The Interplay Among Savings Accounts and Network-based Financial Arrangements: Evidence from a Field Experiment*

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Abstract

This paper studies how access to formal savings accounts affects network-based financial arrangements. We use a field experiment that granted access to a savings account to a random subset of households in 19 Nepalese villages. Exploiting a unique panel dataset that follows all bilateral informal financial transactions before and after the intervention, we show that households that were offered access to an account increased their loans and total transfers to others, independent of the treatment status of the receiver. The increase seemed to occur mostly on the intensive margin and to be driven by treatment households with more assets and greater financial inclusion at baseline.

Keywords: financial access; savings; networks; financial arrangements
JEL Classification: C93, D14, G21, O16, O17

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1 Introduction

A growing body of literature has shown that access to savings accounts has positive direct welfare effects on account holders and their families (Dupas and Robinson 2013a; Prina 2015; Brune et al. 2017; Kast et al. 2018). Nevertheless, little is known about the impact of savings accounts on the network of informal financial transactions of account holders. On the one hand, 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, savings accounts 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 curbing the effect of savings accounts on welfare (Ligon et al. 2000; Platteau 2000). On the other hand, access to savings can foster asset accumulation. Households with greater resources might increase transfers to others either because of altruism or due to a fear of social sanctions (Platteau 2000; Di Falco and Bulte 2011; Hoff and Sen 2006).

In this paper, we study the interplay of formal financial access and network-based informal financial transactions. We take advantage of a field experiment that gave access to savings accounts to households living in 19 villages in Nepal. These savings accounts represented the first access to the formal financial system for the vast majority of the sample. Importantly, the randomization design is within-village, that is, only a random subset of households within the community were granted access to savings accounts. Our study exploits the availability of unique panel network data containing information on informal financial transactions (i.e., loans and gifts given and received) between all sampled households before and after the randomized intervention.[1]

The panel dimension of the network data allows us to document the changes in the network topology with remarkable precision, that is, at the level of the financial flow between any given pair of households (dyad). Overall, our empirical strategy accounts for the fact that decisions to form or sever a financial link over time depends on the treatment status of each of the parts involved, which we are able to disentangle because the allocation of the treatment condition is independent across (old and new) financial partners.

We first consider undirected within-village dyads as the unit of observation. The

[1] By using census data, we avoid making the distributional assumptions necessary for sampled dyadic observations (Chandrasekhar and Lewis 2011).

results from the undirected dyadic regressions show an increase in the magnitude of loans and total transfers among the dyads in which at least one of the households was offered a savings account. We then consider directed dyadic observations to disentangle the direction of the detected effect. Our estimates from the directed sample suggest that the effect is driven by the giver side: households that are offered savings accounts increase loans and overall transfers towards others, independent of the treatment status of the receiver. The increase appears to be driven by the intensive margin, i.e., the magnitude rather than the number of loans increases significantly. Overall, our results suggest that there is no crowding out of informal financial activities due to formal savings, as in Ligon et al. (2000) and Platteau (2000). In contrast, the intervention appears to *increase* the overall level of network-based transactions within the village in an inclusive way, thereby also benefiting the households that were not offered a savings account. In terms of the potential channels at play, our analysis shows that treatment households that were more financially included and had a higher level of total assets prior to the intervention are more likely to give loans and transfers to others in the village. These results are in line with the argument that households that are better off make more transfers to others (Platteau 2000; Di Falco and Bulte 2011; Hoff and Sen 2006).

Our paper adds to the small stream of recent literature studying how access to savings accounts affects informal risk-sharing arrangements. The evidence comes from a handful of studies that have relied mostly on data on financial flows that are either anonymized or collected at one point in time only, and it is not clear cut. Dupas et al. (2019) find positive inter-household spillovers, with account holders sending more to their village network and relying less on relatives outside their village. However, their results rely on risk-sharing information reported by survey respondents (so-called 'ego-centric' network data), while we trace declared partners to the identity (and treatment status) of other respondents. Instead, Dizon et al. (2019) show a reduction in risk-sharing arrangements among a sample of vulnerable women in Kenya. They identify risk-sharing pairs based on the detailed network data elicited before the intervention and find that transfers are reduced by 53% if both partners are offered savings accounts and by 35% if one partner is offered a savings account. By eliciting the network topology at both the baseline and endline, we are also able to document the rewiring of financial links due to the intervention.[2] In addition, our

[2]In the context of microfinance, Banerjee et al. (2020) find a thinning of informal credit networks

paper contributes to the literature showing either that one's social network can be used as a commitment device to save actively in savings account (Kast et al. 2018) or that commitment savings products might make it easier to resist requests to share with friends and family (Dupas and Robinson 2013b; Brune et al. 2017).

Our study also relates to the growing literature studying the effects of social networks on overall economic outcomes.[3] Most previous studies do not have detailed information on social links and identify the individual reference group on the basis of the respondents' social context. Exceptions include, for example, Banerjee et al. (2013), Oster and Thornton (2011), and Cai et al. (2015), which, similar to our case, have detailed data on the links between households in the sample but, unlike us, exploit preintervention network data only.[4] In fact, data sources containing longitudinal information on social links was rarely available in the past. However, digital social interaction data are becoming increasingly accessible (Blumenstock et al. 2016), putting the study of network changes in the spotlight.

The following section describes the field experiment, data, and sample. Section 3 provides evidence that the exogenous expansion of formal financial access impacts the network of informal financial transactions and discusses the potential mechanisms. Section 4 concludes.

2 Experimental Design and Data

2.1 Data collection

We use household survey data from a randomized field experiment that offered access to formal savings accounts to a random sample of poor households in 19 villages surrounding Pokhara, Nepal. We conducted three survey rounds: two baselines and an endline. The

in neighborhoods or villages that were entirely exposed to microfinance relative to those that were not exposed. While they use longitudinal network data as we do, we contribute a different angle by studying the reshuffling of financial links within communities in which only a subset of households are granted formal financial access.

[3] Examples of studies using a randomized intervention to identify the causal effect of social networks are Banerjee et al. (2013); Cai et al. (2015); Duflo and Saez (2003); Duflo et al. (2008); Dupas (2014); Kremer and Levy (2008); Kremer and Miguel (2007); Kling et al. (2007); Oster and Thornton (2011).

[4] Another exception is Patnam (2011), who uses panel data on firms in India to study corporate peer effects. However, her study does not rely on exogenous variation generated by a randomized experiment or on self-declared network data.

first baseline survey was conducted in February 2009 to census all households with a female head aged 18-55.[5,6] Before the introduction of the savings accounts, a second baseline survey was conducted in May 2010. Both baseline surveys collected information on households' socioeconomic characteristics, but only the first survey collected data on the network of informal financial transactions.

