The Effect of Financial Access on Networks: Evidence from a Field Experiment in Nepal*

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October 21, 2012

PRELIMINARY AND INCOMPLETE

Abstract

We study how an exogenous expansion in formal financial access affects the structure of the network of financial transactions. We use a unique panel dataset that contains detailed information on the network of financial transactions before and after a field experiment that randomly gave access to a savings account to half of the households in 19 slums in Nepal. We provide evidence that the intervention has affected the network of financial transactions, and we show that not taking this network change into accounts bias downwards the estimates of a standard peer effect model. While previous literature has mostly taken the network structure as exogenous, our paper attempts to shed light on the individuals strategic incentives to link formation.

JEL codes: D85; G21; O17

Keywords: networks; financial access; savings

*We thank Bernard Fortin for helpful comments. Silvia Prina is grateful for funding from IPA-Yale University Microsavings and Payments Innovation Initiative and the Weatherhead School of Management.

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1 Introduction

Networks play a fundamental role for risk sharing, financial insurance and decision making, getting a job, technology and financial products adoption, and other economic outcomes (e.g., Ambrus, Mobius, and Szeidl 2010; Banerjee, Chandrasekhar, Duflo, and Jackson 2012; Cai 2012; Conley and Udry 2010; Duflo and Saez 2003; Munshi 2004). All studies on peer effects and network externalities implicitly assume that the social structure helps spreading the effect of a policy intervention, due to spillovers. However, the reverse may also be true, namely the social structure may be also be shaped by the policy intervention. Many papers, for example, have studied the effects that social networks have on the expansion of financial products, but, in order to do this they implicitly assume networks remain constant. Nevertheless, anecdotal evidence on small-scale networks and theoretical literature suggest that the architecture of networks evolves, and it is crucial to understand how (Jackson and Watts 2002).

No network paper has explored this issue empirically. One of the reasons has been the lack of clear cut theoretical predictions regarding the patterns of network evolution.[1] Another reason has been the lack of appropriate network data. In fact, all previous models of peer effects identified trough social network structure make use of network data collected at one point in time (Bramoullé, Djebbari, and Fortin 2009; Calvó-Armengol, Patacchini, and Zenou 2009).

This paper intends to fill this gap in the literature. We use a unique panel dataset that contains detailed information on the network's financial transactions before and after a field experiment that randomly gave access to a savings account to half of the households in 19 slums in Nepal. We investigate the network response to the exogenous intervention and study the impact of introducing formal savings account on the household's network of informal financial arrangements. First, we show that the exogenous expansion in formal financial access has affected the structure of the network of financial transactions. Using individual and dyadic regressions, we show that both the treatment status of the individual and his partners prove useful to explain overall

[1]While the issue of how networks evolve has been explored by theorists under specific assumptions (Watts 2001), a fully heterogeneous framework, due to the model complexity, is unable to provide predictions.

level of network activity, link formation and severance, and the probability of giving and receiving loans and gifts. Then, we estimate a dynamic model of peer effect which takes into account the change in the financial network structure and we show that the peer effect estimates obtained differ from a standard peer effect static model. In particular, we show that not taking into account the change that the intervention induces in the network, we underestimate the effect of peer outcome.

The following section summarizes the related literature. Section 3 describes the field experiment, the savings account, and the network data. Section 4 describes our estimation strategy and results. Finally, Section 5 concludes.

2 Literature Review

The effect of informal networks on economic outcomes such as adhesion to a retirement plan, adoption of health products, new technologies, microcredit programs, and weather insurance has been extensively studied, not only in developing country settings. These studies can be divided according to two criteria: whether they use self-declared detailed network data or not, and whether the identification strategy relies on a randomized experimental design or not.

The papers relying on a randomized intervention to identify the causal effect of informal networks include Cai (2012), Duflo and Saez (2003), Duflo, Kremer, and Robinson (2008), Dupas (2010), Kremer and Levy (2008), Kremer and Miguel (2007), Oster and Thornton (2008). Among the studies that use non-experimental methods to identify the causal effects of networks we find Bandiera and Rasul (2006), Banerjee et al. (2012), Conley and Udry (2010), Foster and Rosenzweig (1995), Munshi (2003), Munshi (2004).

Most previous studies do not have self-declared network data and thus identify the individual reference group based on information on the respondents' social context. Few notable exceptions are, for example, Banerjee et al. (2012), Oster and Thornton (2008), and Cai (2012), that similar to our case have self-declared information on the links between individuals in the sample. The studies most related to our paper are Banerjee et al. (2012) and Cai (2012) which consider the introduction of a microcredit and of an insurance product, respectively. Banerjee et al. (2012) examine the role social networks play in the decision to participate in a microfinance program. They have only one round of pre-intervention network data. As they sample up to 50% of the households in each village considered in the analysis, they need to resort to strong distributional assumptions and estimation techniques (Chandrasekhar and Lewis 2011). Moreover, their randomization is at the village level and take-up rate of the microcredit product was 18.5%. Similarly, Cai (2012) studies the influence of social networks on the decision to adopt a weather insurance product. All villages are offered the product, but its price is randomized at the village level. The take-up rate of the product after one year is 44%.

Our study uses a dataset of 19 slums where 99% of the households were surveyed. In addition, it takes advantage of within village randomization and of the high take-up rate of the savings product offered (84%). Hence, the unique combination of a within village randomized experiment with a census data with two round of network data (before and after the intervention) allow us to investigate the evolution of network following the program and to incorporate this information when estimating the overall effect of the intervention. Furthermore, while Banerjee et al. (2012) and Cai (2012) define the network on the basis of social interaction data within each village, we collected financial flows inside and outside the slums.

Our paper is also linked to studies analyzing the importance of informal arrangements for villagers in developing countries who do not have access to formal institutions. Informal arrangements (such as loans and gifts between relatives, neighbors and friends) are used to smooth consumption when hit by idiosyncratic shocks, e.g., health-related expenses, unemployment, and funerals. They are particularly valuable for households with limited or no financial access (either because there are no institutions that provide it or because households cannot meet the collateral required to enter a formal transaction).

3 Experimental Design and Background

3.1 Financial Institutions in Nepal and the Savings Account Offered

Formal financial access in Nepal is very limited. Only 20% of Nepalese households have a bank account, according to the nationally representative i2006 by the World Bank (Ferrari, Jaffrin, and Shrestha 2007). Not surprisingly, access is concentrated in urban areas and among the wealthy. Thus, most households typically save via microfinance institutions, savings and credit cooperatives, and Rotating Savings and Credit Associations (ROSCAs). Also, households commonly have cash at home and save in the form of durable goods and livestock.

In the field experiment by Prina (2012), GONESA bank gave access to savings accounts to a random subsample of poor households in 19 slums in Nepal. The accounts are very basic but have all the characteristics of any formal savings account. The enrollment procedure is simple and account holders are provided with an easy-to-use passbook savings account. Customers can make transactions at the local bank-branch offices in the slums, which are open twice a week for three hours. Account holders have no opportunity to deposit or withdraw money in the slum outside of these working hours. However, they can make any transactions during regular business hours at the bank's main office, located in downtown Pokhara. Nevertheless, this option is inconvenient because it requires customers to spend time and money to travel to the city center. The bank does not charge any opening, maintenance, or withdrawal fees and pays a 6% nominal yearly interest (inflation is above 10% in Nepal), similar to the average alternative available in the Nepalese market (Nepal Rastra Bank 2011). In addition, the savings account does not have a minimum balance requirement. The money deposited in the savings account is fully liquid for withdrawal at any time at the bank's main office, or twice a week at the local bank-branch office. Finally, the savings account is fully flexible and operates without any commitment to save a given amount or to save for a specific purpose.

3.2 Experimental Design and Data

A first baseline household survey was conducted in February 2009 in the 21 slums in which GONESA operates. All households with a female head between the ages of 18-55 were sampled.[2] This round of data contains detailed information on households' socioeconomic characteristics, and their network of informal financial transactions within and outside the village.A second baseline survey was conducted in May 2010 in each slum. This survey collected information on household composition, education, income, income shocks, monetary and non-monetary assets ownership, borrowing, and expenditures on durables and non-durables. This survey however, did not collect networks data.

