Mobile Money and Risk Sharing Against Aggregate Shocks

Emma Riley *

Department of Economics, Manor Road Building, Oxford OX1 3UQ, UK

(email: emma.riley@economics.ox.ac.uk)

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Abstract

Households in developing countries have gained increased access to remittances through the recent introduction of mobile money services. While the benefits of improved risk sharing to the remittance receiver have been examined in past research, benefits to the wider community have not been looked into. I examine the impact of mobile money services on consumption smoothing after an aggregate shock for both users of mobile money and for household that don't use mobile money but who reside in villages with users. This allows me to determine the extent that remittances received via mobile money are shared within villages. Using a difference-in-difference fixed effects specification, I find that after a village-level aggregate shock it is only users of mobile money who are able to prevent a drop in their consumption. This finding has implications for how new technologies might change traditional risk sharing arrangements, with both costs and benefits.

Keywords: risk sharing, mobile money, Tanzania

JEL Classification - O16, O17, O33

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1 Introduction

In developing countries, households use informal risk sharing networks to smooth their consumptions in response to unanticipated idiosyncratic shocks such as illness or death . Households within a network can insure their idiosyncratic shocks through cross-sectional risk sharing which allows a household in a network who is affected by a shock to receive transfers from those who aren't affected. This is on the assumption that when the shocks are reversed a transfer will be made the other way, and crucially relies on not everyone in the same network being subject to the same shock at once. Once network income is controlled for, this means that household income is partially or wholly insured against idiosyncratic income shocks, assuming no information or enforcement constraints. Household consumption will depend on total network consumption, not household income. This sort of cross-sectional risk sharing has been found to exist both within villages and across them (Townsend 1994, Udry 1994, De Weerdt & Dercon 2006, Kazianga & Udry 2006).

However, network consumption is still affected by aggregate shocks which affect everyone in the network at once and against which the network is unable to self-insure itself. Particularly if the network is clustered in one geographical location, such as within a village, aggregate shocks, such as a flood or drought, could occur affecting all of the village at once. Larger risk sharing networks of friends and families in other villages could be used to insure this risk by sending remittances, but in practice it is costly and difficult to send money long distances due to high transaction costs. Mobile money services are a new tool allowing small amounts of money to cheaply, quickly and safely be sent around the country via a mobile phone, dramatically increasing access to a wider remittance network that households can draw from. By allowing risk sharing outside the village with people in other communities which will be less likely to have experienced the same shock, mobile money allows households to insure themselves against aggregate shocks to their village.

This paper examines how the introduction of mobile money services allow remittances to flow into a village after an aggregate shock and to what extent these remittances are shared throughout the village, allowing all households within the village to smooth their consumption. By comparing households in village with and without mobile money, and within villages with mobile money, households that do and do not use mobile money services, I can quantify the benefits of mobile money to both the recipient and to the rest of the village. While previous work has looked at the impact of mobile money on the user, no one has yet looked at the potential benefits to other members of a village when a household uses mobile money. Likewise, previous work has not separated aggregate and idiosyncratic shocks, while I argue a key contribution of mobile money services is enabling risk sharing when an entire village experiences a shock at once. This paper will build upon other work showing the benefit of mobile money use to the user after an idiosyncratic shock (Jack & Suri 2014) and focus on the extent of sharing of the benefits of mobile money use within the village after an aggregate shock.

I begin by looking at aggregate shocks in the form of floods and drought, which are geographically concentrated, large and unexpected and hence cannot be insured within the village. I find that household consumption is significantly negatively affected by these shocks, with household consumption falling approximately 6%.

Secondly, I show that mobile money provides insurance against these aggregate shocks, resulting in household consumption of users no longer being negatively impacted by an aggregate shock to the village. The mechanism proposed here is that mobile money allows the user access to remittances, which I examine in more detail using a single cross section of remittance data. I find that mobile money users are both more likely to receive remittances and, after an aggregate shock, receive an increase in remittances of 10% of per capita income, more than cancelling out the negative effect of the shock. Mobile money therefore allows households to form income sharing networks with others outside their village, resulting in aggregate shocks to village consumption no longer being aggregate shocks to the household's network. When a user experiences an aggregate shock which cannot be insured at the village level, they can ask for help from family and friends in other locations which have not experienced a negative shock and with whom they can reciprocally insure. Remittances can then be sent easily and cheaply via mobile money. This means users of mobile money are able to smooth their consumption after an aggregate shock in a way non users aren't able to.

Thirdly, I examine the wider impact of mobile money transfers within a village, something that has not been looked at before in previous work. If insurance networks cover both users and non-users of mobile money within a village, then if a household uses mobile money and receives remittances after an aggregate shock the remittance will be shared with other non-user members of the insurance network within the village. Hence consumption of non-mobile-money users in villages with other mobile money users will also not decline as much after an aggregate shock as that of households in villages without any mobile money users.

I find that while a user of mobile money is able to perfectly smooth the impact of an aggregate shock, non-users in villages with other mobile money users still experience a fall in consumption. Users of mobile money are not sharing their remittances with other members of the community after an aggregate shock. Possible explanations for this are that recipients of mobile money are able to keep their remittances hidden, or they are choosing not to participate in a risk-sharing network with others in the village and instead are relying on networks outside the village and on the stream of remittances for insurance. I discuss these more extensively in the Discussion section, and these are an exciting area for future research.

The remainder of this paper is organised as follows: I first survey the literature on both informal risk sharing and the emerging literature on mobile money services and their context in Tanzania. I then go through a simple model of risk-sharing. Section 3 summarises the data used in this paper and section 4 , outlines the empirical specifications and makes predictions to be tested in the data. Section 5 covers the main results, robustness checks and mechanisms. Finally, I conclude.

1.1 Literature review

1.1.1 Risk sharing

The literature on the use of mobile money services to smooth consumption ties into a larger literature on how households share risk cross-sectionally. Therefore I begin by looking at why households share risk within a village and under what conditions risk sharing has been shown to occur, before looking at the literature on when risk sharing fails. Lastly, I look at wider risk sharing networks outside the village and the role of remittances.

Households in developing countries are subject to a large amount of variability in income (Dercon and Krishnan, 1996), particularly those reliant on agriculture. In response to this, households have developed strategies for reducing the impact of shocks. These include cross sectional strategies such as informal risk sharing as well as temporal strategies such as income diversification and asset accumulation/de-accumulation (Dercon, 2002). In this paper the focus is on cross-sectional risk sharing.

Under perfect risk sharing within a village, the Pareto efficient outcome results in household income being a monotone increasing function of aggregate village income, so that household transient changes in income are perfectly pooled at the village level (Bardhan and Udry, 1999). With complete markets this Pareto outcome can be achieved by any competitive equilibrium. However, complete markets are unlikely in the presence of information asymmetries and enforcement constraints which prevent credit and insurance markets working.

A large body of research has shown that consumption is at least partially insured at the village level, supported by informal risk networks through mechanisms such as reciprocity within family and community networks. Townsend (1994) finds that household consumption co-moves with village average consumption and isn't affected by factors like contemporaneous own income, sickness, unemployment or idiosyncratic shocks controlling for village consumption. However, he doesn't find that the full Pareto efficient outcome of risk sharing is achieved. Chiappori et al. (2014) find that gifts and insurance transfers through family is the channel for risk sharing in the village and they are unable to reject full risk sharing in Tanzania villages where kin were also present, but strongly reject it when kin were not present.

However there is also a body of work finding little or no risk sharing either at the village or household level. Kazianga and Udry (2006) find far from complete consumption smoothing in Burkina Faso during a severe drought. They find almost no risk sharing in the village even to the idiosyncratic component of the drought and instead households rely almost exclusively on self insurance in the form of grain sales to smooth consumption in a limited way. Likewise Udry (1994) rejects perfect risk sharing in northern Nigeria in informal loan markets. Ravallion and Chaudhuri (1997) question the specification used in Townsend (1994) and highlight the importance of measurement error, concluding there is strong evidence against perfect risk sharing. Even within the household, risk sharing is not complete, with wives experiencing reduced nutrition after a shock (Dercon and Krishnan, 2000).

While idiosyncratic shocks can be insured at the village level, aggregate shocks will still impact village consumption and hence household consumption (Mace, 1991) if consumption can only be insured at the village level. Some papers have questioned whether the village is the right risk-sharing level to consider. De Weerdt and Dercon (2006) find village level risk sharing for food but partial risk sharing via networks for non-food consumption. However, Kinnan (2014) finds strong evidence that village consumption moves together, implying that at least some intra-village insurance is occurring.

Networks of family and friends outside a household's own village are important for smoothing aggregate shocks which affect everyone in the village at once and so cannot be insured within the village. A number of papers have examined these links to others outside the village and how households share risk across a larger network. Rosenzweig (1988) finds that how well households are able to smooth risk ex post doesn't depend on the performance of the village economy but on the extent household have network links with other villages. Fafchamps and Lund (2003) find that households do not receive insurance at the village level but instead mainly insure themselves through networks of family and friends. Shocks are principally insured through informal, state-contingent loans and pure transfers rather than through asset sales. These studies highlight how important networks outside the household's own village are for risk-sharing.

Remittances are a channel through which households with family members outside the village can insure their consumption. Yang and Choi (2007) look at remittance patterns in the Philippines, finding that remittances move in opposite directions to income with 60% of the decline in income after a rainfall shock compensated for by increased remittances. Households without migrant members experience a fall in consumption. Yang (2008), looking at exchange rate shocks in the Philippines, finds that an increase in the value of remittances due to an appreciation of the migrant currency results in more remittances and that these are invested in businesses and child education.

However, sending money across long distances by traditional channels such as through friends or via Western Union can be very costly, slow and unsafe, limiting the effectiveness of this channel. Mobile phone money transfer technology has the potential to overcome these barriers to sending remittances and lower costs (Jack and Suri, 2014), allowing users access to their wider risk-sharing networks and assisting households in smoothing village-level shocks.

1.1.2 Mobile Money Services

Even though mobile money services have been recently introduced, there is a growing literature on their impact (Aron, 2017), particularly on remittances and household consumption smoothing. Mobile money has expanded quickly since the launch of the first such service, M-Pesa, in Kenya in 2007. The quick growth of mobile money has allowed millions of people in developing countries who were otherwise excluded from the formal financial system to transfer money instantly from one phone to another at very low cost. The literature on mobile money is still small, with the first pieces of work focused on describing the patterns of use of mobile money services and how they affect remittance patterns, with recent work exploring the impact of mobile money using panel data. Previous work has focused on Kenya as the initial launch place of mobile money services.

The early literature on mobile money focused on describing its use and correlations with other forms of banking or ways of sending remittances. Mbiti and Weil (2011) describe the impact of M-Pesa in Kenya, finding that M-Pesa changes the pattern of remittance by increasing the frequency and volume of urban-rural transfers while lowering the price of competing remittance services such as Western Union. They find 25% of people report using M-Pesa for savings, and that it lowers the probability of people using informal saving mechanisms, such as ROSCAS, while raising the probability of them being banked. Jack and Suri (2011) also look descriptively at the use of M-Pesa for sending remittances in Kenya and find that remittances sent via M-Pesa are less likely to go to parents and more likely to go to friends and other relatives than other forms of remittance. This could signal that M-Pesa users have/take advantage of a broader network than non-users. They also find over 75% of people use M-Pesa for savings. For those who don't use M-Pesa, the most commonly given reason is not owning a mobile phone followed by not needing the service. Less than 1% report not having access to an agent.