After the completion of the second baseline survey, separate public lotteries were held in each village between the last two weeks of May and the first week of June 2010 to randomly assign the female household heads to either the treatment group or the control group.[7] The women assigned to the treatment group were offered the option to open a savings account at the local bank-branch office; the women assigned to the control group were not given this option but were not barred from opening a savings account at another institution.

A year after the beginning of the intervention, in June 2011, an endline survey was conducted. This survey collected information on households' socioeconomic characteristics and on the network of informal financial transactions. A total of 1,009 households were surveyed in both baseline waves; 91% of these households (*i.e.,* 915) were found and surveyed in the endline survey.[8] Attrition to complete the endline survey for the sample of 1,009 households who completed both baseline surveys does not differ statistically between the treatment and control households and, as Appendix Table A1 shows, is not correlated with observables related to network-based financial activity.

2.2 The savings accounts

Formal financial access in Nepal is very limited and concentrated in urban areas and among the wealthy.[9] Thus, most households typically save informally, storing cash at home, saving in the form of durable goods and livestock, or participating to Rotating

[5]The 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.

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

[7]The random assignment into the treatment and control groups was done publicly with balls in an urn, with no stratification based on observables. For additional details on the lottery and experimental design, see Prina (2015).

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

[9]According to the nationally representative "Access to Financial Services Survey," conducted in 2006 by the World Bank (Ferrari et al. 2007), only 20% of Nepalese households have a bank account.

Savings and Credit Associations (ROSCAs). The savings accounts offered in the context of our randomized intervention has all the characteristics of a formal 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).[10] In addition, the savings account does not have a minimum balance requirement.[11]

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 of 8% of their baseline weekly household income almost once a week for the first year of the intervention. While access to a savings account did not considerably increase total assets, it raised households' investments in health and education and improved their perceived financial situation.[12]

2.3 Sample characteristics and balance check

Appendix Table A2 shows the summary statistics of baseline characteristics for our sample of 915 households separately for the treatment and control groups. The last column reports the *t*-statistics of two-way tests of the equality of the means across the two groups, showing that randomization led to balance along the baseline characteristics.

Panel A shows that the sample comprised households whose female heads were, on average, 37 years old and had less than three years of schooling. Approximately 90% of respondents were married or living with their partner. The average household size at baseline was 5 people, two of whom were children. Weekly household income at baseline averaged Rs. 1,495 (equivalent to about $21).[13] Average total assets had a value of more than Rs. 44,000 (roughly $630). Approximately 15% of the households were banked at baseline, 17% had money in a ROSCA, and more than half stored money in a microfinance

[10] In the country report for Nepal, the International Monetary Fund (2011) indicates a 10.5% rate of inflation during the intervention period.

[11] 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.

[12] See Prina (2015) for a detailed analysis of the effects of providing access to a savings account on asset accumulation and household welfare.

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

institution (MFI). Households also typically had more than one week's worth of income stored as cash in their home. In terms of liabilities, 90% of the households had at least one outstanding loan. Overall, the sample population had a high level of participation in financial activities. However, households seemed to rely mostly on informal financial institutions rather than on banks. This phenomenon is in line with the nationally representative survey conducted in 2006 by the World Bank showing that over two-thirds of Nepalese households had an outstanding loan from a formal or informal institution (Ferrari et al. 2007). It is also consistent with previous literature showing that poor people had a portfolio of financial transactions and relationships (Collins et al. 2009; Rutherford 2000).

2.4 Data on informal financial transactions

In the first baseline survey and the endline survey, we collected detailed information on all informal network-based financial transactions. The female household head was asked to give a list of people (inside or outside the village) who regularly exchange gifts and/or loans with her or other members of her household. Respondents could list as many partners as they wished. 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 $1,200$; $1,200 - 2,400$; $2,400 - 5,000$; and more than $5,000$ rupees.[14] Special attention was devoted to accurately matching the declared partners' identities to sampled households within the village and to circumvent homonymy.[15] Panel B of Appendix Table A2 contains the network descriptive statistics at baseline by treatment status. On average, households reported having 1.50 financial partners: 0.72 within the village and 0.79 outside the village.[16] Loans seem to be more frequent than gifts: the declared number of gift and loan partners are 0.28 and 0.67, respectively.

[14] We also collected information on the exact amount of and the reason for transfers in the month prior to the survey. However, very few respondents reported an exact value for these transfers. Hence, in our main estimations, we use the ordinal measure that spans a longer period and incorporates multiple transactions.

[15] 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' unique identifiers were coded into the questionnaire while in the field rather than during the data cleaning process.

[16] To determine the number of partners within the village, we took the maximum report out of the two parts involved whenever discrepancies arose (this will be explained in Section 3). The number of partners outside the village was self-reported.

Two caveats are in order. First, our study focuses mainly on the network of informal financial transactions. One's social network, however, spans many dimensions of social interactions other than the financial ones, so that the change in the network of informal financial transactions may spill over to other types of social relationships that are beyond the scope of our analysis. Second, our study uses actual (rather than hypothetical) transfer data, (i.e., we asked households '*who did you exchange loans/gifts with?*' rather than '*who would you exchange loans/gifts with, in case of need?*'). Actual transfer data have an advantage in the context of financial exchanges because they limit the amount of measurement error due to subjective evaluations (Comola and Fafchamps 2014). Our results should be interpreted in light of the type of network data we elicited, that is, in terms of the actual transactions that occurred rather than the underlying network of support that can be triggered in case of need.

3 The Impact of the Intervention on the Network

3.1 Network description

We begin by taking an aggregate look at the dynamics of our network data. Our set of $n = 915$ households yields 28,154 dyads, i.e., 28,154 feasible undirected links between unique pairs of sampled households within a given village.[17] For each dyad ij, the variable c_{ij}^t represents a binary financial link between households i and j at time t (where $t = 0$ at baseline and $t = 1$ at endline). These are within-households links: $c_{ij}^t = 1$ if a member of household i declares a transfer (loan or gift, given or received) involving a member of household j at time t.[18] The average number of links within the village per household is

[17]We allow only links within the same village, and villages have different sample sizes. For a given village v of size n_v, the number of undirected dyads is computed as $\frac{n_v(n_v-1)}{2}$. Note that, for undirected transfers, the estimation sample includes only one observation per dyad ij and ji.

[18]For each observation c_{ij}^t, we may have up to eight reports: four reports for gifts (how much i declares to have given/received to/from j and how much j declares to have given/received to/from i) and similarly other four reports for loans. In principle, the answers to these questions should be concordant; in practice, they often are not, i.e., we observe cases where the report from household i does not match the one from j. 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. 2013). Following this literature, we take the maximum from the different reports. This is equivalent to assuming that discrepancies between survey answers correspond to underreporting, perhaps as a result of omission mistakes (Comola and Fafchamps 2014; Comola and Fafchamps 2017).

