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

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Abstract

This paper studies how formal financial access 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 be driven by treatment households with more assets and greater financial inclusion at baseline.

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1 Introduction

A growing body of literature has studied the direct welfare effects of access to savings accounts on account holders and their families (De Mel *et al.* 2020; Dupas *et al.* 2018; Dupas and Robinson 2013; Prina 2015; Brune *et al.* 2017; Kast *et al.* 2018). Nevertheless, little is known about the indirect impact of savings accounts on the informal financial arrangements 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 (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 living in each village was 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 sent and received) between all households before and after the randomized intervention.[1] The panel dimension of the network data allows us to trace changes in transfers at the level of the dyad, and it informs about the dynamics of aggregate flows across periods. Since the treatment allocation is independent across (old and new) financial partners, we are able to quantify social spillovers which operate through the observed network structure.[2]

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

[1]As shown by Chandrasekhar and Lewis (2011), with census data one can avoid making the distributional assumptions needed for sampled dyadic observations.

[2]In our identification strategy combining within-village treatment and detailed network data, the control group is constituted by pairs of 'untreated' households within a given village. Other studies on social networks take a different avenue by exploiting a randomization design at the village level (Banerjee *et al.* 2013, 2021).

results from the dyadic regressions 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. Then, we use 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 sender's side: households that are offered savings accounts increase loans and total transfers towards others, independent of the treatment status of the receiver. 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 network-based transactions within the village and spill over to 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 increase loans and total 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 recent literature studying how access to savings accounts affects informal risk-sharing arrangements. The current 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 self-reported risk-sharing information (so-called 'ego-centric' link data), while we trace declared partners to the identity (and treatment status) of other respondents. Dizon *et al.* (2019) instead, 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. Our study exploits the panel dimension of risk-sharing arrangements: eliciting the network topology at both the baseline and endline enables us to document the rewiring of financial links due to the intervention. In the context of microfinance, Banerjee *et al.* (2021) find a thinning of informal credit networks in communities 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. To our knowledge, our paper is the first to study the interplay of formal savings and informal risk-sharing arrangements by combining longitudinal network data with within-village treatment allocation. This design has advantages and limitations. One main advantage is that it creates exogenous variation in the treatment status of dyads at the village level. This enables us to compare the changes in financial networks for different treatment groups (i.e. treatment-treatment vs. treatment-control dyads) within the same village. On the other side, the randomization scheme implies that we have no 'pure' control villages. This forces us to rely on the changes within control-control dyads to infer what would have happened in a counterfactual world with no randomized access to savings accounts, which can be seen as an intrinsic drawback of our strategy.

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 interpersonal 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 pre-intervention 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.

2 Experimental Design and Data

We exploit the randomized field experiment conducted by Prina (2015) that offered access to formal savings accounts to a random sample of poor households in 19 villages surrounding Pokhara, Nepal.

[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.

We use three survey rounds: two baselines and an endline. The 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, our partner bank progressively began operating in the 19 villages between the last two weeks of May and the first week of June 2010. First, a pre-announced public meeting was held in each village, with the aim of launching the program and providing the entire population with some basic financial literacy on the benefits of savings and savings accounts.[7] Separate public lotteries were then 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.[8] 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 elsewhere.

A year after the beginning of the intervention, in June 2011, an endline survey was conducted in order to collect 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.[9] 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 is not correlated with observables

[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 villages in our sample have about 48 households and 240 inhabitants on average.

[7]At this meeting, participants were told (1) about the benefits of savings; (2) that the bank was about to launch a savings account; (3) the characteristics of the savings account; (4) what the savings account could help them with and how they could use it; and (5) that the savings account would only be initially offered to half of the households via a public lottery. The public talk was given by a bank employee and it was followed by a short session of questions and answers.

[8]The random assignment into the treatment and control groups was done publicly with balls in an urn, with no stratification based on observables.

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

related to network-based financial activity (Appendix Table A1).

2.1 The savings accounts

Formal financial access in Nepal is very limited and concentrated in urban areas and among the wealthy.[10] 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). The savings accounts offered in the 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).[11] In addition, the savings account does not have a minimum balance requirement.[12]

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 children's education and improved their perceived financial situation.[13]

2.2 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 baseline characteristics.

