Online Appendix to Development Projects and Economic Networks: Lessons From Rural Gambia

Simon Heß
Goethe University
Frankfurt*

Dany Jaimovich
Universidad de Talca†

Matthias Schündeln
Goethe University
Frankfurt†

This version: June 5, 2020

*Faculty of Economics and Business Administration, Goethe University Frankfurt. RuW Postbox 46, Theodor-W.-Adorno-Platz 4, 60323 Frankfurt am Main, Germany.

†Facultad de Economía y Negocios, Universidad de Talca, Avenida Lircay s/n, Talca, Chile

A The Gambian CDD

A.1 Institutional Background

This section provides background on the Gambian CDD program to supplement the information provided about the program in the main text.

The Gambian CDD program followed the design of a typical CDD program, which promotes community involvement at all stages of the process from identification of the potential sub-projects to their maintenance after implementation.[1] In order to select the village-level sub-projects, each village had to follow a long decision-making process involving several local and external actors (GoTG, 2006). According to the guidelines, as a first step, a CDD facilitator organized a series of village meetings and consultations with community-based organizations, with the goal of identifying and preparing a Strategic Development Plan (SDP) that summarized a three-year projection of the village's perspectives. The SDP had to be confirmed by the whole village in a special meeting supported by the CDD facilitator. Subsequently, the priorities contained in the SDP were translated into a list of sub-projects, whose feasibility was analyzed by the CDD regional team. As a last step of the decision-making process, feasible sub-projects were presented to all the villagers for the selection of sub-projects to be financed by the CDD program.[2] After project selection, community members were also involved in the implementation and maintenance of the investments. Villages were expected to contribute at least 10% of the project cost in cash and/or in kind, e.g., through labor or complementary infrastructure. The scope of the program-induced village-level activities is best illustrated by the fact that the implementation manuals used by the local facilitators mandated 38 village meetings in the course of project implementation. Twenty of these meetings were intended to involve the whole village, while the other 18 involved meetings of community-based organizations (CBOs), the VDC, or other subgroups of the village (GoTG, 2006).

In contrast to other CDD program designs that put a stronger emphasis on social outcomes (White et al., 2018), the Gambian CDD program focussed more on economic outcomes. According to official program documents, growth and poverty reduction was the first of three focus pillars of the Gambian CDD program. The others were coverage of basic social services needs and capacity building (World Bank, 2006). This is mirrored by the villages' sub-project proposals. Using administrative data, we find that 59% of all CDD villages indicate income generation and growth as a goal for their sub-project.

A.2 Magnitude of the Disbursement

The country-wide average funding per household is approximately US$140. In the sub-sample for this study, because villages have fewer households, it is US$230 (in 2009). Per-household funding in Casey et al. (2012), who study a CDD program that is in many ways comparable to the program in The Gambia, is US$100. A summary provided by Wong (2012) shows that per-household spending in the Gambian CDD is also large relative to CDD programs in other parts of the world (Wong 2012, Table 4). For comparison, for microfinance programs discussed in Banerjee et al. (2015b), the loan size as a proportion of income is between 6 and 43% of annual household income in 5 of the 6 countries covered. To translate this into a subsidy,

[1] A sub-component of the program allocated resources to hamlets smaller than 100 inhabitants, as long as they form clusters of at least 100 inhabitants. Another sub-component also financed ward-level projects. These components followed a slightly different process, which we do not discuss this paper.

[2] Even though the official CDD program documents clearly emphasize the importance of obtaining approval from the community for the SDP and the selected sub-projects, the specific project selection mechanism is not specified. In our surveys 79% of the respondents in treatment villages declared having attended a meeting in which the CDD sub-project was chosen. Among those present at that meeting, 21% mentioned that there had been a vote to decide on the sub-project and 87% agreed that the decision had been taken by the whole community.

consider, for example, Mexico, where the difference in interest rates charged by the program and market interest rates is 35% APR, which translates into subsidy levels of less than 3% of annual household income. As an alternative way to illustrate the magnitude, note that the funds in the Gambian CDD would suffice to provide each household with about eight goats (roughly the same magnitude that is reported, for example, in Banerjee et al. 2015a).