0.72 for both baseline and endline, and these village networks are spread out within small groups that display little clustering. Additionally, the network has undergone a reshuffle over time: out of the 328 links observed at baseline, only 73 (22%) were actually the same at endline, while 255 (78%) were different. In contrast, at endline, we observe 256 newly formed links.

Let us define T_i as the household intent-to-treat dummy variable, which takes a value of one at time $t = 1$ if household i was offered a savings account and zero otherwise.[19] Building on this notation, the treatment status of each undirected dyad is captured by three dyad-level indicators corresponding to different 'treatment groups': TT_{ij} identifies dyads in which both households were offered a savings account ($T_i = T_j = 1$), TC_{ij} identifies dyads in which only one household was offered an account ($T_i = 1$ and T_j=0, or $T_i = 0$ and $T_j = 1$), and CC_{ij} identifies dyads of untreated households ($T_i = T_j = 0$). These three groups consist of 7,208, 14,178 and 6,768 dyads. Appendix Table A3 reports the summary statistics for links c_{ij}^t by the treatment status of the dyad separately for overall transfers, gifts, and loans. Regarding transfers, there is an increase among TT_{ij} dyads from baseline to endline, as well as a slightly smaller increase for the TC_{ij} dyads. These results are accompanied by a dip in the number of CC_{ij} links between baseline and endline, which leads to no overall change in the number of links for the whole sample (328 links at baseline and 329 links at endline). In the lower part of Appendix Table A3, we disaggregate transfers into gifts and loans. Loans follow a similar path to the one for overall transfers. Nevertheless, gifts decrease across all types of dyads. This is likely to originate from two combined factors. First, Nepali festivities are based on a lunar calendar, which is approximately 10-12 days shorter than a Gregorian calendar, and our questions asked about gifts for a Gregorian year. For this reason, the Fagu Purnima and Maha Shivaratri religious festivals were sometimes counted twice in the baseline data (once in 2008 and then again in 2009) depending on the exact date of the interview. Second, the respondents were more likely to remember and report recent transfers. While no significant festivals occurred shortly before the endline survey, the baseline survey was conducted shortly after the Fagu Purnima festival, during which gifts are traditionally exchanged.

[19] In our notation $T_i = 0$ for all households at baseline. At endline, it takes value of zero for households that were not assigned to the treatment group and a value of one for those that were assigned to the treatment group.

3.2 Household-level regressions

Before describing the results of the dyad-level analysis, we first show the estimates of the impact of the intervention on informal financial transactions at the household level. Let $network_i^t$ be a proxy for the intensity of the network-based activity of household i at times $t = 0, 1$. We run the following intent-to-treat linear regression at the household level:

$$network_i^t = \beta_0 + \beta_1 T_i + \beta_2 x_i^t + \delta^t + \alpha_i + \epsilon_i^t \tag{1}$$

where x_i^t represents the demographic characteristics of i at time t, δ^t is the time dummy taking a value of 1 for the endline, α_i represents the household-level fixed effects and ϵ_i^t is the error term.[20]

Estimates are reported in Table 1 for the number of partners within the village and the number of partners outside the village separately.[21] We differentiate our estimates along the nature of the transaction: partners within the village can be defined in terms of gifts only (columns 1-2), loans only (columns 3-4) or both loans and gifts (columns 5-6). Specifications in columns 2, 4, 6 and 8 add controls for sociodemographic characteristics at time t.[22] The error term ϵ_i^t is clustered at the village level and wild-bootstrapped to account for the small number of clusters.[23,24]

The results show that having been offered a savings account increases the total number of partners *within* the village a year after the intervention. However, the estimates are only (marginally) significant for total partners and do not allow us to detect a significant

[20] This panel specification boils down to a first-differenced estimating equation since we rely on a two-wave panel. With respect to a standard intent-to-treat regression where outcomes are only measured at endline, the specification also allows us to document the time trends in aggregate transfers. This finding is in line with the dyadic specifications of Sections 3.3 and 3.4.

[21] Consistent with the rest of the paper, we consider the partners within the village whose identity was traced to respondents in the roster. For partners outside the village, we have to rely on self-reported data.

[22] The set of sociodemographic controls, which is the same throughout the paper, includes a dummy that takes a value of one if the female household head has no formal education, marital status dummies (married and single dummies, where the omitted category is separated/widowed/abandoned), and the number of children under 16 years of age.

[23] Note that when we include fixed effects (at the household level in this case), the estimate of the constant term is actually computed as an average of all estimated effects. Thus, its reported p-value is not bootstrapped (and this is the case throughout the paper).

[24] In the presence of many unlinked populations, clustering is the preferable solution for dyadic network data because it allows for arbitrary cross-observation dependence (Arcand and Fafchamps 2012; Barr et al. 2012).

increase for gift or loan partners. The time dummy for $t = 1$ is negative for gifts, in line with the evidence discussed in Section 3.1. We do not find any statistically significant intent-to-treat effect for the number of declared partners *outside* the village. However, as the coefficients for $t = 1$ show in columns 7 and 8, we find a marginally significant decrease in the number of partners outside the village, which is similar for the treatment and control households. Additionally, individual-level regressions do not account for the fact that the intervention might have spilled over to the network of informal financial transactions. We revisit the point in the following by estimating dyadic-level regressions.

3.3 Binary dyadic regressions

Household-level regressions do not account for the fact that the formation and severance of links are dyadic decisions, where one's outcome also depends on old and new partners. Indeed, by offering access to savings accounts for half of the households in each village, the intervention affected not only the treatment households but also the control households who were connected or could potentially be connected to them. We start by analyzing the dynamics of binary transfers and take the undirected within-village dyad as the unit of observation ($N = 28,154$ for each period $t = 0, 1$). The dependent variable c_{ij}^t equals one if there was a transfer between i and j at time t. Summary statistics for the relative frequency of undirected binary transfers (gifts, loans, and overall transfers) in the undirected dyadic sample are reported in Appendix Table A4.[25] We investigate whether the probability of a nonzero transfer between households i and j is affected by the treatment status of the dyad by running the following linear panel regression:

$$c_{ij}^t = \beta_0 + \beta_1 \cdot TC_{ij} + \beta_2 \cdot TT_{ij} + \beta_4 |x_i^t - x_j^t| + \delta^t + \alpha_{ij} + \epsilon_{ij}^t \tag{2}$$

The two coefficients of interest are those related to the treatment status of the dyad: TC_{ij} (TT_{ij}) takes a value of one at $t = 1$ if one partner (both partners) was offered the savings account and zero otherwise – where CC_{ij} is the excluded category.[26] δ^t represents the time fixed effect, α_{ij} represents the dyad-level fixed effect, and the error term ϵ_{ij}^t is

[25]Note that Appendix Table A4 reports the same information as Appendix Table A3, although in a different format. For example, the gift statistic for the TT group in $t = 0$ is 0.0058, which implies a number of gift links of $0.0058 \times 7,208 = 42$, as reported in Appendix Table A3.

