Intervention-Driven Changes in Social Networks and their Effects on Household Outcomes*

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Abstract

We study how social networks change as a result of an exogenous expansion in formal financial access and show how to estimate the effects of these changes on household outcomes. We use a unique household panel dataset that contains detailed information on the network of informal financial transactions before and after a field experiment that randomized access to savings accounts in Nepal. First, we provide evidence that the exogenous intervention affected the network of informal financial transactions. Second, we propose a dynamic model of peer effects in household expenditure that accounts for changes in the network due to the intervention. We show that disregarding such changes would lead to downward-biased peer-effect estimates.

JEL codes: C31; D85; G2; O16

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1 Introduction

A large literature has documented how new products and technologies spread through social networks.[1] All papers studying how social networks help diffuse the effects of a given intervention implicitly assume that the network structure is fixed. However, one may also argue that social networks change over time in response to interventions. In fact, anecdotal evidence and a few theoretical contributions suggest that the structure of networks evolves strategically with time.[2]

No previous empirical study has explored how a given intervention affects the structure of a pre-existing social network, or estimated the effects of intervention-driven network changes on economic outcomes. Two possible reasons are the lack of a suitable methodology, and the lack of appropriate network data. In fact, all previous studies on networks and diffusion have relied on network data collected at one point in time (*e.g.* Oster and Thornton 2008, Bramoullé, Djebbari, and Fortin 2009, Calvó-Armengol, Patacchini, and Zenou 2009, Banerjee *et al.* 2012, Cai 2012).

Our paper intends to fill this gap. Our contributions are both empirical and methodological. First, we show that an exogenous intervention, namely an expansion in formal financial access, affects the structure of the pre-existing network of informal financial transactions. Second, using a peer-effect estimation framework we propose and illustrate a new method to evaluate the spillover effects of the intervention, *i.e.* the effects of the intervention-driven network changes on household outcomes.

We take advantage of a field experiment that randomized access to savings accounts among all households living in 19 villages in Nepal. The savings account represented the first access to the formal financial system for the vast majority of the population

[1]Existing studies cover several different outcomes. These include financial products' adoption and decision making (Duflo and Saez 2003, Hong, Kubik, and Stein 2003, Banerjee *et al.* 2012, Cai 2012), technology adoption in agriculture (Ryan and Gross 1943, Foster and Rosenzweig 1995, Conley and Udry 2010, Maertens 2012), drug adoption (Coleman, Katz, and Menzel 1966, Kremer and Miguel 2007, Miguel and Kremer 2004), risk sharing (Ambrus, Mobius, and Szeidl 2010), building trust (Karlan *et al.* 2009), getting a job (Pistaferri 1999, Munshi 2004), productivity in the workplace (Mas and Moretti 2009), voting behavior (Lazarsfeld, Berelson, and Gaudet 1944) and criminal behavior (Glaeser, Sacerdote, and Scheinkman 1996, Bayer, Hjalmarsson, and Pozen 2009). See Jackson (2008), Jackson (2010), and Jackson and Yariv (2010) for an extensive review.

[2]The issue of network evolution has been explored by theorists under specific assumptions (*e.g.* Watts 2001, Jackson and Watts 2002).

sample (Prina 2013). This exogenous variation in financial access may have changed the network of financial transactions. The effects could be both positive and negative. On the one side, access to a savings account allows households to accumulate a buffer stock that can be used to smooth consumption or to cope with negative shocks. Hence, it might offer a partial substitute for informal financial arrangements. As a result, informal transactions may be crowded out, reducing the level of mutual insurance and diminishing the effect of access to savings accounts on welfare (Ligon, Thomas, and Worrall 2000, Platteau 2000). On the other hand, access to savings can foster asset accumulation. Hence, households with greater resources might increase transfers to others, either because of altruism, or in fear of social sanction (Platteau 2000, Hoff and Sen 2006, Comola and Fafchamps 2010, Di Falco and Bulte 2011).

Our unique panel dataset of all households living in 19 Nepalese villages contains detailed information on the network of informal financial transactions (*i.e.* all loans and gifts given and received) before and after the randomized intervention. Our study takes advantage of the unique combination of three features of the data: the within-village randomization, the high take-up rate of the savings product offered (84%), and the availability of detailed census network data before and after the intervention. This allows us to investigate the impact of the intervention on the network and to propose and implement a methodology to estimate the spillover effects of the intervention.

Using household and dyadic regressions we show that the intervention had a significant impact on the pre-existing network of informal financial transactions. First, we run household-level regressions to show that exogenous access to a savings account increased many proxies of network activity *within* the village (*e.g.* the number of partners, number of loans and gifts). Second, we take as observations all directed financial transactions between sampled households and we run dyadic regressions, in order to account for the fact that decisions to form or sever a link are two-sided. To the best of our knowledge, this specification is novel to the literature, as it combines the randomized experiment with post-intervention dyadic data. Results show that being offered the savings account increases the probability of giving both loans and gifts as well as their magnitude. Overall, our results show that the intervention has *increased* the network activity within the village, suggesting that there might be complementarity between formal savings and informal network-based financial activities.

The second contribution of our study is methodological. Having shown that the network responded to the exogenous intervention, we argue that it may be incorrect to estimate the impact of an intervention on the basis of the pre-intervention network data only. Hence, we propose a method to incorporate these intervention-driven changes in the network into a model of peer effects. The peer-effect literature aims at distinguishing three effects: the effect of one's exogenous characteristics on her outcome, the effect of her partners' exogenous characteristics, and the effect of her partners' outcomes (Mansky 1993). We argue that there may be another effect to be taken into account: the effect of the intervention-driven network changes on outcome. This additional effect, that we call spillover effects of the intervention, is not accounted for in the standard ('static') peer-effect framework.

We consider household expenditure as the outcome of interest and set up a dynamic peer-effect model of expenditure where the interaction matrix, which represents village-level social interactions, can change over time in response to the exogenous intervention. We show how the change in the mean expenditure of one's partners can be decomposed into three terms: the change in her partners' mean expenditure keeping partners constant, the change in her partners' mean expenditure keeping expenditure constant, and the cross term which accounts for the combined effect of the expenditure change and the network change. While the first term also belongs to a static peer-effect model, the other two terms only appear in the dynamic peer-effect model and represent the spillover effects of the intervention. We argue that omitting the two terms accounting for the spillover effects of the intervention leads to a biased estimate of the peer effect.

We illustrate this dynamic peer-effect model using our data from Nepal. The panel structure of our data allows us to estimate the model in first differences, which helps addressing endogeneity from correlated unobservables. To address endogeneity arising from the model simultaneity we follow the conventional strategy of using 'lagged' household characteristics (*i.e.* the characteristics of partner's partners) as identifying instruments for the outcome of one's partners (Kelejian and Prucha 1998, Bramoullé, Djebbari, and Fortin 2009, Patacchini and Zenou 2012). Overall, we find strong evidence of peer effects in expenditure: an increase in partners' expenditure, whether it comes from old partners or new partners, has a positive effect on one's expenditure. Specifically, we find significant evidence of spillover effects: the intervention-driven changes in the network matter for the household's expenditure, and disregarding these spillover effects leads to downward-biased peer-effect estimates in the static model.

The main message of our paper is that social networks evolve in response to exogenous interventions. Hence, in order to capture the actual importance of social networks in spreading products and technologies, it is crucial to take into account how the increasing availability of these products and technologies changes the pre-existing social network. As such, our study provides novel insights on the way we should draw inference based on social network data. The implicit assumption grounding all studies on social networks and diffusion is that pre-existing relationships matter for economic outcomes, *i.e.* a better social network predicts better future outcomes. This assumption is appropriate in a setting where the network is fixed, or hard to change. However, it is also possible that the social network rewires easily in response to changes in the economic environment, and new links can be formed irrespective of pre-existing relationships. This might be the case when considering financial interventions, as we show in this study. Hence, more caution is recommended in interpreting pre-existing links in a casual manner, and in drawing policy recommendations.[3]

Our paper adds to the growing literature studying the effects of networks on economic outcomes. Among the papers relying on a randomized intervention to identify the causal effect of social networks there are Banerjee *et al.* (2012), Cai (2012), Duflo and Saez (2003), Duflo, Kremer, and Robinson (2008), Dupas (2010), Kremer and Levy (2008), Kremer and Miguel (2007), Kling, Liebman, and Katz (2007), Oster and Thornton (2008). And, among the studies that use non-experimental methods to identify the causal effects of networks, we find Bandiera and Rasul (2006), Bertrand, Luttmer, and Mullainathan (2000), Card and Giuliano (forthcoming), Conley and Udry (2010), Foster and Rosenzweig (1995), Imberman, Kugler, and Sacerdote (2009), Munshi (2003), and Munshi (2004). Most previous studies do not have detailed dyad-level network information. Thus, they identify the individual reference group on the basis of the re-

[3]A similar argument is made by Comola and Fafchamps (2012) in the context of network formation.

spondents' social context. Notable exceptions are, for example, Banerjee *et al.* (2012), Oster and Thornton (2008), and Cai (2012), that, similar to our case, have dyadic data on the links between households in the sample, but, differently from us, exploit pre-intervention network data only.

Since our study is the first one collecting and exploiting panel data on the social network, we are able to challenge the conventional assumption that the network is not affected by the intervention. Furthermore, we contribute to the literature estimating peer effects through social network data in two respects. First, we propose a new dynamic peer-effect framework incorporating the spillover effects of the intervention. Second, by taking differences at the household and dyadic level rather than at the partners' level (Bramoullé, Djebbari, and Fortin 2009, Calvó-Armengol, Patacchini, and Zenou 2009, Lee, Liu and Lin 2010), we are able to address the problem of correlated unobservables more convincingly than before.

Finally, our study also adds new evidence to the large literature in development economics that investigates how access to financial products shapes the lives of the poor (Aportela 1999, Banerjee *et al.* 2010, Banerjee *et al.* 2012, Bruhn and Love 2009, Burgess and Pande 2005, Carvalho, Prina and Sydnor 2013, Dupas and Robinson 2013, Kaboski and Townsend 2011, Karlan and Zinman 2010, Prina 2013). We take a new angle on this issue by exploring whether financial access might have an indirect effect on household behavior through the fact that such access shapes the pre-existing social network. As such, our study relates to the recent literature studying how access to savings accounts interacts with informal financial arrangements. On the one side, research has shown that savings and commitment savings products might make it easier to resist requests for sharing with friends and family (Dupas and Robinson forthcoming, Brune *et al.* 2011). On the other side, one's social network can be used as a commitment device to save actively in a savings account (Kast, Meier, and Pomeranz 2011). Finally, few studies have analyzed how formal financial access might affect sharing arrangements. The evidence is not clearcut. For example, Feigenberg, Field, and Pande (2012) and Heinrich et al. (2010) find positive effects, while Binzel, Field, and Pande (2012) and Conning and Udry (2010) find negative effects.