B Model Example

For illustrative purposes, consider an example with three players $N = \{a, b, c\}$ and assume first that $p_a = p_b = p_c = p$. Assume, costs and benefits are in a range such that (1) some non-empty networks can be sustained in equilibrium but (2) at least some networks cannot be sustained. This is the case if two conditions are met for all households:

$$\frac{\delta}{1-\delta}(mp\nu - mpc) > c > \frac{\delta}{1-\delta}\left(\frac{mp}{2}\nu - mpc\right)$$

or expressed in terms of p:

$$\frac{1-\delta}{\delta m}\left(\frac{\nu}{c} - 1\right)^{-1} < p < \frac{1-\delta}{\delta m}\left(\frac{\nu}{c}\frac{1}{2} - 1\right)^{-1}.$$

The first inequality implies that for each individual i, the costs of making a gift today are *smaller* than the benefit of staying connected to a network where all households linked to i have the same number of links as i has. The second inequality implies that the costs of making a gift today are *larger* than the benefit of staying connected to a network where households linked to i have twice the number of links as i has. These are useful parameter restrictions to study comparative statics. If the first inequality is violated, then no non-empty network is an equilibrium. If the second one is violated, any network can be sustained as an equilibrium.

Under these assumptions two types of non-empty equilibrium networks exist: one where all households are connected and thus $d_i = 2$, and one where two households are connected and a third household is isolated. These are shown in Figure 1. A network where a is connected to both b and c, but b and c are not connected, is not an equilibrium. This is because b and c would prefer autarky, as they expect to receive future gifts only with probability $\frac{mp}{2}$, but have to incur the costs of making gifts with probability mp. By assumption this has a lower expected net utility than saving the costs of making a gift today.

Figure 1: Self-sustaining networks with homogeneous agents

Assume now that the CDD program gets implemented and household a becomes the VDC head, thus has a higher chance of generating gifts, i.e., $p_a = p^* > p_b = p_c = p$. If the change in p^* is small, nothing will change. Non-VDC households who have a link with a VDC household, benefit from more frequent gifts, and the VDC household still prefers staying in the network. However, if p^* is large enough so that

$$c > \frac{\delta}{1-\delta}(mp\nu - mp^*c) \qquad \Leftrightarrow \qquad p^* > p \cdot \frac{\nu}{c} - \frac{1-\delta}{\delta m},$$

the full network ceases to be an equilibrium, because the VDC is not willing to share her (frequent) gifts anymore in exchange for the (rare) gifts received from the other two households. By the same argument, the networks where only one link between the VDC member and one other household exists are not equilibria anymore.

For some ranges of p^*, a new equilibrium emerges, where the VDC is connected to both non-VDC households, but they are not connected among themselves. This is possible if two conditions are met. First, the VDC household would still want to stay connected to the other households, i.e., if

$$c < \frac{\delta}{1-\delta}\left(2mp\nu - mp^* c\right) \qquad \Leftrightarrow \qquad p^* < p \cdot 2\frac{\nu}{c} - \frac{1-\delta}{\delta m}$$

and the other households still want to stay connected to the VDC, i.e., if

$$c < \frac{\delta}{1-\delta}\left(\frac{1}{2}mp^*\nu - mpc\right) \qquad \Leftrightarrow \qquad p^* > p \cdot 2\frac{c}{\nu} + 2\frac{c}{\nu}\left(\frac{1-\delta}{\delta m}\right).$$

We thus establish that within the range of parameters where different networks are possible, a sufficiently large increase in p_a removes the three first networks in Figure 1 from the set of equilibria and, for some range of p_a, adds another network to that set. Then the two equilibrium networks are as shown in Figure 2.