The full-scale field experiment took place in the remaining 19 slums (two slums were initially used to pilot-test the savings account). The populations in the areas considered in the study ranged from 20 to 150 households.

After completion of the baseline survey, the bank progressively began operating in the slums between the last two weeks of May and the first week of June 2010, as described below. A pre-announced community meeting was held in each slum. At this meeting, participants were told (1) about the benefits of savings; (2) that the bank was about to launch a savings account; (3) the characteristics of the savings account; (3) what the savings account could helped them with and how they could have used it; and (4) that the savings account would be initially offered only to half of the households via a public lottery. The short public talk was given by an employee of the bank with the support of a poster and was followed by a session of questions and answers. The main aim of the session was to provide some kind of financial literacy on the benefits of savings and savings accounts to the entire sample so that the effect of the intervention would be mainly caused by the offer of the accounts. Then, separate public lotteries were held in each slum to randomly assign female household heads to either the treatment group (offered the savings account) or the control group (not offered the savings account).[3] Those women who were sampled for the treatment were

[2]Female household head is defined here as the female member taking care of the household. Based on this definition, 99% of the households living in the 19 slums were surveyed by the enumerators.

[3]GONESA required that the random assignment into treatment and control groups be done publicly, making stratification based on occupation or income highly infeasible.

offered the option of opening a savings account at the local bank-branch office.[4] Those women sampled for the control group were not given this option, but were not barred from opening a savings account at another institution.

The endline survey was conducted starting in June 2011, a year after the beginning of the intervention. It contained, in addition to the modules contained at baseline, information on household expenditures, time preferences, and networks.

A total of 1,009 households were surveyed in both the first and second baseline. 91% (i.e., 915) were found and surveyed in the endline survey.[5] Attrition for completing the endline is not correlated with observables, as shown in Appendix Table A1. Hence, performing the analysis on the restricted sample for which there are endline data will not bias the estimates of the treatment effect.

3.3 Sample Characteristics and Balance Check

Table 1A illustrates that female household heads were an average age of about 37 years and had about two years of schooling. Roughly 90% of them were married or living with their partner. The average household size at baseline was 4-5 people, with an average of 2-3 children per household.

Weekly household income at baseline averaged 1,494 Nepalese rupees[6] although there is considerable variation. Households earned their income from varied sources: working as an agricultural or construction worker, collecting sand and stones, selling agricultural products, raising livestock and poultry, running a small shop, or working as a driver. In addition, households received remittances and pensions, and collected rents. Also, the majority of households (86%) reported living in a house owned by a household member, and 80% reports to own the plot of land on which the house was built.

The population of the study seems highly vulnerable to shocks; 42% of the sample indicated having experienced a negative external income shock during the month previous to the baseline survey. Shocks include health shocks, lost job, livestock loss,

[4]The offer did not have a deadline.

[5]Those households that could not be traced had typically moved out of the area, with a minority migrating outside the country.

[6]In 2010-2011, 70 Nepalese rupees were approximately 1 U.S. dollar.

broken/damaged/stolen goods or equipment, low demand for business, decrease in the wage rate, and death of a household member. 51% of the households coped with a shock using cash savings, 18% coped by borrowing from family and friends, and 17% coped by borrowing from a moneylender. Only 1% coped by cutting consumption, possibly suggesting that households have some ability to smooth consumption when facing by a negative shock.[7]

Table 1B shows households' assets and liabilities in May 2010. Total assets owned by the average household had a value of more than 44,000 rupees. Monetary assets accounted for about a third of total assets. Non-monetary assets, consumer durables, and livestock and poultry accounted for the remaining two thirds. Roughly 15% of the households at baseline were banked, 17% had money in a ROSCA, and more than 56% stored money in a microfinance institution (MFI). Households also typically had more than one week's worth of income stored as cash in their home. Furthermore, 90% of the households had at least one outstanding loan. This is in line with the national average from the Access to Financial Services Survey showing that, in 2006, over two-thirds of Nepalese households had an outstanding loan from a formal or informal institutions (Ferrari et al. 2007).

Most loans are taken from shopkeepers (41%), MFIs (39%), family, friends, or neighbors (32%), and moneylenders (12%). Formal loans from banks are rare, with only 5% of the sample reporting an outstanding loan borrowing from a bank. Summary statistics from Table 1B show a high level of participation by the sample population in financial activities. Most transactions were carried out with "informal" partners, such as kin and friends, moneylenders, and shopkeepers rather than with formal institutions like banks. This is consistent with previous literature showing that the poor have a portfolio of transactions and relationships (Banerjee, Duflo, Glennerster, and Kinnan 2010; Collins, Morduch, Rutherford, Ruthven 2009; Dupas and Robinson forthcoming a and b).

Finally, Table 1C highlights that control and treatment groups do not have differences that are statistically significant when considering their attitudes towards saving/spending and in beliefs regarding network support.

[7] An alternative explanation could be that shocks were small in economic terms.

Overall, Tables 1A, 1B, and 1C show that for the final sample considered for the analysis (i.e., those 915 households that completed the two baseline surveys and the endline survey) treatment and comparison groups are balanced along almost all characteristics). In particular, Table 1A shows that treatment households are 5% more likely to have experienced a negative income shock in the month prior to may 2010 than control households (45% treatment households vs. 40% control households experienced a negative shock). And, Table 1B shows that treatment households have more monetary assets than control households (Rs. 16,000 vs. Rs. 11,900). Both differences are statistically significant at the 10% level.

As shown by Prina (2012) take-up and usage rates of the savings accounts offered to the treatment group were very high. In particular, more than 80% of the treatment households offered an account opened one and used it actively, depositing on average of 8% of baseline weekly household income almost once a week for the first year of the intervention. Moreover, access to the savings account considerably increased monetary assets and total assets (Prina, 2012). Thus, such large impacts could potentially affect the network of financial transactions.

3.4 The network data

Detailed information on informal network-based financial transactions, inside and outside the village, with regular and with occasional partners, was collected in the first baseline survey and in the endline survey.

First, each respondent was asked to give a list of people inside or outside the village that she could rely on most and with whom she regularly exchanged gifts and/or loans. For each of these regular partners detailed information was collected on all loans and gifts given and received, both the last 12 months and in the last month. The information regarding loans and gifts given and received in the last 12 months was collected using four brackets: Rs. 1200, 1200-2400, 2400-5000 and >5000. Instead, regarding loans and gifts given and received in the last month, the respondent was asked the exact amount and reason of each transfer given or received.

After completing the information on regular partners, information on loans and gifts given and received within the last month from so-called occasional partners (i.e., individuals not mentioned in the initial list) was elicited.

Special attention was devoted to matching the declared partners identities with other sampled individuals and circumvent homonymy, one of the biggest challenges of network data collection. At the end of each interview the enumerator used an updated list of all households in the village sample (containing detailed individual level information) to determine, jointly with the respondent, the household identity code of the mentioned partners, that were transcribed on the filled questionnaire.

Tables 2A and 2B contain the network descriptive statistics at baseline by treatment status. On average households reported having on average 1.4 regular partners and receiving on average more than one loan from their informal network. The number of occasional partners is smaller. Overall, for the final sample considered for the analysis (i.e., those 915 households that completed the two baseline surveys and the endline survey) treatment and comparison groups are balanced along all network characteristics when considering both regular and occasional partners, Table 2A and 2B, respectively.

Finally, Table 3 shows that there is no differential attrition on treatment or network characteristics.

4 Empirical analysis

First, we provide evidence that the exogenous access to savings accounts has affected the network of financial transactions. In order to do so, we look at the creation and destruction of informal financial links taking both an individual perspective (Subsection 4.2) and a dyadic perspective (Subsection 4.3). Second, in Subsection 4.4, we show that if estimate a model of peer effects by taking into account the change in the network, we obtain different estimates than the ones obtained assuming the network remains fixed.