The following section describes the field experiment, the savings account, and the network data. Section 3 provides evidence that the exogenous expansion in formal financial access impacted the network of informal financial transactions between sampled households. In Section 4 We introduce the dynamic peer-effect model incorporating the spillover effects of the intervention, and we estimate it on our Nepalese data. Section 5 concludes. Tables and appendix tables are reported at the end of the paper.

2 Experimental Design and Background

2.1 Financial Institutions and the Savings Account Offered

Formal financial access in Nepal is very limited. According to the nationally representative "Access to Financial Services Survey," conducted in 2006 by the World Bank (Ferrari, Jaffrin, and Shrestha 2007) only 20% of Nepalese households have a bank account. Not surprisingly, access is concentrated in urban areas and among the wealthy. Thus, most households typically save informally, storing cash at home, saving in the form of durable goods and livestock, or participating to Rotating Savings and Credit Associations (ROSCAs).[4]

In the randomized field experiment described in Prina (2013), GONESA bank gave access to savings accounts to a random sample of poor households in 19 villages surrounding Pokhara, Nepal's second largest city. The accounts 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. The bank does not charge any opening, maintenance, or withdrawal fees and pays a 6% nominal yearly interest, similar to the average alternatives available in the Nepalese market (Nepal Rastra Bank, 2011).[5] In addition, the savings account does not have a minimum balance requirement.[6] Customers can make transactions at the local bank-branch offices

[4]A ROSCA is a savings group formed by individuals who decide to make regular cyclical contributions to a fund in order to build together a pool of money, which then rotates among group members, being given as a lump sum to one member in each cycle.

[5]The International Monetary Fund Country Report for Nepal (2011) indicates a 10.5% rate of inflation during the intervention period.

[6]The money deposited in the savings account is fully liquid for withdrawal. The savings account is fully flexible and operates without any commitment to save a given amount or to save for a specific purpose.

in the villages, which are open twice a week for about three hours, or at the bank's main office, located in downtown Pokhara, during regular business hours.

2.2 Experimental Design and Data

A first baseline survey was conducted in February 2009 in the 19 villages to census all households with a female head aged 18-55.[7,8] This round of data contains information on households' socio-economic characteristics and their network of informal financial transactions. Before the introduction of the savings accounts, a second baseline survey was conducted during May 2010. This survey collected information on households' socio-economic characteristics but did not collect network data. The baseline characteristics of our estimation sample are computed on the basis of these two data rounds.[9]

After completion of the second baseline survey, GONESA bank progressively began operating in the villages between the last two weeks of May and the first week of June 2010. Separate public lotteries were held in each village to randomly assign the female household heads to treatment and control groups. The women assigned to the treatment group were offered the option to open a savings account at the local bank-branch office, while the women assigned to the control group were not given this option.

An endline survey was conducted starting in June 2011, a year after the beginning of the intervention. This survey collected information on households' socio-economic characteristics and on the network of informal financial transactions. A total of 1,009 households were surveyed in both the first and second baseline. 91% of these households (*i.e.* 915) were found and surveyed in the endline survey.[10]

[7]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 villages were surveyed by the enumerators. The female household head is also the survey respondent, and the savings account owner.

[8]The population in the villages ranged from 20 to 150 households.

[9]Network and expenditure data come from the first baseline survey, while most demographic information comes from the second baseline.

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

2.3 Sample Characteristics and Balance Check

Table 1 shows the summary statistics of baseline characteristics, separately for treatment and control groups, for our panel estimation sample of 915 households. The last column in the table shows the t-statistic of two-way tests of the equality of the means across the treatment and control group and reveals that randomization generally led to balance along baseline characteristics. The women participating in the savings experiment are very poor. They have on average 2.5 years of schooling, and live in households whose weekly household income average 1,500 Nepalese rupees (about $20) and with household assets amounting to a little more than 44,000 rupees (about $630).[11,12] Households have on average 4.5 members with 2 children. The sample seems highly vulnerable to shocks: 42% of the households indicated having experienced a negative income shock during the month previous to the survey.

Only 15% of the households had a bank account before the introduction of the program. Given the lack of access to formal savings products, it is not surprising that most households typically save via microfinance institutions (MFIs) and ROSCAs. They also save by either investing in durable goods or livestock or by storing cash at home.[13] Moreover, 90% of them had at least one outstanding loan (most loans are taken from ROSCAs, MFIs, and family, friends, or neighbors). Hence, households seem to rely mostly on informal financial institutions (*e.g.* MFIs, ROSCAs, friends, family, and neighbors) rather than on formal institutions, like banks.[14] This is consistent with previous literature showing that the poor have a portfolio of transactions and financial relationships (Banerjee *et al.* 2010, Collins *et al.* 2009, Dupas and Robinson 2013).

As shown by Prina (2013) take-up and usage rates of the savings accounts offered to the treatment group were very high. In particular, more than 84% of the treatment households offered an account opened one and used it actively, depositing an average

[11]In 2010-2011, 70 Nepalese rupees approximately corresponded to 1 U.S. dollar.

[12]Household members earn income from multiple sources: working as agricultural or construction workers, collecting sand and stones, selling agricultural products, raising livestock and poultry, having a small shop, working as drivers, and receiving remittances, rents and pensions, among others.

[13]Households typically had about one week worth of household income stored at home.

[14]This is in line with the nationally representative survey conducted in 2006 by the World Bank. The survey shows that over two-thirds of Nepalese households had an outstanding loan from a formal or informal institution (Ferrari, Jaffrin, and Shrestha 2007).

of 8% of their baseline weekly household income almost once a week for the first year of the intervention. Moreover, access to the savings account considerably increased total assets. Treatment households reduced the amount of cash savings, but households do not seem to reallocate assets away from other types of savings institutions, formal or informal. Finally, access to the savings account strongly increased households' welfare, *e.g.* investments in health and education and perceived financial situation.[15] Thus, such large impacts could potentially affect the network of informal financial transactions.

2.4 The Network Data

Detailed information on the informal network-based financial transactions of each household was collected both in the first baseline survey and in the endline survey.[16] The respondent was asked to give a list of people (within or outside the village) who have exchanged gifts and/or loans with her or with other members of her household. Respondents could list as many partners as they wished.[17] For each partner, the total amount of loans and gifts given and received in the 12 months prior to the survey was collected using four brackets: less than 1200, $1200 - 2400$, $2400 - 5000$ and more than 5000 rupees.[18] Special attention was devoted to accurately match the declared partners identities to sampled households and to circumvent homonymy.[19]

Table 2 contains the network descriptive statistics at baseline by treatment status. On average, households self-reported having 1.73 financial partners, 0.7 of which within

[15]See Prina (2013) for a detailed analysis of the effects of providing access to a savings account on assets accumulation and household welfare.

[16]The availability of census network data allows us to circumvent the econometric problems outlined by Chandrasekhar and Lewis (2011).

[17]First, the information regarding regular partners (defined as individuals they *regularly* exchange loans and/or gifts with, and they could rely on *most* for financial help/support) was elicited. Then, the respondents were asked to list any other recent loans/gifts exchanged with occasional partners not already listed. Since very few occasional partners were declared (less than 7% of all within-village links at endline), in the analysis we aggregate occasional and regular partners together.

[18]For loans and gifts given or received in the last month, we also collected information on the exact amount and the reason of the transfer. However, very few respondents reported a transfer in the month prior. Hence, given the few non-zero observations, we use the ordinal measure that spans a longer period and may incorporate multiple transactions.

[19]At the end of each interview the enumerator used an updated village roster to determine, jointly with the respondent, the household identity code of the mentioned partners. Thus, the partners codes were coded into the questionnaire in the field, rather than during the data cleaning process.

the village and 0.79 among relatives (within or outside the village). Table 2 also reports the total number of gifts and loans within and outside the village, and the direction of the transfer (received vs. given). Respondents report exchanging on average 0.61 gifts within the village and 0.31 outside. The average number of gifts given and received is rather comparable, 0.42 and 0.50, respectively. Loans seem to be more frequent than gifts (on average 1 within the village and 0.87 outside the village), and more frequently received than given (1.19 vs. 0.68). Overall, Table 2 shows that treatment and comparison groups are well balanced along all network characteristics at baseline. Finally, Table 3 reports the attrition regressions for the sample of 1,009 households who completed both baseline surveys. Results show that the probability of completing the endline survey does not seem to depend neither on the treatment, nor on the network characteristics. The only exception is the number of partners within the village, which appears marginally significant.

3 The impact of the Intervention on the Network

In this section we provide evidence that the exogenous expansion in formal financial access has affected the network of informal financial transactions.

3.1 Notation

We now introduce the notation in use. 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 $\mathbf{A}_{[ij]} \equiv a_{ij}$ to indicate its $(i, j)^{th}$ entry. If $\mathbf{b}$ is a $n \times 1$ vector, we write $\mathbf{b}_{[i]} \equiv b_i$ to indicate its i^{th} row. When a matrix or a vector is indexed by time, this is indicated with a superscript to avoid confusion with the entry notation, *e.g.* we write a^t_{ij} and $\mathbf{A}^t$, where $t = 0$ represents the baseline survey and $t = 1$ represents the endline survey.

In our analysis we use the within-village network data[20] to generate four different interaction matrices: the directed binary matrix $\mathbf{G}^t$, the directed ordinal matrix $\mathbf{C}^t$, the undirected binary matrix $\mathbf{Z}^t$, and the undirected row-standardized matrix $\mathbf{W}^t$.

[20]The partners who live out of the village are omitted from the analysis since they did not participate to the randomized experiment, and hence on who we cannot apply our methodology.