Figure 2: Self-sustaining networks with sufficiently heterogeneous agents

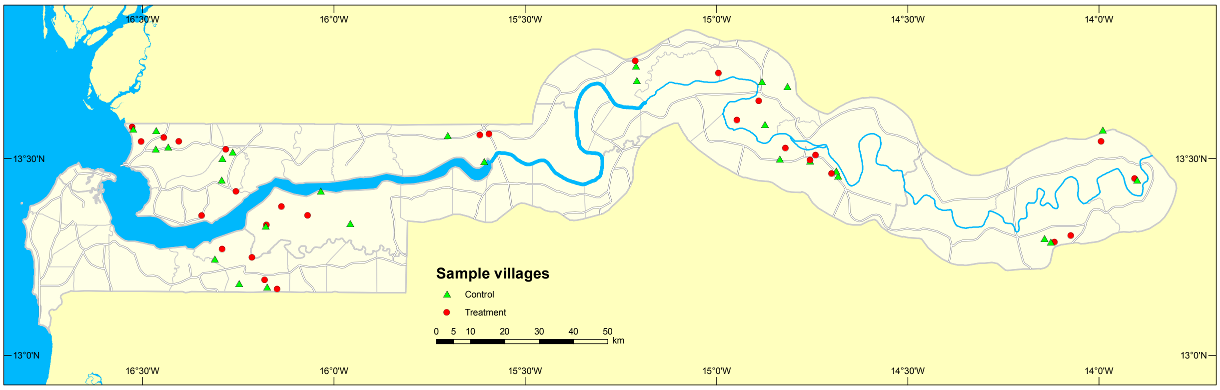
In sum, an increase in heterogeneity in p_i can result in changes in networks. Household a may become more central at the cost links between other households.

C Sampling and Data Collection

C.1 Village Sample

The sample was originally drawn in a first round of network data collection that took place in 2009, during the implementation of the Gambian CDD program (these data are described in detail by Arcand et al., 2010 and Jaimovich, 2015). In this original sample, 62 villages were selected in a two-stage procedure. First, a set of wards was randomly chosen from all wards with eligible villages. Second, within wards an equal number of control and treatment villages was randomly selected from the set of all villages of the desired population size. For the 2014 data collection for this study we excluded one urban ward (four communities) from the sample, in order to focus exclusively on rural villages. Aside from this, two villages had to be excluded from the data collection. In one village that was originally assigned to treatment, *Kerr Mod Ali*, all but a handful of households had followed a local religious leader, relocated right across the border to Senegal, and could not be interviewed. According to disbursement data this village did not receive funds. One control village was excluded because of incomplete data in the 2009 data collection. Our main results remain equally strong when we exclude either one or both wards where these two villages are located (see Appendix Table A3, columns 2-4).

Figure 3: Sample Villages in The Gambia



C.2 Household Structure and Sampling of Households

Our network data uses households as the basic unit of analysis, but household definitions are not always straightforward. This is particularly the case in the West African context (Beaman and Dillon, 2012). In The Gambia, villages are typically organized into *compounds*, a group of huts usually surrounded by a grass fence where members of the same family live. Most compounds are a single household (80% of our sample), but in some cases separate households share one compound. Locally, households are called *sinkiros*, and the intra-compound distinction was, in most cases, clear to the village chief and all other village inhabitants. Considerable care was taken to clarify ambiguous cases for respondents. We always targeted the household head as a respondent. In some cases (where a household head was too old, sick, or absent) we chose the most knowledgeable person available in the household as a proxy respondent.

C.3 Elicitation of Networks in the Main Survey

The full village censuses were carried out through household head gatherings co-organized with the village chief (for details see Jaimovich, 2015). Networks were elicited using a name generator procedure (Campbell and Lee, 1991). Respondents were asked to name villagers with whom they had exchanges in each of the six economic domains: *Land*, *Labor*, *Inputs*, *Food*, (non-food) *Gifts*, and *Credit*. In a comparable manner, the interviews also collected information about social networks created by *Kinship* (first-degree relatives and children's in-laws); and *Friendship* (which we measured with information about gatherings to drink green tea, *Attaya*).

For each response, enumerators looked up the names in a list of households and recorded the corresponding ID. If the other household in a transaction was a village outsider, and thus not on our household list, the transactions was recorded with a generic outsider-ID. For each transaction listed by a respondent, in addition to the direction of the transaction, we recorded quantity and further specifics of the exchange, such as whether there was payment involved. Our enumerators were trained to ensure that any given transaction was recorded in the most suitable category and to assist the respondents in estimating quantities (e.g., hectares of land) using examples.