More recent papers have used panel data to determine the impact of mobile money services and particularly look at how sending remittances via mobile phones can help households respond to shocks. Jack and Suri (2014) use panel data to analyse how mobile money facilitates consumption smoothing in response to negative idiosyncratic income shocks. They find that while the consumption of non-user households falls by 7%-10% after a shock, there is no corresponding fall for user households. They find that this effect is due to the improved ability to smooth risk via remittances; in the face of a negative shock, user households are 13% more likely to receive any remittances, receive more remittances and receive a larger total value amounting to 6-10% of annual consumption. Their proposed channel is that mobile money services reduce transaction costs and hence expand the number of network members a household can receive remittances from.

Blumenstock et al. (2014) look at detailed transactional data on mobile airtime * sent via mobile phones after an earthquake in Rwanda and find that mobile phones reduce transaction costs and enable Rwandans to share risk quickly across long distances. However they also find that wealthier people are more likely to receive transfers after the earthquake suggesting regressive consequences to the rapid uptake of mobile money service. They also show that the pattern of remittances is most consistent with a model of reciprocal risk sharing, where transfers are determined by past reciprocity and geographical proximity rather than one of pure altruism.

Munyegera and Matsumoto (2014) look at the impact of mobile money services on household welfare using panel data from Uganda, finding wider benefits to household consumption than just the smoothing of shocks. They find that adopting mobile money increases per capita consumption by 69% and that the mechanism for this is again through remittance. Households with mobile money are 20 percentage points more likely to receive remittances, receive remittances more frequently and the total value of the remittances received are higher 33% than for non-user households.

Batista and Vicente (2016) are the first to use an experimental design to assess the impact of randomised mobile money dissemination in rural Mozambique. They take advantage of the fact that the recent launch of mobile money allows the determination of a control group and randomise individuals with a migrant family member in the city who receive a training about the new mobile money services. They find no effect of mobile money services on total consumption but the treated group is able to increase consumption after a negative shock. More randomised trials to help determine the causal impact of mobile money services would be beneficial.

1.2 Context: Mobile money in Tanzania

There are 4 mobile money providers in Tanzania ; Vodacom's M-Pesa, Zantel's Z-Pesa and Zain's Zap (now Airtel Money), all of which launched in 2008/9, and Tigo's Tigo Pesa which launched in 2010. M-pesa is by far the largest of these with 72% of the market. Take-up of mobile money took off slowly, with only 0.5% of households having ever used mobile money in 2009 (Finscope, 2013), but after Vodacom initiated some changes at the end of 2009 the service took off, reaching a quarter of the population by the end of 2011 and a third by the end of 2013. From only 900 agents in September 2009, the service had 17,000 by December 2013.

Mobile money requires the user to have a mobile phone and sim card from the mobile money provider. The user must register for a mobile money account and can then deposit money through

*Sending mobile airtime is an earlier, simpler service than mobile money but also allows the transfer of funds between two people via a mobile phone in the form of call balances. However it is much harder to turn the transferred funds into cash and can only informally be done that mobile money providers' agents, which are usually located in shops. The cash is then electronically deposited in the customer's account. Customers can transfer money via SMS to other people even on different networks, and make withdrawals at their network's agents anywhere in the country. Users are charged a step-tariff rate for sending money and for withdrawing money from agents, with fees for M-Pesa of around 10% for withdrawing and 3% for sending $5 and falling with the amount. Depositing money on the account is free.

2 Theoretical framework

In this section, I use the canonical model of Mace (1991) to show how aggregate shocks impact household consumption and the effect that mobile money would have.

Consider a network of risk-averse utility maximising households indexed by $j = 1 \dots J$. There are T periods and village states of nature $s_{\tau t}, \tau = 1 \dots S$. There is a probability $\pi(s_{\tau t})$ that state τ occurs in period t such that $\sum_{\tau=1}^{S} \pi(s_{\tau t}) = 1$. Each household has utility $U(C^j(s_{\tau t}), h^j(s_{\tau t}))$ where $C_t^j(s_{\tau t})$ is consumption and $h^j(s_{\tau t})$ is a preference shock representing changes in taste for consumption and both can be functions of the state of the world over time. I assume each household receives an exogenous amount of the consumption good $y_t^j(s_{\tau t})$ which is visible to everyone in the community and composed of a deterministic ($\bar{y}_t^j$) and stochastic component. The stochastic component contains an aggregate shock to household j's endowment ($\eta_t^j(s_{\tau t})$), which may still differ across households and an idiosyncratic shock experiences by household j only ($\epsilon_t^j(s_{\tau t})$).

For the Pareto efficient outcome to be achieved:

$$\frac{U'(C_t^i(s_{\tau t}))}{U'(C_t^j(s_{\tau t}))} = \frac{\lambda^j}{\lambda^i} \qquad \forall i, j, \tau, t \tag{1}$$

where λ^i is the welfare weight for household i. This condition says that the weighted marginal utilities are equated across households so that any household's consumption is a monotonically increasing function of average network consumption.

Assuming a class of utility functions, the power utility functions, that exhibit constant relative risk aversion (Chiappori and Paiella, 2011):

$$U(c, h) = \exp \sigma h_t^j \frac{1}{\sigma} (C_t^j)^{\sigma} \tag{2}$$

Eq. (1) can be manipulated by aggregating over J households, taking logarithms and substituting in the aggregate resource constraint ($C_t^a = y_t^a$), giving the consumption for a household as:

$$\ln C_t^j(s_{\tau t}) = \ln(\bar{y}_t^a + \eta_t^a(s_{\tau t})) + \frac{1}{1-\sigma}(\ln \lambda^i - \lambda^a) + \frac{\sigma}{1-\sigma}(h^i - h_t^a(s_{\tau t})) \tag{3}$$

where

$$C_t^a = \frac{1}{J}\sum_{j=1}^{J} \ln C_t^j \qquad \lambda^a = \frac{1}{J}\sum_{j=1}^{J} \ln \lambda^j \qquad h_t^a = \frac{1}{J}\sum_{j=1}^{J} \ln h_t^j$$

$$y_t^a(s_{\tau t}) = \frac{1}{J}\sum_{j=1}^{J} y_t^j(s_{\tau t}) \qquad \bar{y}_t^a = \frac{1}{J}\sum_{j=1}^{J} \bar{y}_t^j \qquad \eta_t^a = \frac{1}{J}\sum_{j=1}^{J} \eta_t^j(s_{\tau t}) \qquad \sum_{j=1}^{J} \epsilon_t^j(s_{\tau t}) = 0$$

This shows that household consumption depends on the deterministic component of network consumption $\bar{y}$, an aggregate shock η, plus a time invariant household fixed effect λ, which depends on the relative weight in the Pareto optimum allocation, and preference shifters. Household shocks are perfectly insured at the network level as $J \to \infty$, and do not affect household consumption.

The model assumes that households have no access to savings or credit so the only method of smoothing consumption is via insurance with others in the network. A finding of perfect insurance could in actuality be due to within-village transfers, transfers from outside the village or to self insurance (or some other means). While in reality households use self-insurance, such as savings and credit, to smooth their consumption, these can be controlled for in the empirical specification, allowing the effect of insurance within the network to be examined. Households are also assumed in this model to share a common coefficient of constant risk aversion. If households instead have differing risk preferences, tests of efficient risk sharing will reject efficiency even if the households do share risk efficiently (Mazzocco and Saini, 2012). Additionally, perfect and symmetric information and perfect commitment are needed to reach a full risk sharing result (Ligon et al., 2002; Ligon, 1998).

For a network in which all households experience the same aggregate shock at once, consumption will fall. The case considered here of rainfall shocks is an example of an aggregate shock which would affect everyone in the network at once if the network was confined to a small geographical area, such as the village. If households instead insure through networks both inside and outside the village, as many papers such as Fafchamps and Lund (2003) have found, then households will be able to insure against any shock which doesn't affect everyone in their network at once. This crucially supposes a suitable means of making transfers between network partners in different locations, something that will be examined here in the case of the introduction of mobile money.

3 Data and summary statistics

3.1 Household panel

The data used comes from the Tanzania National Panel survey (NPS) 2008-9, 2010-11 and 2012-13, implemented by the Tanzania National Bureau of Statistics and downloaded from the World Bank LSMS microdata catalogue. The survey covers 3,265 households in 26 districts containing 409 Enumeration Areas (EAs), and is designed to be representative of Tanzania as a whole. Within each EA (village) an average of 8 households were randomly selected. The survey made particular effort to track respondents, with all adult former households members tracked to new location, resulting in over 97% of the round 1 households being re-interviewed in round 2 and a total panel attrition rate of 4.8%. The data includes weightings of the probability that an observation was included in the survey to take into account the fact that some areas were over surveyed to reflect the higher variance of the variables of interest (for example in cities).

The survey included questions on consumption, assets, finance, shocks, household characteristics and village characteristics. I combined the data by household since mobile money use is only recorded at the household level. Looking at the characteristics of the household head in table 1, the average household has 5 people, average years of education of the household head is just under 5 years, increasing slightly during the survey. 60% of household heads worked in agriculture, 10% in the private sector as paid workers and 15% were self employed. In 2008-09 annual real per person consumption was 742,386 TZ Shillings ($450), rising to 1,011,279 ($568) in 2012-13.

I generated a wealth index of assets using principal component analysis (PCA) since the value of assets owned was not asked all the waves of the survey. Different components of wealth, such as the number of chickens owned or bicycle ownership, cannot easily be added up. PCA determines the relative importance of variables when seeking to summarize a set of variables. The first principal component accounts for the largest variance across the variables. In a wealth index, the first principal component is assumed to represent relative wealth. Based on this, each factor is given a factor weight representing its relative importance in constructing the principal component. I generated a wealth index score based on these factor weights.

Looking at mobile phone ownership and mobile money use, in 2008-9, 45% of households owned at least one mobile phone, increasing to 62% in 2010-11 and 71% in 2012-13. 13% of households had used a mobile money service in 2010-11 and 38% had by 2012-13. I am interested in both users and non users in villages with mobile money and non-users in villages without mobile money. Therefore I break down the number of villages and households by these categories in Figure 1. This figure shows the large increase in both villages with any mobile money users and the number of mobile money users and non-users within these villages. By the second round of the survey 47% of

Table 1: HH summary stats by wave

	Wave 1		Wave 2		Wave 3	
	Mean	SD	Mean	SD	Mean	SD
Per capita consumption	743,386	725,334	862,266	782,264	1,011,279	1,090,465
Rainfall shock	0.21	0.41	0.21	0.41	0.29	0.46
Mobile money use	0.00	0.00	0.13	0.33	0.38	0.49
Rural	0.69	0.48	0.69	0.46	0.69	0.46
Education of head (yrs)	4.76	3.57	4.84	3.65	4.96	3.70
Female head	0.25	0.43	0.26	0.43	0.28	0.446
Age of head	45.53	15.05	45.85	15.53	46.71	16.04
Household size	5.08	2.86	5.28	3.13	5.02	3.05
Own mobile	0.45	0.50	0.62	0.48	0.71	0.45
Financial access						
Number of loans	0.07	0.30	0.10	0.35	0.12	0.39
Bank account	0.0	0.0	0.20	0.40	0.20	0.40
ROSCA	0.04	0.20	0.05	0.22	0.04	0.19
Wealthscore	-0.11	3.00	0.14	2.85	-0.05	2.56
Occupational dummies						
Agriculture/ Livestock	0.60	0.49	0.56	0.50	0.55	0.50
Fishing	0.02	0.14	0.01	0.12	0.01	0.12
Mining	0.00	0.05	0.00	0.06	0.00	0.06
Tourism	0.00	0.02	0.00	0.02	0.00	0.02
Employed: Government	0.06	0.23	0.06	0.24	0.06	0.23
Parastatal	0.01	0.08	0.01	0.08	0.01	0.07
Private sector	0.09	0.28	0.11	0.31	0.12	0.33
NGO/religious	0.01	0.09	0.01	0.08	0.01	0.09
Self-employed (non-agri) w employees	0.02	0.16	0.03	0.18	0.03	0.16
Self-employed (non-agri) w/o employees	0.15	0.35	0.14	0.35	0.15	0.36
Unpaid family work	0.01	0.05	0.01	0.11	0.01	0.11
Job seeker	0.00	0.05	0.00	0.07	0.00	0.04