4.1 Framework and definition

For the scope of our analysis let us define the first baseline survey as time t, and the endline survey as time $t+1$. In what follows, vectors are denoted with bold lower case letters and matrices with bold capital letters. If $\mathbf{A}$ is a $n \times m$ matrix, we write $a_{ij} \equiv \mathbf{A}_{[ij]}$ to indicate its (i, j)th entry, which gives the shorthand $\mathbf{A} = [a_{ij}]_{n \times m}$. If $\mathbf{b}$

is a $n \times 1$ vector, we write $b_i \equiv \mathbf{b}_{[i]}$ to indicate its ith row, which gives the shorthand $\mathbf{b} = [b_i]_{n\times 1}$. If a matrix or a vector is indexed by time, it is indicated with a superscript to avoid confusion with the entry notation, *i.e.* we write $\mathbf{A^t} = [a^t_{ij}]_{n\times m}$.

Starting from our set of n survey respondents $(1, ..., n)$, for each pair (*dyad*) of sampled individuals ij we define the binary variable g^t_{ij} representing a directed transfer at time t, setting $g^t_{ij} = 1$ if a transfer is given by i to j at time t, and zero otherwise.[8] Since the transfers are directed and do not need to be symmetric, both dyads ij and ji are included.[9] The binary interaction matrix $\mathbf{G^t} = [g^t_{ij}]_{n\times n}$ represents the directed network at time t. $\mathbf{G^t}$ is block-diagonal because, by construction, only transfers within the same village community are allowed. $\mathbf{G^{t+1}}$ is the binary interaction matrix for the endline data. For the dyadic regressions of Subsection 4.3 we compute $\mathbf{G^t}$ and $\mathbf{G^{t+1}}$ using three different definitions of transfers: loans only, gifts only, loans and gifts (detailed statistics are provided in the Appendix, Table A2).

For the individual regressions of Subsections 4.2 and 4.4 we follow the literature on identification of peer effects through social network data (Bramoullé *et al.* 2009; Liu *et al.* 2012) and use the undirected and row-standardized version of the interaction matrix $\mathbf{W^t} = [w^t_{ij}]_{n\times n}$ where $w^t_{ij} = z^t_{ij}/\sum_i z^t_{ij}$ and $z^t_{ij} = z^t_{ji} = max(g^t_{ij}, g^t_{ji})$. $\mathbf{W^{t+1}}$ is constructed analogously using endline survey data. Note that for Subsections 4.2 and 4.4 we compute $\mathbf{W^t}$ and $\mathbf{W^{t+1}}$ only on the basis of the most general definition of transfers (including both loans and gifts). Under this undirected network, the number of partners at baseline range from 0 to 19.[10]

[8]No self link is allowed, i.e. $g^t_{ij} = 0$.

[9]For each directed observation g^t_{ij} we have two reports: how much i declares to have given to j and how much j declares to have received from i. In principle answers to these questions should agree, in practice they often do not. The problem is common to all empirical literature using self-reported link data, and the solution is typically to assume that a link exists if it is reported by either i or j or a combination of the two (De Weerdt, 2004; De Weerdt and Fafchamps 2011; Fafchamps and Lund 2003; Liu, Patacchini, Zenou, and Lee 2012; Banerjee et al., 2012). In this paper we assume that discrepancies between survey answers are most likely due to omission mistakes and under-reporting (which is the most common assumption across the relevant literature). Therefore, when reports do not coincide, we set $g^t_{ij} = 1$ if any of the parts involved declares so.

[10]The average number of partners is 0.8, and its standard deviation is 1.2.

4.2 Individual intent-to-treat regressions

We first present individual-level results providing evidence that the involvement in the village-level network activities is impacted by the intervention. For each sampled individual $i = 1, ..., n$, let $network_i^{t+1}$ be a given proxy for the intensity of his network-based informal transactions at time $t+1$, and let itt_i be the intent-to-treat dummy which takes value one if i was offered a bank account after the second baseline survey was carried out. Let $\mathbf{x^t}_i$ be a $1 \times Z$ vector of characteristics of i at baseline. To get a preliminary evidence of the effects of the intervention on the network-based informal financial transactions, we run the following individual intent-to-treat linear regression

$$network_i^{t+1} = \beta_0 + \beta_1 itt_i + \beta_2 \mathbf{x^t}_i + \lambda_v + \epsilon_i^{t+1} \tag{1}$$

where λ_v represent village fix-effects and ϵ_i^{t+1} is the exogenous error term. In Table 4 we estimate Equation (1) using 12 different proxies of network transactions, all reported by the respondent: total number of regular (occasional) partners, number of regular (occasional) partners within the slum, number of gifts (loans) from regular (occasional) partners in the last 12 months given and received respectively. Controls at baseline include: age and years of education of the female households head, household size, number of children 16 year of age or less. The descriptive statistics of the network variables used in the analysis are reported in the Appendix, Table A3.

The results show that many proxies of network transactions (e.g., the number of regular partners inside the slum, the number of occasional partners inside and outside the slum, and the number of gifts received from occasional partners) are affected by the treatment dummy.

In Table 5, we run the augmented specification

$$network_i^{t+1} = \beta_0 + \beta_1 itt_i + \beta_2 \mathbf{x^t}_i + \beta_3 w^t itt_i + \lambda_v + \epsilon_i^{t+1} \tag{2}$$

where $w^t itt_i = \mathbf{W^t}\,\mathbf{itt}_{[i]} = \sum_{k=1}^{n} w_{ik}^t \cdot itt_k$ represents the share of i's partners at time t who were offered a bank account. For the sake of the interaction matrix $\mathbf{W^t}$ partners are defined on the basis of all transfers, i.e. including loans and gifts. Since the matrix is undirected, a partner is someone that has given/received a loan/gift from/to the respondent. Because of the intervention design, this regressor is exogenous and it is a good first proxy for the spillover effects of peers' treatment status on individual outcomes. Results of Table 5 reconfirm the findings from Table 4. Moreover, the estimates show that $w^t itt_i$ is positively significant for several network measures. This can be interpreted as preliminary evidence that the effects of the intervention have spilled over the network of informal transactions.

4.3 Dyadic intent-to-treat regressions

Estimating the effects of the intervention on the informal financial transactions by using individual regressions is not entirely satisfactory. In fact, individual regressions do not take into account that the formation and severance of financial links are a dyadic decision. Therefore, the network outcome of an individual depend on the characteristics of his potential partners as well. The intervention considered, that provided access to savings accounts to half or the households in the slums, didn't only affect treatment households, but also control households who were connected or could be potentially connected to them.

In this section we account for the fact that financial links involve two parties. We take the directed dyads as unit of observation and provide evidence that the randomized intervention has shaped the pattern of interactions. We run the following probit regression:[11]

$$\begin{aligned} P(g_{ij}^{t+1} = 1) =& P(\beta_0 + \beta_1 itt_i + \beta_2 itt_j + \beta_3 g_{ij}^t + \beta_4 itt_i \cdot itt_j + \beta_5 itt_i \cdot g_{ij}^t \\ & + \beta_6 itt_j \cdot g_{ij}^t + + \beta_7 itt_i \cdot itt_j \cdot g_{ij}^t + \beta_8 \mathbf{x^t}_i + \beta_9 \mathbf{x^t}_j + \lambda_v + \epsilon_{ij}^{t+1} > 0) \end{aligned} \quad (3)$$

where the dependent variable $g_{ij}^{t+1} \equiv \mathbf{G}_{[ij]}^{\mathbf{t+1}}$ represents a binary transfer from i to j at time $t+1$. The three main dummies of interest are: the treatment status of the

[11]In this specification we are looking at the binary transfer rather than at the magnitude of the transaction, because in order to maximize the number of non-zero observations we are considering regular and occasional partners together (the exchanges to regular partners were collected using 4 brackets, while for occasional partners we collected the continuous value).

potential giver, itt_i; the treatment status of the potential receiver, itt_j; and the directed transfer at time t, g_{ij}^t. The specification also includes all two-way dummies interactions, as well as their three-way interaction. Furthermore, dyadic controls at baseline for i and j (namely: age and years of education of the female household head, household size, number of children 16 years old or younger for given and receiver respectively) and village fix-effects λ_v are also included.