Ambiguities regarding the domain of a specific transaction were rare. For example, for *food* and *gifts*, enumerators were instructed to record all non-edible items as *gifts* and the rest as *food*. One goal of covering multiple domains in the network survey was to be able to elicit links with specific, relatable survey questions, while still covering a wide range of economic transactions. Our main results use the union of all six economic domains, which limits the scope to which ambiguity in the network domains can affect our results.

One technical concern could be that the directionality of good flows, on which the computation of our measure for reciprocity is based, is imprecisely measured via the survey. Bidirectional good flows in the data would then not capture reciprocation. Instead these flows would capture the rate at which two households report a flow but one household reports the incorrect flow direction. There is reason to believe this is not the case. For 80% of the dyads that we code as reciprocal, reciprocity is already implied in the reports made by a single households in this dyad, i.e., a single respondent's report that transactions took place in both direction. Reciprocity, as we measure it, is thus unlikely to be related to miss-measured flow direction.

D VDC Membership Determinants and Random Forest Classifications

The Village Development Committee (VDC) is a group that is central for the CDD program's implementation and for our analysis. While the concepts of VDCs existed before the implementation of the CDD program (Local Government Act, 2002), they were often not fully established. The roll-out of the CDD program was used to enforce the inclusiveness criteria and ensure the establishment of VDCs in all communities (GoTG, 2006). As a result, the members of the VDC in treatment and control communities are not comparable. For the purpose of a treatment effect heterogeneity analysis, VDC membership is thus endogenous. As Table 1, columns 1 and 2 show, VDC members in treatment villages are on average significantly older, more educated, more likely to belong to an ethnic minority and owning less land than their counterparts in control villages. In order to be able to study treatment effect heterogeneity, we compute a measure for the household's likelihood of being in the VDC if a village becomes a CDD village. This likelihood is estimated using several variables that are not affected by the program and is thus not endogenous. We treat the predicted likelihood as a household-level characteristic and study heterogeneity of treatment effects along this dimension. Our approach is inspired by Banerjee et al. (2019), but relies on household characteristics to predict VDC membership instead of take-up of microfinance.

Table 1: VDC Composition Differs Between Treatment and Control Communities

	(1) VDC member	(2) VDC member	(3) VDC member
treatment	-0.177		
	(0.008)$^{\bullet\bullet\bullet}$		
age	-0.001	-0.001	
	(0.063)*	(0.039)**	
education	0.011	0.014	
	(0.545)	(0.503)	
ethn. group <30%	-0.086	-0.071	
	(0.002)***	(0.007)***	
$\mathbb{1}$(land$\leq$ 2ha)	-0.095	-0.107	
	(0.000)***	(0.000)***	
female headed hh	-0.036	-0.038	
	(0.225)	(0.188)	
treatment $\times$ age	0.002	0.002	
	(0.048)$^{\bullet\bullet}$	(0.034)$^{\bullet\bullet}$	
treatment $\times$ education	0.064	0.056	
	(0.022)$^{\bullet\bullet}$	(0.054)$^{\bullet}$	
treatment $\times$ ethn. group <30%	0.098	0.089	
	(0.020)$^{\bullet\bullet}$	(0.021)$^{\bullet\bullet}$	
treatment $\times$ $\mathbb{1}$(land$\leq$ 2ha)	0.057	0.082	
	(0.054)$^{\bullet}$	(0.008)$^{\bullet\bullet\bullet}$	
treatment $\times$ female headed hh	0.027	0.039	
	(0.503)	(0.319)	
$\Pr_{\mathrm{RF}}$[VDC]			0.490
			(0.004)***
controls	✓	✓	✓
village fixed effects		✓	
households	2774	2774	1416
control mean dep. var.	0.2	0.2	

Notes: $^{\bullet}$/* $p < 0.1$, $^{\bullet\bullet}$/** $p < 0.05$, $^{\bullet\bullet\bullet}$/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Where bullets are shown, randomization inference was used to compute p-values: filled bullets ● indicate significance levels with randomization inference; starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The dependent variable is an indicator for VDC membership (being a VDC executive or the head of the committee). For observations where information on land holdings, age or gender of the household head were missing, we imputed the village-level mean (applies to less than 1% of observations). The units of observation are households. Control variables are the village-level variables listed in Appendix Table A1, Panel B, and enumerator fixed effects, ethnicity fixed effects and, in column 1, ward-fixed effects. In column 3 the sample is restricted to only treatment villages, to assess the accuracy of the random forest result for a sub-sample where the outcome is observable.