[10] 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.

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

[12] 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.

[13] Prina (2015) provides a detailed analysis of the effects of the intervention on asset accumulation and welfare at the household level. It does not, however, exploit the network data to assess the intervention's effect on informal financial arrangements as we do in this paper.

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 five people, two of whom were children. Weekly household income at baseline averaged Rs. 1,495 (equivalent to about $21).[14] 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 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 have a portfolio of financial transactions and relationships (Collins *et al.* 2009; Rutherford 2000).

2.3 Data on informal financial transactions

The first baseline survey and the endline survey 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 herself or other members of the household. Respondents could list as many partners as they wished. For each partner, the amount of loans and gifts sent 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 $5,000$ Rs.[15] Special attention was devoted

[14] Amounts are expressed in Nepalese rupees (the exchange rate was roughly Rs. 70 to USD 1 during the study period). 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.

[15] 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 estimations, we use the ordinal measure that spans a longer period and incorporates multiple transactions.

to accurately match the declared partners' identities to sampled households within the village and to circumvent homonymy.[16] 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.[17] The declared number of gift and loan partners are 0.28 and 0.67, respectively.

Two caveats are in order. First, our study focuses 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

Our set of $n = 915$ households yields 28,154 undirected dyads, i.e., 28,154 unique undirected pairs of sampled households within a given village.[18] To take an aggregate look at the dynamics of our network data, we construct a dummy variable $c_{ij,t}$ representing a financial link between households i and j at time t (where $t = 0$ at baseline and $t = 1$ at

[16] At the end of each interview, the enumerator used an updated roster of the village 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.

[17] 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.

[18] 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 $\frac{n_v(n_v-1)}{2}$. Thus the undirected sample includes only one observation per dyads ij and ji.

endline).

We start by defining these financial links on the basis of total transfers: for each dyad ij, we set $c_{ij,t} = 1$ if a member of household i declares a transfer (loan *or* gift, sent or received) involving a member of household j at time t.[19] The resulting network is sparse into small groups that display little clustering. The overall number of links remains remarkably stable across waves (328 links at baseline *vs.* 329 at endline). However, 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.

Next, we look at financial links defined on the basis of loans only, or gifts only. The number of links defined on the basis of loans remains stable across waves (306 links at baseline *vs.* 303 at endline). Nevertheless, links based on gifts are much less frequent at endline (128 links at baseline *vs.* 50 at endline). This drop 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.

3.2 Undirected flows

The formation and severance of informal financial links are dyadic decisions, where one's outcome also depends on the status of her partners. Indeed, by offering access to savings accounts to 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. In what follows we take the undirected dyad as the unit of observation ($N = 28,154$ for each period $t = 0, 1$), and we investigate whether the

[19]For each observation $c_{ij,t}$, we may have up to eight reports: four reports for gifts (how much i declares to have sent/received to/from j and how much j declares to have sent/received to/from i) and similarly other four reports for loans.

magnitude of the transfer between households i and j is affected by the treatment status of the dyad. To do so we estimate the following linear panel regression:

$$z_{ij,t} = \beta_0 + \beta_1 \cdot TC_{ij,t} + \beta_2 \cdot TT_{ij,t} + \delta_t + \alpha_{ij} + \epsilon_{ij,t} \tag{1}$$

For each dyad and for both loans and gifts the survey reports 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 Rs), 2 (1,200-2,400 Rs), 3 (2,400-5,000 Rs), and 4 (more than 5,000 Rs). The dependent variable $z_{ij,t}$ is defined as the maximum of the reports by the respondents, and it represents the strength of the undirected financial flow between i and j over the period of reference.[20] We compute $z_{ij,t}$ for gifts, loans and total transfers (e.g. gifts *or* loans) respectively. By estimating a linear equation we treat these categorical reports as having cardinal meaning – alternative estimation strategies are discussed in Section 3.4.

The coefficients of interest are those related to the treatment status of the dyad: $TT_{ij,t}$ always takes value zero at $t = 0$, and it takes value one at $t = 1$ if both partners were offered the savings account. Conversely, $TC_{ij,t}$ takes value zero at $t = 0$ and takes value one at $t = 1$ if only one of the two households was offered the savings account. The excluded cathegory $CC_{ij,t}$ refers to dyads composed by two control-group households. Summary statistics on transfers in the undirected dyadic sample by treatment status are shown in Appendix Table A3.