Table 6 reports the probit marginal effects for Equation (3) for three different transfers measures: loans only, gifts only, and both loans and gifts. The marginal effects for the binary interaction terms are computed following the Delta method (Ai and Norton 2003). Standard errors are clustered at the slum level to accommodate for arbitrary patterns of residuals correlations within slums.[12]

Not surprisingly, results in Table 6 show that there is a positive and statistically significant effect of past transfers on current transfers: if two individuals were already doing financial transactions in the past they are likely to continue doing so. More interestingly, we also observe a significantly positive effect of the treatment dummies on the transfers probabilities. In particular, concerning loans, the interaction $itt_j \cdot g_{ij}^t$ is significant: the fact that the receiver i was offered a bank account increases his probability of receiving another loan from i if j had already borrowed money from i in the past. Regarding gifts, itt_i is significant, showing that those who were offered the bank account are *ceteris paribus* more likely to make gifts. Moreover, the interaction $itt_i \cdot itt_j$ is also positively significant: for those pairs where both partners benefited from the program, the probability of gifts increases (which is consistent with both a wealth effect or social norms explanation).

The $itt_j \cdot g_{ij}^t$ and itt_i dummies remain significant in column (3) where a binary transfer is defined as either a loan or a gift from i to j. Indeed, these marginal effects are small in absolute terms. They are large however, when compared to the mean value of the dependent variable, which has very few positive observations, since the database include all possible directed dyads. For instance, the itt_i coefficient for gifts is the smallest significant coefficient in our estimates, and yet it corresponds to a 30%

[12]The number of observations of the specification in column (2) of Table 6 is lower than the full directed sample of 56,308 dyads. That is because in two slums there were no declared gifts in $t+1$ and therefore all corresponding observations are dropped.

increase with respect to the baseline value of the dependent variable. To the best of our knowledge, this specification is innovative in the literature, as it exploits in a dyadic framework the combination of the two original features of our data: the randomized treatment and the two-wave network data. These results provide convincing evidence that the randomized intervention had non negligible impact on the pre-existing network structure in the village.

4.4 Network externalities

While Subsection (4.3) provides evidence that the intervention has affected the network of informal financial interactions within the sampled communities, this subsection explores the consequence of its findings. Most previous studies looking at how economic phenomena diffuse through a network implicitly assume that the network structure is not affected by the phenomenon itself. However, in the previous subsections we have provided evidence suggesting that the network structure also responds to a given intervention. Taking advantage of our unique data, which contain information on the network of financial transactions before and after the intervention, we investigate the network response to the exogenous intervention.

Define the $n \times 1$-dimensional vectors $\mathbf{y^t}$, $\mathbf{y^{t+1}}$, $\boldsymbol{\epsilon}^{\mathbf{t}}$, $\boldsymbol{\epsilon}^{\mathbf{t+1}}$ and $\mathbf{itt}$ representing the individual outcomes at time t and $t+1$, the error terms at t and $t+1$, and the treatment status, respectively. $\mathbf{W^t}$ and $\mathbf{W^{t+1}}$ are the two undirected and row-standardized interaction matrices, and $\boldsymbol{\iota}$ is an $n \times 1$ vector of ones. For the sake of notation, let us also define the vectors of changes: $\boldsymbol{\Delta}\mathbf{y} = \mathbf{y^{t+1}} - \mathbf{y^t}$, $\Delta\boldsymbol{\epsilon} = \boldsymbol{\epsilon}^{\mathbf{t+1}} - \boldsymbol{\epsilon}^{\mathbf{t}}$, and $\boldsymbol{\Delta}\mathbf{W} = \mathbf{W^{t+1}} - \mathbf{W^t}$. The peer effect model in matricial form can be written as

$$\mathbf{y^t} = \alpha_0\boldsymbol{\iota} + \beta\mathbf{W^t}\,\mathbf{y^t} + \boldsymbol{\lambda}_v + \boldsymbol{\mu}_c + \boldsymbol{\epsilon}^{\mathbf{t}} \tag{4}$$

$$\mathbf{y^{t+1}} = (\alpha_0+\alpha)\boldsymbol{\iota} + \beta\mathbf{W^{t+1}}\,\mathbf{y^{t+1}} + \gamma\mathbf{itt} + \delta\mathbf{W^{t+1}}\,\mathbf{itt} + \boldsymbol{\lambda}_v + \boldsymbol{\mu}_c + \boldsymbol{\epsilon}^{\mathbf{t+1}} \tag{5}$$

where $\boldsymbol{\lambda}_v$ is the village effect and $\boldsymbol{\mu}_c$ is the network component fixed effect.[13] Following Lee (2007), Bramoullé *et al.* (2009) and Calvo- Armengol, Patacchini and Zenou

[13]A component is a maximal set of indirectly related individuals.

(2009) throughout our analysis we assume that $\boldsymbol{\mu}_c$ at the component level captures all unobserved variable assortativity in link formation, and that net of $\boldsymbol{\mu}_c$ the interaction matrix $\mathbf{W^t}$ can be treated as exogenous. This would be the case, for instance, if women with same ability, background and attitude were linked together. Treating those unobservables as fixed effects helps addressing the problem of correlated effects, that is, the fact that individuals in the same reference group tend to behave similarly because they are alike or face a common environment (Mansky, 1993). For the interpretation purpose, $\mathbf{W^t}\,\mathbf{y^t}_{[i]} = \sum_{k=1}^{n} w_{ik}^t \cdot y_k^t$ represents the average outcome at time t of i's partners at time t.

First we consider the scenario in which we assume that the interaction matrix remains constant across the two periods, *i.e.* $\mathbf{W^{t+1}} = \mathbf{W^t}$. Subtracting (4) from (5) we get to the first-difference equation

$$\Delta\mathbf{y} = \alpha\boldsymbol{\iota} + \beta\mathbf{W^t}\,\boldsymbol{\Delta}\mathbf{y} + \gamma\mathbf{itt} + \delta\mathbf{W^t}\,\mathbf{itt} + \Delta\boldsymbol{\epsilon} \tag{6}$$

In what follows we refer to Equation (6) as to the static peer effect model.

Second, taking advantage of the availability of two survey rounds before and after the intervention, we can allow the interaction matrix to change through time ($\mathbf{W^{t+1}} \neq \mathbf{W^t}$), possibly as a consequence of the intervention. In this scenario we make the additional assumption that, once $\boldsymbol{\mu}_c$ is netted out, the observed change in the interaction matrix $\boldsymbol{\Delta}\mathbf{W}$ only depends on the intervention vector $\mathbf{itt}$ (and on a purely random component). This comes as a natural extension of the exogeneity of $\mathbf{W^t}$ conditional on $\boldsymbol{\mu}_c$, if we believe that the correlated unobservables are time-invariant individual characteristics. We can rewrite:

$$\mathbf{W^{t+1}}\,\mathbf{itt} = \mathbf{W^t}\,\mathbf{itt} + \boldsymbol{\Delta}\mathbf{W}\,\mathbf{itt} \tag{7}$$

$$\mathbf{W^{t+1}}\mathbf{y^{t+1}} - \mathbf{W^t}\mathbf{y^t} = \mathbf{W^t}\,\boldsymbol{\Delta}\mathbf{y} + \boldsymbol{\Delta}\mathbf{W}\,\mathbf{y^t} + \boldsymbol{\Delta}\mathbf{W}\,\boldsymbol{\Delta}\mathbf{y} \tag{8}$$

where the decomposition in Equation (8) suggest that the total change in peers' outcomes between the two periods can be imputed to three componenents: the changes in peers' outcomes keeping their composition constant; the changes in peers' composition keeping their outcome constant; and the cross term effect. By subtracting (4) from (5) we now obtain the first-difference estimating equation:

$$\Delta \mathbf{y} = \alpha \boldsymbol{\iota} + \beta_1 \mathbf{W^t}\, \boldsymbol{\Delta} \mathbf{y} + \beta_2 \boldsymbol{\Delta} \mathbf{W}\, \mathbf{y^t} + \beta_3 \boldsymbol{\Delta} \mathbf{W}\, \boldsymbol{\Delta} \mathbf{y} + \gamma \mathbf{itt} + \delta_1 \mathbf{W^t}\, \mathbf{itt} + \delta_2 \boldsymbol{\Delta} \mathbf{W}\, \mathbf{itt} + \Delta \boldsymbol{\epsilon} \quad (9)$$

where model (6) is nested into model (9). In what follows we refer to Equation (9) as to the dynamic peer effect model. The key message is that If we do not take into account the network changes as in the model of Equation (6) our peer effect estimates are likely to be biased as we are omitting the correlated terms which account for the readjustments d in the financial network due to the intervention.