Matrices $\mathbf{G}^t$ and $\mathbf{C}^t$ are used in the dyadic regressions of subsection 3.3. The matrix $\mathbf{G}^t$ represents the directed *binary* network at time t: starting from our set of n sampled households $(1, ..., n)$ for each pair ("dyad") of households ij we define g_{ij}^t as the directed binary variable which equals one if a transfer is declared from i to j at time t, and zero otherwise.[21] $\mathbf{C}^t$ is the directed *ordinal* interaction matrix, where c_{ij}^t indicates its $(i,j)^{th}$ entry and classifies the transfer from i to j at time t into a five-category scale: 0 (no transfer), 1 (less than 1,200 rupees), 2 (1,200-2,400 rupees), 3 (2,400-5,000 rupees), and 4 (more than 5,000 rupees). Both matrices $\mathbf{G}^t$ and $\mathbf{C}^t$ are block-diagonal because, by construction, only transfers within the same village are allowed. Since directed transfers do not need to be symmetric, both dyads ij and ji are included in the estimation sample.[22] Depending on the specification for $\mathbf{G}^t$ and $\mathbf{C}^t$ transfers are defined in terms of: loans only, gifts only, loans *or* gifts.

Matrices $\mathbf{Z}^t$ and $\mathbf{W}^t$ are used in the household regressions of subsection 3.2 and section 4 respectively. $\mathbf{Z}^t$ is the binary matrix of undirected transfers, whose $(i,j)^{th}$ entry is defined as $z_{ij}^t = z_{ji}^t = max(g_{ij}^t, g_{ji}^t)$. Finally, following the literature on peer effects which model the individual outcome as a function of the *mean* outcome of partners, for the peer-effect regressions of section 4 we use a row-standardized version of the undirected interaction matrix $\mathbf{W^t}$ where $w_{ij}^t = z_{ij}^t / \sum\limits_i z_{ij}^t$. Note that for both $\mathbf{Z}^t$ and $\mathbf{W}^t$ we define transfers in terms of loans *or* gifts.

3.2 Household-level Regressions

We first present a set of reduced-form results on the effect of the intervention on the level of informal financial transactions taking the household as the unit of observation. Let $network_i^1$ be a given proxy for the intensity of the network-based activity of household

[21]No self link is allowed, *i.e.* $g_{ij}^t = 0$.

[22]For each directed observation g_{ij}^t and c_{ij}^t 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, the answers to these questions should be the same, in practice they often are not. This is a common problem in the empirical literature using self-reported link data. The standard solution is 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 *et al.* 2012, Banerjee *et al.* 2012). Following this literature, whenever discrepancies arise, we take the maximum report out of the two parts involved. This is equivalent to assuming that discrepancies between survey answers correspond to under-reporting, perhaps as a result of omission mistakes.

$i = 1, ..., n$ at endline, *i.e.* at $t = 1$. Let itt_i be the intent-to-treat dummy, which takes value one if i was offered a savings account. Let x_i^0 represent the demographic characteristics of household i at baseline, *i.e.* at $t = 0$. We run two household-level intent-to-treat linear regression, the first one being:

$$network_i^1 = \beta_0 + \beta_1 itt_i + \beta_2 x_i^0 + \lambda_v + \epsilon_i^1 \tag{1}$$

where λ_v represents the village fixed effects and ϵ_i^1 is the exogenous error term, clustered at the village level to accommodate for arbitrary patterns of residuals correlations. The second specification corresponds to the augmented linear regression:

$$network_i^1 = \beta_0 + \beta_1 itt_i + \beta_2 x_i^0 + \beta_3 z^0 itt_i + \lambda_v + \epsilon_i^1 \tag{2}$$

where $z^0 itt_i = \sum_{k=1}^{n} z_{ik}^0 \cdot itt_k$ is the number of partners of household i at baseline who were offered the savings account.[23] In order to compute $z^0 itt_i$ we adopt the broadest definition of transfers which includes loans *or* gifts. Hence, a partner of household i is defined as some other household in the village whose members have given/received a loan or a gift from/to i.[24] Because of the randomized design of the intervention, the regressor $z^0 itt_i$ is arguably exogenous and it is a first proxy for the effects of the treatment status of one's partners on her level of network activity.

In Table 4 we estimate equations (1) and (2) taking as dependent variable all network statistics reported in Table 2: number of partners (within the village, outside the village, relatives, total), number of gifts (within the village, outside the village received, given), number of loans (within the village, outside the village, received, given). Socio-demographic controls at baseline include: the age of the female household head, a dummy which takes value one if the female household head has no formal education, household size, and the number of children less than 16 years of age. The

[23] Recall that z_{ik}^0 is the $(i,k)^{th}$ entry of $\mathbf{Z}^0$. In the network represented by $\mathbf{Z}^0$ the number of sampled partners ranges from 0 to 19, with a mean of 0.8 and a standard deviation of 1.2. The network is sparse into small clustered groups: only 8% belong to the same network component and, for those who do, the mean geodesic distance (*i.e.* the mean number of steps in the shortest path between two households) is rather small (1.13).

[24] This is to economize on space, but results stay comparable if we run separate regressions defining transfers on the basis or loans only, or gifts only. Indeed loans and gifts data display a very similar pattern, as will be shown in subsection 3.3.

descriptive statistics of all variables used in Table 4 are reported in the Appendix Table A1.

Results show that several proxies of network activity are affected by one's treatment status, and/or by the number of baseline partners who were offered the savings account. In particular, we observe a significant increase in all proxies accounting for network activity *within* the village. Columns (1) and (2) show that the coefficient of i's treatment status itt_i, and the coefficient of the number of i's partners in the treatment group $z^0 itt_i$ are all positive and statistically significant. Hence, having been offered the savings account and having more baseline partners who were offered the savings account increase significantly the number of partners, loans and gifts within the village. Regarding the network activity outside the village, reported in columns (3) and (4), we do not find any statistically significant direct intent-to-treat effect. Nevertheless, the coefficient of $z^0 itt_i$ is negative and statistically significant for the number of partners and of loans, which may be interpreted as indication of some substitution between informal financial activity within and outside the village. Finally, we also find a positive and significant impact of the number of baseline partners who were offered a savings account, $z^0 itt_i$, on total partners and gifts given. Overall, these results provide some preliminary evidence that the effects of the intervention have spilled over the network of informal financial transactions.

3.3 Dyadic Regressions

Estimating the effects of the intervention on the network with household-level observations is not entirely satisfactory. In fact, household-level regressions do not take into account that the formation and severance of links are dyadic decisions, where one's outcome depends on the characteristics of her (current and potential) partners. By providing access to savings accounts to half of the households in the villages, the intervention did not only affect treatment households, but also the control households who were connected or could potentially be connected to them. Therefore, to account for the fact that directed financial transfers involve two parties and better understand the underlying mechanisms of network formation, in this subsection we exploit the dyadic nature of our data.

We take all directed within-village dyads as the unit of observation, for a total of 56,308 observations.[25] First, we run the following logit regression:

$$P(g_{ij}^1 = 1) = P(\beta_0 + \beta_1 itt_i + \beta_2 itt_j + \beta_3 x_i^0 + \beta_4 x_j^0 + \lambda_v + \epsilon_{ij}^1 > 0) \tag{3}$$

where the directed dependent variable g_{ij}^1 is the $(i, j)^{th}$ entry of $\mathbf{G}^1$ and equals one if a transfer from i to j was reported at endline ($t = 1$). The two dummies of interest are the treatment status of the potential giver and of the potential receiver, itt_i and itt_j respectively. The specification also includes x_i^0 and x_j^0, the controls at baseline for giver and receiver respectively, and λ_v, village fixed effects. Standard errors are clustered at the village level.[26] Table 5 reports the logit marginal effects for equation (3), with and without demographic controls, based on three different definitions of transfers: loans only, gifts only, and loans or gifts.[27]

Results show that, for all dependent variables and all specifications, the treatment status of the giver, itt_i, has a positive and statistically significant effect. This means that the households who were offered the savings account are *ceteris paribus* more likely to make transfers to others. The estimated coefficients may seem small in absolute terms. However, they are large compared to the mean of the dependent variables (reported at the bottom of the table), which is naturally small since dyadic observations include all possible within-village directed pairs. In fact, the specifications with controls predict an increase of 16% for loans and 25% for gifts with respect to the sample mean (columns 2 and 4, respectively). Interestingly, the treatment status of the receiver, itt_j, does not appear significant, suggesting that the increase in network-based transactions is driven by the giver's side: those who were offered the savings account increased their transfers towards other households (regardless of the treatment status of the partner),

[25]The descriptive statistics for the dyadic sample are reported in the Appendix Table A2.

[26]In presence of many unlinked populations, clustering is the preferable solution for dyadic network data as it allows for arbitrary cross-observation dependance (Barr, Dekker and Fafchamps 2012, Arcand and Fafchamps 2012). In our context, clustering may also address the negative correlation arising if households were financially saturated (*i.e.* if having one link would discourage an household to form other links).

[27]The number of observations in columns (3) and (4) of Table 5 is lower than the full directed sample of 56,308 dyads. That is because in two villages there were no declared gifts at endline. Therefore, all corresponding observations are dropped.

possibly because they now manage to accumulate the necessary liquidity.

Next, we explore the effect of the intervention on the magnitude of the dyadic transfers within the village sample, by running the linear regression:

$$c_{ij}^1 = \beta_0 + \beta_1 itt_i + \beta_2 itt_j + \beta_3 x_i^0 + \beta_4 x_j^0 + \lambda_v + \epsilon_{ij}^1 \tag{4}$$

where the ordinal dependent variable c_{ij}^1 is the $(i, j)^{th}$ entry of $\mathbf{C}^1$ and classifies the transfer at endline from i to j as: 0 (no transfer), 1 (less than 1,200 rupees), 2 (1,200-2,400 rupees), 3 (2,400-5,000 rupees), and 4 (more than 5,000 rupees). Estimation results from equation (4) are reported in Table 6, and reconfirm the findings of Table 5 in terms of direction, significance and order of magnitude of the estimated effects. To the best of our knowledge, these dyadic specifications are novel to the literature on policy evaluation, as they jointly exploit the randomized experiment design and the availability of post-intervention network data.[28] Overall the results shown in Tables 5 and 6 provide evidence that the intervention had increased the financial transactions in the sampled villages, suggesting some complementarity between formal savings and informal network-based financial activities.

One caveat is in order. As we consider a field experiment that randomized financial access, our study analyzes the network of informal financial transactions. One's social network however, spans many dimensions of social interactions other than the financial ones. Nevertheless, since these social dimensions are likely to be intertwined, the change in the network of informal financial transactions may spill over to and proxy for other types of social relationships that are not the focus of our analysis.