[26]Recall that $T_i = 0$ for all households (and thus for all dyads) at baseline.

clustered and wild-bootstrapped at the village level, as described above. Time-varying controls are computed as the absolute difference between x_i^t and x_j^t.[27] Because we have a two-period panel, this specification corresponds to a first-difference estimation.[28] The fixed effects absorb all time-invariant dyad-specific attributes, addressing the concern of assortative matching along unobservables.[29]

Results are reported in Table 2. Columns 1-2 consider gifts only, columns 3-4 consider loans only, and columns 5-6 combine these two categories into total transfers. The coefficients for TC and TT are positive and significant for total transfers (columns 5-6). This indicates that the probability of a nonzero transfer (defined in terms of loans or gifts) between i and j increases with respect to the baseline if at least one of them was in the treatment group, i.e., was offered access to a savings account. These estimated coefficients may seem small at first glance because by construction, they represent the increase in the probability of forming *each* link within the village. Nevertheless, the estimate for TT in column 6 represents an increase of 36% over the mean of the dependent variable. The coefficients are not significant for loans and gifts separately. This may be partly due to our restrictive assumptions on the error terms.

Overall, the observed patterns of binary exchanges suggest no major impact on the extensive margin: the detected increase in the frequency of informal transactions for dyads in the treatment group is significant (at the margin) only for overall transfers. This finding is also in line with the individual-level results in Table 1. Note that this effect does not seem to be associated with an increase in the overall number of transfers across waves: the estimated effect for $t = 1$ is nonsignificant in columns (5) and (6) of Table 1 and Table 2. These findings, along with the descriptive statistics at the link level, suggest that the villages have undergone a link reshuffle with no impact on the aggregate level of informal financial activity: the decrease in the overall number of transfers within the

[27] Time-varying controls include a no-education dummy, two marital status dummies (single and married, with divorced or widowed as the omitted variable), and the number of children under age 16.

[28] Since the randomization is balanced, a cross sectional intent-to-treat regression on the dyadic sample at $t = 1$ would capture the differential outcomes at endline among dyadic treatment groups. However, it would not capture the change in total flows across periods or the substitution dynamics within different groups, while our specification does. For example, in a cross-sectional context, a positive coefficient associated with the TT group could be consistent with an increase, a decrease, or a stable level of aggregate flows at the village level (depending on the substitution effects between groups).

[29] We have also run the specification above without dyad-level fixed effects. This corresponds to a standard difference-in-differences estimator, given the way our dyadic treatment dummies are defined. Estimates (available upon request) reconfirm all results reported in Table 2.

dyads in the CC group is offset by the increase in the TC group (Appendix Tables A3 and A4).

3.4 Categorical dyadic regressions

We now consider the categorical measure of undirected transfers as the dependent variable. For each dyad and for both loans and gifts we observe four brackets (what i gives to j as reported by i and j and what i receives from j as reported by i and j), each coded on 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). Summary statistics for the categorical transfers in the undirected dyadic sample are shown in Appendix Table A5.

We re-estimate Equation (2) for a categorical dependent variable z_{ij}^t, defined as the maximum of the four brackets reported above, which represent the strength of the undirected flow between i and j over the period of reference. Note that this measure incorporates both the number of financial exchanges and the monetary amounts that flow through these links.[30] The estimates are reported in Table 3.

Consistent with our findings in Table 2, the coefficients for TC and TT are positive and statistically significant for total transfers. As the dependent variable is categorical, this finding indicates that the probability and/or the magnitude of the transfer between i and j increases if at least one of them is offered access to a savings account. There is also a similarly positive and statistically significant effect for loans. The remaining results are in line with those in Table 2, namely, no significant time trend for loans and transfers. Overall, the evidence in Table 3 suggests that the intervention has noticeably increased the amount of loan money flowing through the financial links whenever there is at least one treatment household in the dyad.

3.5 Directed flows

The earlier results show an increase in the magnitude of loans and total transfers for the dyads in which at least one of the households was offered a savings account. This leaves an open question regarding the direction of the detected effect, which we explore below. We

[30]The undirected estimates for categorical variables raise some interpretation concerns for total transfers because the measure aggregates over two distinct dimensions (ij and ji, as well as loans and gifts). Note that this issues does not apply to the directed estimates shown later in Table 4.

now consider directed categorical transfers to analyze the direction of the flows. Summary statistics for directed categorical transfers are reported in Appendix Table A6. Note that the estimation sample has now doubled (i.e. $N = 56,308 \times 2$): as directed transfers do not need to be symmetric, both dyads ij and ji are included separately.[31] We estimate the following linear panel regression:

$$k_{ij}^t = \beta_0 + \beta_1 TT_{ij}^d + \beta_2 TC_{ij}^d + \beta_3 CT_{ij}^d + \beta_4 (x_i^t - x_j^t) + \delta^t + \alpha_{ij} + \epsilon_{ij}^t \tag{3}$$

where the dependent variable k_{ij}^t represents the strength of the directed monetary flow *from* household i *to* household j at time t, coded using the same brackets as above: from 0 (no transfer) to 4 (more than 5,000 rupees), respectively.[32] We can now expand the set of variables accounting for the treatment status of the dyad because the treatment status of the potential giver and receiver can enter the regression separately. All dyadic treatment dummies in this directed sample now have a superscript d to avoid confusion: TT^d refers to dyads in which both giver and receiver households were in the treatment group; TC^d refers to dyads in which only the giver was offered a savings account; and CT^d to dyads in which only the receiver was offered a savings account. As discussed previously, the omitted category is CC^d. This is our preferred regression specification, as it allows us to split the estimation sample to investigate the direction of the effect for the undirected TC coefficients in Table 3. The specification also includes time-variant controls for both the giver and the receiver (x_i^t and x_j^t, respectively) in a simple difference,[33] time fixed effects (δ^t), and dyad fixed effects α_{ij}. As before, the standard errors are wild-bootstrapped at the village level.

The estimates reported in Table 4 suggest that the giver's treatment status (rather than the receiver's status) determines the amount of loans and total transfers: households offered access to savings account appear to make larger loans and overall transfers to their partners. The detected effect for the TT^d and TC^d dyads displays the same magnitude,

[31] For a given village v of size n_v, the number of undirected dyads is computed as $n_v(n_v - 1)$.

[32] Since we now have two statements about the transfer (loan/gift/all transfers) from i to j at time t (as reported by i and j, respectively), in line with the methodology above, we take the maximum of these two quantities. As discussed in Footnote 18, this corresponds to imputing discrepancies to recall mistakes, which seems appropriate in our context because in the majority of cases in which the two reports diverge we have one side reporting nothing.