The estimates of the static (6) and dynamic (9) models are reported in Tables 7, 8 and 9. We consider six different outcomes in logs. For each outcome, we run three different specifications: column (1) reports the estimates from a first-difference baseline model with no peer effects (corresponding to the estimating equation $\Delta \mathbf{y} = \alpha \boldsymbol{\iota} + \gamma \mathbf{itt} + \Delta \boldsymbol{\epsilon}$), column (2) reports the results for the static peer effect model, column (3) reports the estimates of for the dynamic peer effect model. All descriptive statistics are reported in the Appendix, Table A3.

The general conclusions we can draw from these three tables is that all three peer effect components enter with positive sign (when significant) as we would have expected. Moreover, the estimated coefficients for the term $\mathbf{W^t}\, \boldsymbol{\Delta} \mathbf{y}$ are biased downward in the static model when we omit the other two terms accounting for the change in the network driven by the intervention itself.

Table 7 contains the results for monetary assets and non-monetary assets. For what concerns monetary assets, the intent-to-treat individual dummy is significantly positive and of the same magnitude as in Prina (2012). When we run the peer effect model of column (2), the peer effect term $\mathbf{W^t}\, \boldsymbol{\Delta} \mathbf{y}$ does not appear significant, while the treatment status of peers $\mathbf{W^t}\, \mathbf{itt}$ is significantly positive. A higher share of ex-ante friends who get offered a saving account has a negative impact on the individual increase in monetary assets. This unexpected negative sign deserves further investigation, however it seems in line with previous results. in fact, in Table 5 we found that $\mathbf{W^t}\, \mathbf{itt}$ is associated with an increase in the overall number and loans to occasional partners in and out the village. In addition, in Table 6 we found that treated individuals tend to give more loans to ex-ante friends. When we get to the dynamic peer effect model of column (3), the term $\mathbf{W^t}\,\mathbf{\Delta y}$ becomes significantly positive: an increase in monetary assets of baseline friends has a positive impact on individual assets. Also the cross-term product $\mathbf{\Delta W}\,\mathbf{\Delta y}$ (comparing the increase in assets for endline vs. baseline partners, respectively) is positive and statistically significant. On the other hand, the second term $\mathbf{\Delta W}\,\mathbf{y^t}$, which measures baseline assets for endline versus baseline partners, is not statistically significant. As the cross term product is negatively correlated with $\mathbf{W^t}\,\mathbf{\Delta y}$ by construction, the lack of statistical significance of column (2) can be imputed to omitted variable bias. Results for non-monetary assets are smaller (only the interaction term becomes significant).

Table 8 reports results for children and educational expenditures: in both cases we do not find direct impact of the intervention, but a strongly significant peer effect, that gets underestimated in column (2) with respect to column (3) when the additional terms $\mathbf{\Delta W}\,\mathbf{y^t}$ (significant for children expenditures) and $\mathbf{\Delta W}\,\mathbf{\Delta y}$ (significant for both dependent variables) are omitted. Results in Table 9 can be interpreted in a similar fashion. Overall, these results show that the peer effect estimates significantly increase in sign and/or statistical significance when we take into account that the network structure is potentially linked to the intervention itself.

5 Conclusions

Financial products, like information and new technologies, spread through networks. The existing literature has shown that networks help spreading financial products, assuming that the structure of the network is fixed. However, the structure of the network might change precisely because of the availability of those products.

In this paper we challenge the assumption that the network remains constant. We use a unique panel data on the network of financial transactions gathered before and after an exogenous expansion in formal financial access. We find evidence supporting the claim that exogenous access to a savings account changed the network. In fact, using individual and dyadic regressions, we show that the treatment status of the individual and his partners prove useful to explain overall level of network activity, link formation, severance, and probability of giving and receiving loans and gifts.

Then, we estimate a dynamic model of peer effect which takes into account the change in the financial network structure and we show that the peer effect estimates obtained differ radically from a standard peer effect static model. In particular, we show that assuming a fixed network biases downwards the peer effect estimates.

This paper shows that taking into account of both, how financial access influences networks and how networks change because of financial access, is necessary to capture the actual importance of networks in spreading products and technologies. While our results are still preliminary, this information can serve as a foundation for the design of successful interventions that take advantage of networks to spread new products and technologies.

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Table 1A: Descriptive Statistics at Baseline by Treatment Status

		Mean			
	Obs.	**Sample**	**Control**	**Treatment**	**T-stat**
Characteristics of the Female Head of Household)					
Age	915	36.80	36.77	36.82	0.05
		(12.51)	(12.16)	(12.85)	
Years of Education	913	2.52	2.44	2.59	0.79
		(2.82)	(2.67)	(2.96)	
Percent Married/Living with Partner	915	0.89	0.88	0.90	0.77
		(0.32)	(0.33)	(0.31)	
Household Characteristics					
Household size	915	4.55	4.58	4.52	-0.51
		(1.66)	(1.68)	(1.64)	
Number of Children	915	2.21	2.26	2.18	-0.86
		(1.30)	(1.30)	(1.29)	
Total Income Last Week	915	1,494.73	1,472.84	1,515.64	0.13
		(4,833.91)	(4,598.50)	(5,053.36)	
Log(Total Income Last Week + 1)	915	3.50	3.50	3.49	-0.06
		(3.68)	(3.67)	(3.70)	
Total Income in a Typical Week	915	2,864.19	2,952.04	2,780.28	-1.19
		(2,167.27)	(2,491.06)	(1,803.37)	
Log(Total Income in a Typical Week + 1)	915	7.71	7.71	7.71	-0.13
		(0.77)	(0.82)	(0.73)	
Experienced a Negative Income shock	915	0.42	0.40	0.45	1.68*
		(0.50)	(0.49)	(0.50)	
Coped Using Cash Savings	388	0.51	0.51	0.51	-0.03
		(0.50)	(0.50)	(0.50)	
Coped Borrowing from Family/Friends	388	0.18	0.20	0.16	-0.93
		(0.38)	(0.40)	(0.37)	
Coped Borrowing from a Moneylender	388	0.17	0.15	0.18	0.6
		(0.37)	(0.36)	(0.38)	
Coped Cutting Consumption	388	0.01	0.01	0.01	-0.71
		(0.09)	(0.11)	(0.07)	
Owns the house	913	0.86	0.86	0.85	-0.41
		(0.35)	(0.34)	(0.35)	
Owns the land on which the house is built	910	0.80	0.80	0.80	-0.03
		(0.40)	(0.40)	(0.40)	
House has a toilet	913	0.82	0.83	0.82	-0.40
		(0.38)	(0.38)	(0.39)	

Note: differences statistically significant at the *10%, **5%, or ***1% level.