The specification of Equation (1) also includes a time dummy δ_t (taking value 1 for endline), and α_{ij} which represents the dyad-level fixed effect. The error term ϵ^t_{ij} is clustered at the village level and wild-bootstrapped to account for the small number of clusters.[21,22] The fixed effects absorb all time-invariant unobserved heterogeneity at the level

[20] Taking the maximum value of the reports by i and j is equivalent to assuming that discrepancies are due to under-reporting, perhaps as a result of omission mistakes (Comola and Fafchamps 2014; Comola and Fafchamps 2017). This solution is standard to the literature using dyadic data (De Weerdt 2004; De Weerdt and Fafchamps 2011; Fafchamps and Lund 2003; Liu *et al.* 2012; Banerjee *et al.* 2013), and it appears appropriate in our context because for the majority of discordant reports we have one side reporting nothing. However, we have re-estimated Equation (1) under alternative assumptions regarding mis-reporting, and reached conclusions consistent with the discussion reported below.

[21] In the presence of many unlinked populations, clustering is suitable for dyadic network data because it allows for cross-observation dependence of arbitrary form in link-formation decisions within the village beyond direct partnerships (Arcand and Fafchamps 2012; Barr *et al.* 2012).

[22] The standard errors we report throughout the paper are clustered at the village level, while the thresholds for statistical significance (e.g., * for 10%) are based on (clustered) wild-bootstrap p-values

of the dyad.[23] Since we have a two-period panel, this specification corresponds to a first-difference estimator.

Results are reported in Table 1. Columns (1) and (2) consider loans and gifts respectively, while column (3) combines these two categories into total transfers. The coefficients for TT and TC are positive and significant for loans and total transfers (columns 2-3), while we find no effect for gifts. This suggests that the magnitude of the transfer between i and j increases if at least one of them was offered access to a savings account. These estimated coefficients may seem small at first glance because the dyadic sample accounts by construction for all possible links within the village, but they represent an increase of roughly 50% over the mean of the dependent variable. Note that this detected effect does not seem to be associated with a decrease for the CC group: the estimated effect for $t = 1$ is non-significant in columns (2) and (3), in line with the statistics reported in Appendix Table A3. Overall, the evidence in Table 1 suggests that the intervention has increased the amount of loan and total money flowing through the financial links whenever there is at least one treatment household in the dyad.

3.3 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 flows, which we explore below by considering directed transfers. Note that the estimation sample has now doubled (i.e. $N = 56,308$ for each period $t = 0, 1$), since both dyads ij and ji are included separately.[24] We estimate the following linear panel regression:

$$k_{ij,t} = \beta_0 + \beta_1 TT^d_{ij,t} + \beta_2 TC^d_{ij,t} + \beta_3 CT^d_{ij,t} + \delta_t + \alpha_{ij} + \epsilon_{ij,t} \tag{2}$$

where the dependent variable $k_{ij,t}$ represents the strength of the directed monetary flow

(Roodman *et al.* 2019). Note that the constant term is computed as an average of all estimated fixed effects, thus its p-value is not bootstrapped.

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

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

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.[25] We can now expand the set of variables accounting for the treatment status of the dyad because the treatment status of the potential sender and receiver can now enter the regression separately. All dyadic treatment dummies in this directed sample now have a superscript d to avoid confusion with Table 1: $TT^d_{ij,t}$ refers to dyads in which both sender and receiver households were in the treatment group. Following the same convention as before it takes value zero at $t = 0$, and at $t = 1$ it takes value one if both partners were offered the savings account. Similarly, $TC^d_{ij,t}$ refers to dyads in which only the sender was offered the savings account; and $CT^d_{ij,t}$ to dyads in which only the receiver was offered the savings account. As discussed previously, the omitted category is $CC^d_{ij,t}$ (which takes value zero throughout). Equation (2) is our preferred specification, as it allows us to disentangle the direction of the detected effect for the TC coefficients in Table 1. The equation also includes the time dummy (δ^t) and dyad fixed effects α_{ij}. As before, the error terms are wild-bootstrapped at the village level. Summary statistics for directed transfers by treatment group are reported in Appendix Table A4.