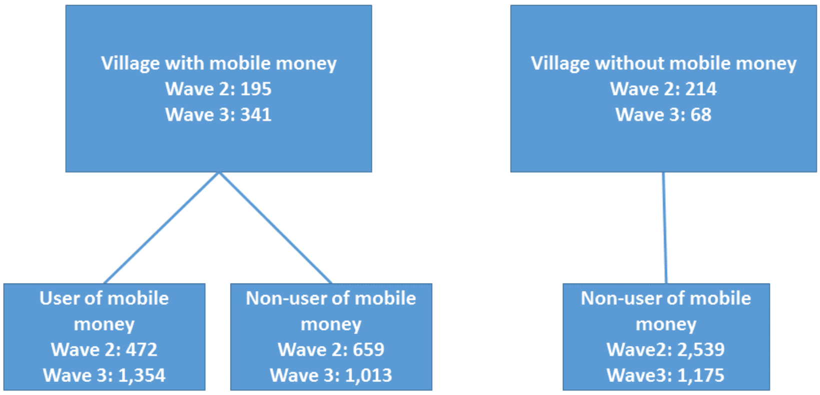


Figure 1: Break down of villages and households by mobile money use

the communities had at least one person using mobile money. By 2012-13, 83% of the communities had at least one person using mobile money. The agent network also expanded rapidly during this period. In the second wave of the data 20% of villages had an agent in the village but this increased to 50% by the third wave.

Sending and receiving money are by far the most popular uses of mobile money, with 67% of users saying they send money and 82% of users saying they receive money. These two uses are also given as the most important use of mobile money services by 80% of respondents. 20% of people report using mobile money services to save up for emergencies and 12% have used it to pay for a good or service. 40% of people use the service at least monthly. The most common reason for not using mobile money was no mobile phone, given by just over 60% of respondents. Lack of proximity to an agent was only given as the reason for not using mobile money by 8% of respondents, also equal to those citing they don't understand the service.

In the third round of the survey there was detailed data on who sent the remittances to whom using what channel, where from and what their relationship was to the sender. 40% of remittances were sent physically via friends and family and 35% by mobile money. Only 2% was sent using a bank account, 1% using Weston Union and 0.4% using the Post Office. In the past it's probable that the majority of remittances were sent via friends and family with very little sent using any more formal channels. 40% of remittances were sent by a son or daughter with only 3.5% sent via a spouse, 7% by a parent and 17% by a sibling. 30% of remittances are sent from Dar Es Salaam and less than 3% are from abroad. This is consistent with a pattern of a family member migrating to another location such as the city within Tanzania and then sending remittances back to their family.

3.2 Rainfall measure

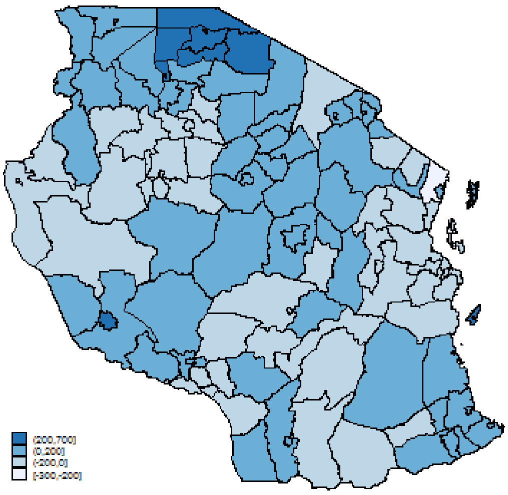
The panel data contains information on self reported shocks, including whether a household has experienced a drought or flood. This is a dummy equal to one if the household reported that they experienced a drought or flood in the year proceeding the survey wave. To the extent that households misreport or subjectively interpret a rainfall shock, for example saying they experienced a rainfall shock to justify a poor crop yield or exaggerating the importance of a rainfall shock in a year when they have no other shocks, this measure of rainfall shocks will be subject to bias and measurement error. I therefore also calculate a rainfall shock measure per village using data from the NOAA's climate prediction centre FEWS (Famine Early Warning System). This is available at 0.1 degree resolution by latitude and longitude across Africa and was included in the Tanzania NPS summarized at the EA level.

I define a rainfall shock as more than a 1 standard deviation in absolute values from the 15 year mean by the nearest rainfall station to the village, as used in Jensen (2000). Deviations from the historic mean capture the extent that rainfall is different from what is expected, and 1 standard deviation is a large difference from normal (on average 200mm difference from an average annual rainfall of 800mm across the entire country). The absolute value is used because either too much or two little rainfall can be harmful. Only deviations greater than 1 standard deviation in absolute value are examined since a little bit too much or too little rain is unlikely to have a big effect, and initially more rain can have a positive effect on crop yields Paxson (1992). I am only interested in extreme, abnormal, rainfall deviations which could be classified as a drought or flood. The rainfall deviations in millimetres by year are shown in figures 2a to 2d.

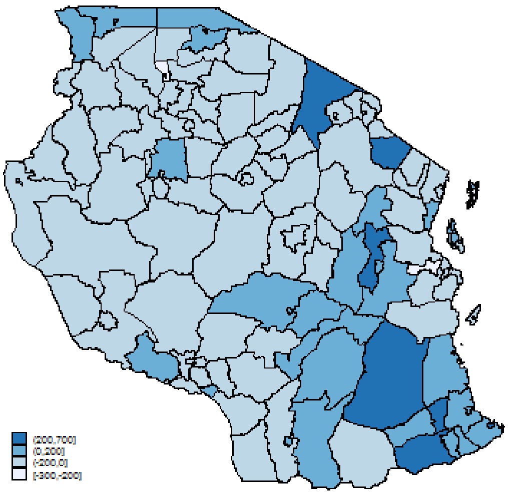
The mechanism through which rainfall shocks negatively affect income can be varied as I look at both rural and urban households. In rural households, droughts and floods will destroy crops leading to loss of income. In urban areas, flooding is likely to be the main mechanism though which rainfall shocks affect income by preventing people from working and by destroying property. For example, in Dar es Salaam in December 2011 there were severe floods resulting in over 6,000 people being displaced and left homeless. I examine the size of these different mechanisms by examining separately droughts and floods for both urban and rural households. In both these examples remittances can be sent to alleviate the loss of consumption, from urban to rural households after crop losses and from rural to urban households in the case of severe floods.

Figure 2: Deviations from mean rainfall, mm

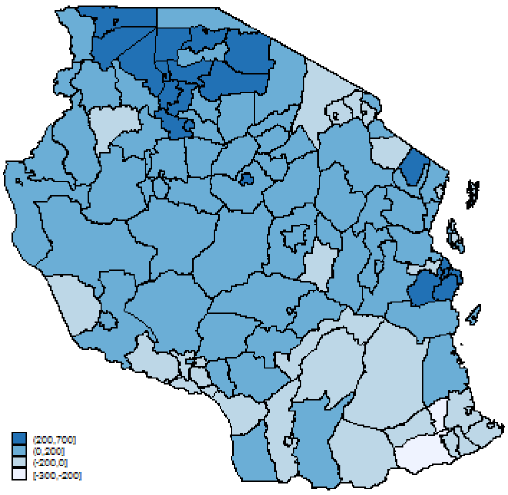
(a) July 2009-10



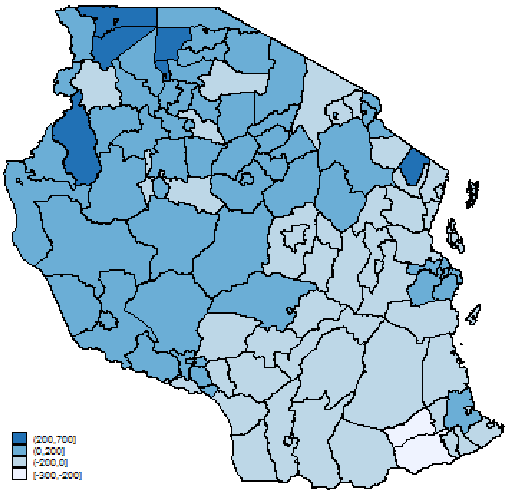
(b) July 2010-11



(c) July 2011-12



(d) July 2012-13



4 Empirical framework

4.1 Empirical Specification

If mobile money allows for transfers to be made in response to an aggregate shock, then consumption will no longer respond to aggregate shocks. If these transfers are shared with others in the village then there will be a positive spillover to non-users from other members of the community using mobile money. If transfers are kept by the user of mobile money then only the user will be able to smooth consumption after an aggregate shock. In order to examine each of these potential impacts of mobile money, I first write equation 3 as a specification I can estimate.

I follow the literature [†] by writing equation 3 as an empirical specification where the aggregate shock η_t^a can be captured by a measure of unexpected shocks affecting the whole village $AggShock_{jvt}$, the pareto weight by a household fixed effect α_j, the deterministic component of income $\bar{y}$ by household characteristics $\boldsymbol{X_{jvt}}$, and the preference shock for both individual households and in aggregate by an error term ε_{jvt}. This error term will also contain any measurement error. I add to this specification measures of the impact of being the recipient of mobile money transfer, using an indicator variable for mobile money use MM_{jvt}, and the impact of being in a village with other mobile money users but not using mobile money yourself (which I'll refer to from now as being a mobile money spillover household), using an indicator variable VMM_{jvt}.

With these assumptions, the empirical specification can be written as:

$$
\begin{aligned}
C_{jvt} =& \gamma_a AggShock_{jvt} + \mu MM_{jvt} + \lambda VMM_{jvt} \\
&+ \beta_m MM_{jvt} \cdot AggShock_{jvt} + \beta_v VMM_{jvt} \cdot AggShock_{jvt} \\
&+ \boldsymbol{\theta X_{jvt}} + \boldsymbol{\psi X_{jvt}} \cdot AggShock_{jvt} + \alpha_j + \delta_t + \varepsilon_{jvt} \qquad (4)
\end{aligned}
$$

where C_{jvt} is household j's per capita log consumption in village v, $AggShock_{jvt}$ is a rainfall shock in village v, MM_{jvt} is mobile money use by household j in village v, VMM_{vt} is an indicator if household j doesn't use mobile money themselves but resides in a village v with at least one other mobile money user, $\boldsymbol{X_{jvt}}$ is a vector of controls consisting of household demographics, financial service use and occupation dummies to control for any other variables which might enable households to better smooth consumption, α_j is a household fixed effect, δ_t is a time trend and ε_{jvt} is a time varying error.

The parameters of interest are β_m, which allows for use of mobile money to affect the household's ability to smooth shocks and β_v, which allows for being a mobile money spillover household to impact the household's ability to smooth shocks.