Table 1B: Descriptive Statistics at Baseline by Treatment Status

		Mean			
	Obs.	**Sample**	**Control**	**Treatment**	**T-stat**
Assets					
Total Assets	915	44,469.26	42,510.10	46,340.51	1.14
		(50,891.76)	(45,540.07)	(46,340.51)	
Total Monetary Assets	915	13,993.67	11,872.42	16,019.74	1.66*
		(38,013.67)	(30,582.810	(43,892.17)	
Log(Total Assets + 1)	915	10.23	10.20	10.25	0.72
		(1.03)	(1.02)	(1.05)	
Log(Total Monetary Assets + 1)	915	7.92	7.91	7.92	0.03
		(2.17)	(2.07)	(2.25)	
Percentage of Households with Access to the Financial System (Formal and/or Informal)	915	0.68	0.69	0.66	-0.65
		(0.47)	(0.46)	(0.47)	
Percentage of Households with Money in a ROSCA	915	0.17	0.17	0.18	0.47
		(0.38)	(0.37)	(0.38)	
Log(Total Money in ROSCA + 1)	915	1.50	1.45	1.56	0.47
		(3.32)	(3.27)	(3.37)	
Percentage of Households with Money in an MFI	915	0.56	0.58	0.54	-1.25
		(0.50)	(0.49)	(0.50)	
Log(Total Money in MFIs + 1)	915	4.49	4.62	4.36	-0.96
		(4.10)	(4.05)	(4.14)	
Percentage of Households with Money in a Bank	915	0.15	0.14	0.17	0.89
		(0.36)	(0.35)	(0.37)	
Log(Total Money in Bank Accounts + 1)	915	1.38	1.25	1.50	1.15
		(3.30)	(3.12)	(3.46)	
Log(Total Amount of Cash at Home + 1)	915	6.39	6.32	6.45	1.07
		(1.93)	(1.91)	(1.95)	
Total Nonmonetary Assets	915	30,475.59	30,637.68	30,320.77	-0.17
		(28,595.00)	(29,368.14)	(27,867.15)	
Log(Total Nonmonetary Assets + 1)	915	9.87	9.88	9.87	-0.12
		(1.30)	(1.24)	(1.36)	
Log(Nonmonetary Assets from Consumer Durables + 1)	915	9.87	9.88	9.87	-0.12
		(1.30)	(1.24)	(1.36)	
Log(Nonmonetary Assets from Livestock + 1)	915	3.56	3.35	3.76	1.45
		(4.25)	(4.24)	(4.26)	
Grams of Gold in Savings	915	11.99	11.67	12.30	0.61
		(15.59)	(14.89)	(16.24)	
Liabilities					
Total Amount Owed BY the Household	915	43,269.18	38,889.30	47,452.53	1.36
		(95,422.21)	(92,431.79)	(98,109.61)	
Log(Total Amount Owed BY the Household + 1)	915	8.56	8.38	8.73	1.56
		(3.33)	(3.45)	(3.21)	
Percentage of Households with Outstanding Loans	915	0.90	0.88	0.91	1.42
		(0.31)	(0.32)	(0.29)	
Received Loan from Grocery/Shop	915	0.41	0.40	0.42	0.77
		(0.49)	(0.49)	(0.49)	
Received Loan from MFI	915	0.39	0.38	0.40	0.53
		(0.49)	(0.49)	(0.49)	
Received Loan from Family/Friends/Neighbors	915	0.32	0.35	0.30	-1.54
		(0.47)	(0.48)	(0.46)	
Received Loan from Moneylender	915	0.12	0.10	0.15	2.06*
		(0.33)	(0.30)	(0.33)	
Received Loan from Bank	915	0.05	0.04	0.05	0.48
		(0.21)	(0.20)	(0.22)	
Received Loan from Dhukuti	915	0.03	0.02	0.03	0.90
		(0.16)	(0.15)	(0.18)	

Note: differences statistically significant at the *10%, **5%, or ***1% level.

Table 1C: Descriptive Statistics at Baseline by Treatment Status

	Obs.	Mean Sample	Mean Control	Mean Treatment	T-stat
Attitudes Towards Saving/Spending					
I never save.	910	0.18	0.20	0.17	-1.11
		(0.39)	(0.40)	(0.38)	
When I have a little cash, I spend it rather than save it.	903	0.41	0.42	0.40	-0.39
		(0.49)	(0.49)	(0.49)	
My spouse or any adult member of the household typically wants to save more than I do.	861	0.40	0.44	0.38	-1.79*
		(0.49)	(0.50)	(0.49)	
I often find that I regret spending money.	899	0.47	0.47	0.47	-0.14
		(0.50)	(0.50)	(0.50)	
I typically want to save more than my spouse or any other adult member of the household.	886	0.56	0.55	0.58	0.79
		(0.50)	(0.50)	(0.50)	
I like to have savings that I can control so I can spend on priorities over emergencies.	902	0.67	0.65	0.70	1.58
		(0.47)	(0.48)	(0.46)	
I would like to have a mechanism to separate money from my spouse or any other adult	891	0.55	0.55	0.55	0.02
		(0.50)	(0.50)	(0.50)	
Network Support and Beliefs					
If I ask someone (a relative, friend, or neighbor) for money, and s/he has some, then s/he	911	0.74	0.76	0.72	-1.17
		(0.44)	(0.43)	(0.45)	
If I start saving money, people will think that I am rich and will ask me for more money.	895	0.37	0.35	0.38	1.06
		(0.48)	(0.48)	(0.49)	
It is difficult to save because when I have some money set aside, my relatives ask for it.	893	0.45	0.48	0.43	-1.45
		(0.50)	(0.50)	(0.50)	
It is difficult to save because when I have some money set aside, my friends ask for it.	892	0.40	0.41	0.38	-0.95
		(0.49)	(0.49)	(0.49)	
It is difficult to save because when I have some money set aside, my neighbors ask for it.	890	0.44	0.46	0.42	-1.14
		(0.50)	(0.50)	(0.50)	
If someone (a relative, friend, or neighbor) is in need and asks me for money, then I help him/her and give up saving.	903	0.62	0.63	0.62	-0.16
		(0.49)	(0.49)	(0.49)	
If I do not help someone (a relative, friend, or neighbor) with money if they are in need, then s/he will not help me in the future when I need help.	899	0.72	0.72	0.73	0.13
		(0.45)	(0.45)	(0.45)	
If someone (a relative, friend, or neighbor) that has some money does not help me when I am in need, then I will not help him/her in the future.	903	0.59	0.59	0.58	-0.44
		(0.49)	(0.49)	(0.49)	

Note: all variables are indicators equal to 1 if "strongly agree" or "agree," and 0 if "neutral," "disagree," or "strongly disagree." Differences statistically significant at the *10%, **5%, or ***1% level.

Table 2A: Network Descriptive Statistics at Baseline by Treatment Status

		Mean			
		Sample	**Control**	**Treatment**	**T-stat**
regular partners	*total*	1.422	1.394	1.449	0.61
		(1.369)	(1.352)	(1.387)	
	within village	0.636	0.620	0.652	0.53
		(0.915)	(0.944)	(0.888)	
	females	0.981	0.953	1.009	0.73
		(1.156)	(1.127)	(1.183)	
	relatives	0.540	0.544	0.536	-0.13
		(0.853)	(0.856)	(0.851)	
	within Pokara region	0.557	0.546	0.568	0.39
		(0.872)	(0.837)	(0.905)	
	outside Pokara region	0.125	0.136	0.113	-0.78
		(0.446)	(0.484)	(0.406)	
gifts received	*total*	0.473	0.447	0.498	0.84
(12 months, reg.)		(0.914)	(0.858)	(0.965)	
	within village	0.235	0.221	0.248	0.65
		(0.619)	(0.593)	(0.643)	
gifts given	*total*	0.321	0.277	0.363	1.71
(12 months, reg.)		(0.761)	(0.724)	(0.794)	
	within village	0.173	0.148	0.197	1.36
		(0.548)	(0.497)	(0.592)	
loans received	*total*	1.207	1.181	1.231	0.6
(12 months, reg.)		(1.251)	(1.248)	(1.255)	
	within village	0.526	0.508	0.543	0.64
		(0.820)	(0.839)	(0.802)	
loans given	*total*	0.692	0.658	0.724	0.93
(12 months, reg.)		(1.089)	(1.064)	(1.112)	
	within village	0.343	0.313	0.372	1.22
		(0.729)	(0.728)	(0.731)	