The estimates reported in Table 2 suggest that the sender'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 total transfers. The detected effects for the TT^d and TC^d dyads display the same magnitude, suggesting that treated households increase their money outflow in a like manner regardless of the treatment status of their partners. This implies that the effect of TC in Table 1 is actually due to TC^d dyads rather than CT^d dyads. In fact, loans and total transfers from control households (to other control households or treated households) do not vary significantly over time. This evidence is consistent with the statistics reported in Appendix Table A4 showing a significant increase in the amount of loans for TT and TC dyads. This result points at positive spillovers for control households and it is associated with a net increase of transfers within the villages.

[25]In line with the estimation strategy above, we take the maximum of the directed reports about the transfer from i to j at time t (as reported by i and j, respectively).

3.4 Alternative specifications

We now discuss changes in the econometric specification along several lines. First, our linear estimates of Tables 1 and 2 use a dependent variable which is categorical and groups transfers into bins of different 'size.' As such, this strategy may raise concerns related to the interpretation of the resulting coefficients. To shed light on the issue, we take an alternative approach by running a set of binary regressions defined over different thresholds of the categorical dependent variable. Appendix Table A5 does so for the undirected dyadic sample, and thus it compares to Table 1. The binary dependent variable equals one if there was a transfer between i and j above a given threshold: any non-zero transfer (columns 1-3), above 1,200 rupees (columns 4-6), above 2,400 rupees (columns 7-9), above 5,000 rupees (columns 10-12). The estimates reported in Appendix Table A5 show that the TT and TC coefficients for loans and total transfers are positive and significant for the highest thresholds only (above 2,400 rupees, and above 5,000 rupees). These results, along with the ones from Table 1, indicate that the treatment did not affect the likelihood of *any* transfer being made, but only of large transfers being made.

In Appendix Table A6 we apply the same estimation strategy to the directed dyadic sample, which gives a straightforward comparison with Table 2. When we define the binary variable in terms of any transfer (columns 1 to 3) the treatment variables do not appear significant. As we increase the threshold, we get closer to the main results of current Table 2 (i.e. both TT^d and TC^d highly significant). Taken together, results from Appendix Tables A5 and A6 validate the main message of the paper and suggest that the detected effect for the treatment group comes from transfers of large size.[26]

Finally, our results appear robust to several alternative specifications. For instance, we removed the dyad-level fixed effects to estimate a standard difference-in-differences regression on the dyadic panel.[27] Also, we experimented with alternative specifications including time-varying controls (i.e., socio-demographic attributes of the households). In all cases above, our main conclusions stay unaffected (results available upon request).

[26] We also relaxed the linearity assumption by estimating an ordered probit model, and obtained results which are in line with the findings discussed above.

[27] Since in our notation the treatment dummies always take value zero at baseline, our estimation strategy could be seen as a difference-in-differences specification 'augmented' for dyad-level fixed effects.

3.5 Mechanisms

Next we explore the channels that could explain the observed data patterns focusing on our preferred set of results, namely, the directed dyadic regressions in Table 2. We discuss some of the 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 indicating whether the household had access to a formal or informal financial institution at baseline, prior to the intervention. We constructed the financial access variable by taking the maximum out of three separate questions in the baseline survey which ask whether the household had any money in formal financial institutions (banks), MFIs or savings organizations, or ROSCAs. Given that the answer to each question was coded as a dummy variable, the final financial access variable is also a dummy variable. In addition, we constructed a standardized variable measuring total assets for each household at baseline. In order to do so, first we combined the values of all monetary assets (i.e. savings at formal financial institutions, ROSCAs, MFIs along with cash at hand) and non-monetary assets (i.e. livestock and poultry and consumer durables) owned by each household at the baseline.[28] Then, we standardized the total assets variable.