4 The Spillover Effects of the Intervention

Having shown that the exogenous expansion in formal financial access has affected the network, it may be misleading to evaluate whether social networks help diffusing the effect of the intervention using pre-intervention network data only. In fact, doing so,

[28] In a different context, Fafchamps and Quinn (2012) use dyadic regressions to investigate whether exogenous group assignment fostered social links between managers of African manufacturing firms.

we might miss a possibly important effect: the spillover effect of the intervention, *i.e.* the effect of the intervention-driven network changes on household outcomes.

We now explore this idea and show how to internalize these spillover effects within a model of peer effects. All previous peer-effect studies exploiting the network structure of social interactions have used network data collected at one point in time (*e.g.* Bramoullé, Djebbari, and Fortin 2009, Calvó-Armengol, Patacchini, and Zenou 2009), thus the validity of their results relies on the assumption that the network structure is not affected by the variables of interest. Challenging this assumption is the scope of this section. In subsection 4.1 we first introduce the benchmark model with time-invariant interaction matrix, hereafter called static peer-effect model. Then, in subsection 4.2, we derive our dynamic peer-effect model where we allow the interaction matrix to vary exogenously. We show that the static model is nested in the dynamic model and that we obtain biased peer-effect estimates if we disregard the network changes.

4.1 A Static Model of Peer Effects

4.1.1 The Model

In this subsection we follow closely the empirical strategy by Bramoullé, Djebbari, and Fortin (2009), who show how to estimate peer-effect models where social interactions are structured through the social network.[29] Define $\mathbf{y}^t$ as the $n \times 1$ vector representing the household expenditure decision at time t, *e.g.* total non-food expenditure, in natural logs. Recall that $\mathbf{W}^t$ represents the undirected and row-standardized $n \times n$ matrix of social interactions, and that $\mathbf{itt}$ denotes the intent-to-treat vector. Call $\boldsymbol{\epsilon}^t$ the vector of disturbances, and $\boldsymbol{\iota}$ a $n \times 1$ vector of ones. We use Δ to denote a change in a given variable from baseline to endline.[30] The peer-effect model for period $t = 0$ can be written in matricial form as:

$$\mathbf{y}^0 = \alpha_0 \boldsymbol{\iota} + \beta \mathbf{W}^0 \mathbf{y}^0 + \boldsymbol{\lambda}_v + \boldsymbol{\mu} + \boldsymbol{\epsilon}^0 \tag{5}$$

[29] In particular, they provide the identification conditions and motivate the moment restrictions we use in this paper, and they formalize the analogy with differencing techniques for linear panel data that we develop in subsection 4.2.

[30] For instance, $\Delta \mathbf{y} = \mathbf{y}^1 - \mathbf{y}^0$ represents the change in expenditure from baseline to endline. Note that the term is positive when $\mathbf{y}$ has *increased* with time.

where $\boldsymbol{\lambda}_v$ and $\boldsymbol{\mu}$ represent village and household fixed effects, respectively. The expenditure of household i is assumed to depend linearly on the mean expenditure of its partners: $\mathbf{W}^0\mathbf{y}^0_{[i]} = \sum_{k=1}^n w^0_{ik} \cdot y^0_k$ where w^0_{ik} is the $(i,k)^{th}$ entry of $\mathbf{W}^0$.[31] $\mathbf{W}^0\mathbf{y}^0$ is usually referred to as the first lag of the dependent variable, and its coefficient β represents the strength of the peer effect.[32] The corresponding equation for $t = 1$ is:

$$\mathbf{y}^1 = (\alpha_0 + \alpha)\boldsymbol{\iota} + \beta\mathbf{W}^1\mathbf{y}^1 + \gamma\mathbf{itt} + \delta\mathbf{W}^1\mathbf{itt} + \boldsymbol{\lambda}_v + \boldsymbol{\mu} + \boldsymbol{\epsilon}^1 \tag{6}$$

where we add the intercept coefficient α to allow for time trends, and we introduce two additional terms: $\mathbf{itt}$ and $\mathbf{W}^1\mathbf{itt}$. The intent-to-treat vector, $\mathbf{itt}$, represents the direct effect of one's treatment status. The first lag of the intent-to-treat vector, $\mathbf{W}^1\mathbf{itt}$, represents the share of one's partners at endline that was offered the savings account. This term captures any effect of the treatment status of one's partners which does not transit through their expenditure.

Let us first assume that the interaction matrix is non-stochastic, *i.e.* has a fixed and known structure: $\Delta\mathbf{W}$ =0. Hence, subtracting (5) from (6), we obtain the first-difference estimating equation:

$$\Delta\mathbf{y} = \alpha\boldsymbol{\iota} + \beta\mathbf{pe}_A + \gamma\mathbf{itt} + \delta\mathbf{W}^0\mathbf{itt} + \Delta\boldsymbol{\epsilon} \tag{7}$$

where the peer-effect term that we write $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the mean expenditure change of one's partners.[33] We refer to equation (7) as to the static peer-effect model. This model exploits the full structure of the social network data to identify the peer effect through non-overlapping reference groups.[34] This feature, combined with

[31] We follow here the standard approach of most papers on social interactions (independently of whether they use network data or not) which use a linear-in-means identification strategy, that is, which model the outcome of each individual as a linear function of the *mean* outcome of her partners. Few notable exceptions include Brock and Durlauf (2001) and Liu *et al.* (2012).

[32] In analogy with time series econometrics, it is customary to assume that the process is stationary, *i.e.* that $\beta < |1|$ (Bramoullé, Djebbari, and Fortin 2009, Kelejian and Prucha 1998).

[33] In the terminology of Mansky (1993), $\mathbf{W}^0\mathbf{itt}$ is called the exogenous social effect, and $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ the endogenous social effect.

[34] Most earlier studies on peer effects have used data where individuals are partitioned into mutually-exclusive fully-overlapped reference groups (*e.g.* all children belonging to the same class, all workers in the same census area). Doing so, they assume that individuals are equally affected by all other individuals belonging to their group and by nobody outside their group. Our model belongs to the

the fact that our randomization is *within* villages (rather than *across* villages), allows us to disentangle the effect of the random treatment allocation at different levels. That is, exploiting the fact that two households with similar pre-intervention network characteristics may score differently in terms of their treatment status, and also their partners may score differently in terms of their treatment status, we can identify separately the effect of own treatment and of the treatment of partners.

4.1.2 2SLS Strategy

Note that, because of the model's simultaneity (*i.e.* the outcomes of the household and its partners are jointly determined), here we have a problem of endogeneity: the peer-effect term $\mathbf{pe}_A$ is correlated with the disturbance vector $\Delta\boldsymbol{\epsilon}$, which may invalidate OLS inference. However this equation can be consistently estimated by 2SLS using as instruments the lagged households characteristics, that is, the exogenous attributes of the partners of one's partners. As Bramoullé, Djebbari, and Fortin (2009) show, this exclusion restriction is valid as long as the interaction matrix is either non-stochastic (as for equation (7)) or stochastic but strictly exogenous (a case that will be discussed in the next subsection), and it is not partitioned into mutually-exclusive fully-overlapped reference groups. Said otherwise, as long as there are households who are excluded from one's reference group but are included in the reference group of her partners, their exogenous characteristics may affect one's outcome only through her partners and thus are a natural set of instruments to overcome the reflection problem (Mansky 1993). This instrumentation strategy is rather standard to spatial and network interaction models (Kelejian and Prucha 1998, Bramoullé, Djebbari, and Fortin 2009, Drukker, Egger and Prucha forthcoming, Calvò-Armengol, Patacchini, and Zenou 2009, Patacchini and Zenou 2012). Here we use two lagged household characteristics as identifying instruments for the change in mean expenditure of partners $\mathbf{pe}_A$: for the partners of partners, we compute the share that was offered the savings account (call it $\mathbf{iv}_1$) and the mean baseline expenditure of those who were offered the savings account

group of peer-effect models where interaction is structured though social networks, such that the reference group has individual-level variation: if i and j are connected and j and k are connected, it does not necessarily imply that i and k are also connected.

(call it $\mathbf{iv}_2$).[35,36] For the estimation we assume that disturbances $\Delta\boldsymbol{\epsilon}$ are exogenous, heteroskedastic, and arbitrarily correlated within villages.[37]

4.2 A Dynamic Model of Peer Effects

4.2.1 The Model

The validity of the static peer-effect model of subsection (4.1) relies on the assumption that the social interaction matrix has a fixed and known structure, which in our context is potentially misleading. In what follows we develop a dynamic peer-effect model in which we allow the interaction matrix to change as a result of the exogenous intervention, and we show that disregarding the network changes leads to biased peer-effect estimates. Let us assume that the interaction matrix is stochastic and changes due to the randomized experiment: $\Delta\mathbf{W} = f(\mathbf{itt})$. In order to write the estimating equation, note that the total change in mean expenditure of partners can be written as

$$\mathbf{W}^1\mathbf{y}^1 - \mathbf{W}^0\mathbf{y}^0 = \mathbf{W}^0\Delta\mathbf{y} + \Delta\mathbf{W}\,\mathbf{y}^0 + \Delta\mathbf{W}\,\Delta\mathbf{y} \tag{8}$$

where for a given household $\mathbf{W}^0\Delta\mathbf{y}$ represents the change in its partners' *mean* expenditure keeping *partners* constant, $\Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in its partners' *mean* expenditure keeping *expenditure* constant, and $\Delta\mathbf{W}\,\Delta\mathbf{y}$ accounts for the combined effect of the expenditure change and the network change.[38] In order to improve the readability, in what follows we write $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$, $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ and $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ respectively. Also, note that we can write $\mathbf{W}^1\mathbf{itt} = \mathbf{W}^0\mathbf{itt} + \Delta\mathbf{W}\,\mathbf{itt}$, where $\boldsymbol{\Delta}\mathbf{W}\,\mathbf{itt}$ represents the change in the share of partners that was offered the savings account. Subtracting (5) from (6) we now obtain the first-difference estimating

[35]The count of the partners of one's partners does not include the household itself.

[36]For instance, let us imagine a network composed of six agents $\{a, b, c, d, e, f\}$ where we observe 4 links $g^0_{ac} = g^0_{fc} = g^0_{ec} = g^0_{db} = 1$ (and zero elsewhere) and only three agents $\{b, e, f\}$ are offered the savings account. The only partner of a is c and c's partners are e and f, who were both offered the savings account. Thus, we get $\mathbf{iv}_{\mathbf{1}[\mathrm{a}]} = 2/2 = 1$ and $\mathbf{iv}_{\mathbf{2}[\mathrm{a}]} = (\mathbf{y_e}^0 + \mathbf{y_f}^0)/2$.