Table 2B: Network Descriptive Statistics at Baseline by Treatment Status

		Mean			
		Sample	**Control**	**Treatment**	**T-stat**
occasional partners	*total*	0.309	0.311	0.308	-0.07
		(0.676)	(0.696)	(0.657)	
	relatives	0.245	0.239	0.250	0.28
		(0.576)	(0.571)	(0.581)	
	within village	0.068	0.060	0.075	0.83
		(0.264)	(0.238)	(0.287)	
gifts received	*total*	0.069	0.072	0.066	-0.27
(1 month, occ.)		(0.304)	(0.291)	(0.317)	
	average value	68.470	102.908	35.577	-1.3
		(764.113)	(1071.624)	(209.224)	
gifts given	*total*	0.130	0.132	0.128	-0.14
(1 month, occ.)		(0.413)	(0.427)	(0.399)	
	average value	134.030	172.754	97.045	-0.6
		(1874.034)	(2392.278)	(1185.392)	
loans received	*total*	0.082	0.083	0.081	-0.08
(1 month, occ.)		(0.294)	(0.284)	(0.303)	
	average value	1671.247	1540.183	1796.429	0.26
		(14666.660)	(14017.050)	(15275.300)	
loans given	*total*	0.048	0.043	0.053	0.75
(1 month, occ.)		(0.219)	(0.213)	(0.225)	
	average value	244.856	202.573	285.242	0.62
		(2035.334)	(1843.246)	(2204.459)	

Table 3: Attrition Regressions with Network Variables

		Completed Endline	
		(1)	(2)
regular partners	*total*	-0.0038	-0.0037
		(0.787)	(0.796)
	within village	0.0124	0.0125
		(0.241)	(0.244)
occasional partners	*total*	0.0499	0.0503
		(0.384)	(0.378)
	within village	-0.0666	-0.0674
		(0.164)	(0.156)
gifts 12 months (reg.)	*received*	-0.0128	-0.0127
		(0.370)	(0.376)
	given	0.0155	0.0149
		(0.310)	(0.335)
loans 12 months (reg.)	*received*	0.0015	0.0014
		(0.894)	(0.903)
	given	0.0147	0.0147
		(0.258)	(0.258)
gifts 1 month (occ.)	*received*	-0.0017	-0.0019
		(0.971)	(0.967)
	given	-0.0051	-0.0053
		(0.935)	(0.932)
loan 1 month (occ.)	*received*	-0.0615	-0.0613
		(0.368)	(0.364)
	given	-0.0520	-0.0523
		(0.475)	(0.471)
itt_i			0.0086
			(0.598)
Constant		0.9231***	0.9193***
		(0.000)	(0.000)
Village dummies		yes	yes
Observations		1,009	1,009
R-squared		0.069	0.069

Note: Robust standard errors, clustered at the village level, reported in parenthesis. Statistically significant coefficients are indicated as follows: *10%; **5%; ***1%.

Table 4: Individual Intent-To-Treat Regressions

	Regular Partners		Occasional Partners	
	Total	Village	Total	Village
	(1)	(2)	(3)	(4)
itt_i	0.0378	0.0893**	0.0837**	-0.0214**
	(0.0873)	(0.0377)	(0.0321)	(0.00907)
Village Dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	915	915	915	915
R-squared	0.187	0.061	0.156	0.047

	Gifts from Regular Partners (12 months)		Loans from Regular Partners (12 months)	
	Received	Given	Received	Given
	(5)	(6)	(7)	(8)
itt_i	-0.000919	0.0209	0.0182	0.00703
	(0.0377)	(0.0156)	(0.0665)	(0.0370)
Village Dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	915	915	915	915
R-squared	0.104	0.062	0.132	0.085

	Gifts from Occasional Partners (1 month)		Loans from Occasional Partners (1 month)	
	Received	Given	Received	Given
	(9)	(10)	(11)	(12)
itt_i	0.0484***	0.0250	0.00882	0.00955
	(0.0151)	(0.0219)	(0.0231)	(0.0136)
Village Dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	915	915	915	915
R-squared	0.064	0.062	0.061	0.043

Note: Robust standard errors, clustered at the village level, reported in parenthesis. Statistically significant coefficients are indicated as follows: *10%; **5%; ***1%. Controls include age, years of education, household size, number of children less than 16 years of age.

Table 5: Individual Intent-To-Treat Regressions with Spillovers

	Regular Partners		Occasional Partners	
	Total	**Village**	**Total**	**Village**
	(1)	(2)	(3)	(4)
itt_i	0.0298	0.0854**	0.0675**	-0.0231**
	(0.0875)	(0.0379)	(0.0313)	(0.00917)
$W^t\, itt_i$	0.157**	0.0774	0.318***	0.0336
	(0.0669)	(0.0535)	(0.0744)	(0.0241)
Village Dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	915	915	915	915
R-squared	0.192	0.063	0.180	0.050
	Gifts from Regular Partners (12 months)		**Loans from Regular Partners (12 months)**	
	Received	**Given**	**Received**	**Given**
	(5)	(6)	(7)	(8)
itt_i	-0.000975	0.0194	0.0114	0.00384
	(0.0391)	(0.0154)	(0.0653)	(0.0390)
$W^t\, itt_i$	0.00110	0.0294	0.135**	0.0629
	(0.0412)	(0.0300)	(0.0504)	(0.0541)
Village Dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	915	915	915	915
R-squared	0.104	0.065	0.136	0.087
	Gifts from Occasional Partners (1 month)		**Loans from Occasional Partners (1 month)**	
	Received	**Given**	**Received**	**Given**
	(9)	(10)	(11)	(12)
itt_i	0.0480***	0.0225	0.00821	0.00845
	(0.0154)	(0.0213)	(0.0236)	(0.0132)
$W^t\, itt_i$	0.00706	0.0485*	0.0121	0.0215
	(0.0231)	(0.0273)	(0.0226)	(0.0158)
Village Dummies	yes	yes	yes	yes
Controls	yes	yes	yes	yes
Observations	915	915	915	915
R-squared	0.064	0.067	0.061	0.046

Note: Robust standard errors, clustered at the village level, reported in parenthesis. Statistically significant coefficients are indicated as follows: *10%; **5%; ***1%. Controls include age, years of education, household size, number of children less than 16 years of age.

Table 6: Directed Dyadic Intent-To-Treat Regressions

Transfer:	**Loan**	**Gift**	**Loan or Gift**
	(1)	(2)	(3)
itt_i	0.001	0.0004*	0.001*
	(0.001)	(0.000)	(0.001)
itt_j	0.000	0.000	0.000
	(0.001)	(0.000)	(0.001)
g^t_{ij}	0.126***	0.025**	0.125***
	(0.021)	(0.012)	(0.014)
$itt_i \cdot itt_j$	0.001	0.001**	0.001
	(0.001)	(0.000)	(0.001)
$itt_i \cdot g^t_{ij}$	-0.039	-0.005	-0.033
	(0.032)	(0.024)	(0.032)
$itt_j \cdot g^t_{ij}$	0.054*	-0.003	0.044**
	(0.027)	(0.021)	(0.020)
$itt_i \cdot itt_j \cdot g^t_{ij}$	0.030	0.032	0.049
	(0.051)	(0.038)	(0.040)
Village Dummies	yes	yes	yes
Controls	yes	yes	yes
Mean of Dep. Var.	0.0067	0.0012	0.0074
Observations	56308	50970	56308

Note: Marginal effects computed with the Delta method (Ai and Norton 2003). Robust standard errors, clustered at the village level, reported in parenthesis. Statistically significant coefficients are indicated as follows: *10%; **5%; ***1%. Controls include age, years of education, household size, number of children less than 16 years of age, for both i and j.