[33] In a directed dyadic sample, we can use the simple difference among exogenous attributes, as regressors could differ for dyads ij and ji.

suggesting that treated households increase their money outflow of the same amount regardless of the treatment status of their partners. This suggests that the effect of TC in Table 3 is actually due to TC^d dyads and not to CT^d dyads. In fact, transfers from control households (to other control households or treated households) appear stable over time. This evidence is consistent with the statistics reported in Appendix Table A6 showing a sizable increase in the amount of loans for TT and TC dyads. This implies positive spillovers for control households and is associated with a net increase in the magnitude of transfers in the villages.

3.6 Mechanisms

We now focus on our preferred set of results, namely, the directed dyadic regressions in Table 4, to explore the mechanisms that could explain the observed data patterns. We discuss some complementary mechanisms that could explain our findings and test for the ones for which our data allow.

A first channel could be that treatment households that were better off at baseline, proxied by financial inclusion and total assets, make more transfers. To test this hypothesis, we built a variable measuring a household's initial level of financial inclusion prior to the intervention. In particular, we built a dummy variable indicating whether the household had access to a formal or informal financial institution at baseline (i.e., banks, MFIs or savings organizations, or ROSCAs). In addition, we constructed a standardized variable measuring total assets (i.e., monetary and nonmonetary assets) for each household at baseline.

A second explanation could be that households offered access to a savings account might face more redistributive pressure than those not offered a savings account. It is possible then that treatment households that had stronger beliefs about network support at baseline are more likely to give transfers to others afterward. To test this, we built an index measuring network beliefs by combining the answers to four questions about the respondent's beliefs about sharing in the community.[34]

[34] We first converted each answer on a Likert scale (1 to 5 based on how much a respondent agreed with each statement) to a dummy variable indicating whether they agreed or strongly agreed with any statement that would point to their preferences leaning towards sharing income in the village. We then added these dummy variables and standardized the outcome. The questions used to measure network support and beliefs were as follows: (a) '*If I ask someone (a relative, friend or neighbor) for money, and she/he has some, then she/he should help me.*'; (b) '*If someone (a relative, friend or neighbor) is in need*

We use these variables (hereafter 'V') to estimate the following equation:

$$\begin{aligned} k_{ij}^t = {} & \beta_0 + \beta_1 TT_{ij}^d + \beta_2 TC_{ij}^d + \beta_3 CT_{ij}^d \\ & + \beta_4 TT_{ij}^d * V_i + \beta_5 TC_{ij}^d * V_i + \beta_6 CT_{ij}^d * V_i \\ & + \beta_7 TT_{ij}^d * V_j + \beta_8 TC_{ij}^d * V_j + \beta_9 CT_{ij}^d * V_j + \alpha_{ij} + \epsilon_{ij}^t \end{aligned} \tag{4}$$

where V measures either the household's level of financial inclusion, its wealth, or its network beliefs at baseline, depending on the specification. This specification is an augmented version of Equation (3) aimed at identifying the 'heterogeneous treatment effect' in the context of our directed dyadic regressions: in fact, it enables us to see how these attributes of interest interplay with the treatment status of both the giver and the receiver household.

The results reported in Table 5 show that treatment households with a higher level of total assets at baseline are more likely to give loans and transfers (independently of the treatment status of the receiver). Similarly, more financially included treatment households are more likely to give loans and transfers to others. Network beliefs, however, do not appear to matter for inter-household transfers: treatment households that had stronger beliefs about network support at baseline were not more likely to give out transfers afterward.

An additional explanation could be that women now manage their finances more independently than before and are thus capable of transferring outside the household funds that had been flowing within the household previously (e.g., forced transfers to the husband). This hypothesis is consistent with the evidence that net transfers between households increased at the village level. Unfortunately, we cannot test this channel because we do not have data on within-household transfers.

Overall, while there might certainly be other channels at play, our evidence seems to show that the wealthier and more financially included treatment households were before the start of the intervention, the more transfers they make to others.

and asks me for money, then I help her/him and give up saving.'; (c) '*If I do not help with some money someone (a relative, friend or neighbor) in need, then she/he will not help me in the future when I need help.*'; (d) '*If someone (a relative, friend, or neighbor) who has some money does not help me when I am in need, then I will not help him/her in the future.*'

4 Conclusions

A growing body of research has documented the private benefits for account users from obtaining access to savings accounts. The effects on informal financial arrangements existing prior to the introduction of formal savings, however, are unclear. In particular, does access to a savings account crowd out network-based financial arrangements?

Using both household-level and directed and undirected dyadic regressions, we provide evidence that a randomized intervention that offered access to savings accounts to half of the households in 19 villages in Nepal positively affected informal financial arrangements. Our results show that being offered access to a savings account increases the number and size of transfers to financial partners within the village, regardless of their treatment status. Furthermore, transfers appear to be associated with treatment households with more assets and greater financial inclusion at baseline.

These results suggest some complementarity between formal savings and informal network-based financial activities. These findings point to additional benefits of access to savings accounts that go beyond the direct effects for recently banked households. In particular, interventions aimed at expanding access to bank accounts might have positive indirect effects on other households within account holders' financial networks. However, the studies assessing the potential spillover effects of access to savings accounts are scarce, and their findings vary (Dizon et al. 2019; Dupas et al. 2019). Thus, further research is needed to understand when and why access to savings accounts crowds out or complements pre-existing informal financial arrangements.

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Table 1: Household-level regressions, number of partners

	N. of gift partners within the village		N. of loan partners within the village		N. of total partners within the village		N. of total partners outside the village	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T_i	0.0166	0.0131	0.0790	0.0705	0.1355*	0.1288*	-0.0720	-0.0777
	(0.0773)	(0.0799)	(0.0667)	(0.0661)	(0.0684)	(0.0681)	(0.0933)	(0.0928)
$t = 1$	-0.1790**	-0.1724***	-0.0470	-0.0247	-0.0671	-0.0529	-0.2058*	-0.2015*
	(0.0685)	(0.0587)	(0.1039)	(0.1038)	(0.1201)	(0.1150)	(0.0937)	(0.0964)
Constant	0.2798***	0.3827***	0.6689***	0.7696***	0.7169***	0.8591***	0.7858***	0.9678***
	(0.0439)	(0.0799)	(0.0477)	(0.1718)	(0.0597)	(0.1796)	(0.0549)	(0.2407)
Controls	no	yes	no	yes	no	yes	no	yes
Observations	1,830	1,830	1,830	1,830	1,830	1,830	1,830	1,830
Household effects	yes	yes	yes	yes	yes	yes	yes	yes
N. of Households	915	915	915	915	915	915	915	915
R-squared	0.050	0.053	0.001	0.004	0.003	0.005	0.018	0.026

Notes: This table reports the estimates of the household-level intent-to-treat regressions measuring the effect of the intervention on the number of partners. OLS coefficients are reported.Fixed effects at the household level are included. Robust standard errors are in parentheses and clustered at the village level. The p-values are calculated using a clustered wild-bootstrap procedure. The coefficients for statistical significance are given as follows: * 10%, ** 5%, and *** 1%. Controls include a no-education dummy, marital status, and the number of children under age 16.