This gives the following predictions for the empirical estimation:

[†]This is the specification form used by Jack and Suri (2014) which also follows Gertler and Gruber (2002)

Prediction 1 For households in villages without mobile money (when $MM_{jvt} = 0$ and VMM_{vt}=0), $\gamma_a < 0$ so that rainfall shocks have negative effects on consumption

Prediction 2 If users of mobile money receive remittances after an aggregate shock then $\beta_m > 0$.

Prediction 3 For households in villages with other mobile money user that don't use mobile money themselves, if there is some sharing of remittances within the village after an aggregate shock then $\beta_v > 0$.

I estimate equation (4) using difference-in-differences on a household panel dataset.

4.2 Identification strategy

In this section I explain how I implement the estimating equation (4) using my household panel data set and the assumptions required for identification.

To control for household characteristics, all regressions include the full sets of controls from Table 1. Standard errors are clustered at the village level since mobile money agents are located by village and so the decision to use mobile money will be correlated within villages but not across villages. All regressions also control for village characteristics which could affect the ease of sending remittances. These are the distance to the nearest main road, distance to nearest population centre and distance to nearest market. The data is also weighted in all regressions by the inverse of the probability that the observation is included in the survey. The survey was stratified in order to produce estimates for different sub populations, for example between rural and urban households, with similar confidence intervals. The weights take this into account.

The use of fixed effects allows for unobserved time constant household characteristics, α_j, to be removed and hence controls for selection effects into mobile money use. This will account for time invariant unobservables but not for time varying unobservable characteristics e.g changing risk preference or changing technology preference which influence mobile money use and risk sharing capacity. The solution here is to instrument for mobile money use with something that can only influence consumption smoothing through the decision to use mobile money services. This will be covered in the Robustness section.

I estimate (4) using a panel data difference-in-difference specification. In the panel data case, difference-in-difference subtracts the average change in the control group (households in villages without mobile money) from the average change in the treatment group (users of mobile money or non-users in villages with mobile money), therefore removing biases from permanent differences between the two groups and changes due to a time trend.

To estimate equation (4) using a difference-in-difference specification requires the common trends assumption. This assumes that there are no differences in the trends of users and non-users, had the users not actually used mobile money i.e. there are no time varying variables that differentially affect the mobile money using and non-using households. An example of such a violation would be local prices and supply side effects. The counterfactual levels for the two groups can be different but the time trends must be the same so that in the absence of the use of mobile money the change in per capita consumption would have been the same for the two groups. I test this by running a placebo test (see Robustness section), examining if people who went on to adopt mobile money were already better able to smooth risk in the past. I find no effect of future mobile money use on risk sharing in the past and therefore cannot reject common trends.

The interaction term with the shock variable, $\boldsymbol{X_{jvt}} \cdot AggShock_{jvt}$, controls for any changes in observable household characteristics which might impact the household's ability to smooth shocks. It can be seen from Table 1 that many of the demographic variables changed over time including education, mobile phone ownership and loans which all increased across the three waves. These could help a household smooth shocks, for example by mobile phone ownership providing access to information about shocks which makes it easier for households to smooth shocks. Including a set of covariates and interactions of these covariates with the shock controls for any effects of these variables on consumption smoothing.

For the above specification to identify the impact of mobile money use on consumption smoothing following a shock, the interaction term $MM_{jvt} \cdot AggShock_{jvt}$ must also be uncorrelated with the error term ϵ_{jvt}. Since I show that aggregate shocks are exogenous (see table 12), this means that unobserved factors which cause a household to use mobile money cannot also help them smooth consumption following a shock. For equation (4) to identify the impact of being a spillover household, $AggShock_{jvt} \cdot MMV_{vt}$ must be uncorrelated with the error term. This means that unobserved factors which cause a household to not use mobile money itself but to be residing in a village with at least one mobile money user cannot also help them smooth consumption following a shock.

There are two self-selection effects with regards to mobile money which could violate the above conditions and bias my results. The first is self-selection by a household to use mobile money. Self-selection effects into using mobile money are absorbed into the coefficient μ on MM_{jvt}, which is not the focus of my analysis, as I am only interested in the effects of using mobile money after an exogenous aggregate shock. Looking at the average marginal effect from a logistic regression of household characteristics on mobile money use (Table 2), the table shows that being wealthier, owning a mobile phone, having more loans having and a bank account all increase the probability of a household using mobile money. A rural household is 6% less likely to use mobile money and a larger household size and older household head decrease the probability of using mobile money. I control for all these variables in all the regressions. I also control for static unobservable characteristics of households by using fixed effects. Time varying unobserved characteristics are addressed using a placebo test and a instrumental variable regression (see Robustness Section).

Secondly there is self-selection by mobile money agents into villages. If mobile money agents are more likely to select into villages with certain citizen characteristics, such as wealthier inhabitants, who's income is also less sensitive to shocks, this could confound my results. The roll-out of the agent network can shed some light on this. The majority of mobile money agents, especially early on in the launch of mobile money services, were existing sellers of airtime and sim cards. These small businesses already had links with the mobile operators and were spread throughout the country where mobile phone ownership was an already high 45% and cellular coverage was 75% of the population (Shkaratan, 2010). The only requirement for these existing sellers to become an agent was that the owner had a mobile phone.

Vodacom, the first and by far the largest mobile money operator in Tanzania, used aggregators to sign-up their existing airtime sellers as agents extremely quickly rather than dealing directly with thousands of outlets spread across the country (GSMA, 2010; International financial Coporation, 2010; USAID, 2013). This also allowed Vodacome to launch its mobile money services simultaneously nationwide instead of a regional roll-out. These aggregators provide liquidity to agents, allowing agents to be located in areas without bank access, and provide their initial training. Agents take a commission on the transactions and pay no fixed costs for being an agent, meaning that agents do not need a minimum number of mobile money users in their area to make the business viable. Since most agents operate out of an existing business selling airtime there is little movement of agents to, for example, wealthier locations, though there is a higher density of agents in wealthier and more populated locations such as cities.

According to the Finscope (2013), in 2013 74% of households were within 1 hour of a mobile money agent, varying between 94% in urban areas and 64% in rural areas. This shows just how quickly the mobile money network was set up and how good the coverage is, especially compared to an alternative financial services such as a MFI, which only 22% of the population were within 1 hours journey of in 2013.In the survey data used here only 8% of respondents reported lack of access to an agent as their reason for not using mobile money.

To examine whether mobile money agent presence is correlated with characteristics of the village they are located in I run a logistic regression of the presence of a mobile money agent within the village on average observed characteristics of the village inhabitants and the aggregate shock indicators (with each covariate a separate regression) and village fixed effects to control for non-time varying characteristics of the village. In Table 3, I show the average marginal effect of each covariate.

Of these mobile phone ownership and bank account ownership are significant at the 1% level,

Table 2: Correlations of mobile money use

	MM use=1 if household uses mobile money
Rural	-0.063***
	(0.012)
Wealthscore	0.003***
	(0.001)
Head education	0.001
	(0.001)
female head	0.020**
	(0.009)
Head age	-0.001***
	(0.000)
Household size	-0.004***
	(0.002)
Mobile phone ownership	0.180***
	(0.019)
Number of loans	0.044***
	(0.009)
Bank account	0.062***
	(0.009)
ROSCA	0.030*
	(0.017)
Observations	9,278

Average marginal effects from a logit regression of correlates with mobile money use. Errors are clustered at the village level and covariates are at the household level

Village clustered standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: Correlations of mobile money agent presence

	MM agent=1 if mobile money agent within that village
Rain shock 1sd	-0.011
	(0.067)
Rain shock self-reported	-0.039
	(0.212)
log per capita consumption	0.000
	(0.000)
Wealth	-0.007
	(0.027)
Head education	-0.019
	(0.027)
Head age	-0.001
	(0.001)
Mobile phone ownership	0.219***
	(0.018)
Number of loans	0.259
	(0.180)
Bank account	1.115***
	(0.164)
ROSCA	0.370
	(0.344)
Agricultural worker	-0.291
	(0.222)
Fishing	0.329
	(0.685)
Public sector	0.042
	(0.444)
Private sector	0.000
	(0.225)
Self-employed	-0.123
	(0.211)
F-test p value	0.147

Each coefficient is run as a separate fixed effect logit regression at the village level with village fixed effects to control for non-varying characteristics of the villages.

Standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

suggesting agents are locating in villages which show a tendency to adopt technology. Since mobile money use requires a mobile phone, it would be surprising if agent presence in a village was no correlated with mobile phone ownership. However the full set of covariates are jointly insignificant at the 15% level and other factors potentially correlated with the ability to smooth shocks such as wealth, credit access (loans) and education are not significant. Importantly, the rain shocks are both insignificant suggesting agents are not locating in areas which experience more or less aggregate shocks, supporting my use of mobile money use interacted with the rainfall shock as exogenous.

This gives an indication of whether contemporaneous characteristics of the village are correlated with agent presence but says nothing about the direction of the relationship or causation. It could equally be that mobile phone ownership increases if there is an agent in the village! I therefore also examine in the Robustness section whether the introduction of an agent in a village was predicted by the change of village characteristics and services the previous year. This provides causal evidence using the time series nature of my data to see whether agents are responding to changing village characteristics.

I assume aggregate shocks are exogenous, as is usual in this literature, a reasonable assumption since in self reported data shocks are unexpected large events and in the rainfall constructed data they are large unusual events one standard deviation away from the mean in absolute value. I check rainfall shocks are exogenous by regressing the shock measures on household characteristics (see Robustness section, Table 12) and find that these do not predict a rainfall shock (for example poorer villages are not in places which experience more rainfall shocks and aren't more likely to report a shock).

I also confirm that the rainfall shocks are in fact affecting most people in the village at once. To do this I look at the intra-class correlation, which measures the proportion of overall variance explained by within group variance, where the group was the village or enumeration area in Table 4). An intra-class correlation of 1 means the variable is the same for everyone in the class. An intra-class correlation of 0 means the variable is no more similar within the class than in different classes. Here the classes are the enumeration areas.

The intra-class correlation for the one standard deviation rainfall shock is 0.849 which is not surprising since rainfall was defined for an enumeration area. The fact that it is not one likely results from the clustering of city based enumeration areas together. The self-reported rainfall shock has an intra-class correlation of 0.13 showing some correlation within a village in terms of households reporting a rainfall shock. The fact this is not higher could be because rainfall shocks have different effects on households depending on their characteristics and plot characteristics, resulting in a house reporting a flood or drought when others in the village don't also or vice versa.

This also suggests the self-reported rainfall shock is less good as a measure of aggregate shocks and so the main results will be based on the 1 standard deviation rainfall shock definition, with the self-reported shock included for comparability with other research [‡].

Other possible aggregate shocks are also shown, but again the intra-class correlation is only around 0.1, suggesting these are also imperfect measures. Some examples of idiosyncratic shock intra-class correlations are also shown to illustrate that there is no or very little correlation within a village for these shocks.

Table 4: Intra-class correlation coefficients (ICC) for different shocks

	ICC
Aggregate shocks	
1 sd rain shock	0.849
Self reported rain shock	0.133
Large fall crop price	0.092
Large rise food price	0.087
Large rise agri input price	0.119
Idiosyncratic shocks	
Household business failure	0.054
Loss of salaried employment	0.002
Chronic/severe illness	0.012
Death of a household member	0.014
Fire in home	0.002

The intra-class correlation coefficient gives the proportion of the overall variance that is explained by within enumeration area variance.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

[‡] such as Jack and Suri (2014)

5 Results

This section begins by examining the impact of aggregate shocks on users of mobile money, non-users and non-users within villages with other mobile money users. I then look at heterogeneous effects by distance to the nearest agent, urban and rural households and droughts and floods. I finish by looking at remittances as the proposed mechanism through which mobile money allows shocks to be insured.