Table 7: Peer Effects Regressions, Monetary and Non-Monetary Assets

	Monetary Assets			**Non-monetary Assets**		
	(1)	(2)	(3)	(1)	(2)	(3)
itt	0.421***	0.450***	0.450***	0.0866	0.0866	0.0924
	(0.131)	(0.119)	(0.109)	(0.0637)	(0.0635)	(0.0661)
$\mathbf{W^t\,\Delta y}$		0.0168	0.111**		-0.0243	0.0141
		(0.0532)	(0.0397)		(0.0241)	(0.0334)
$\mathbf{\Delta W\,y^t}$			-0.00539			0.00131
			(0.00443)			(0.00151)
$\mathbf{\Delta W\,\Delta y}$			0.127**			0.0538*
			(0.0518)			(0.0293)
$\mathbf{W^t}$ **itt**		-0.442***	-0.572***		-0.00294	-0.0899
		(0.102)	(0.167)		(0.0886)	(0.115)
$\mathbf{\Delta W}$ **itt**			-0.0910			-0.133
			(0.201)			(0.114)
Constant	0.393**	0.476***	0.460***	0.112*	0.116	0.132*
	(0.147)	(0.151)	(0.141)	(0.0601)	(0.0682)	(0.0641)
Observations	915	915	915	915	915	915
R-squared	0.011	0.017	0.029	0.002	0.002	0.006

Note: Robust standard errors, clustered at the village level, reported in parenthesis. Statistically significant coefficients are indicated as follows: *10%; **5%; ***1%. All variables in natural logs.

Table 8: Peer Effects Regressions, Children and Education Expenditures

	Children Expenditures			Education Expenditures		
	(1)	(2)	(3)	(1)	(2)	(3)
itt	-0.311	-0.318	-0.314	0.473	0.421	0.401
	(0.271)	(0.299)	(0.291)	(0.340)	(0.347)	(0.330)
$\mathbf{W^t\,\Delta y}$		0.149***	0.263***		0.150*	0.229***
		(0.0348)	(0.0379)		(0.0732)	(0.0754)
$\mathbf{\Delta W\,y^t}$			0.102***			0.0407
			(0.0307)			(0.0393)
$\mathbf{\Delta W\,\Delta y}$			0.225***			0.160**
			(0.0388)			(0.0585)
$\mathbf{W^t}$ **itt**		-0.173	-0.312		-0.113	-0.307
		(0.492)	(0.567)		(0.489)	(0.513)
$\mathbf{\Delta W}$ **itt**			-0.537			-0.389
			(0.339)			(0.366)
Constant	0.0993	0.209	0.221	-0.273	-0.226	-0.189
	(0.433)	(0.424)	(0.405)	(0.491)	(0.392)	(0.363)
Observations	915	915	915	915	915	915
R-squared	0.001	0.025	0.064	0.003	0.027	0.053

Note: Robust standard errors, clustered at the village level, reported in parenthesis. Statistically significant coefficients are indicated as follows: *10%; **5%; ***1%. All variables in natural logs.

Table 9: **Peer Effects Regressions, Health and Total Non-food Expenditures**

	Health Expenditures			**Total Non-food Expenditures**		
	(1)	(2)	(3)	(1)	(2)	(3)
itt	-0.164	-0.0680	-0.0729	-0.000220	0.0306	-0.0675
	(0.269)	(0.282)	(0.289)	(0.157)	(0.178)	(0.158)
$\mathbf{W^t\,\Delta y}$		0.0477	0.0832		0.206***	0.261***
		(0.0537)	(0.0667)		(0.0609)	(0.0630)
$\mathbf{\Delta W\,y^t}$			0.0131			0.0379***
			(0.0371)			(0.0108)
$\mathbf{\Delta W\,\Delta y}$			0.0584			0.156***
			(0.0617)			(0.0475)
$\mathbf{W^t}$ **itt**		-1.323***	-1.244**		-0.742**	-0.575*
		(0.372)	(0.544)		(0.280)	(0.285)
$\mathbf{\Delta W}$ **itt**			0.0361			-0.106
			(0.475)			(0.188)
Constant	0.681*	0.946**	0.908**	1.351***	1.357***	1.244***
	(0.389)	(0.350)	(0.362)	(0.430)	(0.393)	(0.352)
Observations	915	915	915	915	915	915
R-squared	0.000	0.018	0.020	0.000	0.050	0.110

Note: Robust standard errors, clustered at the village level, reported in parenthesis. Statistically significant coefficients are indicated as follows: *10%; **5%; ***1%. All variables in natural logs.

Appendix Table A1: Attrition

	Completed Endline		
	(1)	(2)	(3)
itt^i: Offered the Savings Account	0.011	0.009	0.011
	(0.016)	(0.015)	(0.015)
Age of female HH head			0.001
			(0.001)
Years of schooling			-0.000
			(0.004)
Married/living with partner			0.002
			(0.041)
# children below 16			0.010
			(0.010)
# HH members			0.004
			(0.006)
Main source of HH income			-0.001
			(0.001)
Constant	0.901***	0.878***	0.837***
	(0.026)	(0.008)	(0.051)
Village dummies	No	Yes	Yes
Obs.	1,009	1,009	1,005
R^2 (overall)	0.001	0.056	0.059
Mean of Dependent Variable		0.91	

Note: Robust standard errors clustered at the village level in parenthesis. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

Appendix Table A2: Descriptive Statistics of Variables Used in the Analysis

	Mean	**Min**	**Max**	**S.D.**
itt_i	0.508	0	1	0.500
itt_j	0.508	0	1	0.500
g^{t}_{ij}: gifts	0.008	0	1	0.091
g^{t}_{ij}: loans	0.004	0	1	0.062
g^{t}_{ij}: loans and gifts	0.010	0	1	0.098
g^{t+1}_{ij}: gifts	0.007	0	1	0.082
g^{t+1}_{ij}: loans	0.001	0	1	0.035
g^{t+1}_{ij}: loans and gifts	0.008	0	1	0.087

Appendix Table A3: Descriptive Statistics at of Individual Variables

		Mean	Min	Max	s.d.
regular partners (t+1)	total	1.11	0	6	0.84
occasional partners (t+1)	total	0.22	0	5	0.55
regular partners (t+1)	village	0.57	0	5	0.75
occasional partners (t+1)	village	0.04	0	2	0.22
gifts (12 months, reg., t+1)	received	0.16	0	3	0.43
gifts (12 months, reg. t+1)	given	0.04	0	2	0.23
loans (12 months, reg . t+1)	received	0.86	0	6	0.74
loans (12 months, reg. t+1)	given	0.29	0	4	0.56
gifts (1 months, occ. t+1)	received	0.08	0	3	0.33
gifts (1 months, occ. t+1)	given	0.04	0	3	0.25
loans (1 months, occ. t+1)	received	0.08	0	2	0.30
loans (1 months, occ. t+1)	given	0.02	0	2	0.16
years of education (t)		2.52	0	14	2.82
age (t)		36.80	16	99	12.51
children <16 yrs (t)		1.97	0	7	1.25
household size (t)		4.55	1	12	1.66
itt		0.51	0	1	0.50
$W^t\, itt$		0.24	0	1	0.39
$\Delta W\, itt$		0.03	-1.00	1.00	0.50
log non monetary assets	$W^t\, \Delta y$	0.12	-2.21	8.56	0.72
	$\Delta W\, y^t$	-0.22	-92.42	60.79	12.73
	$\Delta W\, \Delta y$	0.01	-11.49	8.53	1.00
log monetary assets	$W^t\, \Delta y$	0.46	-10.99	13.93	1.94
	$\Delta W\, y^t$	-0.19	-91.63	50.77	10.78
	$\Delta W\, \Delta y$	0.04	-11.21	15.81	2.29
log children expenditures	$W^t\, \Delta y$	-0.44	-20.10	25.70	4.43
	$\Delta W\, y^t$	-0.52	-48.83	21.92	4.91
	$\Delta W\, \Delta y$	0.62	-21.92	22.79	5.10
log health expenditures	$W^t\, \Delta y$	0.02	-19.84	24.82	4.11
	$\Delta W\, y^t$	-0.51	-19.68	16.52	3.92
	$\Delta W\, \Delta y$	0.45	-24.82	22.63	4.80
log education expenditures	$W^t\, \Delta y$	0.04	-20.50	28.14	4.52
	$\Delta W\, y^t$	-0.21	-33.81	20.50	4.95
	$\Delta W\, \Delta y$	0.23	-21.06	30.50	5.29
log total non-food expenditures	$W^t\, \Delta y$	0.75	-9.87	33.33	3.22
	$\Delta W\, y^t$	-0.72	-77.63	26.99	8.40
	$\Delta W\, \Delta y$	0.72	-15.68	37.79	4.47