

Development Programs and General Equilibrium Effects: Experimental Evidence on Formal Savings and Informal Insurance In Villages

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Abstract

I use a randomized field experiment in Malawi to identify the effects of formal savings adoption in villages on inter-household transfers, and to isolate *spillover* effects on non-savers. My findings highlight that general equilibrium effects of development projects, though commonly overlooked, can be very important. They also explore the effectiveness of a novel method to spur formal services adoption and accelerate financial deepening. A panel analysis of 2,006 households reveals a randomly assigned encouragement raises savings account adoption among all but the bottom strata of households. This boost in formal savings-use increases inter-household transfers during peak periods of hunger. Rises in transfer receipts are highest among the poorest, a *de facto* financial services-ineligible group, among whom increased cash help is linked to large improvements in food consumption. The strong impacts I find on *non* service-users means formal finance has immediate-term effects which are much broader than indicated by analyses that restrict attention to impacts on service-users. The findings also highlight the sensitivity of safety nets and welfare outcomes among the village “underclass” to the spread of financial markets. Little is known about how microfinance affects pre-existing informal insurance arrangements, and those who rely on such practices. My findings help address this gap.

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

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

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1 Introduction

Much energy is devoted to understanding the process of economic growth in low-income countries and how to foster development. Analyses of development aid projects form a critical part of this endeavor. They can help uncover general relationships, enrich our theoretical understanding, and assess the effects of particular policies or programs. Yet the focus in much of the project assessment literature leaves open many questions, and misses opportunities to develop a deeper understanding of the development process and effects of specific interventions. These studies commonly examine how certain programs affect those directly “treated”, and ignore general equilibrium effects. This paper presents direct evidence that general equilibrium effects of even modest-sized development projects can be strong, and deserve more attention.

One prominent class of development aid programs promotes increased access to formal financial markets. While most of the research on financial services for the poor has focused on credit, a growing set of studies indicates formal savings can have positive effects on outcomes such as ability to handle shocks, and investments in health and education (Dupas and Robinson, 2012; Prina, 2012). An important question that remains open in this area is how to get the poor to start using new formal savings services. This paper examines evidence on a novel and effective method for inducing account adoption and increasing savings rates in villages.

More broadly, as modern capital markets penetrate ever deeper into isolated areas, there remain important gaps in our understanding of effects. In particular, how formal services interact with pre-existing local practices key to the welfare of many households remains ill-understood. In his analysis of risk-bearing capacities among villages in northern Thailand, Townsend (1995a) observes that the communities most integrated into markets outside the village also had weaker informal credit and insurance institutions, as well as sharper consumption shocks among their

worst-off households. The suggestion that the spread of formal financial markets may harm local social safety nets and increase vulnerability to adverse income and consumption shocks is echoed elsewhere (e.g. Besley (1995), Morduch (1999)). Yet the question of a causal connection remains open.

This paper uses an exogenous boost to savings account adoption rates in rural Malawi to examine these questions more closely. In late 2007, a local microfinance organization rapidly expanded savings access to villages through a fully-equipped bank on wheels, which drove to more remote areas. While access increased for the entire area, a randomly assigned information treatment I designed with the bank raised adoption rates by an additional 33% - 43%, enabling identification of causal impacts of formal savings on informal insurance practices. My findings shed new light on the interaction of capital markets and indigenous social safety nets in villages.

Contrary to the existing correlative evidence, which suggests formal finance may weaken local safety nets, increased local use of formal savings in Malawi has a clear and significant *positive* effect on inter-household wealth flows over the immediate-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 2.3 percentage-point increase in the probability of receiving a cash gift. The proportion of highly vulnerable households receiving loans from friends and relatives also increases by about 16 to 22 percentage points in savings-encouraged villages over a two year period, with no change in loan

receipts among the highly vulnerable in matched control villages. Receipts of other types of assistance, such as in-kind gifts, also mildly increase.

These increases in assistance from other households are associated with significant welfare impacts. Among the worst-off, households in communities that received the savings encouragement are 12 to 16 percent more likely to exit the worst food-security category and enter one of the less severe categories, over the two year period. They also experience a reduction in continuous food-insecurity scores relative to comparable households in the information-control areas, amounting to a 10-12% improvement over baseline values. In addition, the worst-off households living in savings-encouraged communities were 12 to 17 percent less likely to report any members of the household as recently unwell.

Research on financial services for the poor has remained focused on financial service-users themselves. Yet the non-using population is often significant during early stages of financial development, and the poorest households – a subpopulation of key importance for many policy objectives – may never start using formal services. This highlights the importance of considering the institutional context of financial services adoption, and the possibilities that high intra-community interdependence creates for spillover effects. Introducing new services may affect indigenous practices that evolved to fulfill important economic roles, with sharp consequences for non-users, and there remains scant evidence to serve as a guide.

My findings help advance our understanding of the interaction of formal and informal institutions by identifying causal effects through a clean and direct estimation strategy. First, by relying on a randomly assigned instrument in the form of an information intervention, the analysis avoids important endogeneity concerns. Second, the analysis isolates the effects of formal savings from other formal financial services, such as credit or insurance.

Most project evaluations focus on effects among direct beneficiaries. By identifying a group of *de facto* ineligible and the effects they experience, this paper finds broader local effects of microfinance. Beyond its policy and theoretical import, the fact that non-users can be strongly affected by formal finance also suggests greater caution in designing and interpreting empirical studies of microfinance, as spillovers onto comparison groups can strongly bias estimates.

The rest of the paper is organized as follows. The next section briefly reviews the literature on risk and uncertainty in village life, indigenous responses, and the possible effects of microfinance on these practices. In section 3, I describe the empirical setting, data, and identification strategy used to test for the effects of formal savings services on inter-household assistance practices. In section 4, I analyze the effects of a novel information intervention on financial services adoption. Section 5 examines the relationship between the information-treatment and receipts of cash and in-kind gifts among the most vulnerable households (i.e. the reduced form effect of the instrument on transfers). In section 6, I use an instrumental-variables analysis to estimate the magnitude of the effect of increasing the proportion of local formal-savers 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 transfers 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. In section 9, I conclude with a discussion on mechanisms underlying the findings on incidence of transfers, and implications for expanding the scope of experiments in development to better understand general equilibrium effects.

2 Formal Finance and 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 long-lasting or permanent effects, such as serious chronic 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).¹ Negative impacts are often sharpest among the poorest, already at very low consumption levels, highlighting the importance of understanding how the development of formal financial markets affects consumption insurance among the worst-off.

In the face of adverse income and consumption shocks, a variety of informal methods often help individuals in villages avoid low consumption. Methods pursued in isolation include saving through durable goods and other non-financial assets to address future uncertainty (Paxson, 1992; Deaton, 1992; Rosenzweig and Wolpin, 1993; Fafchamps et. al., 1998), as well as 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 potential 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”,

¹For 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 et. al. 2006, Karlan and Morduch (2009) p.57.

and “informal insurance arrangements”, these practices can fulfill a crucial function for individuals in poor, rural communities. Studies across a wide variety of settings show, for example, that households frequently address short-falls in income through informal loans from friends and 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 additional motivations can also play an important role (Hoff and Sen, 2006; Baland et. al., 2007; Comola and Fafchamps 2010). The exchanges embodied in these informal methods are often largely confined within the local community.

A growing literature explores whether formal financial markets can improve on existing informal insurance and consumption-smoothing options to help the poor better address their vulnerability.² Stepping back from this question, in this paper I explore a complementary issue that is commonly overlooked, despite its critical importance for anti-poverty policy: how the spread of formal financial services affects informal insurance institutions, and the effects this has on households unable to use formal services. Several studies have observed strong *correlations* between formal capital markets and weaker inter-household assistance practices (Townsend 1995a, 1995b; Besley, 1995, Morduch, 1999). However, the existence and nature of a *causal* relationship between formal finance and informal systems in villages remains a wide open question.

² See Karlan and Morduch (2009) for a review of this emerging literature.