It is also possible that households offered access to a savings account might face more redistributive pressure than those not offered a savings account. In addition, treatment households that had stronger beliefs about network support at baseline might be more likely to give transfers to others afterward. To test this, we built a measure of network beliefs by combining the survey answers to four questions about the respondent's beliefs about sharing in the community.[29] Given that each question was on a Likert scale from

[28]Livestock and poultry include goats, pigs, baby cows/bulls/buffaloes, cows, bulls, buffaloes, chickens, and ducks. Consumer durables include radios, cassette players, sewing machines, busses and trucks, motorcycles, bicycles, phones and cell phones, televisions, stoves, gas ranges, beds, sofas/sofa sets, electric fans, refrigerators, tables and chairs and cupboards.

[29]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 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.*'

1 to 5 (strongly disagree to strongly agree), we first converted each answer to a dummy variable equal to one if the respondent agreed or strongly agreed with each statement. We then added these four variables and standardized the outcome.[30]

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

$$\begin{aligned}
k_{ij,t} = {} & \beta_0 + \beta_1 TT^d_{ij,t} + \beta_2 TC^d_{ij,t} + \beta_3 CT^d_{ij,t} \\
& + \beta_4 TT^d_{ij,t} * V_i + \beta_5 TC^d_{ij,t} * V_i + \beta_6 CT^d_{ij,t} * V_i \\
& + \beta_7 TT^d_{ij,t} * V_j + \beta_8 TC^d_{ij,t} * V_j + \beta_9 CT^d_{ij,t} * V_j + \delta_t + \alpha_{ij} + \epsilon_{ij,t}
\end{aligned} \tag{3}$$

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 (2) aimed at identifying the 'heterogeneous treatment effect' in the context of our panel dyadic regressions: in fact, it enables us to see how these attributes of interest interplay with the treatment status of both the sender and the receiver household.

The results reported in Table 3 show that treatment households with a higher level of total assets at baseline are more likely to increase loans and transfers (independently of the treatment status of the receiver). Similarly, more financially included treatment households are more likely to increase loans and transfers to others. Network beliefs of the sender do not appear to matter for inter-household transfers: households with stronger beliefs about network support at baseline were not more likely to give out transfers afterward. We find a marginally significant effect of the receiver's beliefs, however, for TC^d dyads. This result points at the redistributive pressure exerted by households with strong beliefs about network support who were not offered a savings account.

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.

[30] As a robustness check, we have also conducted a principal component analysis on the four survey responses about the respondent's network beliefs. The estimates from a regressions that takes the first principal component as the beliefs variable are consistent with the results shown in Table 3 (available upon request).

Overall, while other channels might be 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 after being offered the savings account.

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?

We provide evidence that a randomized intervention that offered access to savings accounts to half of the households in 19 villages in Nepal increased informal financial arrangements. Our identification strategy combines within-village treatment and detailed network data and uses directed and undirected dyadic regressions. Our results show that being offered access to a savings account raises the magnitude 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.[31]

Our findings suggest some complementarity between formal savings and informal network-based financial activities, and point to spillover effects of access to savings accounts that go beyond the direct effects for recently banked households. 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 under which circumstances granting access to savings accounts crowds out or complements pre-existing informal financial arrangements, such as reciprocal risk sharing and informal loan repayment.

[31] These findings complement the ones by Prina (2015) suggesting that the intervention had an indirect effect beyond the treatment households.

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Table 1: Undirected dyadic regressions

	Gifts only	Loans only	Total transfers
	(1)	(2)	(3)
TT	-0.0006	0.0144**	0.0161***
	(0.0032)	(0.0053)	(0.0048)
TC	-0.0006	0.0116*	0.0123**
	(0.0017)	(0.0054)	(0.0054)
$t=1$	-0.0031*	-0.0019	-0.0030
	(0.0012)	(0.0051)	(0.0053)
Constant	0.0065***	0.0243***	0.0260***
	(0.0009)	(0.0018)	(0.0018)
Mean of dependent variable	0.005	0.028	0.030
Number of observations	56,308	56,308	56,308
Number of dyads	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). The total transfers variable is the maximum of the loans and gifts variables. 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%.