[37]In this context the exogeneity condition on the error term writes $E(\Delta\boldsymbol{\epsilon}|\mathbf{itt}, \mathbf{W}^0, \boldsymbol{\lambda}_v, \boldsymbol{\mu}) = 0$. Note that the Generalized 2SLS strategy first proposed by Kelejian and Prucha (1998) reduces to standard 2SLS whenever disturbances are not spatially correlated.

[38]This is close in spirit to the Oaxaca (1973) decomposition, which aims at decomposing earnings gaps into differences in characteristics and in performances.

equation that we call the dynamic peer-effect model

$$\Delta \mathbf{y} = \alpha \boldsymbol{\iota} + \beta_1 \mathbf{pe}_A + \beta_2 \mathbf{pe}_B + \beta_3 \mathbf{pe}_C + \gamma \mathbf{itt} + \delta_1 \mathbf{W}^0 \mathbf{itt} + \delta_2 \Delta \mathbf{W}\, \mathbf{itt} + \Delta \epsilon \tag{9}$$

Note how the static model of equation (7) is nested into the dynamic model of equation (9). The first peer-effect term $\mathbf{pe}_A$, which appears in both models, represents the mean expenditure change of one's baseline partners. The other two peer-effect terms, which only appear in equation (9), accounts for the spillover effects of the intervention, that is, the effect of the intervention-driven network changes on household expenditure. $\mathbf{pe}_B$ represents the network changes in terms of the baseline expenditure: for a given household, $\mathbf{pe}_B$ is positive if at baseline the mean expenditure of its new partners was higher than the mean expenditure of its old partners. Said otherwise, this term is positive whenever as a results of the randomized intervention the household formed links with new partners who were already better endowed *ex ante* (regardless of the intervention). The third term $\mathbf{pe}_C$ accounts for the combined effect of the expenditure change and the network change: for a given household, $\mathbf{pe}_C$ is positive if the mean expenditure of new partners has increased more than the mean expenditure of old partners from baseline to endline. In other words, the term is positive whenever as a results of the randomized intervention the household formed new links with those households whose expenditure increased the most. The three peer-effect terms are correlated*:* $\mathbf{pe}_C$ is negatively correlated with $\mathbf{pe}_A$ and $\mathbf{pe}_B$ by construction, while the correlation between $\mathbf{pe}_A$ and $\mathbf{pe}_B$ depends on the specific context. Thus, not accounting for the spillover effects of the intervention may lead to biased estimates in the static model of equation (7).

4.2.2 Correlated Unobservables

By exploiting the panel dimension of our data, we are able to address more convincingly than before the issue of endogeneity from correlated unobservables, which stems from the fact that linked individuals tend to behave similarly because they are alike (Mansky 1993). Bramoullé, Djebbari, and Fortin (2009) show that equation (7) can be consistently estimated as long as the interaction matrix is conditionally exogenous (*i.e.* strictly exogenous conditional on the model's fixed effects) and argue that, in analogy with linear panel data, correlated unobservables can be treated as fixed effects. However, since they use cross-sectional data, their *within* transformation is implemented at the partners level: they express their model in deviation from the mean equation of the individual's partners.[39] Therefore their identification strategy is valid as long as the interaction matrix is strictly exogenous conditional on the partner-level fixed effects, that is, as long as all correlated unobservables affecting both household outcome and link formation are common to all connected partners. In our context, the panel information on the household and on the network allows us to implement a *within* transformation *both* at the household level (by estimating equation 9 in first differences) *and* at the dyad level (by computing the peer-effect terms on the basis of $\Delta\mathbf{W}$). In our study, the conditional exogeneity assumption therefore requires $\Delta\mathbf{W}$ to be exogenous conditionally on the household-level fixed effects $\boldsymbol{\mu}$. This assumption is rather reasonable if one believes that all confounding unobservables which may simultaneously affect household outcome and link formation (such as homophyly between partners in risk attitude, savings and spending behavior, and financial literacy) are time-invariant within the duration of our study. To the best of our knowledge this approach is novel to the literature, and allows us to model the spillover effects of the intervention we aim to study.

4.2.3 2SLS Strategy

Note that equation (9) also suffers from endogeneity because of the model's simultaneous structure: even under the conditionally exogeneity assumption, since **itt** simultaneously affects $\Delta\mathbf{y}$ and $\Delta\mathbf{W}$, all three peer-effect terms $\mathbf{pe}_A$, $\mathbf{pe}_B$, and $\mathbf{pe}_C$, as well as $\Delta\mathbf{W}\,\mathbf{itt}$ are endogenous because in equilibrium they are jointly determined with the dependent variable.[40] We address this issue combining the estimation strategy of

[39] Bramoullé, Djebbari, and Fortin (2009) also provide similar results for the *within* transformation implemented at the network level (which corresponds to the village level in our illustration), to which our discussion applies as well.

[40] The Generalized 2SLS strategy first proposed by Kelejian and Prucha (1998) delivers consistent estimates even in presence of additional endogenous regressors (Drukker, Egger and Prucha forthcoming). Lee (2003) proposes an asymptotically optimal adjustment that we do not apply here, because

subsection 4.1 with the panel dimension of our network data. We use four instruments. The first two instruments are the same ones described for the static peer-effect model, that is, the exogenous characteristics of the partners *at baseline* of one's partners *at baseline* (note that now we need to specify that we refer to the baseline network data, since the interaction matrix can change). Thus, for the partners at baseline of one's partners at baseline, we compute the share that was offered the savings account ($\mathbf{iv}_1$) and the mean baseline expenditure of those who were offered the savings account ($\mathbf{iv}_2$). The other two instruments are built on the same intuition, *i.e.* using exogenous lagged characteristics. However, since now we exploit both rounds of interaction data, we can also include the exogenous characteristics of the partners *at baseline* of one's partners *at endline*, which can only affect one's expenditure through her partners at endline. The intuition is that, if linking decisions are interconnected, one's new partners *at endline* may be such because the intervention has changed *their* ex-ante network, pushing them to form new links. Thus, we use the following two instruments: for the partners at baseline of one's partners at endline, we compute the share that was offered the savings account (call it $\mathbf{iv}_3$), and the mean baseline expenditure of those who were offered the savings account (call it $\mathbf{iv}_4$).[41]

4.3 Main Results

We now present the main empirical results. The dependent variable we consider is the household total non-food expenditure, in natural logs. The first and second-stage estimates of the static and dynamic peer-effect models are reported in Tables 7 and 8, respectively.[42]

Table 7 shows the first-stage estimates for the static peer-effect model of equation

it has not been formally proven that Lee's estimator can accommodate endogenous regressors other than the peer-effect term.

[41]Let us continue with the same example used above of a network composed by six agents $\{a, b, c, d, e, f\}$ where at $t = 0$ we observe 4 links $g^0_{ac} = g^0_{fc} = g^0_{ec} = g^0_{db} = 1$ (and zero elsewhere) and only agents $\{b, f, e\}$ are offered the savings account. The first two instruments only exploit the network structure at $t = 0$, and thus we get $\mathbf{iv_{1[a]}} = 2/2 = 1$ and $\mathbf{iv_{2[a]}} = (\mathbf{y_e}^0 + \mathbf{y_f}^0)/2$ as before. Suppose that at $t = 1$ agent a has only one link $g^1_{ad} = 1$. Thus, the only partner at $t = 1$ of a is d and d's only partner at $t = 0$ was b who was offered the savings account. Hence, $\mathbf{iv_{3[a]}} = 1/1 = 1$ and $\mathbf{iv_{4[a]}} = \mathbf{y_b}^0$.

[42]Descriptive statistics are reported in the Appendix Table A3.

(7) in column (1), and the first-stage estimates for the dynamic peer-effect model of equation (9) in columns (2)-(5). Overall, Table 7 shows that the instruments in use are statistically significant. In the last line of Table 7 we report the weak identification tests.[43] All test results are significant at 10% level or lower, reassuring that no endogenous variable is under or weakly identified.

Table 8 reports five specifications, all with standard errors clustered at the village level. For the sake of comparison, in column (1) we report the estimates from a benchmark intent-to-treat model in first differences with no peer effects, which corresponds to the estimating equation $\Delta\mathbf{y} = \alpha\boldsymbol{\iota} + \gamma\mathbf{itt} + \Delta\boldsymbol{\epsilon}$. The estimates of the static peer-effect model of equation (7) are reported in columns (2) and (3), via OLS and 2SLS, respectively. The estimates of the dynamic peer-effect model of equation (9) are reported in columns (4) and (5), via OLS and 2SLS respectively.

The results are highly consistent across all specifications: while the direct intent-to-treat dummy does not seem to affect total non-food expenditure,[44] the peer-effect terms $\mathbf{pe}_A$ and $\mathbf{pe}_C$ appear positive and strongly significant in all specifications. This is true whether we use OLS or the 2SLS instrumentation strategy - however, OLS estimated coefficients appear biased downwards. The estimated coefficient for $\mathbf{pe}_A$ in column (5) suggests that a 1% increase in the expenditure of baseline partners increases one's expenditure by 0.83%. Interestingly, the peer-effect term $\mathbf{pe}_C$, which accounts for the interaction of the expenditure change and the network change, is also significant: getting new partners whose expenditure increased 1% more than the expenditure of old partners leads to an increase of 0.35% in one's expenditure. Taken together, these results suggest that the increase of partners' expenditure, whether it comes from old partners (as reflected by $\mathbf{pe}_A$), or from new partners via the changes in the village network due to the intervention (as reflected by $\mathbf{pe}_C$), has a positive effect on one's expenditure. Our results are in line with recent studies finding a positive peer effect for expenditure and social spending (Moretti 2011, Chen 2011, Brown, Bulte and Zhang 2011, De Giorgi, Frideriksen and Pistaferri 2012). Note that the variable $\mathbf{pe}_C$ has a positive mean (see Appendix Table A3), meaning that as a result of the randomized

[43] We use the Kleibergen-Paap F-test for column (1) and the Angrist-Pischke multivariate F-test for the remaining columns, where we have multiple endogenous regressors.