In this paper, I isolate the effects of formal savings on transfers between households, using a randomly assigned inducement. The analysis shows formal savings adoption strongly affects assistance-receipts 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

3 Data and Empirical Approach

To test the empirical effects of introducing formal savings vehicles to rural areas of the developing world, I use household survey data from Malawi. Malawi is among the poorest countries, has low participation in formal financial markets in rural areas, and significant

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.

incidence of inter-household assistance, gifts, and loans.⁶ In late 2007, a local microfinance bank began expanding formal savings 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 made regular weekly stops at six different trading centers.

This expansion of formal savings into the thin financial environment of rural Malawi provides an ideal setting to examine the effects of financial services on local informal insurance markets. Or, more precisely, 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 information campaign designed to encourage use of 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 villages⁹. Clusters of villages were first categorized based on distance from the mobile van-bank stop: (i) within 5 km; (ii) 5-10 km; (iii) more than 10 km. They were then further split into two population categories: high versus

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.

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.

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.

3.1 The Information Intervention: Financial Services “Extension Worker”

It was not feasible to directly randomize access to the bank’s services. Even if it had been feasible, it is not clear this would have been desirable. Allowing access for some and preventing it for others might have easily caused perceptions of favoritism and discrimination and led to changes in household interactions unrelated to use of financial services, thereby confounding effects. Instead of randomizing access, drawing from focus group discussions on how people obtain trustworthy information from sources outside the village, I designed a formal savings encouragement with the bank that would mirror these other methods of information dissemination, to serve as an instrument. The backbone of this information campaign consisted of periodic visits (via foot and bicycle) to each village from a Field-Based Promotional Assistant (FBPA). The FBPA brought informational materials on the bank’s services, talked with village residents, and left promotional materials in each village assigned to the information-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 FBPAAs – would have had to introduce elements to the information-treated clusters not also present in the control 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 FBPAAs would introduce anything not already present in both the information-treated and control 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 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 information campaign therefore represents nothing new to these areas.¹²

¹⁰ The FBPAAs 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.

¹¹ This was, in fact, the primary inspiration for how I designed the encouragement. After learning this is the standard way villages receive information from outside, I intentionally fashioned the information intervention to mimic these

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 the campaign 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 campaign 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 information-campaign 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 personal wealth as an avenue to future prosperity might be passed on by the FBPA's 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

pre-existing methods. A virtue of fashioning the encouragement in this way is that it fits right in with other commonly experienced "interventions" in these communities, minimizing the risk that it did anything new to the information-treated areas, besides the provision of information on formal financial services.

¹²It 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.

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.

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.

3.2 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 information-treated and non-treated. 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.

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 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.

Table 1: Characteristics of Households in Baseline, Overall and by Treated & Control Clusters

	Overall	Control	Treated	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 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	

Notes: The above table reports descriptive statistics for households in the 2008 cross-section. Except where indicated in parentheses, units are proportions.

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).

3.3 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 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 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.

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 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 bank stop, no 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 bank stop, no cell-phone, and either: (i) no HH member is literate in Chichewa; or (ii) household head is female.	141	131

Notes: Groups A, B, C, and D are mutually exclusive. Group E is a subset of D, F is a subset of E, and G is a subset of F.

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.

4 Assessing the Inducement: Information Effects on Financial Services Use

Access to credit through the microfinance organization's "bank on wheels" was not available to most communities.¹⁵ The information intervention thus served essentially as an encouragement to open a formal savings account with the bank. If it raised general awareness and literacy with respect to financial services, however, it is plausible the campaign might induce individuals to start using services of other financial organizations within the region. In addition, as other organizations might be able to offer loans, it is possible the information campaign could have induced higher formal credit use. The analyses therefore test for changes in use of formal savings or formal credit at any financial organization.

4.1 Effect of Information 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 linear regression of the decision to adopt (quit) use of formal savings (credit) on a dummy variable indicating assignment of the community to the information 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 at least one current 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 information

¹⁵ Access to formal credit from the van-bank is expanded slowly, on a village-by-village basis. It 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, first targets areas closest to the bank's stop, and those with the greatest economic activity.

¹⁶The fixed effects account for the possibility that pairs experience the 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.

dummy, accounting for pair-level effects, and explicitly correcting for heteroskedasticity across clusters due to variation in number of households in each cluster (FGLS).

Column 1 shows results when the sample is restricted to households which did not have formal savings accounts in 2008 (i.e. the baseline non formal-savers). The coefficient estimate for the information dummy shows that the information-treatment increased the proportion of previously non-saving households that adopted formal savings by about 3.1 percentage-points overall. As 9.3% of households in control clusters without formal savings in 2008 adopted formal savings by 2010, the 3.1 percentage-point effect of the inducement represents a boost to savings adoption rates of 33%.

Result 1: *The provision of information on financial services through periodic visits by a field-based assistant increased formal savings adoption rates by 33% overall.*

People living nearest the bank's weekly location are likely to know more about its services than those living further away, independent of whether they receive the information treatment. To test whether the information is indeed more effective in more remote areas, each specification is also run on the sample restricted to clusters for which both members of the cluster-pair are three or more kilometers from the nearest bank-stop.

As column 2 of Table 3 shows, among clusters three or more kilometers from the bank's stop, the information treatment increased the proportion of non-savers that adopted by 3.7 percentage points. Since 8.6% of the non-savers in 2008 living this distance threshold adopted formal savings over the two-year period, this boost to adoption rates in information-treated villages represents an increase of 43% over the control villages. The larger

magnitude and significance of estimated effects as distance increases confirms that the information is more effective at promoting savings-adoption in more remote locations.¹⁷

Result 2: *Information on financial services is more effective in more remote locations. In villages 3 or more kilometers from the bank-stop, information raised adoption rates by 43%.*

Columns 3 and 4 show results from regressions analogous to those in columns 1 and 2, but for which the sample is instead restricted to households that had a formal savings account in 2008. Here, the coefficient on the information dummy represents any effect of the information intervention on the proportion of previous formal-savers that stopped formal savings-use. As the estimates clearly show, the information had no effect on stopping use of formal savings.

Results from regressions analogous to those for columns 1-4, but for changes in formal credit use, are reported in columns 5-8. For columns 5 and 6, the sample is restricted to households with no formal loan in 2008; for columns 7 and 8, the sample is restricted to those with a formal loan in 2008. The estimates show the information intervention had no effect on changes in use of formal credit.

4.2 Effect of Information on the Total Proportion of the Community Using Formal Services

Table 4 reports results on the effect of the information intervention from a different perspective – its impact on changes in the overall proportion of households in the community using formal services. The response variable takes a value of -1 if a household

¹⁷The increasing effect of the information intervention with distance is even more clear 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.

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

VARIABLES	Formal Savings				Formal Credit			
	Start Formal Savings		Stop Formal Savings		Start Formal Credit		Stop Formal Credit	
	(1) All Distances Has Svgs	(2) 3+km Has Svgs	(3) All Distances Has Svgs	(4) 3+km Has Svgs	(5) All Distances Has Loan	(6) 3+km Has Loan	(7) All Distances Has Loan	(8) 3+km Has Loan
Information	0.0306** (0.0138)	0.0371** (0.0147)	-0.0441 (0.0637)	-0.0298 (0.0663)	-0.00708 (0.00868)	-0.00693 (0.00874)	0.00782 (0.120)	-0.0752 (0.151)
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

Notes: Cluster-robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates from linear regressions of the decision to start or stop formal financial service-use. 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 Information Campaign on Local Proportion of Households with Formal Savings and Formal Credit

VARIABLES	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) All Distances Chg in FSAV	(2) 3+ km Chg in FSAV	(3) All Distances Chg in FSAV	(4) 3+ km Chg in FSAV	(5) All Distances Chg in FCRED	(6) 3+ km Chg in FCRED	(7) All Distances Chg in FCRED	(8) 3+ km Chg in FCRED
Information	0.0143 (0.0135)	0.0185 (0.0147)	0.0283* (0.0154)	0.0323* (0.0164)	-0.00250 (0.00868)	-0.00414 (0.00874)	0.00357 (0.120)	-0.000404 (0.151)
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

Notes: Cluster-robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates for linear regressions of the change in the total proportion of households in the community using formal savings (columns 1-4) or formal credit (columns 5-8). All regressions include cluster-pair fixed effects.

