapter 2 in M. Rosenzweig and D. Rodrik (eds.) *Handbook of Development Economics, Volume 5*.

Kletzer and Wright (1992) "Sovereign Debt Renegotiation in a consumption-smoothing model"

Ligon, Ethan, Jonathan P. Thomas and Tim Worrall. 2000. "Mutual Insurance, Individual Savings, and Limited Commitment." *Review of Economic Dynamics*. 3, pp. 216-246.

Lucas, Robert E.B. and Oded Stark (1985). "Motivations to Remit: Evidence from Botswana". *Journal of Political Economy*, 93, 901-18.

Malawi Integrated Household Survey (IHS2) 2004-2005 Volume 1: Household Socio-Economic Characteristics Zomba , National Statistical Office of Malawi; 2005.

Miguel, E., and M. Kremer, (2004), “Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities,” *Econometrica*, 72(1), 159-217.

McGuinness, Elizabeth (2008). “Malawi’s Financial Landscape: Where Does Opportunity International Bank of Malawi Fit in?”, Microfinance Opportunites, July 2008. Available online at: http://fsassessment.umd.edu/publications/pdfs/OIBM-Financial-Landscape.pdf

Meagher, Patrick (2010). “Constant Gardening: A Study of Malawi’s Enabling Enviornment for Microfinance. “ IRIS Center, University of Maryalnd, July 2010. Available online at http://fsassessment.umd.edu/publications/pdfs/Malawi-Environment-Studies.pdf

Morduch, Jonathan (1990), "Risk, Production, and Saving: Theory and Evidence from Indian Households," Mimeo, Harvard University, 1990.

Morduch, Jonathan. 1995. "Income Smoothing and Consumption Smoothing," J. Econ. Perspect. 9:3, pp. 103-14.

Morduch, J. 1999a. "Between the state and the market: can informal insurance patch the safety net?". World Bank Research Observer 14:2 187-207.

Morduch, J. 1999b. “The Microfinance Promise,” *Journal of Economic Literature*, 37, pp 1569-1614.

Morduch, J. (2006). "Micro-insurance: the next revolution? What have we learned about poverty?". In: Banerjee, A., Benabou, R., Mookherjee, D., (Eds.), Understanding Poverty. Oxford University Press, New York.

Paulson, Anna, “Insurance Motives for Migration: Evidence from Thailand,” Kellogg Graduate School of Management, Northwestern University, mimeo, 2000.

Paxson, Christina,(1992) "Using Weather Variability to Estimate the Response of Savings to Transitory Income in Thailand," American Economic Review, 82:1, 15-33.

Platteau, Jean-Philippe, and Anita Abraham.1 987. "An Inquiry into Quasi-Credit Contracts: The Role of Reciprocal Credit and Interlinked Deals in Small-Scale Fishing Communities." Journal of Development Studies 23(4):461-90.

Rose, E. (1999). "Consumption smoothing and excess female mortality in rural India". Review of Economics and Statistics 81**,** 41-49.

Rosenzweig, Mark R. and Oded Stark. 1989. “Consumption Smoothing, Migration and Marriage: Evidence from Rural India.” Journal of Political Economy. 97, 4 pp. 905-927.

Rosenzweig, Mark and Kenneth Wolpin (1993) "Credit Market Constraints, Consumption Smoothing, and the Accumulation of Durable Production Assets in Low: income Countries: Investment in Bullocks in India", Journal of Political Economy, 101 (21): 223-244.

Stiglitz, Joseph E., 1990. "Peer Monitoring and Credit Markets," World Bank Economic Review, 4:3, 351-66.

Townsend, R. (1994). "Risk and Insurance in Village India". Econometrica 62**,** 539-591.

Townsend, Robert. 1995a. "Consumption Insurance: An Evaluation of Risk-Bearing Systems in Low-Income Countries."Journal of Economic Perspectives 9: 83-102.

Townsend, Robert. 1995b." Financial Systems in Northern Thai Villages."Quarterly Journal of Economics 110(November): 1011-46.

Townsend, Robert M., and Kenichi Ueda. 2006. “Financial Deepening, Inequality and Growth: A Model-Based Quantitative Evaluation.” *Review of Economic Studies*, 73 (1): 251–93.

Udry, C. (1994). "Risk and Insurance in a Rural Credit Market: An Empirical Investigation in Northern Nigeria". Review of Economic Studies 61, 495-526.

Wade, Robert, 1988. *Village Republics: Economic Conditions for Collective Action in South India*. Cambridge: Cambridge University Press,

Walker, Thomas S., and James G. Ryan, Village and Household Economies in India's Semi-Arid Tropics. Baltimore, Md.: Johns Hopkins University Press, 1990.

Zeller, M. (1999). The role of micro-finance for income and consumption smoothing. Paper Presented at the IADB Conference on Social Protection and Poverty. Washington DC, 5 February.

Appendix 1

Table A.1. Proportion of HHs in EA with Formal Savings, Controlling for Initial Local Savings Penetration Only

	All Districts				Dedza & Mchinji			
	All Distances		3+km		All Distances		3+km	
VARIABLES	(1) FSAV in 2010	(2) Δ Pctg Fml Savers	(3) FSAV in 2010	(4) Δ Pctg Fml Savers	(5) FSAV in 2010	(6) Δ Pctg Fml Savers	(7) FSAV in 2010	(8) Δ Pctg Fml Savers
Mktg Dummy	0.0275	0.0288	0.0402*	0.0416*	0.0423*	0.0429**	0.0541**	0.0545**
	(0.169)	(0.148)	(0.0748)	(0.0634)	(0.0528)	(0.0493)	(0.0183)	(0.0174)
FSAV in 2008	0.728***	-0.272**	0.625***	-0.376*	0.645***	-0.353***	0.456*	-0.541**
	(3.07e-08)	(0.0277)	(0.00249)	(0.0639)	(6.01e-06)	(0.00959)	(0.0501)	(0.0206)
Constant	-0.103	-0.103	-0.0858	-0.0863	-0.0913	-0.0920	-0.0547	-0.0556
	(0.598)	(0.594)	(0.622)	(0.618)	(0.602)	(0.599)	(0.699)	(0.695)
Pair Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	112	112	96	96	85	85	72	72
R-squared	0.799	0.551	0.765	0.531	0.791	0.609	0.714	0.623

Heteroskedasticity-robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Appendix

{This table shows how strong the intervention's effect on the boost in informal loan receipts by vuln really is – it's robust to excluding fixed effects.}

Table A.2. Change in Percentage of Highly Vulnerable Receiving an Informal Loan, Restricted to 3+ km from Bank-Stop

	All Districts				Excluding Lilongwe			
Variables	(5) Δ Loan	(6) Δ Loan	(7) Δ Loan	(8) Δ Loan	(5) Δ Loan	(6) Δ Loan	(7) Δ Loan	(8) Δ Loan
Marketing Dummy	0.144* (0.0876)	0.146* (0.0748)	0.165** (0.0414)	0.158** (0.0281)	0.193* (0.0512)	0.197** (0.0325)	0.218** (0.0180)	0.224*** (0.00845)
Change in Date	-0.00475 (0.196)	-0.00693* (0.0588)	-0.00732** (0.0394)	-0.00913** (0.0325)	-0.00775 (0.203)	-0.0122** (0.0360)	-0.0117** (0.0493)	-0.00689 (0.209)
Fixd Effcts		District	Zone	Clust-Pair		District	Zone	Clust-Pair
Obsv.	250	250	250	250	187	187	187	187