Table 2: Dyadic regressions, binary dependent variable

	Gifts only		Loans only		Total transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
TT_{ij}	0.0005	0.0003	0.0026	0.0025	0.0045*	0.0043*
	(0.0026)	(0.0026)	(0.0021)	(0.0022)	(0.0022)	(0.0022)
TC_{ij}	0.0007	0.0006	0.0029	0.0028	0.0039*	0.0038*
	(0.0014)	(0.0013)	(0.0021)	(0.0021)	(0.0023)	(0.0023)
t=1	-0.0033**	-0.0033**	-0.0022	-0.0024	-0.0031	-0.0033
	(0.0010)	(0.0011)	(0.0021)	(0.0021)	(0.0023)	(0.0024)
Controls	no	yes	no	yes	no	yes
Constant	0.0045***	0.0069***	0.0109***	0.0181***	0.0117***	0.0188***
	(0.0006)	(0.0022)	(0.0008)	(0.0038)	(0.0010)	(0.0041)
Mean of dep. var.	0.003	0.003	0.011	0.011	0.012	0.012
Obs.	56,308	56,308	56,308	56,308	56,308	56,308
N. of dyads	28,154	28,154	28,154	28,154	28,154	28,154

Notes: This table reports the estimates of the (undirected) dyadic intent-to-treat regressions. The binary dependent variable equals one if there was a transfer between i and j at time t. All undirected within-village dyads are taken as observations. Dyad-level fixed effects are included. OLS coefficients are reported. Robust standard errors are in parentheses and clustered at the village level. The p-values are calculated using a clustered wild-bootstrap procedure. The coefficients of statistical significance are given as follows: * 10%, ** 5%, and *** 1%. Controls include differences in marital status, the number of children under age 16 and no-education dummies.

Table 3: Dyadic regressions, categorical dependent variable

	Gifts only		Loans only		Total transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
TT_{ij}	-0.0006	-0.0008	0.0144**	0.0143**	0.0161***	0.0159***
	(0.0032)	(0.0032)	(0.0053)	(0.0055)	(0.0048)	(0.0049)
TC_{ij}	-0.0006	-0.0007	0.0116*	0.0114*	0.0123**	0.0120*
	(0.0017)	(0.0017)	(0.0054)	(0.0054)	(0.0054)	(0.0054)
t=1	-0.0031*	-0.0032*	-0.0019	-0.0022	-0.0030	-0.0033
	(0.0012)	(0.0013)	(0.0051)	(0.0050)	(0.0053)	(0.0053)
Controls	no	yes	no	yes	no	yes
Constant	0.0065***	0.0100**	0.0243***	0.0348***	0.0260***	0.0369***
	(0.0009)	(0.0037)	(0.0018)	(0.0066)	(0.0018)	(0.0071)
Mean of dep. var.	0.005	0.005	0.028	0.028	0.030	0.030
Obs.	56,308	56,308	56,308	56,308	56,308	56,308
N. of dyads	28,154	28,154	28,154	28,154	28,154	28,154

Notes: This table reports the estimates of the (undirected) dyadic intent-to-treat regressions. The dependent variable is coded on 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). All undirected within-village dyads are taken as observations. Dyad-level fixed effects are included. OLS coefficients are reported. Robust standard errors are in parentheses and clustered at the village level. The p-values are calculated using a clustered wild-bootstrap procedure. The coefficients of statistical significance are given as follows: * 10%, ** 5%, and *** 1%. Controls include differences in marital status, the number of children under age 16 and no-education dummies.

Table 4: Directed dyadic regressions, categorical dependent variable

	Gifts only		Loans only		Total transfers	
	(1)	(2)	(3)	(4)	(5)	(6)
TT^d_{ij}	-0.0016	-0.0019	0.0081**	0.0078**	0.0088***	0.0083***
	(0.0025)	(0.0026)	(0.0031)	(0.0031)	(0.0028)	(0.0027)
TC^d_{ij}	-0.0002	-0.0004	0.0079*	0.0078*	0.0085**	0.0083**
	(0.0013)	(0.0013)	(0.0034)	(0.0034)	(0.0035)	(0.0033)
CT^d_{ij}	-0.0004	-0.0006	0.0054	0.0052	0.0055	0.0052
	(0.0012)	(0.0013)	(0.0033)	(0.0032)	(0.0034)	(0.0033)
$t = 1$	-0.0029**	-0.0022**	-0.0026	-0.0014	-0.0039	-0.0023
	(0.0009)	(0.0008)	(0.0039)	(0.0040)	(0.0042)	(0.0041)
Controls	no	yes	no	yes	no	yes
Constant	0.0050***	0.0090***	0.0164***	0.0184*	0.0184***	0.0225**
	(0.0008)	(0.0022)	(0.0016)	(0.0091)	(0.0017)	(0.0088)
Mean of dep. var.	0.003	0.003	0.018	0.018	0.019	0.019
Obs.	112,616	112,616	112,616	112,616	112,616	112,616
N. of dyads	56,308	56,308	56,308	56,308	56,308	56,308

Notes: This table reports the estimates of the directed dyadic intent-to-treat regressions. The dependent variable is coded on 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). All directed within-village dyads are taken as observations. OLS coefficients are reported. Dyad-level fixed effects are included. Robust standard errors are in parentheses and clustered at the village level. The p-values are calculated using a clustered wild-bootstrap procedure. The coefficients of statistical significance are given as follows: * 10%, ** 5%, and *** 1%. Controls include differences in marital status, the number of children under age 16 and no-education dummies.