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. Here, the coefficient on the information dummy represents the effect of the encouragement on the change in proportion of the *entire* community that has a formal savings account or a formal loan. 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. Columns 3 and 4 show that, 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, and a 3.2 percentage-point increase when restricting to the more remote village clusters. This represents a boost in prevalence of formal saving by 23% and 31%, respectively, over the control villages. Estimates in columns 5-8 confirm that the encouragement had no effect on use of formal credit.

There are two potential reasons for the lack of significance of the estimated effect of the information intervention on formal savings prevalence in Lilongwe. The first is that, due to their closer proximity to the capital city and its financial centers, the 24% of the sample in Lilongwe district may have better access to information and are therefore less sensitive to the information campaign. The second is that expansion of formal savings-use may be partly a function of initial penetration rates. As previously noted, pre-intervention formal savings-use was already higher in

the information-treated communities of Lilongwe. Communities in which a relatively high proportion of households already use formal savings should have less potential for savings-use to grow. This would dampen the instrument's ability to induce changes in the total proportion of households with formal savings over the two-year period. Regression results reported in Appendix Table A show that controlling for initial savings prevalence increases the estimated impact of the encouragement on total proportion of the community with formal savings, and causes it to be significant across all three districts.

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 that communities in Lilongwe are less responsive to the information due to being closer to the capital and its institutions. Since the most conservative approach suggests the encouragement may have not had a significant effect on the two-year change in proportion of communities with formal savings in Lilongwe, all regressions which follow are accompanied by results when restricting to just Dedza and Mchinji, to serve as a robustness check.

4.3 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 uptake of formal savings among this group (see Appendix Table C).

A different set of regressions which restricts to the non-vulnerable categories (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 than those reported above. For example, in Dedza and Mchinji, the information campaign increased the change in proportion of non-vulnerable households with formal savings by 3.3 percentage points ($p=0.06$) across all clusters, and by 4.0 percentage-points ($p=0.03$) among clusters beyond the 3 km threshold. (See Appendix Table D.) This represents a 24% and 34% increase, respectively, over the matched control clusters.¹⁹

5 Reduced-Form Effect of Savings Encouragement on Assistance Receipts

The data contain information on transfers made during the pre-harvest hungry season, the time of year when household resources are generally most restricted. Transfers received during this period are likely to have the highest marginal impacts. It is also the time requests for assistance are arguably most abundant. The randomly assigned information treatment serves as an instrument for local adoption rates, enabling unbiased inferences about the impact of formal savings on transfers. The data also include 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* ineligible (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 by others, induced by formal savings.

Analysis of the effects on transfers focuses mostly on *receipts*, particularly by the most vulnerable households. This is due mainly to the study's focus on empirically testing for the presence of *spillover* effects. As the data do not identify recipients of outgoing

¹⁹ In the control clusters, the average prevalence is 14.9% overall and 11.9% at the 3 km threshold, among the non-vulnerable population.

transfers (or whether they are formal-savers), examining transfers-out provides less information regarding indirect effects of savings adoption.

While the baseline includes data on a broad range of financial services and transactions, the detailed questions on inter-household transfers were only asked during the endline survey. Discussion thus begins with a look at simple cross-sectional differences in prevalence of transfers between the savings-encouraged and non-encouraged clusters, in the endline. I then proceed to a deeper analysis of reduced-form effects of the encouragement on transfer receipts. In the context of assessing direct effects, this would be analogous to intention to treat (ITT) effects. However, since analyzing *indirect* effects, it may be more appropriate to call this an “indirect intention to treat” effect, or “IIT”.²⁰ The indirect intention to treat effects are preliminary evidence on the causal effects of savings accounts.

Since the savings-encouragement is randomly assigned, it is valid to interpret relationships between the encouragement and household outcomes as causal. Fortunately, I do not have to simply rely on this assumption. The full panel does contain information for both years on certain transfers that are similar to those captured only in the endline. Section 8 uses a difference-in-differences analysis of these related types of transfers to explore panel results which serve as a robustness check for the results presented in this section.

Data was gathered on cash gifts of 50 kwacha (about \$.30) or more, received over a 90-day recall period preceding the interview.²¹ The vast majority are from within the local community. While I do not have information on the actual distances between giving and

²⁰ This estimand is similar in spirit to the “ITE” estimand defined by Angelucci et. al. (2009), as the indirect treatment effect. The ultimate object of analysis is of course not the IIT. It is used as an intermediate step to get to the spillover effect of formal savings adoption (the “treatment”) on the “non-treated”.

²¹ 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.

receiving households, 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)²². Given that the standard mode of transport in these areas is walking, or sometimes cycling, this suggests that most of these transfers are between households within the same village, or at most from neighboring villages.

Table 5 shows simple comparisons of the percentage of households receiving cash gifts in the control and savings-encouraged clusters – overall and by household vulnerability type. Before analyzing separately by vulnerability level, we already see a large difference in receipts of cash gifts from other households across savings-encouraged and non-encouraged areas. While 20.8% of all households in the non-encouraged areas received a cash gift in the last 90 days, 30.6% of those in the encouraged areas received one. (Significant with a t-test at the .01 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-encouraged areas received more than one cash gift, 12.0% of all those in the savings-encouraged areas received multiple cash gifts – a difference of 62%. This difference is also highly significant ($p < .001$; results not shown). These findings lead to my third major result.

Result 3: *Receipts of cash gifts during the hungry season are significantly more common in savings-encouraged villages than control villages. Both the likelihood of (i) ever receiving a cash gift; and (ii) receiving multiple cash gifts is higher in savings-encouraged communities.*

²² 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. There are no differences in this proportion across vulnerability categories; for the highly vulnerable, travel times of 5-15 minutes also represent 80% of transfers received.

Note that these figures include households that did adopt formal services. It is possible these differences could be driven partly by some direct effect that formal savings-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.

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

HH Type (Based on 2008 Characteristics)	Control Clusters (#HHs)	Info-Treated 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)
A & B Combined (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)

Notes: 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.

The figures in Table 5 show that the relationship between the formal savings instrument and incidence of cash-gift receipts depends heavily on household vulnerability level. When we

restrict our focus to the least vulnerable groups, for example, the difference between savings-encouraged and non-encouraged areas in cash gift receipts attenuates substantially. Among those households that were food-secure (category A) or mildly food insecure (category B) in 2008, the percentage of households receiving at least one cash gift is not significantly higher in the savings-encouraged villages than in the non- encouraged villages ($p=.294$).

There is a remarkably consistent pattern of an increasingly high encouraged/non-encouraged difference as we move towards indicators of increasing vulnerability. The amount by which the percentage of households receiving gifts is higher in savings-encouraged than non-encouraged 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- encouraged to savings- encouraged (rather than just the change in the proportions), the pattern of increases is even more striking. Gift receipts in savings-encouraged 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 4: *The difference in incidence of cash-gift receipts between savings-encouraged and non-encouraged villages increases as vulnerability rises. The positive impact on the likelihood of (i) ever receiving a cash gift; and of (ii) receiving multiple cash gifts increases with vulnerability.*

5.1 Impact on the De Facto Ineligibles

I now focus on the highest vulnerability category (group G), which consists of 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.

Restricting attention to the highest vulnerability group simplifies the interpretation of 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, 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.²³ This group is therefore assumed to not have access to formal savings, making them a *de facto* “ineligible” group.²⁴

The data confirm that 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. Effects on group G households stemming from local formal savings adoption therefore cannot be caused by direct effects of having an account, but are instead attributable to indirect effects of others in the community having accounts.²⁵

Table 6 reports marginal effects from a set of Probit regressions to help clarify the findings reported in Table 5. These regressions account for intra-cluster correlation within communities, analyze how the effect changes with distance from the bank-stop, and examine

²³ 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 wealth-storage alternatives for low deposit amounts.

²⁴ 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.