5.1 Main result

Table 5 shows the primary results of this paper. It shows regression results of the impact of aggregate shocks on consumption for mobile money users and non-users in villages with and without other mobile money user, as in equation (4). The first 2 columns show results using self reported droughts or floods, whereas the final two columns show results using the measure of a rainfall shock as a greater or less than 1 standard deviation difference from the mean.

Columns (1) and (3) are difference-in-difference regressions with household fixed effect and columns (2) and (4) also include interaction terms of all the control variables with the rainfall shock. MM spillover refers to a household residing in a village with mobile money users but who doesn't use mobile money itself. The total effect of the rainfall shock for someone with the mean value of all covariates (an average household) for mobile money using households, spillover households and households in villages without mobile money are reported underneath the regression results for easy comparability across regression specifications. For example, at the mean value for the covariates, the self reported rainfall shock will have a negative 7.4% impact on per capita consumption for non-mobile-money-users in villages with no other users significant at the 5% level.

The rainfall shock causes a drop in consumption of approximately 6% and is significant at at least the 10% level for households in villages without mobile money (line C) in all specifications. Hence large rainfall shocks have a strongly negative effect on consumption per capita, confirming prediction 1 that in the absence of any mobile money use by the household or village, rainfall shocks have a negative effect on consumption.

The coefficient on being a spillover household is not significant and is very small and precisely estimated. It therefore seems that non-mobile money users do not gain from having mobile money users residing in the same village as them. The coefficient on mobile money use is also small and insignificant in all the regressions. Therefore there is no increase in consumption for a village simply from using mobile money services, which could have been the case if for example using mobile money services allowed a new form of payments for a business or when selling agricultural goods, increasing sales and incomes. This is an interesting result of itself and conflicts with Munyegera

Table 5: Impact of rainfall shocks on consumption for mobile money users and non-users

Dependent variable: Log consumption per capita

	Self-reported shock		1 sd rainfall shock	
	(1)	(2)	(3)	(4)
Rain shock	-0.064**	-0.205	-0.068***	-0.040
	(0.029)	(0.164)	(0.020)	(0.144)
Mobile money spillover	0.002	0.003	0.005	-0.012
	(0.027)	(0.027)	(0.025)	(0.025)
Shock*MM spillover	-0.057	-0.072	0.005	-0.012
	(0.049)	(0.052)	(0.040)	(0.040)
Mobile money use	0.005	0.005	-0.003	-0.007
	(0.026)	(0.026)	(0.026)	(0.027)
Shock*MM use	0.088*	0.042	0.121***	0.139***
	(0.051)	(0.058)	(0.041)	(0.045)
Observations	9,281	9,281	9,281	9,281
Number of households	3,807	3,807	3,807	3,807
R-squared	0.194	0.198	0.196	0.202
(A) Negative Shock MM user	0.024	0.007	0.053	0.059*
	(0.044)	(0.045)	(0.036)	(0.033)
(B) Negative Shock spillover	-0.121***	-0.126***	-0.063*	-0.043
	(0.041)	(0.040)	(0.033)	(0.034)
(C) Negative Shock Non MM village	-0.064**	-0.074**	-0.068***	-0.042*
	(0.029)	(0.032)	(0.020)	(0.022)
F stat (A)=(C)	3.00*	0.53	8.80***	9.77***
F stat (A)=(B)	6.31**	3.95**	5.55**	9.36***
F stat (B)=(C)	1.37	1.90	0.01	0.09
Interactions with shock		YES		YES

Regressions include full set of household control variables from Table 1, errors clustered at the village level and control for village characteristics which could affect the ease of sending remittances (distance to the nearest main road, distance to nearest population centre and distance to nearest market). Mobile money spillover refers to a household in a village where others use mobile money but who doesn't use themselves. Mobile money use is a dummy variable equal to one if that household uses mobile money. Interactions are the control variables from table 1 interacted with the shock. Village clustered standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

and Matsumoto (2014) but agrees with Batista and Vicente (2016) who find no impact of mobile money services on consumption.

Turning to the interactions with the shock dummy; when a rainfall shock occurs, being a spillover household is insignificant but has a large negative coefficient for the self-reported shock. Row B below the main results shows that spillover households experience a large fall in consumption of as much as 12% after an aggregate shock. However, the F-statistics comparing row B to C show that I cannot reject that the impact of the shock is the same (negative impact) as that of households in a village without mobile money . Hence households do not benefit from having other people using mobile money in the village when a rainfall shock occurs. This invalidates predicted 3.

In contrast, the household mobile money use interaction with the aggregate shock is 9%-14% and significant in three out of four regressions. When a rainfall shock occurs the household using mobile money no longer experiences a drop in consumption and may even get a slight increase in consumption, as seen from the coefficients at the bottom of the table in row A being positive in all cases for both shock definitions. The F-test comparing A to B and C below confirms in 7 out of 8 cases that the shock impact for mobile money users is significantly different from non-users, both in their own village and other villages. This confirms prediction 2; mobile money users are able to smooth consumption when a rainfall shock occurs.

Overall this suggests that when a negative aggregate shock occurs users keep any increase in remittances for themselves and do not any detectable amount to the rest of the village to help others smooth the shock. There are many potential explanations for this result, such as changing risk sharing networks within the village, hidden income or lack of norms to share after an aggregate shock. For example, if mobile money users are choosing to insurance themselves with a migrant in another location, due to a lower covariance of shocks, they may no longer participate in risk sharing within the village. Alternatively they may still participate in the village risk sharing network but either hide remittances after an aggregate shock or not see this income as part of the norm for sharing. I explore these in detail in the Discussion section, but understanding how mobile money services are affecting traditional risk sharing relationships is vital to understand if there are winners and losers from this new technology.

A comparison of the self-reported shock definition and actual rainfall shock definition shows that both result in an approximate 6% fall in consumption. The coeffient on the shock interaction with mobile money use is smaller and only significant in one of the specifications though. Since the self-reported shock is not highly covariate at the village level (as shown in Table 4) this could be due to it reflecting a shock that does not have a sufficient aggregate impact and that can be partially smoothed within the village. This is in contrast to the 1 sd rainfall shock which impacts consumption of the entire village at once. This suggests that the 1 sd rainfall shock is a superior shock to use as an aggregate shock and so only this rainfall shock definition will be used from here onwards where possible. Since the coefficients are very similar between specifications (3) and (4), only specification (3) will be used when reporting further results, though all regressions have also been run using specification (4) without any differences in results.

5.1.1 Heterogeneous effects

I examine a number of different effects to understand which factors might be driving the main result. These are reported in Table 6. I examine whether the results depend on the distance to the nearest mobile money agent, whether the effects differ by rural and urban households and lastly whether there are differential effects of rainfall shocks defined as a drought or flood. For all these regressions I only consider the 1 standard deviation rainfall shock measure.

The distance to the nearest mobile money agent could also impact how easy it is for someone to send and receive remittances via mobile money and hence the benefit they receive from using this service. I therefore run specification (4) with interactions with dummy variables for whether an agent is within 1km of the village, between 1km and 5km, between 5km and 10km away, with agents more than 10km away as the exclusion category. This will also indicate whether the distance to the nearest mobile money agent changes the pattern of sharing remittances within the village. For example, it might be easier to hide remittances the further away the mobile money agent is from the village.

The results for distance dummies interacted with each variable are reported in columns (1) and (2) of table 6. I find that the coefficients of interest are only significant if an agent is within 1km of the village and so for brevity I only report these coefficients and coefficients if the agent is greater than 1km away.

Looking at column (1), when there is an agent within 1 km: Households using mobile money themselves benefit by 12% of per capita consumption when a rainfall shock occurs, more than cancelling out the negative impact of the shock of -6%. If an agent is further than 1km there is no benefit to mobile money for either users or non-users. The F tests at the bottom of the table show that I can reject equality of the shock impact for mobile money users and non-users in villages without mobile money (group A compared to group C) if an agent is within 1km of the mobile money users only. This suggests there are only benefits on mobile money for risk sharing if the user can easily access their mobile money account and make withdrawals, for which a nearby agent is required.

Table 6: Heterogeneous effects of the impact of rainfall shocks on consumption for mobile money users and non-users

	Dependent variable: Log consumption per capita					
	agent<1km	agent>1km	rural	urban	drought	flood
	(1)	(2)	(3)	(4)	(5)	(6)
Rain shock	-0.063***	-0.063***	-0.048**	-0.111***	-0.078***	-0.014
	(0.019)	(0.019)	(0.023)	(0.033)	(0.022)	(0.033)
MM spillover	0.004	-0.018	0.028	-0.076**	-0.009	0.006
	(0.030)	(0.033)	(0.037)	(0.033)	(0.024)	(0.026)
Shock*MM spillover	0.031	-0.045	-0.022	0.058	0.000	-0.051
	(0.049)	(0.045)	(0.056)	(0.047)	(0.053)	(0.048)
Mobile money use	-0.011	-0.022	-0.017	-0.047	0.017	0.007
	(0.029)	(0.041)	(0.039)	(0.041)	(0.025)	(0.026)
Shock*MM use	0.125***	0.034	0.161**	0.120**	0.120*	0.053
	(0.046)	(0.077)	(0.066)	(0.052)	(0.065)	(0.049)
Observations	9,281	9,281	6,518	2,763	9,281	9,281
Number of idn	3,807	3,807	2,632	1,175	3,807	3,807
(A) Shock MM user	0.062	-0.029	0.113*	0.009	0.042	0.039
	(0.042)	(0.077)	(0.062)	(0.042)	(0.063)	(0.036)
(B) Shock spillover	-0.032	-0.108**	-0.070**	-0.052	-0.078	-0.065
	(0.043)	(0.043)	(0.049)	(0.033)	(0.049)	(0.037)
(C) Shock Non MM village	-0.063***	-0.063***	-0.048**	-0.111***	-0.078***	-0.014
	(0.019)	(0.019)	(0.023)	(0.032)	(0.022)	(0.033)
F test (A)=(C)	7.37***	0.19	5.95**	5.26**	3.42*	1.16
F test (A)=(B)	2.32*	0.92	5.25**	1.29	2.19	3.94**
F test (B)=(C)	0.39	1.03	0.16	1.54	0.00	1.13

Each column refers to a household fixed effects regression run when that condition is true. Rain shock is the 1 standard deviation rainfall shock dummy. Regressions include full set of household control variables from Table 1, errors clustered at the village level and control for village characteristics which could affect the ease of sending remittances (distance to the nearest main road, distance to nearest population centre and distance to nearest market). MM spillover refers to a household in a village where others use mobile money but who doesn't use themselves. Mobile money use is a dummy variable equal to one if that household uses mobile money. F tests compare whether the shock effect is equal for the three comparison groups. Village clustered standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

Next I look at whether the results vary by whether the households resides in a rural or urban area by running the results separately for urban and rural households to see if there are differential effects for these groups. Mobile money services would be expected to benefit rural households more since they have less access to other ways to send remittances such as banks or designated money transfer shops (such as Weston Union) and are less likely to have friends or relative passing by regularly who could bring remittances. They are also more reliant on agriculture and so affected by rainfall shocks more directly in terms of crop losses than households in urban areas.