[44] This is in line with the results of Prina (2013) on the same data.

intervention those with the highest expenditure increase have also formed more new links. This is in line with the results of Section 3, suggesting that network-based informal financial transactions and total non-food expenditure show complementarity. On the other hand, the peer-effect term $\mathbf{pe}_B$ is marginally significant in column (4), but is not significant once we instrument: the baseline expenditure of new partners does not seem to have an effect on one's expenditure. Importantly, note that for both OLS and 2SLS estimates, the coefficient associated with $\mathbf{pe}_A$ for the static peer-effect model is smaller than the corresponding coefficient for the dynamic peer-effect model. As $\mathbf{pe}_A$ and $\mathbf{pe}_C$ are by construction negatively correlated, this is evidence of omitted variable bias. Thus, in our context, not accounting for the spillover effects of the intervention generates peer-effect estimates that are biased downwards. The other two regressors $\mathbf{W}^0\mathbf{itt}$ and $\Delta\mathbf{W}\,\mathbf{itt}$ are not significant, suggesting that there is no direct effect of the partners' treatment status once we have taken into account their expenditure.

4.4 Further Results

Finally, in the Appendix Tables A4-A9 we report separate estimation results for each of the six expenditure categories which compose total non-food expenditure, namely: social spending (*e.g.* festivals, funerals, marriages, dowry payments), maintenance (*e.g.* personal care, cleaning, house repair and maintenance), medicines and traditional remedies, health services (*e.g.* hospital charges), children school fees, clothing and footwear. Overall, the 2SLS results for social spending, maintenance and medicine expenditure reconfirm the main results of Table 8: the instrumented coefficients for $\mathbf{pe}_A$ and $\mathbf{pe}_C$ appear positive and significant with comparable magnitude, while $\mathbf{pe}_B$ is non-significant. The term $\mathbf{W}^0\,\mathbf{itt}$, which account for the direct effect of the treatment among baseline partners, is negative and significant for social spending only. This suggests that the more baseline partners were offered the savings account, the less one spends for festivals, funerals, marriages, and dowry. This effect is consistent with the results of Tables 5 and 6, which show that households offered the savings account significantly increase the amount and the number of gifts given. Since in our villages gifts have mainly a social motive,[45] having more partners in the treatment group should increase the amount of received gifts related to the social spending categories of interest, which may lower one's expenditure. For what concerns health services, school fees and clothing and footwear in column (5) the first peer effect term $\mathbf{pe}_A$ is always significant, but the spillover term $\mathbf{pe}_C$ is not significant. In all six tables the coefficient for $\mathbf{pe}_A$ is consistently underestimated in the static model where the other peer-effect terms are omitted.

5 Conclusions

A large literature has shown that social networks help spread information, technologies, and products, assuming that the structure of the network is fixed. However, we argue that the structure of the network might change because of the availability of such information, technologies, and products. In this paper we investigate whether social networks change as a result of an exogenous intervention. We do so by exploiting a field experiment that randomized access to a savings account and using a unique panel data on the network of informal financial transactions before and after this exogenous expansion in formal financial access.

First, using household-level and dyadic regressions, we provide evidence that the financial intervention changed the network. Consequently, we propose and estimate a dynamic model of peer effects which incorporates the spillover effects of the intervention, showing that the peer-effect estimates we obtain differ radically from a those of the static peer-effect model. In particular, our results show that assuming a fixed network we obtain downward-biased peer-effect coefficients.

This paper shows that, in order to capture the actual importance of networks in spreading products and technologies, it is necessary to take into account how the increasing availability of such products may change the pre-existing network. Hence, our study provides an important insight on drawing inference on networks and diffusion

[45] We have collected detailed information on the reasons of all gifts which took place within one month prior to the survey. The declared reasons are wedding, festival or funeral for 62% of gifts at baseline and for 49% at endline.

and can serve as a foundation for the design of successful interventions that spread through the social network.

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Tables

Table 1: Household Descriptive Statistics at Baseline

	Sample (N=915)	Control (N=447)	Treatment (N=468)	T-stat
Age of the female household head	36.80	36.77	36.82	0.05
	(12.51)	(12.16)	(12.85)	
Years of education of the female household head	2.52	2.44	2.59	0.79
	(2.82)	(2.67)	(2.96)	
Percent married/living with partner	0.89	0.88	0.90	0.77
	(0.32)	(0.33)	(0.31)	
Household size	4.55	4.58	4.52	-0.51
	(1.66)	(1.68)	(1.64)	
Number of children	2.21	2.26	2.18	-0.86
	(1.30)	(1.30)	(1.29)	
Total income last week	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)	3.50	3.50	3.49	-0.06
	(3.68)	(3.67)	(3.70)	
Experienced a negative income shock	0.42	0.40	0.45	1.68*
	(0.50)	(0.49)	(0.50)	
Owns the house	0.86	0.86	0.85	-0.41
	(0.35)	(0.34)	(0.35)	
Owns the land on which the house is built	0.80	0.80	0.80	-0.03
	(0.40)	(0.40)	(0.40)	
Total assets	44,469.26	42,510.10	46,340.51	1.14
	(50,891.76)	(45,540.07)	(46,340.51)	
Log(total assets + 1)	10.23	10.20	10.25	0.72
	(1.03)	(1.02)	(1.05)	
Percentage of households with money in a ROSCA	0.17	0.17	0.18	0.47
	(0.38)	(0.37)	(0.38)	
Log(total money in ROSCA + 1)	1.50	1.45	1.56	0.47
	(3.32)	(3.27)	(3.37)	
Percentage of households with money in an MFI	0.56	0.58	0.54	-1.25
	(0.50)	(0.49)	(0.50)	
Log(total money in MFIs + 1)	4.49	4.62	4.36	-0.96
	(4.10)	(4.05)	(4.14)	
Percentage of households with money in a bank	0.15	0.14	0.17	0.89
	(0.36)	(0.35)	(0.37)	
Log(total money in bank accounts + 1)	1.38	1.25	1.50	1.15
	(3.30)	(3.12)	(3.46)	
Log(total amount of cash at home + 1)	6.39	6.32	6.45	1.07
	(1.93)	(1.91)	(1.95)	
Log(non-monetary assets from consumer durables + 1)	9.87	9.88	9.87	-0.12
	(1.30)	(1.24)	(1.36)	
Log(non-monetary assets from livestock + 1)	3.56	3.35	3.76	1.45
	(4.25)	(4.24)	(4.26)	
Percentage of households with outstanding loans	0.90	0.88	0.91	1.42
	(0.31)	(0.32)	(0.29)	

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

Table 2: Network Descriptive Statistics at Baseline

		Sample (N=915)	Control (N=447)	Treatment (N=468)	T-stat
Number of partners:	Total	1.73	1.70	1.76	0.48
		(1.62)	(1.64)	(1.61)	
	Within the village	0.70	0.68	0.73	0.73
		(0.95)	(0.99)	(0.92)	
	Outside the village	1.03	1.02	1.03	0.06
		(1.32)	(1.31)	(1.34)	
	Relatives	0.79	0.78	0.79	0.08
		(1.07)	(1.11)	(1.05)	
Number of gifts:	Within the village	0.61	0.57	0.64	0.81
		(1.24)	(1.15)	(1.32)	
	Outside the village	0.31	0.28	0.34	1.06
		(0.83)	(0.73)	(0.92)	
	Received	0.50	0.47	0.52	0.70
		(0.93)	(0.90)	(0.95)	
	Given	0.42	0.38	0.46	1.49
		(0.83)	(0.81)	(0.84)	
Number of loans:	Within the village	1.00	0.95	1.05	1.04
		(1.49)	(1.52)	(1.46)	
	Outside the village	0.87	0.87	0.87	-0.03
		(1.35)	(1.33)	(1.38)	
	Received	1.19	1.18	1.21	0.35
		(1.21)	(1.24)	(1.19)	
	Given	0.68	0.64	0.71	1.04
		(1.05)	(1.03)	(1.06)	

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

Table 3: Attrition Regressions

	Completed endline			
	(1)	(2)	(3)	(4)
itt	0.0111	0.0109	0.0101	0.0085
	(0.016)	(0.016)	(0.016)	(0.016)
Number of partners: total		0.0040		
		(0.004)		
Number of partners: within the village			0.0174*	
			(0.009)	
Number of partners: outside the village			-0.0046	
			(0.009)	
Number of partners: relatives			0.0028	
			(0.014)	
Number of gifts: within the village				0.0174
				(0.013)
Number of gifts: outside the village				0.0090
				(0.013)
Number of gifts: received				-0.0079
				(0.019)
Number of loans: within the village				0.0007
				(0.011)
Number of loans: outside the village				0.0166
				(0.014)
Number of loans: received				0.0129
				(0.021)
Constant	0.9012***	0.8944***	0.8924***	0.8644***
	(0.026)	(0.030)	(0.030)	(0.035)
Observations	1,009	1,009	1,009	1,009
R-squared	0.000	0.001	0.004	0.021

Notes: Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. All regressors are computed at $t = 0$. *itt* represents the intent-to-treat dummy, which takes value one if the household was offered the savings account.

Table 4: Household-level Intent-To-Treat Regressions

	Within the village		Outside the village		Relatives		Total	
Number of partners at endline	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
itt	0.0680**	0.0533*	0.0719	0.0786	0.1531	0.1496	0.1399	0.1319
	(0.0307)	(0.0288)	(0.0903)	(0.0913)	(0.0971)	(0.0961)	(0.1068)	(0.1051)
$z^0 itt$		0.1921***		-0.0868***		0.0457		0.1053***
		(0.0480)		(0.0264)		(0.0264)		(0.0357)
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Village f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	915	915	915	915	915	915	915	915
R-squared	0.150	0.177	0.147	0.152	0.088	0.090	0.174	0.178

	Within the village		Outside the village		Received		Given	
Number of gifts at endline	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
itt	0.0822*	0.0759*	0.0107	0.0128	0.0460	0.0459	0.0468	0.0429
	(0.0400)	(0.0388)	(0.0423)	(0.0440)	(0.0423)	(0.0431)	(0.0358)	(0.0351)
$z^0 itt$		0.0824**		-0.0283		0.0019		0.0521**
		(0.0359)		(0.0266)		(0.0169)		(0.0206)
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Village f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	915	915	915	915	915	915	915	915
R-squared	0.079	0.089	0.090	0.092	0.103	0.103	0.085	0.095

	Within the village		Outside the village		Received		Given	
Number of loans at endline	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
itt	0.1182*	0.1056*	-0.0623	-0.0559	0.0352	0.0335	0.0207	0.0162
	(0.0575)	(0.0565)	(0.0532)	(0.0537)	(0.0650)	(0.0635)	(0.0437)	(0.0439)
$z^0 itt$		0.1648***		-0.0843***		0.0212		0.0593
		(0.0459)		(0.0287)		(0.0464)		(0.0381)
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Village f.e.	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	915	915	915	915	915	915	915	915
R-squared	0.072	0.086	0.115	0.120	0.105	0.105	0.084	0.089

Notes: this table reports the estimates of the household-level intent-to-treat regressions measuring the effect of the intervention on the intensity of network-based financial transactions. OLS coefficients reported. Robust standard errors in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. Controls at baseline include: age, no-education dummy, household size, number of children less than 16 years of age. Constant term included. *itt* represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $z^0 itt$ is the number of partners at baseline who were offered the savings account.