²⁵ Inter-household assistance receipts by the handful of households in this category in fact 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.

whether the results are sensitive to excluding Lilongwe district. The dependent variable is a 0-1 indicator for whether a household received a cash gift. Results are reported for the entire sample, and then restricting to just the highly vulnerable category. Errors are clustered at the village-cluster level.²⁶

The conclusions are unaltered by accounting for intra-cluster correlation, and are the same as those based on the simpler tests presented in Table 5. Among all vulnerability categories taken together, the information treatment 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 18 percentage points. Both estimates remain highly significant. In addition, there is evidence that the effect is slightly stronger in more remote communities (where the treatment has a stronger effect on savings adoption). Finally, note the results are nearly identical when the sample is restricted to Dedza and Mchinji districts.

Result 5: *There is a strong spillover effect onto non-users of formal savings. Among the highly vulnerable group (de facto ineligible for formal savings), the proportion receiving cash aid is 180% higher in the villages with exogenously increased formal savings adoption.*

5.2 Cash Gift Amounts Received

The data include the value of 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

²⁶ Pair-level fixed effects are not included here, as this is a Probit. Appendix Table E shows the absence of pair-level effects is inconsequential. The table reports results from a set of linear regressions for which the results are nearly identical to those of the Probit estimations. The results of the linear estimations are unaltered by the inclusion or omission of pair fixed effects. See appendix for further discussion.

Table 6. Reduced-Form Effect of Information-Treatment on Percentage of Households in Cluster Receiving A Cash Gift

VARIABLES	All Household Types				Vuln HHs (G)			
	All Districts		Excluding Lilongwe		All Districts		Excluding Lilongwe	
	(1) All Distances Rcv Csh Gft	(2) 3+km Rcv Csh Gft	(3) All Distances Rcv Csh Gft	(4) 3+km Rcv Csh Gft	(5) All Distances Rcv Csh Gft	(6) 3+km Rcv Csh Gft	(7) All Distances Rcv Csh Gft	(8) 3+km Rcv Csh Gft
Information	0.0979*** (0.0216)	0.110*** (0.0221)	0.0885*** (0.0253)	0.0961*** (0.0260)	0.178*** (0.0421)	0.180*** (0.0452)	0.166*** (0.0478)	0.169*** (0.0518)
No. Clusters	112	96	85	72	99	91	76	69
Observations	1,992	1,754	1,519	1,330	271	250	205	187

Notes: The table reports estimated marginal effects for a set of Probit regressions. All regressions use standard errors clustered at the village-cluster level; cluster-robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1. Columns 1-4 include households of all types; columns 5-8 restrict the sample to the highly vulnerable category. Number of observations differs slightly due to missing information on cash gifts for 14 households.

Table 7. Reduced-Form Effect of Information-Treatment on Amount of Cash-Gift Received (Conditional on Receiving a Cash Gift)

VARIABLES	All Household Types				Vuln HHs (G)			
	All Districts		Dedza & Mchinji		All Districts		Dedza & Mchinji	
	(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
Information	-173.8 (131.1)	-118.8 (129.3)	-177.4 (169.1)	-100.0 (163.1)	113.0 (89.63)	113.0 (86.93)	130 (101.3)	130 (98.75)
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

Notes: The table reports estimates from a linear regression of the value of the most recently received cash gift, restricting the sample to those who received at least cash gift. Standard errors are clustered at the village-cluster level, and pair-level fixed effects are included. Cluster-robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1.

value of cash gifts received 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.

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 7. These results show that, conditional on receiving a cash gift, amounts are no higher in the treated villages than the control villages. When restricting the sample to those households that report receiving a cash gift, and regressing the amount of the gift on the information-treatment dummy, the coefficient estimate for the dummy is not significantly different from zero.²⁷

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 consumption-support. At the very least, there appear to exist commonly accepted gift amounts, which may vary by wealth-level of the recipient.

Result 6: *The formal savings encouragement is not associated with an increase in the value of cash-gifts received, but instead only the probability of receipt.*

5.3 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,

²⁷ Mann-Whitney U-tests also show no significant difference in the distributions of amounts of cash received between treated and control villages (conditional on receiving a gift), overall or within any vulnerability categories.

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 Table F reports results from regressions analogous to those reported for cash gifts. They show that the estimated effects of the information-treatment on the proportion of highly vulnerable receiving in-kind gifts or help paying fees are generally not significant at conventional levels. However, the signs are positive, with coefficient estimates ranging from 1.1 to 8.9 percentage points.

This finding yields two important insights. On the one hand, it appears that *direct monetary transfers to households* are more sensitive to changes introduced by formal savings than non-monetary wealth transfers and 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 caused by 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.

Result 7: *Increased cash assistance does not appear to be driven by a reduction in other forms of assistance or by a substitution of cash-assistance in place of other types of help.*

This is important, as it suggests the change in transfer behaviors induced by the boost in formal savings may improve welfares of the highly vulnerable, an issue taken up in greater detail in Section 8.

6 Quantifying Indirect Effects on Transfers to the Most Vulnerable

The findings above show that the information intervention boosted local formal savings-use among the less vulnerable and significantly increased the proportion of the highly vulnerable receiving cash gifts from other households. I now turn to a more careful analysis of the spillover effect of formal savings adoption on the highly vulnerable non-savers.

In order to quantify the effects of increased penetration of formal savings on transfer receipts by the vulnerable, a two-stage least squares regression is used. The percentage of highly vulnerable receiving cash gifts is regressed on the percentage of households in the community using formal savings, instrumenting for the latter with the randomly assigned information intervention.²⁸ The unit of observation in these regressions is the village-cluster, and the variables are therefore cluster-level aggregates. The dependent variable is the proportion of the given village cluster's category-G households that receive a cash gift. The chief regressor of interest is the proportion of households (among *all* households in the cluster) that report having one or more formal savings accounts. As this variable is likely to be endogenous, the instrument is used as a source of exogenous variation for causal inference.²⁹

²⁸ Since the analysis is restricted to category-G households, any clusters that do not have category-G households are forcibly dropped. This results in dropping 13 village clusters, or 11% of the sample. Regressions of the instrument's effect on local formal savings and loans prevalence when these 13 clusters are excluded show that dropping them has little to no effect on the estimated effects of the information intervention. The intervention's estimated impact on local percentage of formal savers in the end-line remains 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.

²⁹ Endogeneity of local formal savings-use has several possible sources. As one example, integration into the modern economy may change assistance norms or weaken traditional safety nets and informal aid networks. It may also simultaneously increase the probability of using formal savings. Communities whose residents are more integrated into modern life may therefore have higher formal savings rates, and lower gift-receipts among non-users, but not due to an effect of formal savings on assistance. Alternatively, self-selection into savings adoption may be correlated with less generosity. If so, lower aid receipts might occur in areas of high formal savings penetration, but because of the unobserved lower levels of local generosity.

The first stage is a simple linear regression of the percentage of households in the village cluster with formal savings on the dummy for information-treatment. The regression includes pair-level fixed effects, and uses heteroskedasticity-robust standard errors.³⁰ The second stage is a simple cross-sectional Linear-IV, which regresses the percentage of category-G households that receive a cash gift on the predicted percentage of households in the cluster with formal savings. This regression includes pair fixed effects and heteroskedasticity-robust standard errors.

Table 8 reports the simple linear and the IV-estimates, at both distance thresholds, with and without Lilongwe district. The variables have been scaled up so that they are in terms of percentage points (they are multiplied by 100). The non-instrumented estimates suggest a positive relationship between local formal savings-use 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 0.5 to 0.9 percentage points. This is only significant, however, when including all districts and restricting to the three kilometer threshold.

Instrumenting for local formal savings sharply increases the magnitude and significance of its effects, suggesting a negative bias in the non-instrumented estimates. After removing the endogeneity, estimates show that a 1 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 excluding Lilongwe district, the magnitude of the effect grows to 3.2 percentage points. The effect is highly significant in all four subsamples.