Table 2: Directed dyadic regressions

	Gifts only	Loans only	Total transfers
	(1)	(2)	(3)
TT^d	-0.0016	0.0081**	0.0088***
	(0.0025)	(0.0031)	(0.0028)
TC^d	-0.0002	0.0079*	0.0085**
	(0.0013)	(0.0034)	(0.0035)
CT^d	-0.0004	0.0054	0.0055
	(0.0012)	(0.0033)	(0.0034)
$t = 1$	-0.0029**	-0.0026	-0.0039
	(0.0009)	(0.0039)	(0.0042)
Constant	0.0050***	0.0164***	0.0184***
	(0.0008)	(0.0016)	(0.0017)
Mean of dependent variable	0.003	0.018	0.019
Number of observations	112,616	112,616	112,616
Number of dyads	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). The total transfers variable is the maximum of the loans and gifts variables. 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%.

Table 3: Directed 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)
$TT^d * V_i$	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)
$TC^d * V_i$	-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)
$CT^d * V_i$	-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)
$TT^d * V_j$	-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)
$TC^d * V_j$	-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)
$CT^d * V_j$	-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	105,564	105,564	105,564
N. of dyads	56,308	56,308	56,308	56,308	56,308	56,308	52,782	52,782	52,782

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). The total transfers variable is the maximum of the loans and gifts variables. 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)
$T_i = 1$	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 i was offered a savings account. Please refer to sections 2.3 and 2.4 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	1.97	1.94	2	0.71
	(1.25)	(1.27)	(1.22)	
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)	

Appendix Table A3: Summary statistics on undirected transfers

	Sample	t=0	t=1	t=1 minus t=0	T-stat	N. of dyads
Gifts						
TT	0.0065	0.0083	0.0046	-0.0037	-2.1554	7,208
	(0.1087)	(0.1221)	(0.0934)	(0.1475)		
TC	0.0038	0.0056	0.0020	-0.0037	-3.6786	14,178
	(0.0864)	(0.1034)	(0.0650)	(0.1187)		
CC	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	0.0343	0.0280	0.0405	0.0125	2.6211	7,208
	(0.3318)	(0.2778)	(0.3781)	(0.4044)		
TC	0.0249	0.0200	0.0297	0.0097	3.2275	14,178
	(0.2766)	(0.2313)	(0.3154)	(0.3565)		
CC	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	0.0361	0.0296	0.0427	0.0132	2.7357	7,208
	(0.3373)	(0.2843)	(0.3829)	(0.4090)		
TC	0.0263	0.0217	0.0310	0.0093	3.0244	14,178
	(0.2838)	(0.2417)	(0.3204)	(0.3665)		
CC	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 transfers variable is the maximum of the loans and gifts variables. The column “t-stat” reports the t-statistics of the difference in means between baseline and endline for each treatment group.

Appendix Table A4: Summary statistics on directed 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 transfers variable is the maximum of the loans and gifts variables. The column “t-stat” reports the t-statistics for the difference in means between the baseline and endline for each treatment group.

Appendix Table A5: Undirected dyadic panel, binary regressions for different thresholds

	At least one transfer			At least Rs. 1,200			At least Rs. 2,400			At least Rs. 5,000		
	Gifts	Loans	Transfers	Gifts	Loans	Transfers	Gifts	Loans	Transfers	Gifts	Loans	Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TT	0.0005	0.0026	0.0045*	-0.0004	0.0017	0.0017	-0.0004	0.0048***	0.0048**	-0.0003	0.0053***	0.0052***
	(0.0026)	(0.0021)	(0.0022)	(0.0004)	(0.0019)	(0.0017)	(0.0004)	(0.0015)	(0.0014)	(0.0002)	(0.0015)	(0.0014)
TC	0.0007	0.0029	0.0039	-0.0006**	0.0025	0.0023	-0.0003	0.0027*	0.0027*	-0.0004	0.0036***	0.0033**
	(0.0014)	(0.0021)	(0.0023)	(0.0002)	(0.0017)	(0.0016)	(0.0002)	(0.0014)	(0.0014)	(0.0003)	(0.0009)	(0.0010)
t=1	-0.0033**	-0.0022	-0.0031	-0.0003	0.0001	-0.0001	0.0001	0.0007	0.0007	0.0003**	-0.0006	-0.0004
	(0.0010)	(0.0021)	(0.0023)	(0.0006)	(0.0012)	(0.0013)	(0.0002)	(0.0012)	(0.0012)	(0.0002)	(0.0011)	(0.0011)
Cons.	0.0045***	0.0109***	0.0117***	0.0014***	0.0070***	0.0076***	0.0005***	0.0039***	0.0042***	0.0001**	0.0025***	0.0026***
	(0.0006)	(0.0008)	(0.0010)	(0.0003)	(0.0005)	(0.0005)	(0.0000)	(0.0005)	(0.0005)	(0.0000)	(0.0005)	(0.0005)
Mean y	0.003	0.011	0.012	0.001	0.008	0.008	0.000	0.006	0.006	0.000	0.004	0.004
Obs.	56,308	56,308	56,308	56,308	56,308	56,308	56,308	56,308	56,308	56,308	56,308	56,308
Dyads	28,154	28,154	28,154	28,154	28,154	28,154	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 above a certain level: any non-zero transfer (columns 1-3), above 1,200 rupees (columns 4-6), above 2,400 rupees (columns 7-9), above 5,000 rupees (columns 10-12). The total transfers variable is the maximum of the loans and gifts variables. 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, 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 Table A6: Directed dyadic panel, binary regressions for different thresholds