Table 5: Directed Binary Dyadic Intent-To-Treat Regressions

	Loans at endline		Gifts at endline		Loans or gifts at endline	
	(1)	(2)	(3)	(4)	(5)	(6)
itt_i	0.0011*	0.0011*	0.0004**	0.0003**	0.0013**	0.0013**
	(0.0006)	(0.0006)	(0.0002)	(0.0001)	(0.0006)	(0.0006)
itt_j	0.0005	0.0005	0.0001	0.0001	0.0007	0.0007
	(0.0006)	(0.0006)	(0.0002)	(0.0001)	(0.0006)	(0.0006)
Controls	no	yes	no	yes	no	yes
Village dummies	yes	yes	yes	yes	yes	yes
Mean of dep. var.	0.0068	0.0068	0.0012	0.0012	0.0075	0.0075
Observations	56,308	56,308	50,970	50,970	56,308	56,308

Notes: this table reports the estimates of the directed binary dyadic intent-to-treat regressions. The dependent variable equals one if a transfer (loans only, gifts only, loans or gifts) from i to j was reported at endline. All directed within-village dyads are taken as the unit of observation. Logit marginal effects are reported. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. Controls at baseline include, for both i and j: age, no-education dummy, household size, number of children less than 16 years of age. Constant term included. itt_i is the intent-to-treat dummy of the potential giver i. It takes value one if household i was offered a savings account. itt_j is the intent-to-treat dummy of the potential receiver j. It takes value one if household j was offered a savings account.

Table 6: Directed Ordinal Dyadic Intent-To-Treat Regressions

	Loans at endline		Gifts at endline		Loans or gifts at endline	
	(1)	(2)	(3)	(4)	(5)	(6)
itt_i	0.0052**	0.0053**	0.0009*	0.0010*	0.0057**	0.0058**
	(0.0024)	(0.0025)	(0.0005)	(0.0006)	(0.0024)	(0.0025)
itt_j	0.0028	0.0028	-0.0000	-0.0001	0.0029	0.0029
	(0.0023)	(0.0023)	(0.0005)	(0.0005)	(0.0024)	(0.0024)
Controls	no	yes	no	yes	no	yes
Village dummies	yes	yes	yes	yes	yes	yes
Mean of dep. var.	0.0200	0.0200	0.0019	0.0019	0.0212	0.0212
Observations	56,308	56,308	56,308	56,308	56,308	56,308
R-squared	0.003	0.004	0.002	0.003	0.004	0.004

Notes: this table reports the estimates of the directed ordinal dyadic intent-to-treat regressions. The ordinal dependent variable classifies the transfer (loans only, gifts only, loans or gifts) from i to j at endline in a five-category scale from 0 (no transfer) to 4 (more than 5,000 rupees). All directed within-village dyads are taken as the unit of observation. OLS coefficients are reported. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. Controls at baseline include, for both i and j: age, no-education dummy, household size, number of children less than 16 years of age. Constant term included. itt_i is the intent-to-treat dummy of the potential giver i. It takes value one if household i was offered a savings account. itt_j is the intent-to-treat dummy of the potential receiver j. It takes value one if household j was offered a savings account.

Table 7: Peer-effect Model, First Stage Regressions

	Static PE	Dynamic PE	Dynamic PE	Dynamic PE	Dynamic PE
Dependent var.	$\mathbf{pe}_A$	$\mathbf{pe}_A$	$\mathbf{pe}_B$	$\mathbf{pe}_C$	$\mathbf{\Delta W\,itt}$
	(1)	(2)	(3)	(4)	(5)
itt	-0.101	-0.171	0.166	0.135	-0.010
	(0.147)	(0.172)	(0.468)	(0.143)	(0.034)
$\mathbf{W}^0\mathbf{itt}$	0.512	0.456	-4.138***	-0.058	-0.812***
	(0.727)	(0.634)	(1.095)	(0.403)	(0.038)
$\mathbf{iv}_1$	3.265*	3.021*	2.350	-2.607***	-0.009
	(1.782)	(1.654)	(1.995)	(0.890)	(0.075)
$\mathbf{iv}_2$	-0.456**	-0.418**	-1.329***	0.274***	-0.011
	(0.207)	(0.185)	(0.268)	(0.088)	(0.007)
$\mathbf{iv}_3$		2.272**	1.982	4.631*	0.364***
		(1.102)	(2.358)	(2.751)	(0.139)
$\mathbf{iv}_4$		-0.213**	0.811***	-0.418**	0.020*
		(0.103)	(0.242)	(0.185)	(0.012)
Constant	0.239	0.255	0.667	0.448	0.154***
	(0.241)	(0.233)	(0.426)	(0.331)	(0.022)
Weak id. test (P>F)	0.054	0.074	0.000	0.044	0.000

Notes: This table reports the first-stage estimates of the static and dynamic peer-effect models. The weak identification tests are the Kleibergen-Paap F-test for column (1) and the Angrist-Pischke multivariate F-test for the remaining columns. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* **itt** reads $\mathbf{itt}_{[i]}$. $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ represent the interaction of the expenditure change and the network change. **itt** represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\mathbf{\Delta W\,itt}$ represents the change in the share of one's partners that was offered the savings account. $\mathbf{iv}_1$ is the share of the partners at baseline of one's partners at baseline that was offered the savings account. $\mathbf{iv}_2$ is the mean baseline expenditure of the partners at baseline of one's partners at baseline who were offered the savings account. $\mathbf{iv}_3$ is the share of the partners at baseline of one's partners at endline that was offered the savings account. $\mathbf{iv}_4$ is the mean baseline expenditure of the partners at baseline of one's partners at endline who were offered the savings account.

Table 8: Peer-effect Model, Main Results

	Benchmark OLS (1)	Static PE OLS (2)	Static PE 2SLS (3)	Dynamic PE OLS (4)	Dynamic PE 2SLS (5)
$\mathbf{itt}$	-0.0380	-0.0209	-0.0266	-0.0984	-0.1093
	(0.132)	(0.138)	(0.171)	(0.132)	(0.184)
$\mathbf{pe}_A$		0.2403***	0.7033***	0.2945***	0.8272***
		(0.070)	(0.099)	(0.054)	(0.188)
$\mathbf{pe}_B$				0.0174*	0.0011
				(0.010)	(0.051)
$\mathbf{pe}_C$				0.2013***	0.3513**
				(0.023)	(0.141)
$\mathbf{W}^0\mathbf{itt}$		-0.3499	-0.3051	-0.1601	-0.4851
		(0.215)	(0.390)	(0.196)	(1.149)
$\mathbf{\Delta W\, itt}$				0.1282	-0.2153
				(0.197)	(1.566)
Constant	0.2794	0.3619	0.3696	0.2387	0.2457
	(0.407)	(0.356)	(0.272)	(0.294)	(0.381)
Observations	915	915	915	915	915
R-squared	0.000	0.069	-	0.149	-

Notes: This table reports the estimates of a benchmark model with no peer effect, the static and the dynamic peer-effect models. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* $\mathbf{itt}$ reads $\mathbf{itt}_{[i]}$. $\mathbf{itt}$ represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{pe}_A \equiv \mathbf{W}^0 \Delta \mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta \mathbf{W}\, \mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta \mathbf{W}\, \Delta \mathbf{y}$ represent the interaction of the expenditure change and the network change. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\mathbf{\Delta W\, itt}$ represents the change in the share of one's partners that was offered the savings account.

Appendix Tables

Appendix Table A1: Descriptive Statistics for Table 4

	t	Mean	Min	Max	Std. Dev.
Number of partners: total	1	1.33	0	6	1.07
Number of partners: within the village	1	0.61	0	5	0.78
Number of partners: outside the village	1	0.72	0	6	0.88
Number of partners: relatives	1	0.57	0	6	0.85
Number of gifts: within the village	1	0.20	0	5	0.55
Number of loans: within the village	1	0.71	0	5	0.93
Number of gifts: outside the village	1	0.12	0	3	0.39
Number of loans: outside the village	1	0.51	0	6	0.75
Number of gifts: received	1	0.24	0	3	0.53
Number of loans: received	1	0.92	0	6	0.81
Number of gifts: given	1	0.09	0	3	0.35
Number of loans: given	1	0.30	0	4	0.59
Age	0	36.80	16	99	12.51
No education	0	0.34	0	1	0.48
Number of children <16 yrs	0	1.57	0	6	1.20
Household size	0	4.55	1	12	1.66
itt	-	0.51	0	1	0.50
$z^0 itt$	0	0.39	0	9	0.73

Notes: this table reports the descriptive statistics for the full household sample (N=915).

Appendix Table A2: Descriptive Statistics for Tables 5 and 6

	t	Mean	Min	Max	Std. Dev.
Loans (binary variable)	1	0.007	0	1	0.082
Gifts (binary variable)	1	0.001	0	1	0.035
Loans or gifts (binary variable)	1	0.008	0	1	0.087
Loans (ordinal variable)	1	0.020	0	4	0.261
Gifts (ordinal variable)	1	0.002	0	4	0.063
Loans or gifts (ordinal variable)	1	0.021	0	4	0.265
itt_i	-	0.508	0	1	0.500
itt_j	-	0.508	0	1	0.500

Notes: this table reports the descriptive statistics for the full dyadic sample (N=56,308).