Tables 9 and 10 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

³⁰ The analysis of the information intervention's effects on formal services-use discussed estimates from regressions run at the household-level (since these estimates are more efficient). However, the first stage in these IV-regressions are run at the village-cluster level.

Table 8. Effect of Increase in Pctg of HHs Using Formal Savings on Pctg of Vulnerable HHs in Cluster Receiving a Cash Gift

VARIABLES	All Districts				Dedza & Mchinji			
	Non-Instrumented		IV		Non-Instrumented		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pctg Vuln Rcv Cash Gift	Pctg Vuln Rcv 3+km Cash Gift	Pctg Vuln Rcv Cash Gift	Pctg Vuln Rcv 3+km 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 (0.337)	0.547* (0.323)	2.382** (0.938)	2.382** (0.938)	0.927 (0.589)	0.927 (0.561)	3.191** (1.254)	3.191** (1.254)
Constant	-4.101 (0.428)	-4.101 (0.408)	-17.87 (0.217)	-17.87 (0.217)	-6.954 (0.434)	-6.954 (0.412)	-23.93 (0.216)	-23.93 (0.216)
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

Notes: The table reports linear and two-stage least-squares estimates of the effect of a one percentage-point increase in local formal savings use on the proportion of highly vulnerable households that receive a cash gift during the hungry season. The unit of observation is the village-cluster. Units are in percentage-points. All regressions include pair-level fixed-effects. Heteroskedasticity-robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 9. Effect of Increase in Pctg of HHs With Formal Savings on Pctg of Vulnerable HHs Receiving HELP PAYING FEES

VARIABLES	All Districts				Dedza & Mchinji			
	Non-Instrumented		IV		Non-Instrumented		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	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 (0.3628)	0.514 (0.3478)	1.011* (0.5286)	1.011* (0.5286)	0.392 (0.3534)	0.392 (0.3368)	0.713 (0.6385)	0.713 (0.6385)
Constant	-3.857 (0.444)	-3.857 (0.425)	-7.582 (0.255)	-7.582 (0.255)	-2.940 (0.485)	-2.940 (0.463)	-5.351 (0.381)	-5.351 (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

Notes: The table reports linear and two-stage least-squares estimates of the effect of a one percentage-point increase in local formal savings use on the proportion of highly vulnerable households that receive help paying fees to a third party. The unit of observation is the village-cluster, and units are in percentage-points. All regressions include pair-level fixed-effects. Heteroskedasticity-robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 10. Effect of Increase in Pctg of HHs With Formal Savings on Pctg of Vulnerable HHs Receiving An IN-KIND Gift

VARIABLES	All Districts				Dedza & Mchinji			
	Non-Instrumented		IV		Non-Instrumented		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pctg Vuln Rcv In-Kind Gift	Pctg Vuln Rcv In-Kind Gift	Pctg Vuln Rcv In-Kind Gift	Pctg Vuln Rcv In-Kind Gift	Pctg Vuln Rcv In-Kind Gift	Pctg Vuln Rcv In-Kind Gift	Pctg Vuln Rcv In-Kind Gift	Pctg Vuln Rcv In-Kind Gift
Pctg HHs w FSAV	0.451 (0.4652)	0.451 (0.4460)	0.314 (0.7468)	0.314 (0.7468)	1.006 (0.9013)	1.006 (0.8588)	-0.398 (1.068)	-0.398 (1.068)
Constant	-3.383 (0.508)	-3.383 (0.490)	-2.358 (0.687)	-2.358 (0.687)	-7.547 (0.484)	-7.547 (0.462)	2.986 (0.718)	2.986 (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

Notes: The table reports linear and two-stage least-squares estimates of the effect of a one percentage-point increase in local formal savings use on the proportion of highly vulnerable households that receive in-kind gifts. The unit of observation is the village-cluster, and units are in percentage-points. All regressions include pair-level fixed-effects. Heteroskedasticity-robust standard errors are in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 11. Reduced-Form Effect of Information-Treatment: Change in Percentage of Households Receiving an Informal Loan

Variables	Among Highly Vulnerable Households				Among All Households			
	All Districts		Excluding Lilongwe		All Districts		Excluding Lilongwe	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Distances Δ Loan	3+km Δ Loan	All Distances Δ Loan	3+ km Δ Loan	All Distances Δ Loan	3+km Δ Loan	All Distances Δ Loan	3+ km Δ Loan
Information	0.158** (0.0715)	0.158** (0.0710)	0.222*** (0.0834)	0.224*** (0.0824)	0.0372 (0.0302)	0.0478 (0.0323)	0.0547 (0.0387)	0.0611 (0.0419)
Change in Date	-0.00912** (0.0268)	-0.00913** (0.0325)	-0.00628 (0.253)	-0.00689 (0.209)	-0.00194 (0.219)	-0.000550 (0.789)	0.00298 (0.212)	0.00363 (0.133)
Obsv.	271	250	205	187	1,988	1,750	1,516	1,328

Notes: Cluster-robust standard errors are 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.

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 as strong, and not significant in all subsamples. The results for receipt of in-kind gifts are more mixed, the coefficient estimate for local formal savings-use having a positive sign in some cases and negative in others. However, it is never significant in any of the subsamples, suggesting 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 7” above. That is, the indirect effect on transfer receipts appears strongest for *direct monetary transfers to households*, and the effect on receipts of in-kind gifts and help paying fees to a third party are either positive or 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, at 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%.

Analyzing the changes, adding controls, and controlling for intra-cluster correlation sharpens the picture. Table 11 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.³¹ 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 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 reduced-form effect of the savings encouragement is highly significant across all four samples, increasing the change in percentage of vulnerable households that received an

³¹ Omitting fixed effects does not alter the main results. 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.

informal loan by an estimated 15.8 to 22.4 percentage points over the comparable control villages. This is remarkably similar in scale 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 for formal savings-use grows. Grouping the treated and 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.³² The 16 to 22 percentage point increase is thus large in both absolute and relative terms.

Columns 5-8 report results from analogous regressions, but instead using the total sample of all households. Here, while the sign on the coefficient estimate for the savings encouragement is positive, the magnitudes are much smaller, and never significant. The reduced-form impact of the savings encouragement on the two-year change in proportion of households receiving a loan from friends or relatives is therefore limited to the highly vulnerable group.³³

Result 8: *The instrument for local rates of formal savings adoption is associated with a large increase in the two-year change in the proportion of highly vulnerable households receiving cash-loans from friends or relatives. However, the savings encouragement shows no link with changes in the proportion of non-vulnerable households receiving such loans.*

The patterns for two other types of assistance deserve brief mention. Both years of data also include information on whether a household received cash help in response to a specific

³² 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.

³³ 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.

shock, and whether a household received cash help specifically in order to buy food. Analyses paralleling that for informal loans shows a similar link between the savings encouragement and receipts of these types of aid. The signs of estimated coefficients are consistently positive, but they are significant at conventional levels in only a few specifications.

8 Welfare Changes Among the Highly Vulnerable

The preceding analyses show an exogenous boost in formal savings rates among the non-vulnerable sharply increases monetary transfers to the highly vulnerable, with no evidence this reduces other types of aid. This suggests positive consumption and welfare effects among the worst-off. Yet, as Townsend (1994) points out, certain smoothing options may remain invisible to the researcher, making it harder to determine whether improvements in one mechanism is accompanied by weakening others. By examining final consumption and welfare outcomes, however, we can measure the effect on all available institutions jointly. I do this in this section.

About 40% of the highly vulnerable in savings-encouraged areas rose from the “severely food-insecure” HFIAP category to one of the three other categories (“moderately insecure”, “mildly insecure”, “secure”) over the two-year period between interviews, while only 29% of the highly vulnerable in control clusters did.³⁴ This difference is significant (two-sided Fisher’s exact test, $p=.057$). Table 12, columns 1-4, report results from a first-differenced linear regression that examines this relationship more closely. The dependent variable is a dummy which takes a value of one if the household exits the “severely insecure” category in 2010.³⁵ The coefficient on the treatment dummy thus represents the reduced-form effect of the savings encouragement on the proportion of highly vulnerable households that exit the severely-insecure

³⁴ 30.6% of the sample moved in the opposite direction, from one of the less-severe categories into the most severe.