Separate urban and rural results are shown in columns (3) and (4) of Table 6. From the results it can be seen that rainfall shocks have negative effects in both rural and urban areas. You benefit from using mobile money when there is a rain shock in both rural and urban areas. In both these specifications I reject equality of the shock for mobile money users and non-users in villages with no mobile money users. These results suggest that both rural and urban households benefit from mobile money, possibly because they are engaging in a reciprocal relationship to share risk across urban and rural spaces, as the remittance data suggests, and because even in urban areas some households often undertake income generating activities reliant on rainfall[§].

To see whether too much or too little rainfall have differential effects on consumption and the ability of mobile money to smooth these impacts, I separate out the effects of droughts compared to floods. A drought is defined as the difference in rainfall from the mean being more than one standard deviation below the mean and a flood as the difference in rainfall from the mean being more than one standard deviation above the mean. This is reported in Table 6, where droughts are reported in column (5) and floods in column (6).

It can be seen that it is only droughts which have a significant negative effect of 8% of per capita consumption. Floods have no significantly different effect from zero. Mobile money use is significant at the 10% level when there is a drought and the F test shows that mobile money using households are protected against the effects of a drought compared to households without mobile money users in their village. The lack of negative impact of floods might also indicate the presence of non-linearities in the impact of rainfall, with too much rain initially increasing crop yields, whereas too little rain always has a negative impact on crop yields.

5.1.2 Other shocks

Finally I look at whether the results found are particular to floods and droughts or also extend to other aggregate shocks. The theoretical predictions I made based on equation (4) apply to any aggregate shock, not only rainfall shocks, and so I check that my results generalise by running this

[§]11% of households in urban areas report the main income of the household head to come from agriculture or livestock compared to 89% in rural areas

Table 7: The impact of other aggregate shocks on consumption

	Dependant variable: Log consumption per capita		
	(1)	(2)	(3)
	Large fall crop price	Large rise food price	Large rise agri input price
Shock	0.074***	0.035**	0.072***
	(0.028)	(0.017)	(0.26)
MM spillover	0.003	0.033	0.010
	(0.024)	(0.030)	(0.025)
Shock*MM spillover	-0.067	-0.092**	-0.164***
	(0.063)	(0.038)	(0.046)
Mobile money use	0.016	0.025	0.013
	(0.026)	(0.027)	(0.026)
Shock*MM use	0.030	-0.014	0.058
	(0.056)	(0.035)	(0.052)
Observations	8,475	8,475	8,475
R-squared	0.178	0.175	0.178
Number of households	3,803	3,803	3,803
Mean of Shock	0.123	0.423	0.121
(A) Shock MM user	0.104**	0.022	0.129***
	(0.047)	(0.030)	(0.047)
(B) Shock spillover	0.007	-0.057*	-0.092**
	(0.056)	(0.035)	(0.057)
(C) Shock Non MM village	0.074***	0.35**	0.071**
	(0.028)	(0.019)	(0.025)
F stat (A)=(C)	0.28	0.16	1.23*
F stat (A)=(B)	2.08**	2.77*	14.68***
F stat (B)=(C)	1.14	5.93**	12.68***

All regressions include full set of household control variables from Table 1, household fixed effects and errors clustered at the village level. All regressions also control for village characteristics which could affect the ease of sending remittances. These are the distance to the nearest main road, distance to nearest population centre and distance to nearest market. Village MM use refers to the proportion of households in the village using mobile money. Mobile money use is a dummy variable equal to one if that household uses mobile money.

Village clustered standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

specification again with different aggregate shock measures. These results are reported in Table 7. I have self-reported data on three other potential aggregate shocks: Large fall in crop prices, large rise in food prices and large rise in agricultural input prices. It should be noted that from the intra-class correlations, which are on average around 0.1, these shocks do not seem to meet the definition of a completely aggregate shock at the village level. However they might affect entire areas more widely to conceivably affect a large part of a risk sharing network at once.

None of the three shocks have a negative effect on per capita consumption, and in fact they all have significant and positive effects. The fact that 42% of the sample report experiencing a large rise in food prices might suggest that this would be a good definition of an aggregate shock. However since the shock has no negative effect it might be that households, particularly agricultural households, do not suffer from food prices rising and actually benefit from being able to sell their produce for more. For two of the shocks being a spillover household makes the household worse off than if there hadn't been any mobile money users in the village.

Overall then,these results suggest it might be rainfall shocks which households currently have the most difficulty smoothing themselves since only these seem to negatively impact consumption in the first place.

5.2 Mechanisms

The proposed mechanism tested in this paper is that mobile money allows remittances to be sent by friends and family in other locations in response to a rainfall shock at the village level and that this allows consumption smoothing. However, it is possible that mobile money affects consumption smoothing in other ways. One possible alternative is that mobile money allows funds to be safely stored on a mobile phone as savings which can be run down in response to a shock. A second is that households might be considered more creditworthy if they use mobile money and are able to borrow more when an adverse event happens.

In the survey data used here, over 80% of respondents said they send and receive money as the main reason for using mobile money, with a very equal spilt between sending and receiving for most respondents. 20% said they had ever used mobile money for savings but only 5% said this was the main reason they use mobile money. In the third round of the survey, questions were asked on who sent remittances, by what channel, from where and what their relation was to the recipient. 40% of remittances were sent by a son or daughter, 35% were sent via mobile money and 30% came form Dar es Salaam, the capital city. This suggests a story of families sharing risk with a migrant in the city via mobile money is a plausible one.

In order to test whether remittances are driving the way that mobile money protects against adverse shocks I use the data available on remittances in the third round of the survey to run the following specification:

$$\begin{aligned} r_{jv} = &C_{jv} + \gamma_a AggShock_{jv} + \mu MM_{jv} \\ &+ \beta_m MM_{jv} \cdot AggShock_{jv} + \boldsymbol{\theta X_{jv}} + \varepsilon_{jv} \end{aligned} \tag{5}$$

where r_{jv} is whether a household received any remittances, and if they did receive any remittances the amount received by a household, and the other variables are as defined previously. Log consumption per capita, C_{jv}, is included to control for income effects. Unfortunately, data on remittance amounts is only available for the final wave of the panel and so this specification can only be run as an OLS regression for one period. It still gives an indication though whether remittances are responding to negative shocks. If remittances are the channel through which mobile money smooths aggregate shocks then the following prediction will hold:

Prediction 4 $\beta_m > 0$

so that remittances increase for mobile money users in response to an aggregate shock.

Table 8 shows the OLS regressions of a dummy variable equal to one if any remittances were received (using any method of sending remittances) and, if any remittances were received, the amount received in Tanzanian Shillings, in wave 3. Mobile money use results in the households being 15% more likely to receive remittances, as can be seen in column (1). This shows how mobile money has increased the probability of receiving remittances compared to other forms of receiving remittance such as using friends and neighbours to transport money physically [¶]. A rain shock does not increase the probability of receiving remittances.

Looking at column (2), when there is a rainfall shock the value of remittances received increases but only for users of mobile money. When there is a rainfall shock, mobile money using households receive 100,000 shillings more ($40), approximately 10% of an average household's per capita income in wave 3 and of a similar magnitude to the fall in per capita consumption found in Table 5. This strongly supports the mechanism that mobile money users are able to smooth rainfall shocks since they receive more remittances and confirms prediction 4. The point estimates for the rain shock is negative but insignificant and for mobile money users negative and significant at the 10% level. The negative coefficient on the rain shock could possibly be explained by the difficulty of sending remittances using traditional channels, such as physically with friends, when there is a rain shock and roads are flooded. Mobile money users may receive smaller values of remittances for the very reason that remittances can be sent so much more easily with mobile money, and so more frequent (as seen in column (1)) and smaller value remittances can be sent rather than large one-off amounts.

[¶]35% of households send remittances via friends or family and 35% via mobile money

Overall, the results on remittances are supportive of the argument that mobile money allows the smoothing of rainfall shocks through remittance flows.

Table 8: OLS regression of remittances received after an aggregate shock

	(1)	(2)
	received remittances	value of remittances received
Rain shock	0.012	-24,254
	(0.024)	(29,878)
4MM use	0.144***	-49,967*
	(0.020)	(28,369)
Rain shock*MM use	-0.010	101,125**
	(0.030)	(42,005)
Observations	3,486	769
R-squared	0.131	0.24

Full set of control variables as in Table 1 and errors clustered at the village level. All regressions also control for village characteristics which could affect the ease of sending remittances. These are the distance to the nearest main road, distance to nearest population centre and distance to nearest market. MM use is a dummy variable equal to one if the household used mobile money in a given year. The values for amount of remittances received have been winsorized at 1% and 99%.

Village clustered standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

5.3 Robustness

I run a number of difference tests to confirm the validity of my findings. First I run a placebo regression using two periods of data before the introduction of mobile money to check that people who went on to use mobile money, and areas which gained agents, were not already different from places with no users and agents before mobile money was introduced. Then I run an IV regression to further control for any potential self-selection into mobile money use. These tests confirm my earlier results and support their implications.

5.3.1 Placebo test

The placebo test allows me to check that future mobile money exposure does not predict past changes in consumption and the ability of households to smooth shocks, thus confirming the common trends assumption required for a difference-in-difference specification to be valid. The placebo test can confirm whether there is something different about households and villages which will use mobile money in the future and if this difference allows households to better smooth shocks. For example, a location is more industrialised, mobile money agents are more likely to locate in industrial areas, households are more likely to use mobile money and households working in industry are better able to smooth rain shocks. This could create a spurious correlation between mobile money use and smoothing shocks, whereas actually the industrialisation is allowing shocks to have less impact on consumption. If this case were true then the placebo test would show a positive effect of future mobile money use for smoothing rainfall shocks in the past.

I run a placebo test using the 2007 Tanzanian Household Budget survey (HBS) combined with the NPS 2008-9 wave 1 to construct two rounds of data prior to the introduction of mobile money services. A subsample of the 2007 HBS was re-sampled in the NPS, so by combining this sample with the first wave of the NPS I created a panel of 1,200 households in 191 villages covering 2 periods before the introduction of mobile money, which I call wave 0 (2007) and wave 1 (2008-9). I created a dummy variable for whether the household ever uses mobile money after it's introduction in 2009 and whether the household is ever a spillover household (lives in a village with other users but doesn't use themselves) and I use this to estimate the following equation:

$$\begin{aligned} C_{jit} = &\gamma_a AggShock_{jvt} + \mu MME_{jvt} + \beta_m MME_{jvt} \cdot AggShock_{jvt} \\ &+ \mu MMSE_{jvt} + \beta_m MMSE_{jvt} \cdot AggShock_{jvt} \\ &+ \boldsymbol{\theta X_{jvt}} + \boldsymbol{\psi X_{jvt}} \cdot AggShock_{jvt} + \alpha_j + \varepsilon_{jvt} \end{aligned} \tag{6}$$

where MME_{jvt} is a dummy variable equal to zero in wave 0 and one in wave 1 if the household uses mobile money in the future and $MMSE_{jvt}$ is a dummy variable equal to one in wave 1 if the household lives in a village with other users but doesn't use itself in the future.

If the common trends assumption holds then the dummy for future mobile money use interacted with the shock will not be significant for the past data, confirming that it is not unobservable characteristics of households which choose to use mobile money that is driving my results. Likewise, the dummy for being a spillover household interacted with the shock will not be significant for the past data. A finding of no significance on these coefficients suggests that the households and villages where mobile money are used were similar in their ability to smooth shocks to the household and villages where no-one uses mobile money before the introduction of mobile money services, which is crucial for the validity of my results.