	At least one transfer			At least Rs. 1,200			At least Rs. 2,400			At least Rs. 5,000		
	Gifts	Loans	Transfers	Gifts	Loans	Transfers	Gifts	Loans	Transfers	Gifts	Loans	Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
TT^d	-0.0005	0.0013	0.0024	-0.0005	0.0007	0.0004	-0.0005	0.0031***	0.0029***	-0.0001	0.0031***	0.0030***
	(0.0019)	(0.0015)	(0.0017)	(0.0004)	(0.0011)	(0.0010)	(0.0003)	(0.0010)	(0.0009)	(0.0001)	(0.0010)	(0.0009)
TC^d	0.0003	0.0016	0.0025	-0.0002	0.0015	0.0013	-0.0001	0.0023**	0.0023**	-0.0002	0.0025***	0.0024**
	(0.0011)	(0.0014)	(0.0015)	(0.0002)	(0.0012)	(0.0011)	(0.0002)	(0.0009)	(0.0009)	(0.0002)	(0.0007)	(0.0008)
CT^d	0.0003	0.0016	0.0022	-0.0005**	0.0013	0.0009	-0.0001	0.0011	0.0010	-0.0001	0.0013*	0.0013*
	(0.0010)	(0.0014)	(0.0016)	(0.0002)	(0.0009)	(0.0009)	(0.0002)	(0.0009)	(0.0009)	(0.0001)	(0.0006)	(0.0005)
t=1	-0.0026**	-0.0026	-0.0038*	-0.0004	-0.0001	-0.0003	0.0000	0.0002	0.0002	0.0001	-0.0001	-0.0001
	(0.0008)	(0.0017)	(0.0019)	(0.0005)	(0.0009)	(0.0010)	(0.0001)	(0.0009)	(0.0009)	(0.0001)	(0.0008)	(0.0009)
Cons.	0.0035***	0.0079***	0.0090***	0.0011***	0.0045***	0.0051***	0.0004***	0.0026***	0.0027***	0.0000**	0.0015***	0.0015***
	(0.0005)	(0.0007)	(0.0008)	(0.0003)	(0.0004)	(0.0004)	(0.0001)	(0.0004)	(0.0004)	(0.0000)	(0.0004)	(0.0004)
Mean y	0.002	0.007	0.008	0.001	0.005	0.005	0.000	0.004	0.004	0.000	0.002	0.002
Obs.	112,616	112,616	112,616	112,616	112,616	112,616	112,616	112,616	112,616	112,616	112,616	112,616
Dyads	56,308	56,308	56,308	56,308	56,308	56,308	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 binary dependent variable equals one if there was a transfer from i to j above a certain level: any non-zero transfer (columns 1-3), above 1,200 rupees (columns 4-6), above 2,400 rupees (columns 7-9), above 5,000 rupees (columns 10-12). The total transfers variable is the maximum of the loans and gifts variables. All directed within-village dyads are taken as observations. Dyad-level fixed effects are included. OLS coefficients are reported. Robust standard errors are in parentheses, 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%.