³⁵ Recall that all of the highly vulnerable households, by definition, were in the “severely food insecure” category in 2008.

category. Change of interview-date is included as an additional control, since being interviewed later in the pre-harvest “lean” season may affect food consumption.³⁶ Errors are clustered at the village-cluster level, and pair fixed effects are included.

Columns 1 and 2 show the results across the full sample, then restricted to pairs three or more kilometers from the bank-stop. Columns 3 and 4 show results for analogous regressions, excluding Lilongwe. The effect is large and significant in all 4 specifications. The estimated impact of the savings encouragement on the proportion of vulnerable households exiting the severely food-insecure category ranges from 11.8 to 16.3 percentage points. This represents an increase of 39% - 55% over the proportion of highly vulnerable exiting the severely insecure status in the control clusters.

In addition to the impact on this discrete indicator, results reported in columns 5-8 of Table 12 suggest formal savings adoption also caused improvements in the more continuous food-security indicator, HFIAS, among the highly vulnerable. These results are from a regression of the change in each vulnerable household’s HFIAS score on the dummy for the savings encouragement and change in the interview date, with location fixed-effects and errors clustered at the village-cluster level. The coefficient on the information-treatment dummy represents the average effect of living in a savings-encouraged community on a highly vulnerable household’s change in HFIAS score. Recall that the HFIAS indicator runs from 0 to 21, with higher numbers indicating worse food-security.

As columns 5-8 show, the effect is significant across all four samples. Living in a community exposed to the savings-encouragement reduces the HFIAS score of highly vulnerable households by an estimated 1.25 to 1.40 points, relative to comparable households in control-

³⁶ Change-of-date is calculated as the difference between the calendar date of the interview in 2010 and that in 2008. The results are robust to omitting this variable.

Table 12. Changes in Food-Security Outcomes: Exiting Most Severe Food-Deficiency Status, and Lowering Deficiency Scores

VARIABLES	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)
	Exit Severe	Exit Severe	Exit Severe	Exit Severe	Δ HFIAS	Δ HFIAS	Δ HFIAS	Δ HFIAS
Information	0.162*** (0.0526)	0.163*** (0.0522)	0.118* (0.0662)	0.120* (0.0655)	-1.251** (0.504)	-1.252** (0.501)	-1.393** (0.633)	-1.402** (0.626)
Change Date	-0.00351 (0.332)	-0.00389 (0.301)	-0.00249 (0.704)	-0.00290 (0.655)	0.0468 (0.138)	0.0473 (0.151)	0.0801* (0.0552)	0.0827** (0.0477)
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

Notes: Columns 1-4 report linear estimates of the reduced-form impact of the savings-encouragement on the proportion of highly vulnerable households that exit the worst food-security category over the two-year period. Columns 5-6 report linear estimates of the reduced-form effect of the savings-encouragement on the two-year change in household food-security scores (HFIAS) among the highly vulnerable. Unit of observation is at the household-level. Regressions include pair-level fixed effects and errors are clustered at the village-cluster level. Cluster-robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table 13. Change in the Proportion Vulnerable Households Reporting a Member Unwell

VARIABLES	All Districts		Mchinji & Dedza Districts	
	All Distances	3+ km	All Distances	3+ km
	(1)	(2)	(3)	(4)
Information	-0.120* (0.0660)	-0.116* (0.0652)	-0.174** (0.0861)	-0.174** (0.0853)
Change Date	-0.00502 (0.175)	-0.00618* (0.0938)	-0.00958 (0.171)	-0.00965 (0.167)
Observations	272	251	206	188
R-squared	0.278	0.280	0.291	0.294

Notes: The table reports linear estimates of the reduced-form effect of the savings-encouragement on the two-year change in proportion of highly vulnerable households that report one or more members as sick or injured over the 14 days preceding the interview. Unit of observation is at the household-level. Pair-level fixed effects are included, and errors are clustered at the village-cluster level. Cluster-robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

communities. As the average baseline HFIAS score among the highly vulnerable group in 2008 was 12.0, this represents a 10-12% improvement in food-security as measured by this scale.

Result 9: *Two-year improvements in food security among the highly vulnerable are higher in villages with exogenously increased savings-adoption. The probability of exiting the “severely insecure” category is 39%-50% higher, and continuous food-security scores are 10-12% better.*

One of the top reasons people in these areas provide cash aid to others outside the household is to help them obtain medical treatment.³⁷ One commonly cited example is covering bike-taxi fees to get to clinics difficult to access on foot when unwell. This may make it easier for households to seek treatment for ailments such as Malaria, common and often severe in rural Malawi.³⁸ There is also recent evidence indicating possession of a high-liquidity savings account can increase investments in health (Prina, 2012), as well as help individuals smooth their labor supply over health shocks by withdrawing from their accounts for medical expenses (Dupas and Robinson, 2011). The improved access of the highly vulnerable in savings-encouraged villages to cash transfers from other households may help them deal with health in a similar manner. It is also possible that food-security improvements may be directly linked to short-term health effects. For example, going long periods without eating may cause people to feel unwell

One simple measure of health outcomes the data contain is whether any household member was injured or sick over the 14 days preceding the interview. In control communities, the percentage of highly vulnerable reporting at least one household member unwell increased by

³⁷ This information is drawn from qualitative interviews in the research area. The other two most commonly cited reasons for cash transfers to other households are to help individuals buy food and to help cover funeral expenses.

³⁸ Informal health-insurance based on inter-household transfer arrangements have been documented elsewhere. Dercon et. al. (2008), for example, find that households in Ethiopian villages provide each other assistance to help cover medical expenses and other observable components of health-related shocks.

19.9 percentage points over the two-year period. In savings-encouraged clusters, the percentage increased only 6.1 percentage-points.³⁹ This 13.8 percentage-point improvement over comparable households in the control villages, is significant with a two-sided t-test ($p=.029$).

A simple first-differenced linear regression once again examines the effect more closely, controlling for location fixed-effects and any changes in interview date, as well as adjusting for intra-cluster correlation. The results are reported in Table 13. The effect is significant in all four subsamples, and its magnitude is large. The proportion of highly vulnerable households reporting an unwell member drops by 11.6 to 17.4 percentage points in communities that received the savings encouragement. Note that the scale of the impact is quite similar to that observed for exiting the worst food-security category, as well as the increase in percentage of highly vulnerable households that received cash gifts and informal loans.

Result 10: *The highly vulnerable in savings-encouraged areas experience a 13.8 percentage-point reduction in likelihood someone was unwell over the 14 days preceding the interview.*

9 Discussion

“You can withdraw from a bank any time. If you want to sell a goat, you must first find a buyer, and then you need to settle on a price.” (Formal-saver MW, 2010)

That policies often affect prices in one market that cause changes in related markets, is no less true in villages than other settings. Indeed, the insular nature of certain markets in villages can make them more sensitive, so that even “small” or local changes can yield large effects. While a

³⁹ Among the non highly-vulnerable households, there is absolutely no difference. In fact, the increase is mildly higher in the savings-encouraged communities: the proportion of non-vulnerable reporting a member unwell increased by 4.3 percentage points in control-villages, and increased by 5.7 percentage points in information-treated (two-sided t-test, $p=.60$).

program or policy may intend to affect just one specific domain, general equilibrium implies that any changes caused in one market may affect others outside the intended purview of the intervention. Moreover, if individuals besides those visibly “treated” participate in secondary markets affected, there will be spillover effects that go unnoticed if only the directly treated are studied. Whether increasing net rewards of school, decreasing costs of medical treatment, or lowering the cost of accessing financial services, the segmented nature of many markets in village settings provides considerable scope for spillovers. The findings I present in this paper show that these types of broader effects can be strong, and deserve more attention.