The HBS includes different compositions of goods in the measure of non-food consumption (for example in the inclusion and depreciation of durable assets) and hence is not directly comparable to the expenditure measure used in the NPS. I therefore look only at food consumption per capita when I estimate (6). Food consumption is by far the largest share of overall consumption, with on average in the main sample food consumption being 73% of total consumption. To show that any lack of significance isn't just because food consumption is better smoothed than non-food consumption after a shock, I also run my main specification (4) on the full data sample using log food consumption per capita instead of total consumption. These are recorded alongside the placebo test for easy comparability.

I constructed the same measures of control variables to match those used for in the main analysis such as household head education, household size, financial use and occupation dummies. The rainfall shock variable used here is a self-reported drought or flood. This information was asked in the NPS for the last 5 years so I was able to create a shock dummy for the 2007 data.

The results in Table 9 show the placebo test in column (1) alongside the equivalent regressions of actual mobile money use on log food consumption per capita in column (2). First checking that the main results carry for food consumption per capita; I find that food consumption reacts to rainfall shocks in column (2), falling by 7%. Using mobile money allows you to smooth the rainfall shock impact. Turning to the placebo result in column (1), there is no differential effect for households which use mobile money in the future on their consumption or ability to respond to a shock in the past. This lends support to the common trend assumption necessary for using a difference-in-difference specification and suggests the finding of a significant relationship between the ability to smooth shocks and mobile money is due to the use of mobile money and not another variable correlated with future mobile money use.

Likewise, being in a village with other mobile money users in the future does not predict your consumption or ability to smooth consumption after a shock in the past. This evidence suggests there does not seem to be something correlated with having mobile money users in the village in the future which also allows you to smooth shocks better.

I also test whether agents choose to locate in areas with certain characteristics. To do this I use the wave 0 data to examine whether changes in the average characteristics of households in a village or village services changes predict mobile money agent presence in a future period. The time series nature of the data therefore allows me to see whether the roll-out of agents was effectively random or could be causing spurious results by being correlated with village characteristics which facilitate risk sharing. In particular, this allows me to test if other aggregate shocks, which appear as changes in total village consumption, influence agent location choice. These results are shown in Table 10

I find no effect of changes in any of the average characteristics of the households or the village services on either agent presence in a village or within 5km. Only one service is significant, with places that gained a primary school less likely to get a mobile money agent, but this could be due

Table 9: Placebo test of future mobile money use 2007-2009

Dependent variable: Log food consumption per capita		
	Placebo pre-mobile money data	Actual mobile money use data
	(1)	(2)
	FE	FE
Rain shock	-0.046	-0.070**
	(0.096)	(0.030)
MM spillover	-0.038	-0.020
	(0.060)	(0.032)
Shock*MM spillover	0.032	0.102*
	(0.149)	(0.053)
MM use	0.004	-0.016
	(0.056)	(0.031)
Rain shock*MM use	-0.025	0.139**
	(0.169)	(0.057)
Observations	2,186	9,079
Number of households	1,204	3,758
R-squared	0.224	0.122

All regression include a full set of control variable from Table 1, time dummies and clustered errors at the village level. The shock variable here is a self-reported drought or flood dummy. Regression (1) runs specification (6) on 2007 and 2008-9 data with a dummy variable MM use if a household ever used mobile money after 2009 and regression (2) runs specification (4) on the three waves of NPS data with a dummy variable MM use if the household uses mobile money in that year. MM spillover refers in the placebo specification (1) to a household who will live in a village with at least one mobile money user in the future but never uses mobile money itself and in specification (2) as a household in a village where others use mobile money but who doesn't use themselves in that time period.

Village clustered standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

to chance considering the number of variables tested. Jointly, all the variables are not significant at predicting whether an agent decides to set-up in that villages, as shown by the very small F statistic. In particular, the coefficient on average village consumption changes and on rainfall shocks are both insignificant meaning that agents are not choosing to locate in areas that have been more or less exposed to aggregate shocks, or where consumption has grown more or less in the past.

5.3.2 IV regressions

If mobile money use is endogenous to household's ability to smooth consumption after a shock then my results will be biased. I can use instrumental variable regression to control for this. Given there are two potentially endogenous variables, mobile money use and the shock interaction with mobile money use, I need two instruments. An instrument would need to be correlated with mobile money use (relevant) but not affect consumption smoothing independently (exogenous).

An instrument that has also been used in the literature is distance to the nearest mobile money agent. Distance to the nearest agent is correlated with mobile money use since it is easier to use mobile money if an agent is nearby. And it shouldn't be correlated with unobserved determinants of the ability of households in a village to smooth risk. In addition to the distance to the nearest agent, which is only present in the case there is no agent within the village, I also have a dummy indicating whether there is a mobile money agent in the village or not. Together these two variables define access to a mobile money agent, since it is only when there is no agent in the village that the distance to the nearest agent matters. I therefore instrument with the distance to and presence of a mobile money agent and the interactions of distance to and presence of a mobile money agent with the rainfall shock.

The instrumental variable results in Table 11 confirm that the rainfall shock has a negative effect on consumption per capita of 12%. None of the other coefficients are significant though the point estimate on the mobile money shock interaction is positive and large and significant at a 13% significance level.

I report a number of different tests beneath the regression results to confirm the validity of my instruments. Firstly, the F-statistics on the first stage regressions for mobile money use and the interaction with the shock are moderately large, showing that the instruments do explain variation in these variables and hence supporting their use as valid instruments. The Cragg-Donald Wald F statistic is used to test the strength of more than one excluded instruments. For the IV regressions to have less than 5% of the bias of OLS the critical value for the F-statistic is 13.43. My Cragg-Donald Wald F statistic exceeds this critical value, again supporting the validity of my instruments.

The underidentification test tests the null hypothesis that the equation is underidentified. The

Table 10: Agent location choice

	(1) Agent in village	(2) Agent within 5km
Consumption	-0.005	-0.078
	(0.089)	(0.087)
Rainfall shock	-0.016	-0.120
	(0.263)	(0.257)
Distance to road	0.001	0.001
	(0.001)	(0.001)
Head age	-0.004	-0.010
	(0.010)	(0.010)
Head Education (yrs)	0.033	-0.001
	(0.035)	(0.034)
Household size	0.040	0.037
	(0.033)	(0.032)
Mobile phone	-0.013	0.029
	(0.182)	(0.178)
Loan	-0.017	-0.064
	(0.221)	(0.216)
ROSCA	-0.215	-0.260
	(0.220)	(0.215)
Occupations		
Agricultural	-0.234	-0.262
	(0.186)	(0.182)
Fishing	-0.272	-0.325
	(0.715)	(0.700)
Public sector	-0.130	0.057
	(0.774)	(0.757)
Private sector	-0.248	-0.073
	(0.284)	(0.279)
NGO/religious	0.422	0.721
	(1.014)	(0.993)
Self-employed	0.296	0.224
	(0.183)	(0.180)
Village services		
Bank	0.085	0.105
	(0.131)	(0.128)
Health centre	0.075	0.023
	(0.063)	(0.062)
Hospital -0.052	-0.081	
	(0.097)	(0.094)
Primary school	-0.207**	-0.278***
	(0.095)	(0.093)
Secondary school	0.041	0.028
	(0.068)	(0.069)
Police station	0.109	0.045
	(0.087)	(0.085)
Post office	-0.179	-0.149
	(0.126)	(0.123)
Observations	382	382
F statistic	0.75	0.71

In columns (1) and (2) each coefficient is run as a separate fixed effect regression at the village level with village fixed effects to control for non-varying characteristics of the villages
Standard errors in brackets, *** p<0.01, ** p<0.05, * p<0.1

null is rejected at the 1% level. The Sargan-Hansen test is an over-identification test which determines if the instruments are exogenous. The null hypothesis is that the instruments are uncorrelated with the error term. Since my errors are clustered, the Hansen J statistic is reported. I cannot reject the null hypothesis that all my instruments are valid.

Overall, my IV results cannot reject my main findings and provide support for my results by finding effects of a similar magnitude and sign to my main results, even though they are not quite significant.

5.3.3 Exogeneity of rainfall shock

I confirm the exogeneity of the rainfall shock by showing that the rainfall shock is not correlated with any characteristics of the households. Separate fixed effect regressions for different household characteristics for each of the rainfall shocks are shown in Table 12. For the self reported rainfall shocks in column (1), agricultural households are slightly more likely to report a drought or flood, perhaps due to the importance of rainfall for their livelihood, as are larger households.

In column (2), the 1 standard deviation rainfall shock, places with larger households and less ROSCA membership are correlated with more rainfall shocks, though this is no more than would be expected by chance. This suggest my 1 standard deviation rainfall shock is not correlated with characteristics of the households, confirming its use as an exogenous aggregate shock.

Table 11: IV results

Dependent variable: Log consumption per capita	
	(1)
	IV FE
Rain shock	-0.120**
	(0.058)
MM spillover	-0.037
	(0.111)
Rain shock*MM spillover	0.057
	(0.072)
Mobile money use	-0.184
	(0.295)
Rain shock*MM use	0.297
	(0.198)
Observations	4,448
Number of idn	2,224
F-stat on excluded instruments for MM use	19.7
F-stat on excluded instruments for Rain shock*MM use	27.9
Cragg-Donald Wald F statistic	25.1
Underidentification test χ^2 p-value	0.00
Sargan-Hansen test χ^2 p-value	0.43

Instruments were distance to nearest mobile money agent and a dummy indicating a mobile money agent within the village, and their interactions with the shock variable. All regressions include full set of household control variables from Table 1, household fixed effects and errors clustered at the village level. All regressions also control for village characteristics which could affect the ease of sending remittances. These are the distance to the nearest main road, distance to nearest population centre and distance to nearest market. MM spillover refers to a household in a village where others use mobile money but who doesn't use themselves. Mobile money use is a dummy variable equal to one if that household uses mobile money. Rainfall shock is the 1 standard deviation rainfall shock.

Village clustered standard errors in brackets, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 12: Shock correlations

	(1) Self reported drought/flood	(2) 1sd rain shock
log per capita consumption	-0.016	0.001
	(0.023)	(0.006)
Wealthscore	0.018*	0.025
	(0.008)	(0.023)
Head education	0.006	0.010
	(0.009)	(0.023)
Head age	-0.001	-0.005
	(0.001)	(0.005)
Household size	0.017***	0.026**
	(0.004)	(0.012)
Mobile phone	0.003	0.077
	(0.029)	(0.126)
Loan	0.010	-0.164
	(0.028)	(0.161)
Bank account	0.060	0.103
	(0.048)	(0.125)
ROSCA	0.077	-1.071***
	(0.061)	(0.308)
Agriculture	0.071**	-0.041
	(0.029)	(0.197)
Fishing	0.060	-0.155
	(0.124)	(0.450)
Government	-0.075	0.328
	(0.105)	(0.326)
Private sector	0.026	0.087
	(0.066)	(0.193)
Self employed	-0.097*	0.014
	(0.050)	(0.172)
Family work	-0.181	0.446
	(0.120)	(0.424)
Unemployed	0.091	-0.497
	(0.246)	(0.911)
Observations	2,644	819

Each row is the marginal effect from a separate fixed effect logit regressions. Column (1) has household fixed effects and village clustered errors and is at the household level since each household reports whether it experienced a shock or not. Column (2) has village fixed effects and is at the village level for village averages of the household characteristics, since the one standard deviation rainfall shock is constant within a village.