Table 5: Dyadic regressions with heterogeneous treatment effects

	Access to Financial Institutions			Standardized Total Assets			Standardized Network Preference		
	Gifts	Loans	Transfers	Gifts	Loans	Transfers	Gifts	Loans	Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$TT_i^d * V_i$	0.0005	0.0176***	0.0175***	-0.0018	0.0096***	0.0088***	0.0011	-0.0018	-0.0022
	(0.0033)	(0.0064)	(0.0064)	(0.0010)	(0.0030)	(0.0028)	(0.0011)	(0.0031)	(0.0028)
$TC_i^d * V_i$	-0.0023	0.0086*	0.0088	-0.0020	0.0087**	0.0081**	0.0013	0.0019	0.0029
	(0.0024)	(0.0049)	(0.0054)	(0.0011)	(0.0025)	(0.0024)	(0.0013)	(0.0024)	(0.0024)
$CT_i^d * V_i$	-0.0031*	0.0023	0.0016	-0.0016	0.0033	0.0022	0.0016	0.0049	0.0048
	(0.0012)	(0.0046)	(0.0049)	(0.0009)	(0.0029)	(0.0027)	(0.0010)	(0.0030)	(0.0031)
$TT_j^d * V_j$	-0.0012	0.0019	0.0022	-0.0024	-0.0008	-0.0014	0.0009	-0.0031	-0.0028
	(0.0038)	(0.0046)	(0.0061)	(0.0011)	(0.0046)	(0.0045)	(0.0012)	(0.0042)	(0.0039)
$TC_j^d * V_j$	-0.0030	0.0020	0.0022	-0.0015	-0.0002	-0.0007	0.0009	0.0071*	0.0075*
	(0.0020)	(0.0049)	(0.0055)	(0.0009)	(0.0035)	(0.0035)	(0.0015)	(0.0036)	(0.0038)
$CT_j^d * V_j$	-0.0039	0.0016	-0.0005	-0.0018	0.0006	-0.0005	0.0021	0.0002	0.0012
	(0.0027)	(0.0044)	(0.0057)	(0.0014)	(0.0018)	(0.0022)	(0.0013)	(0.0018)	(0.0017)
Mean dep.v.	0.003	0.018	0.019	0.003	0.018	0.019	0.003	0.018	0.019
Obs.	112,616	112,616	112,616	112,616	112,616	112,616	109,016	109,016	109,016
N. of dyads	56,308	56,308	56,308	56,308	56,308	56,308	54,508	54,508	54,508

Notes: This table reports the estimates of the directed dyadic intent-to-treat regressions. The dependent variable is coded on 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). All directed within-village dyads are taken as observations. OLS coefficients are reported. Dyad-level fixed effects are included. Robust standard errors are in parentheses and clustered at the village level. The p-values are calculated using a clustered wild-bootstrap procedure. The coefficients of statistical significance are given as follows: * 10%, ** 5%, and *** 1%.

Appendix Tables

Appendix Table A1: Attrition regressions

	Completed endline		
	(1)	(2)	(3)
T_i	0.009	0.007	0.007
	(0.015)	(0.015)	(0.015)
N. of partners - Total		0.005	
		(0.005)	
N. of partners - Village			-0.012
			(0.038)
N. of partners - Outside village			0.006
			(0.005)
N. of gift partners - Village		0.001	0.005
		(0.012)	(0.014)
N. of loan partners - Village		0.018	0.034
		(0.012)	(0.038)
Constant	0.919***	0.909***	0.909***
	(0.007)	(0.009)	(0.009)
Village dummies	yes	yes	yes
Observations	1,009	1,009	1,009
R-squared	0.056	0.066	0.066

Notes: Robust standard errors are in parentheses and clustered at the village level. The coefficients of statistical significance are given as follows: * 10%, ** 5%, and *** 1%. All regressors are computed at $t = 0$. T_i represents the intent-to-treat dummy, which takes a value of one if the household was offered a savings account. Please refer to Section 3 for details on the construction of the variables.

Appendix Table A2: Household descriptive statistics at baseline

	Sample (N=915)	Control (N=447)	Treatment (N=468)	T-stat
Panel A: Household statistics				
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)	
No formal education for the female household	0.34	0.33	0.35	0.68
	(0.48)	(0.47)	(0.49)	
Percent married/living with partner	0.89	0.88	0.90	0.77
	(0.32)	(0.33)	(0.31)	
Household size	4.99	4.98	4.99	0.12
	(1.80)	(1.78)	(1.82)	
Number of children	2.22	2.22	2.23	0.11
	(1.09)	(1.11)	(1.07)	
Total income last week	1,494.73	1,472.84	1,515.64	0.13
	(4,833.91)	(4,598.50)	(5,053.36)	
Total assets	44,469.26	42,510.10	46,340.51	1.14
	(50,891.76)	(45,540.07)	(46,340.51)	
Proportion of households with money in a bank	0.15	0.14	0.17	0.89
	(0.36)	(0.35)	(0.37)	
Proportion of households with money in a ROSCA	0.17	0.17	0.18	0.47
	(0.38)	(0.37)	(0.38)	
Proportion of households with money in an MFI	0.56	0.58	0.54	-1.25
	(0.50)	(0.49)	(0.50)	
Total amount of cash at home	2,054.72	1,947.51	2,157.13	0.81
	(3,883.55)	(4,054.65)	(3,714.25)	
Total liabilities	43,269.18	38,889.30	47,452.53	1.36
	(95,442.21)	(92,431.79)	(98,109.61)	
Percentage of households with outstanding loans	0.90	0.88	0.91	1.42
	(0.31)	(0.32)	(0.29)	
Panel B: Network statistics				
N. of partners - Total	1.50	1.50	1.50	0.03
	(1.55)	(1.47)	(1.63)	
N. of partners - Village	0.72	0.73	0.71	-0.26
	(1.17)	(1.08)	(1.25)	
N. of gift partners - Village	0.28	0.27	0.29	0.61
	(0.65)	(0.61)	(0.69)	
N. of loan partners - Village	0.67	0.66	0.67	0.12
	(1.14)	(1.03)	(1.23)	
N. of partners - Outside village	0.79	0.77	0.80	0.32
	(1.07)	(1.02)	(1.12)	

Please refer to the text in Section 3 for details on the construction of the variables.

Appendix Table A3: Summary statistics on links

	Sample	t=0	t=1	t=1 minus t=0	N. of dyads
Transfers					
Whole sample	657	328	329	1	28,154
TT_{ij}	198	94	104	10	7,208
TC_{ij}	298	143	155	12	14,178
CC_{ij}	161	91	70	-21	6,768
Gifts					
Whole sample	178	128	50	-78	28,154
TT_{ij}	64	42	22	-20	7,208
TC_{ij}	70	53	17	-36	14,178
CC_{ij}	44	33	11	-22	6,768
Loans					
Whole sample	609	306	303	-3	28,154
TT_{ij}	181	89	92	3	7,208
TC_{ij}	283	137	146	9	14,178
CC_{ij}	145	80	65	-15	6,768