Appendix Table A3: Descriptive Statistics for Tables 7 and 8

	Mean	Min	Max	Std. Dev.
Δ y	0.26	-9.82	10.01	2.61
$\mathbf{pe}_A$	-0.03	-15.53	24.09	2.79
$\mathbf{pe}_B$	-0.63	-89.91	30.20	9.53
$\mathbf{pe}_C$	0.63	-14.05	37.07	3.69
itt	0.51	0	1	0.50
$\mathbf{W}^0\mathbf{itt}$	0.24	0	1	0.39
$\boldsymbol{\Delta}\mathbf{W}\,\mathbf{itt}$	0.03	-1	1	0.50
$\mathbf{iv}_1$	0.17	0	1	0.32
$\mathbf{iv}_2$	1.96	0	11.36	3.47
$\mathbf{iv}_3$	0.17	0	1	0.32
$\mathbf{iv}_1$	1.81	0	11.97	3.38

Notes: This table reports the descriptive statistics for the full household sample (N=915).

Appendix Table A4: Peer Effect Model, Social Spending

	Benchmark	Static PE		Dynamic PE	
	OLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
itt	-0.2111	-0.1919	-0.2240	-0.2709	-0.2676
	(0.333)	(0.350)	(0.387)	(0.356)	(0.323)
$\mathbf{pe}_A$		0.2161***	0.5621***	0.3294***	0.6929***
		(0.046)	(0.105)	(0.050)	(0.099)
$\mathbf{pe}_B$				0.0564	0.1222
				(0.046)	(0.103)
$\mathbf{pe}_C$				0.2322***	0.6279***
				(0.037)	(0.146)
$\mathbf{W}^0\mathbf{itt}$		-1.0894*	-1.6663***	-1.0336	-3.8433**
		(0.530)	(0.557)	(0.633)	(1.730)
$\mathbf{\Delta W}\,\mathbf{itt}$				-0.0656	-3.5631
				(0.405)	(2.457)
Constant	1.5247*	1.6049**	1.4875***	1.4059*	1.6381*
	(0.844)	(0.734)	(0.575)	(0.740)	(0.885)
Observations	915	915	915	915	915
R-squared	0.000	0.076	-	0.127	-

Notes: This table reports the estimates of a benchmark model with no peer effect, the static and the dynamic peer-effect models. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* **itt** reads $\mathbf{itt}_{[i]}$. **itt** represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ represent the interaction of the expenditure change and the network change. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\mathbf{\Delta W}\,\mathbf{itt}$ represents the change in the share of one's partners that was offered the savings account.

Appendix Table A5: Peer-effect Model, Maintenance Expenditure

	Benchmark	Static PE		Dynamic PE	
	OLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
itt	0.0814	0.0978	0.0800	0.0452	0.0232
	(0.148)	(0.144)	(0.161)	(0.124)	(0.143)
$\mathbf{pe}_A$		0.2170**	0.6955***	0.3086***	0.7842***
		(0.096)	(0.144)	(0.066)	(0.137)
$\mathbf{pe}_B$				0.0042	0.0209
				(0.013)	(0.053)
$\mathbf{pe}_C$				0.2149***	0.2711***
				(0.019)	(0.082)
$\mathbf{W}^0\mathbf{itt}$		-0.4604**	-0.4688	-0.2892	-0.6078
		(0.178)	(0.391)	(0.193)	(0.791)
$\mathbf{\Delta W}\,\mathbf{itt}$				0.1813	-0.3335
				(0.225)	(1.054)
Constant	0.3140	0.3946	0.3603*	0.2679	0.3048
	(0.333)	(0.269)	(0.201)	(0.222)	(0.280)
Observations	915	915	915	915	915
R-squared	0.000	0.063	-	0.142	-

Notes: This table reports the estimates of a benchmark model with no peer effect, the static and the dynamic peer-effect models. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* **itt** reads $\mathbf{itt}_{[i]}$. **itt** represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ represent the interaction of the expenditure change and the network change. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\mathbf{\Delta W}\,\mathbf{itt}$ represents the change in the share of one's partners that was offered the savings account.

Appendix Table A6: Peer-effect Model, Expenditure for Medicines

	Benchmark	Static PE		Dynamic PE	
	OLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
itt	0.4902***	0.5095***	0.5363***	0.5285***	0.5731***
	(0.140)	(0.129)	(0.144)	(0.123)	(0.216)
$\mathbf{pe}_A$		0.1304**	0.5013	0.1825***	0.7676**
		(0.047)	(0.368)	(0.056)	(0.348)
$\mathbf{pe}_B$				0.0282	0.4977
				(0.064)	(0.449)
$\mathbf{pe}_C$				0.0799**	0.6922*
				(0.036)	(0.354)
$\mathbf{W}^0\mathbf{itt}$		-0.3059	-0.6534	-0.5543*	-1.2122
		(0.242)	(0.531)	(0.282)	(1.105)
$\mathbf{\Delta W}\,\mathbf{itt}$				-0.3785	-1.7046
				(0.362)	(1.743)
Constant	0.3283	0.3266	0.2118	0.3742	0.3924
	(0.265)	(0.259)	(0.270)	(0.285)	(0.393)
Observations	915	915	915	915	915
R-squared	0.005	0.024	-	0.029	-

Notes: This table reports the estimates of a benchmark model with no peer effect, the static and the dynamic peer-effect models. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* **itt** reads $\mathbf{itt}_{[i]}$. **itt** represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ represent the interaction of the expenditure change and the network change. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\mathbf{\Delta W}\,\mathbf{itt}$ represents the change in the share of one's partners that was offered the savings account.

Appendix Table A7: Peer-effect Model, Health Services

	Benchmark	Static PE		Dynamic PE	
	OLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
itt	-0.5884*	-0.4493	-0.2709	-0.4884	-0.3144
	(0.309)	(0.344)	(0.408)	(0.355)	(0.468)
$\mathbf{pe}_A$		0.1771***	0.6433***	0.2610***	0.9376**
		(0.052)	(0.141)	(0.078)	(0.378)
$\mathbf{pe}_B$				0.0333	-0.2471
				(0.044)	(0.598)
$\mathbf{pe}_C$				0.1590**	0.4556
				(0.066)	(0.292)
$\mathbf{W}^0\mathbf{itt}$		-1.0210**	-0.2123	-0.6886	0.1466
		(0.421)	(0.590)	(0.518)	(1.754)
$\mathbf{\Delta W}\,\mathbf{itt}$				0.3546	0.8089
				(0.385)	(2.507)
Constant	0.3605	0.6196	0.5654*	0.5019	0.2288
	(0.457)	(0.406)	(0.344)	(0.382)	(0.666)
Observations	915	915	915	915	915
R-squared	0.006	0.053	-	0.072	-

Notes: This table reports the estimates of a benchmark model with no peer effect, the static and the dynamic peer-effect models. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* **itt** reads $\mathbf{itt}_{[i]}$. **itt** represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ represent the interaction of the expenditure change and the network change. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\mathbf{\Delta W}\,\mathbf{itt}$ represents the change in the share of one's partners that was offered the savings account.

Appendix Table A8: Peer-effect Model, School Fees

	Benchmark	Static PE		Dynamic PE	
	OLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
itt	0.4732	0.4205	0.2940	0.4011	0.2693
	(0.340)	(0.347)	(0.380)	(0.330)	(0.340)
$\mathbf{pe}_A$		0.1500*	0.5287***	0.2294***	0.6336***
		(0.073)	(0.155)	(0.075)	(0.157)
$\mathbf{pe}_B$				0.0407	0.0512
				(0.039)	(0.226)
$\mathbf{pe}_C$				0.1601**	0.3438
				(0.058)	(0.252)
$\mathbf{W}^0\mathbf{itt}$		-0.1125	-0.5209	-0.3066	-0.4612
		(0.489)	(0.566)	(0.513)	(0.926)
$\boldsymbol{\Delta}\mathbf{W}\,\mathbf{itt}$				-0.3891	-0.1628
				(0.366)	(2.291)
Constant	-0.2731	-0.2258	-0.0801	-0.1885	-0.1481
	(0.491)	(0.392)	(0.291)	(0.363)	(0.422)
Observations	915	915	915	915	915
R-squared	0.003	0.027	-	0.053	-

Notes: This table reports the estimates of a benchmark model with no peer effect, the static and the dynamic peer-effect models. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* **itt** reads $\mathbf{itt}_{[i]}$. **itt** represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ represent the interaction of the expenditure change and the network change. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\boldsymbol{\Delta}\mathbf{W}\,\mathbf{itt}$ represents the change in the share of one's partners that was offered the savings account.

Appendix Table A9: Peer-effect Model, Clothing and Footwear

	Benchmark	Static PE		Dynamic PE	
	OLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
itt	-0.3272	-0.3411	-0.5190	-0.4259	-0.5409*
	(0.281)	(0.274)	(0.318)	(0.261)	(0.279)
$\mathbf{pe}_A$		0.1296***	0.6168***	0.1975***	0.9741***
		(0.038)	(0.222)	(0.049)	(0.338)
$\mathbf{pe}_B$				0.0967**	0.1084
				(0.034)	(0.247)
$\mathbf{pe}_C$				0.1741***	0.8444
				(0.036)	(0.559)
$\mathbf{W}^0\mathbf{itt}$		-0.3953	0.4562	-0.0165	0.1517
		(0.587)	(0.675)	(0.581)	(2.255)
$\mathbf{\Delta W}\,\mathbf{itt}$				0.1050	-0.4087
				(0.329)	(3.803)
Constant	0.0075	0.2123	0.4898	0.0996	0.0090
	(0.674)	(0.690)	(0.573)	(0.659)	(0.785)
Observations	915	915	915	915	915
R-squared	0.001	0.022	-	0.053	-

Notes: This table reports the estimates of a benchmark model with no peer effect, the static and the dynamic peer-effect models. Robust standard errors are in parentheses, clustered at the village level. Statistically significant coefficients are indicated as follows: *** 10%, ** 5%, * 1%. The i^{th}subscript has been dropped for all vectors, *i.e.* **itt** reads $\mathbf{itt}_{[i]}$. **itt** represents the intent-to-treat dummy, which takes value one if the household was offered the savings account. $\mathbf{pe}_A \equiv \mathbf{W}^0\Delta\mathbf{y}$ represents the change in partners' mean expenditure keeping partners constant. $\mathbf{pe}_B \equiv \Delta\mathbf{W}\,\mathbf{y}^0$ represents the change in partners' mean expenditure keeping expenditure constant. $\mathbf{pe}_C \equiv \Delta\mathbf{W}\,\Delta\mathbf{y}$ represent the interaction of the expenditure change and the network change. $\mathbf{W}^0\mathbf{itt}$ represents the share of one's partners at baseline that was offered the savings account. $\mathbf{\Delta W}\,\mathbf{itt}$ represents the change in the share of one's partners that was offered the savings account.