The interaction of formal financial markets with informal insurance arrangements in villages illustrates this point: while spillover effects on non-users can be large, this question is commonly ignored. Households in the developing world face frequent adverse income and consumption shocks, which they are often able to smooth across with the use of informal institutions. To date, there is scant evidence on how these pre-existing assistance practices are affected by the spread of formal market-based instruments. My results indicate that expanding formal savings affects inter-household transfers, with particularly strong effects on assistance receipts and welfare outcomes by the poor non-using population. Estimates indicate, for example, that increasing the number of new formal-savers in a village by 3.7 percentage points can raise the proportion of highly vulnerable receiving cash-gifts by 18 percentage-points, and the proportion receiving informal loans by 16 percentage points. These are linked to large improvements in food-consumption and short-term health indicators. As this household category is unable to adopt formal savings whether in the savings-encouraged or control villages, these estimates provide a clean indication of savings adoption spillover effects onto non formal-savers.

These positive spillovers onto non-users stand in contrast to correlations between financial market integration and *weaker* local informal support systems documented elsewhere (e.g. Townsend, 1995a). The negative bias I find in the non-instrumented estimates helps explain this discrepancy. However, the novelty of these findings underscores the importance of understanding the mechanism driving these broader effects on non-users.

People in villages of Malawi often state that they use durable goods and livestock (e.g. goats, radios, or bicycles) to save. They also note, however, that it can take more than a week to find a buyer when they need to sell an asset to use the cash for something else. In addition, market fragmentation can mean liquidating assets during the pre-harvest period will cause depreciation, since others nearby may be dissaving from similar assets at the same time and local demand is likely to be lower due to low incomes during this period. Someone who saves through durables, when asked by a friend or relative for help, must consider not only the amount of assistance provided, but also the time and effort required to liquidate stored wealth in order to share it, as well as potential depreciation.

As households monetize their wealth in bank accounts, the cost of providing a transfer may drop considerably. As one formal saver so aptly puts it, “You can withdraw from a bank any time. If you want to sell a goat, you must find a buyer, and you need to settle on a price.” This suggests the following interaction between formal savings markets and local “informal” insurance markets. By reducing the cost of using formal savings (bringing the bank closer, or by reducing information acquisition costs – as done in the encouragement), the quantity of households adopting accounts increases. These households have higher liquidity, which reduces their cost of making transfers, increasing their transfers out, and raising transfer receipts even

among *non*-savers. This causal chain is consistent with the findings. The results not definitively rule out other potential mechanisms, however. Further research on this question is needed.

It is important to note that the mechanism hypothesized will lead to increased inter-household transfers, only so long as it remains in the interest of the formal-saving household to make such transfers. It is possible that as people gain more experience in the formal financial sector, they will see less value in informal insurance methods and withdraw from local insurance markets. This would be consistent with the earlier evidence already cited on correlations between financial deepening and weaker informal institutions. One of the most salient findings of this study is the sensitivity of food-security and health outcomes of the worst-off households to changes in local safety nets. While I find a strong positive effect over the immediate-term, the long-term effect may be that better-off households abandon these networks, leaving the most vulnerable households with *weakened* safety nets over the long run. This represents another important avenue for future research, one that is critical for effective anti-poverty policy.

More generally, this study highlights opportunities to expand the experimental approach to learn more about village economies and the process of economic modernization, and to better understand the full effects of development projects. RCTs are already increasingly used in the development literature to evaluate the effects of projects on the “treated” and to shed light on causal mechanisms in important dimensions of behavior. While research on project effects is typically silent on the question of general equilibrium and spillovers, this need not be the case.

This paper presents a step in the direction of using experiments in development to test for general equilibrium effects when programs are introduced. Using a market that is local and largely insular – informal financial arrangements for dealing with risk – even a modest-sized program evaluation is able to show large effects. In domains where there is high intra-

community interdependence, the experimental approach can be easily extended to take advantage of opportunities to learn more about the interaction of different markets, potential spillover channels, and changes in behavior, to help build more comprehensive theory. Exploring general equilibrium effects in other localized markets and household behaviors represents a potentially rich avenue for future research.

The results also highlight the dangers inherent in ignoring indirect effects. On the one hand, it can introduce biases that complicate conclusions drawn from empirical analyses, as interpretations may not be as clean as is often assumed. This depends on the counterfactuals used. If control units participate in some market that is shared with the treated, if this market is affected by the treatment, and if this affects outcome variables, then estimated effects of the treatment on the outcomes of interest will be biased. In the case studied in this paper, had randomization been at the *household* level, comparing account-adopters to matched control households in the same community would bias any effects of the account on consumption.

On the other hand, ignoring spillovers onto the “non-treated” can have serious negative policy consequences. General equilibrium adjustments may cause effects on non-participants of a program to be as large as (or larger than) effects on participants. Failing to account for these other effects, which may hurt or help policy goals, may cause a severe under-appreciation of program impacts and lead to ill-informed policy decisions.

Finally, the results of this paper also provide several key lessons specific to the class of development projects concerned with extending formal financial markets access to the poor. First, they demonstrate the effectiveness of a novel method for spurring adoption of formal services and increasing financial deepening. Providing information through periodic visits by a formal finance “extension officer” increases formal savings adoption rates by as much as 43%,

the effect largest in more remote locations and among the second-most well-off category of households. Second, introducing formal financial services can have substantial immediate-term impacts on the *non service-using* population living in these communities. While this paper highlights effects on food consumption of non-users due to strengthened local safety nets, it is likely there are also effects on other outcomes. Exploring other types of spillovers of formal finance onto non-users represents an additional opportunity for future work in this area.

Third, intensity of impacts on the safety nets of non-users vary by household vulnerability level. This contributes to our understanding of the presence and importance of heterogeneous effects in microfinance. Fourth, the worst-off households appear particularly sensitive to spillover effects of formal finance, with strong effects on their safety nets and welfare measures. While the effects I find in the present study are positive, the more general lesson is that the poorest of the poor are highly sensitive to changes in informal insurance systems caused by formal finance.

The spread of high-liquidity savings accounts in rural Malawi increased receipts of cash assistance. However, it is not clear what the effects of other changes in financial markets will be, such as increasing access to long-term fixed deposits (which may raise the cost of sharing wealth). Understanding the effects of other types of financial services on local safety nets thus represents a further important avenue for work in this area.

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Appendix

Table A shows that, conditional on the village cluster's initial proportion of households with formal savings, the savings encouragement has a significant positive effect on the two-year change in proportion of households in the cluster with formal savings. This holds across all districts at the three-kilometer threshold, and across all districts and all distances when also controlling for distance. The significant coefficient estimates for the initial percentage of the cluster with formal savings indicate that the higher the initial percentage of the community with formal savings in 2008, the smaller the increase in that percentage over the ensuing two-year period. For example, column 2 suggests that for every 1 percentage point increase in the initial savings prevalence, the 2-year increase in percentage of households with formal savings will be 0.4 percentage points lower. (The average change in formal savings prevalence across the entire sample is +5.3%.) This suggests that communities may have an inherent capacity for the fraction of households that will take up formal savings, and that as usage nears that capacity, the rate at which new people convert to formal savings technology decreases. It also appears that distance affects the rate of conversion to formal savings use. Those communities closer to the bank-stop appear more responsive to the increased accessibility provided by the bank.

The results of Table B confirm that the encouragement has no effect on formal credit when conditioning on initial prevalence of formal credit-use.