*** p<0.01, ** p<0.05, * p<0.1

5.4 Discussion

Exploring why only mobile money users can smooth aggregate shocks would advance our understanding of how informal risk sharing arrangements operate in developing countries and how they are changing in an increasingly connected world. For example, are mobile money using households choosing to hide remittances or are they opting out entirely from the risk sharing network in the village? Looking at transfers within the village and from a migrant in response to shocks would allow these effects to be separated. My data was not detailed enough on specific network partners and remittance flows to answer this question, but the answer is important for understanding how new technologies change traditional risk sharing patterns and for a deeper understanding of how risk sharing networks are sustained.

There are many potential explanations for why remittances might not be shared after an aggregate shock. Three main ones discussed here are hidden income, mobile money using households no longer participating in risk sharing networks within the village and the temporary breakdown of risk sharing after an aggregate shock.

Coate and Ravallion (1993) looked at risk sharing as a repeated game and determined the conditions under which an informal risk sharing network is self-sustaining. To be self-sustaining, at every point in time the benefits from remaining in the relationship must outweigh the benefit of defection. These conditions include the ability to punish deviations from risk sharing which requires information on network member's income. In regards to mobile money, information on the amount of money received may be hard for other villagers to obtain, particularly if the mobile money agent is outside the village itself. Mobile money may therefore make it easier to hide money from the others in the village.

Without accurate information on if remittances have been received and the amount, it is very hard for the rest of the villagers to ask for more transfers when a shock has occurred or to punish the mobile-money-using household for not sharing remittances. Differences in the impact of an aggregate shock on each individual household also creates uncertainty for households to know the extend to which other households' consumption falls after a village level shock. Again this means other households in the village cannot be sure that a mobile money using household is smoothing its consumption due to remittances received or because it simply suffered less from the aggregate shock by chance.

If mobile money does allow households to hide remittance income then this would reduce the amount of risk sharing that can take place with others in the village or lead to a breakdown of risk sharing entirely. It could also widen disparities between well connected households with a migrant and those without social ties beyond the village (Chandrasekhar et al., 2011).

This leads to a second reason for not seeing sharing of remittances after an aggregate shock is that the benefit from defection from the village risk sharing network might increase for a mobile-money-using household, because they can then keep any remittances received for themselves at every point in time. They could insure both aggregate and idiosyncratic shocks through family migrants in other locations, and the benefits of this risk sharing relationship might exceed those of the village risk sharing relationship as a cross-locational network allows for a lower correlation in income shocks. In this case, mobile money using households might no longer participate at all in village risk sharing.

The migrant might also only send enough to allow that one household to smooth consumption, not enough to share with others in the village, and could give restrictions on how the money should be used. Assuming the migrant is a close family member (the data shows 91% of remittances are sent by a relative), the migrant would be expected to know the consumption of the receiving household and the average potential shortfall due to an aggregate shock. This fits in with a literature showing migrants have a strong preference for more control over how their remittances are spent and will incur costs to monitor how their remittances are spent (Ashraf et al., 2011).

Whether mobile money using households still participate in the village risk sharing network is testable by looking at the significance of village by time dummies in a regression of changes in consumption, as shown by (Kinnan, 2014):

$$C_{jvt} = \gamma_i IdioShock_{jvt} + \boldsymbol{\theta X_{jvt}} + \delta_{vt} + \alpha_j + \varepsilon_{jvt} \tag{7}$$

where $Idioshock_{jvt}$ is an idiosyncratic shock experiences by household j in village v at time t, $\boldsymbol{X_{jvt}}$ is a vector of controls consisting of household demographics, financial service use and occupation dummies to control for other variables which allow households to smooth consumption, δ_{vt} are village-time dummies to control for village level changes, including variation of aggregate village consumption as in Ravallion and Chaudhuri (1997), α_j is a household fixed effect picking up fixed characteristics of the household (including the Pareto weight) and ε_{jvt} is a time varying error. The results of this specification for different idiosyncratic shocks are shown in the appendix, but the F-statistics on the village-time dummies are always jointly significant even if the sample is confined to mobile money using households. This means that villages are still providing some insurance to mobile money users and so they are not leaving the village risk sharing network entirely, even if their network extends to other locations.

A third potential reason why remittances might not be shared after an aggregate shock is that norms for sharing in the village don't extend to aggregate shock. Norms develop to address situations which an individual cannot solve alone but that the village as a whole can overcome. Traditionally, the norm has been that only individual shocks are shared by the village. Aggregate shocks were not shared because they impacted the entire village at once and so could not be smoothed. The introduction of mobile money has enabled households to have links with other households in other locations and hence for some households in a village to smooth aggregate shocks. However the social norm might still required households to only share income for idiosyncratic shocks, not aggregate, explaining why I don't see sharing in the data.

Coate and Ravallion (1993) also showed that after a big event like a famine in which incomes fall substantially, the discount factor required for a household to remain in a risk sharing arrangement goes up as the income of the others goes down. This results in the break down of informal risk sharing relationships if the contribution of a relative fortunate household is insufficient to keep the other households from starvation. It is possible that a similar situation occurs after an aggregate shocks, with some households experiencing larger income falls than others and some households getting a gain in income through remittances and risk sharing breaking down temporarily.

While all of these explanations are possible, it would be interesting future work to examine them in detail and determine which are the strongest effects. This links into the question of determining how, if at all, improved access to remittances changes traditional risk sharing networks in villages, for better and for worse. Particularly if mobile money using households are choosing to increasingly opt-out the village risk sharing network, this could lead to a shrinking pool of non-mobile-money-using households in the village without a potential migrant or too old or poor to make use of mobile money struggling to share risk with each other. While mobile money benefits users, it could increasingly have costs for those unable or not willing to use it.

6 Conclusion

Household in developing countries are subject to large changes in consumption due to aggregate shocks, such as rainfall shocks, which negatively affect the consumption of the entire village at one. Droughts and floods are a major source of risk to developing households, and measures which help protect against these, ranging from social protection to micro-insurance, are key new areas of research. Mobile money services are a new and fast growing technology which can help households insure their consumption against aggregate shocks by providing access to remittances from other locations not affected by the shock.

In this paper I show that large rainfall shocks negatively affect household consumption but that mobile money use can mitigate this impact by allowing the easy sending and receiving of remittances. This effect is found only if a mobile money agent is within 1km of the village, works equally for rural and urban households and is found to come from smoothing droughts, which are the type of rainfall shock which actually lead to a fall in consumption. I confirm the robustness of my findings using a placebo test from before the introduction of mobile money to test that villages which did and didn't go on to have mobile money users didn't already differ in their ability to smooth risk. I also use instrumental variable techniques to address potential self-selection effects into mobile money use. Both of these confirm my main results.

However, mobile money only helps smooth aggregate shocks if your household uses mobile money, it is not enough for neighbours in your village to be users. This raises the question of why remittances aren't being shared with others in the village after an aggregate shock.

One potential explanation discussed is that mobile money allows easy sending of money across networks with members in different locations, such as between family migrants to cities and the remaining family in a village, therefore decreasing reliance on traditional village risk sharing networks or networks confined to a small geographical area. However, the idea of mobile money using households completely exiting village risk sharing relationships is questioned by the finding that their consumption still depends to a degree on aggregate village consumption. This suggests mobile money using households still have members of their village in their risk sharing network.

An alternative is that mobile money using households hide their remittance income or that there isn't a norm requiring them to share remittances after an aggregate shock. An information asymmetry such as this has important implications because it means full risk sharing won't take place and could shut it down entirely. Either explanation would be interesting to understand in more detail and in particular to understand if this additional option for mobile money using households comes at the expense of non-mobile-money-using households in their villages who are more reliant on risk sharing with members of their village.

Research on the impact of mobile money services is still in its infancy and further work could explore in more detail how access and use of mobile money services changes traditional risk sharing arrangements and who the winners and losers are from this. Understanding this would suggest whether more formal measures need to be put in place to replace traditional village-based risk sharing networks.

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A Village risk sharing of idiosyncratic shock

I first want to determine if idiosyncratic risk is perfectly shared within the village. To do this I look at the impact of a variety of different household shocks on household per capita consumption, controlling for the level of village consumption. I run the following specification:

$$C_{jvt} = \gamma_i IdioShock_{jvt} + \boldsymbol{\theta X_{jvt}} + \delta_{vt} + \alpha_j + \varepsilon_{jvt} \tag{8}$$

where $Idioshock_{jvt}$ is an idiosyncratic shock experiences by household j in village v at time t, $\boldsymbol{X_{jvt}}$ is a vector of controls consisting of household demographics, financial service use and occupation dummies to control for other variables which allow households to smooth consumption, δ_{vt} are village-time dummies to control for village level variations, including variation of aggregate village consumption as in Ravallion and Chaudhuri (1997), α_j is a household fixed effect picking up fixed characteristics of the household (including the Pareto weight) and ε_{jvt} is a time varying error. If household risk is perfectly pooled within the village then idiosyncratic shocks should have no impact on household consumption once village consumption is controlled. This means that the coefficient on $IdioShock_{jvt}$ should be insignificantly different from zero:

Prediction 1 $\gamma_i = 0$

I estimate this specification using a fixed effect regression with three waves of household panel data.

Table 13 shows OLS fixed effect regressions of different idiosyncratic shocks on per capita consumption. These were the shocks reported in the household survey to affect predominantly this household only, for example a major illness of, or loss of employment by, a household member. I confirm that these shocks are not correlated within a village by calculating the intra-class correlation (see Table 4). For the idiosyncratic shocks, the within group correlation was very small in all cases, confirming these are idiosyncratic shocks affecting individual households. Each shock type enters as a dummy for whether the household has experience that shock in the year proceeding each wave of the survey. The regressions include village-time dummies to control for village level variations of aggregate resources, household fixed effects to remove unobserved factors affecting household consumption smoothing ability and a vector of household demographic variables to account for other household differences which could affect consumption smoothing and to improve precision.

The results shows that none of the idiosyncratic shocks cause a drop in consumption, with three of the coefficients actually positive, though nothing is significant. I therefore cannot reject the hypothesis that idiosyncratic shocks are insured perfectly. The F statistic on the significance of the village by time dummies is large showing that consumption of households is moving with other's in the village. I also confirm these results by running the same regression with a dummy for any idiosyncratic shock, which is equal to 1 if the household experienced any of the 5 idiosyncratic shocks in the year proceeding the survey wave. This is also insignificant and close to zero (results not shown).

Table 13: Household fixed effects regressions of different idiosyncratic shocks on consumption

Dependent variable: Log consumption per capita

	(1) Household business failure	(2) Loss of salaried employment	(3) Chronic/ severe illness	(4) Death of household member	(5) Fire in home
Shock	0.124	0.0861	0.0475	-0.018	-0.010
	(0.100)	(0.110)	(0.084)	(0.041)	(0.084)
Observations	6,769	6,769	6,769	6,769	6,769
Number of households	3,427	3,427	3,427	3,427	3,427
R-squared	0.231	0.171	0.171	0.171	0.171
F test	456.1	178.5	217.1	126.3	168.5
F test MM households	27.9	40.9	54.4	21.9	30.1

All regression include a full set of control variable from Table 1, household fixed effects, village-time dummies and clustered errors at the village level.The F tests refer to tests of the joint significance of the village by time dummies, for both the entire sample and a sample restricted to only mobile money using households

Village clustered standard errors in brackets*** $p<0.01$, ** $p<0.05$, * $p<0.1$