Appendix Table A4: Summary statistics on undirected binary transfers

	Sample	t=0	t=1	t=1 minus t=0	T-stat	N. of dyads
Gifts						
TT_{ij}	0.0044	0.0058	0.0031	-0.0028	-2.5830	7,208
	(0.0665)	(0.0761)	(0.0552)	(0.0912)		
TC_{ij}	0.0025	0.0037	0.0012	-0.0025	-4.4342	14,178
	(0.0496)	(0.0610)	(0.0346)	(0.0682)		
CC_{ij}	0.0033	0.0049	0.0016	-0.0033	-3.5720	6,768
	(0.0569)	(0.0697)	(0.0403)	(0.0749)		
$Whole\ sample$	0.0032	0.0045	0.0018	-0.0028	-6.0947	28,154
	(0.0561)	(0.0673)	(0.0421)	(0.0763)		
Loans						
TT_{ij}	0.0126	0.0123	0.0127	0.0004	0.2582	7,208
	(0.1113)	(0.1104)	(0.1123)	(0.1369)		
TC_{ij}	0.0100	0.0097	0.0103	0.0006	0.5973	14,178
	(0.0994)	(0.0978)	(0.1010)	(0.1265)		
CC_{ij}	0.0107	0.0118	0.0096	-0.0022	-1.3868	6,768
	(0.1029)	(0.1081)	(0.1032)	(0.1315)		
$Whole\ sample$	0.0108	0.0109	0.0108	-0.0001	-0.1371	28,154
	(0.1034)	(0.1037)	(0.1032)	(0.1304)		
Transfers						
TT_{ij}	0.0137	0.0130	0.0144	0.0014	0.8276	7,208
	(0.1164)	(0.1135)	(0.0975)	(0.1423)		
TC_{ij}	0.0105	0.0101	0.0109	0.0008	0.7778	14,178
	(0.1020)	(0.1000)	(0.1040)	(0.1296)		
CC_{ij}	0.0119	0.0134	0.0103	-0.0031	-1.8638	6,768
	(0.1084)	(0.1152)	(0.1012)	(0.1370)		
$Whole\ sample$	0.0117	0.0117	0.0117	0.0000	0.0442	28,154
	(0.1074)	(0.1073)	(0.1075)	(0.1347)		

Notes: $t = 0$ refers to statistics at the baseline, while $t = 1$ refers to statistics at endline. Standard deviations are reported in parentheses. The column “t-stat” reports the t-statistics of the difference in means between the baseline and endline for each treatment group.

Appendix Table A5: Summary statistics on undirected categorical transfers

	Sample	t=0	t=1	t=1 minus t=0	T-stat	N. of dyads
Gifts						
TT_{ij}	0.0065	0.0083	0.0046	-0.0037	-2.1554	7,208
	(0.1087)	(0.1221)	(0.0934)	(0.1475)		
TC_{ij}	0.0038	0.0056	0.0020	-0.0037	-3.6786	14,178
	(0.0864)	(0.1034)	(0.0650)	(0.1187)		
CC_{ij}	0.0047	0.0062	0.0031	-0.0031	-2.0306	6,768
	(0.0937)	(0.0971)	(0.0901)	(0.1257)		
$Whole\ sample$	0.0047	0.0065	0.0029	-0.0036	-4.6441	28,154
	(0.0943)	(0.1071)	(0.0795)	(0.1283)		
Loans						
TT_{ij}	0.0343	0.0280	0.0405	0.0125	2.6211	7,208
	(0.3318)	(0.2778)	(0.3781)	(0.4044)		
TC_{ij}	0.0249	0.0200	0.0297	0.0097	3.2275	14,178
	(0.2766)	(0.2313)	(0.3154)	(0.3565)		
CC_{ij}	0.0281	0.0291	0.0272	-0.0019	-0.4155	6,768
	(0.2978)	(0.2996)	(0.2960)	(0.3804)		
$Whole\ sample$	0.0281	0.0243	0.0319	0.0076	3.4005	28,154
	(0.2967)	(0.2613)	(0.3283)	(0.3751)		
Transfers						
TT_{ij}	0.0361	0.0296	0.0427	0.0132	2.7357	7,208
	(0.3373)	(0.2843)	(0.3829)	(0.4090)		
TC_{ij}	0.0263	0.0217	0.0310	0.0093	3.0244	14,178
	(0.2838)	(0.2417)	(0.3204)	(0.3665)		
CC_{ij}	0.0298	0.0313	0.0284	-0.0030	-0.6318	6,768
	(0.3032)	(0.3055)	(0.3009)	(0.3848)		
$Whole\ sample$	0.0297	0.0260	0.0334	0.0074	3.2274	28,154
	(0.3030)	(0.2694)	(0.3332)	(0.3822)		

Notes: $t = 0$ refers to statistics at baseline, while $t = 1$ refers to statistics at endline. Standard deviations are reported in parentheses. The column "t-stat" reports the t-statistics of the difference in means between baseline and endline for each treatment group.

Appendix Table A6: Summary statistics on directed categorical transfers

	Sample	t=0	t=1	t=1 minus t=0	T-stat	N. of dyads
Gifts						
TT^d	0.0049	0.0071	0.0026	-0.0044	-4.0994	14,416
	(0.0576)	(0.1140)	(0.0696)	(0.1300)		
TC^d	0.0030	0.0045	0.0014	-0.0031	-3.5242	14,178
	(0.0760)	(0.0919)	(0.0557)	(0.1049)		
CT^d	0.0023	0.0039	0.0006	-0.0033	-4.4640	14,178
	(0.0636)	(0.0830)	(0.0346)	(0.0884)		
CC^d	0.0031	0.0045	0.0016	-0.0029	-3.3092	13,536
	(0.0746)	(0.0837)	(0.0643)	(0.1013)		
Whole sample	0.0033	0.0050	0.0016	-0.0034	-7.6132	56,308
	(0.0781)	(0.0942)	(0.0576)	(0.1074)		
Loans						
TT^d	0.0223	0.0196	0.0251	0.0054	1.9940	14,416
	(0.2655)	(0.2276)	(0.2986)	(0.3342)		
TC^d	0.0162	0.0135	0.0188	0.0054	2.1033	14,178
	(0.2215)	(0.1810)	(0.2556)	(0.3035)		
CT^d	0.0154	0.0140	0.0168	0.0028	1.2161	14,178
	(0.2154)	(0.1936)	(0.2352)	(0.2762)		
CC^d	0.0175	0.0188	0.0163	-0.0026	-0.9927	13,536
	(0.2332)	(0.2361)	(0.2304)	(0.3030)		
Whole sample	0.0179	0.0164	0.0193	0.0029	2.2240	56,308
	(0.2348)	(0.2106)	(0.2568)	(0.3051)		
Transfers						
TT^d	0.0241	0.0216	0.0265	0.0049	1.7110	14,416
	(0.2718)	(0.2375)	(0.3021)	(0.3407)		
TC^d	0.0175	0.0152	0.0197	0.0046	1.7454	14,178
	(0.2288)	(0.1926)	(0.2600)	(0.3128)		
CT^d	0.0166	0.0158	0.0174	0.0016	0.6487	14,178
	(0.2211)	(0.2034)	(0.2375)	(0.2848)		
CC^d	0.0189	0.0209	0.0170	-0.0039	-1.4763	13,536
	(0.2383)	(0.2424)	(0.2342)	(0.3086)		
Whole sample	0.0193	0.0184	0.0202	0.0018	1.4023	56,308
	(0.2410)	(0.2199)	(0.2604)	(0.3125)		

Notes: $t = 0$ refers to statistics at baseline, while $t = 1$ refers to statistics at endline. Standard deviations are reported in parentheses. The column "t-stat" reports the t-statistics for the difference in means between the baseline and endline for each treatment group.