Table A. Change in Proportion of Households in Cluster with Formal Savings, Controlling for Initial Savings Penetration

VARIABLES	All Distances		3+km		Excluding Lilongwe District			
	(2)		(4)		(6)		(8)	
	Δ Pctg Savers	Δ Pctg Savers	Δ Pctg Savers	Δ Pctg Savers	Δ Pctg Savers	Δ Pctg Savers	Δ Pctg Savers	Δ Pctg Savers
Information Dummy	0.0288 (0.148)	0.0360* (0.0586)	0.0416* (0.0634)	0.0366* (0.0999)	0.0429** (0.0493)	0.0430** (0.0355)	0.0545** (0.0174)	0.0480** (0.0389)
Pctg with FSAV in 2008	-0.272** (0.0277)	-0.403** (0.0127)	-0.376* (0.0639)	-0.371* (0.0690)	-0.353*** (0.00959)	-0.509*** (0.00449)	-0.541** (0.0206)	-0.531** (0.0248)
Distance		-0.0255** (0.0433)		-0.0353* (0.0907)		-0.0292* (0.0756)		-0.0308 (0.174)
Observations	112	112	96	96	85	85	72	72
R-squared	0.799	0.591	0.531	0.565	0.609	0.656	0.623	0.651

Notes: All regressions include pair-level fixed effects. Heteroskedasticity-robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table B. Change in Proportion of Households in Cluster with Formal Loans, Controlling for Initial Penetration

VARIABLES	All Distances		3+km		Excluding Lilongwe District			
	(2)		(4)		(6)		(8)	
	Δ Pctg Fml Borrowers	Δ Pctg Fml Borrowers	Δ Pctg Fml Borrowers	Δ Pctg Fml Borrowers	Δ Pctg Fml Borrowers	Δ Pctg Fml Borrowers	Δ Pctg Fml Borrowers	Δ Pctg Fml Borrowers
Information Dummy	8.32e-05 (0.996)	0.00336 (0.816)	-0.00288 (0.847)	-0.00713 (0.611)	0.00926 (0.654)	0.00566 (0.740)	0.00375 (0.839)	-0.00231 (0.896)
Pctg with FCRED in 2008	-0.540*** (0.00914)	-0.555*** (0.000928)	-0.408** (0.0347)	-0.414** (0.0229)	-0.609** (0.0133)	-0.652*** (0.000227)	-0.458* (0.0524)	-0.495** (0.0189)
Distance		-0.0307*** (0.00512)		-0.0306*** (0.00739)		-0.0372*** (0.00197)		-0.0289** (0.0111)
Observations	112	112	96	96	85	85	72	72
R-squared	0.533	0.648	0.608	0.654	0.521	0.662	0.593	0.633

Notes: All regressions include pair-level fixed effects. Heteroskedasticity-robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table C. Effects of Information on Local Proportion of *Highly Vulnerable* Households with Formal Savings and Formal Credit

VARIABLES	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) All Distances	(2) 3+ km	(3) All Distances	(4) 3+ km	(5) All Distances	(6) 3+ km	(7) All Distances	(8) 3+ km
	Chg in FSAV	Chg in FSAV	Chg in FSAV	Chg in FSAV	Chg in FCRED	Chg in FCRED	Chg in FCRED	Chg in FCRED
Information	-0.00834 (0.684)	-0.00834 (0.682)	-0.0195 (0.395)	-0.0195 (0.390)	-0.00309 (0.893)	-0.00309 (0.893)	-0.00845 (0.767)	-0.00845 (0.767)
Observations	270	249	204	186	269	269	203	203
R-squared	0.178	0.162	0.084	0.084	0.332	0.332	0.301	0.301

Notes: Cluster-robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates for linear regressions of the change in the total proportion of group G households in the community using formal savings (columns 1-4) or formal credit (columns 5-8). All regressions include cluster-pair fixed effects. Sample restricted to the households in the highly vulnerable category.

Table D. Effects of Information on Local Proportion of *Non-Vulnerable* Households with Formal Savings and Formal Credit

VARIABLES	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) All Distances	(2) 3+ km	(3) All Distances	(4) 3+ km	(5) All Distances	(6) 3+ km	(7) All Distances	(8) 3+ km
	Chg in FSAV	Chg in FSAV	Chg in FSAV	Chg in FSAV	Chg in FCRED	Chg in FCRED	Chg in FCRED	Chg in FCRED
Information	0.0151 (0.324)	0.0219 (0.187)	0.0334* (0.0584)	0.0401** (0.0322)	-0.00179 (0.885)	-0.00385 (0.741)	0.00300 (0.843)	-0.00176 (0.897)
Observations	1,731	1,513	1,319	1,148	1,709	1,608	1,303	1,236
R-squared	0.039	0.038	0.042	0.042	0.036	0.046	0.036	0.048

Notes: Cluster-robust pval in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table shows estimates for linear regressions of the change in the total proportion of households *not* in group G using formal savings (columns 1-4) or formal credit (columns 5-8). All regressions include cluster-pair fixed effects. Sample restricted to the households not in the highly vulnerable category.

Table E reports results from linear regressions of a $\{0,1\}$ indicator for whether a household received a cash gift on the information intervention, with cluster-pair fixed effects, and errors clustered at the village cluster-level. Results are reported for the entire sample (columns 1-4), and then restricting to just the highly vulnerable category (columns 5-8). The main conclusions are the same as those based on the Probit regressions in Table 6. While the estimated magnitudes mildly differ, the estimated effects all remain highly significant. Among all vulnerability categories, the reduced-form effect of the savings encouragement is an increase in the percentage of households in the cluster receiving a cash gift of about 10 percentage points. It increases the percentage of highly vulnerable households receiving a cash gift by about 15 or 16 percentage points. Note that the results are nearly identical when the sample excludes Lilongwe district.

The inclusion or omission of pair-level fixed effects do not alter the results of the linear regression. When fixed effects are omitted, estimated magnitudes of effects are almost identical, and they remain significant at 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 magnitudes between specifications with and without fixed effects is .02 (an estimated coefficient of .178 versus .159).

Table E. Linear Reduced-Form Effect of Information on Percentage of Households in Village-Cluster Receiving A Cash Gift

VARIABLES	All Household Types				Vuln HHs (G)			
	All Districts		Excluding Lilongwe		All Districts		Excluding Lilongwe	
	(1) All Distances Rev Csh Gft	(2) 3+km Rev Csh Gft	(3) All Distances Rev Csh Gft	(4) 3+km Rev Csh Gft	(5) All Distances Rev Csh Gft	(6) 3+km Rev Csh Gft	(7) All Dist Rev Csh Gft	(8) 3+km Rev Csh Gft
Information	0.0989*** (1.17e-08)	0.111*** (4.82e-09)	0.0953*** (2.31e-06)	0.0983*** (1.23e-05)	0.159*** (0.000371)	0.159*** (0.000346)	0.153*** (0.00314)	0.153*** (0.00295)
Constant	0.0409 (0.397)	0.0315 (0.539)	0.0438 (0.367)	0.0413 (0.407)	-0.106* (0.0989)	-0.106* (0.0963)	-0.102 (0.117)	-0.102 (0.114)
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

Notes: The table reports estimates from a set of linear regressions. The dependent variable is a {0,1} indicator for whether a household received a cash gift. All regressions include cluster-pair fixed effects, and errors clustered at the village-cluster level. Pval in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Table F. Reduced-Form Effect of Savings-Encouragement on Percentage of Vulnerable Receiving In-Kind Gift or Help with Fees

VARIABLES	In-Kind Gifts				Help Paying Fees			
	All Districts		Excluding Lilongwe		All Districts		Excluding Lilongwe	
	(1) All Distances Receive Gift	(2) 3+km Receive Gift	(3) All Distances Receive Gift	(4) 3+km Receive Gift	(5) All Distances Receive Help	(6) 3+km Receive Help	(7) All Distances Receive Help	(8) 3+km Receive Help
Information	0.0891 (0.103)	0.0891 (0.100)	0.0599 (0.325)	0.0599 (0.320)	0.0449 (0.206)	0.0449 (0.202)	0.0105 (0.742)	0.0105 (0.740)
Constant	-0.0594 (0.220)	-0.0594 (0.216)	-0.0399 (0.385)	-0.0399 (0.380)	-0.0299 (0.295)	-0.0299 (0.291)	-0.00699 (0.746)	-0.00699 (0.744)
Observations	271	250	205	187	271	250	205	187
R-squared	0.247	0.227	0.266	0.237	0.300	0.299	0.327	0.329

The above table reports coefficient estimates from a linear regression of the percentage of highly vulnerable households in a village cluster receiving assistance on an indicator for exposure to the savings-encouragement. All regressions include cluster-pair fixed effects, errors clustered at the village-cluster level. Standard errors are clustered at the village-cluster level. *** p<0.01, ** p<0.05, * p<0.1.