To obtain a measure for the likelihood of being a member of the VDC, we use a random forest (Breiman, 2001). Random forests are a family of algorithms that are useful for classification and regression problems. Contrary to parametric methods, random forests, like many other supervised machine learning techniques, do not make functional form assumptions and were developed for (out-of-sample) prediction rather than model-based inference (Mullainathan and Spiess, 2017). Consequently, they can pick up highly non-monotonous and complex patterns in the data, thereby offering the potential to thereby increase the prediction accuracy at the expense of providing less easy to interpret insights about the subject matter. Random forests fit a large number of decision trees to the data, whereby each tree uses a bootstrap sample of observations, and each split uses only a random subset of the available variables. To obtain predictions, observations are run through the full set of trees, and the fraction falling into each class is treated as the predicted probability for that outcome.

In order to train our model, we use a rich set of $p = 23$ variables. In Main Text Table 4 we apply the same procedure to the census data, where we are restricted to using variables that are recorded in both the census and our data. For this we train a reduced random forest with only $p = 10$ variables. Those variables are marked with an asterisk (*) below. The variables we use broadly fall into three categories.

- Survey-based household characteristics: being the village chief's household, being the household of a first degree relative of the chief, being the imam's household, age*, sex*, formal education*, Koranic education, household size*, owned land, number of wives*, being the compound head, ethnicity* and a how long a household has been living in that village*.
- Network data-based household characteristics: inverse distance to the chief in the family network and in the joint family and neighborhood network, average (inverse) distance to all households in the family network, the neighborhood network, and in the joint family and neighborhood network, the number of other households in the village a household is connected to through kinship, and the number of households outside the village household is connected to through kinship.
- Other household characteristics: the ward a village/household belongs to*, the share a household's ethnicity makes up in a village*, and whether or not a household belongs to the village's ethnic majority*.

We train the model by growing 1000 trees and follow standard procedures for classification trees (Friedman et al., 2001), by sampling cases with replacement, randomly drawing $\lfloor\sqrt{p}\rfloor = 4$ variables as candidates at each split and not restricting the size of terminal nodes. Because the feature we are trying to predict (post-CDD VDC membership) is only observable in treatment communities, we exclude control communities from the training sample.

Special care is taken to ensure comparability across treatment and control. Using the random forest we trained on the full treatment data to predict probabilities for treatment and control households would imply that for treatment villages we use within-sample predictions, while using out-of-sample predictions for the control group. This would likely imply substantially different prediction errors for treatment and control, which could lead to attenuation bias that is correlated with treatment assignment. This could likely result in spurious estimates for treatment effect heterogeneity. To avoid this, we predict the probability of VDC membership in control villages using a leave-one-out procedure. That is, to predict VDC membership probabilities for households in village v, we train a new random forest using the data from all treatment villages except village v. This process is repeated for all treatment villages. Probabilities in control villages are predicted using a random forest trained with the full set of all treatment villages. This procedure ensures comparability of the predicted probabilities across treatment and control.

The predicted probabilities resulting from this process have a mean of 0.188 and a standard deviation of 0.096. Column 3 of Table 1 shows that households who are classified by the out-of-sample prediction from the random forest to have a high probability of being in the VDC actually are significantly more likely to report being in the VDC.

E Details for Randomization Inference

Randomization inference uses the logic that counterfactual outcomes and observed outcomes are identical under the *exact null hypothesis* of no treatment effect:

$$y_i(D_i = 0) = y_i(D_i = 1), i = 1, \dots, n,$$

to derive the distribution of a test statistic under that null hypothesis. The observed sample realization of the test statistic is compared against the derived distribution to assess whether the results significantly differ from what would be observed under the null. We implement randomization inference as follows (for details see Heß, 2017):

1. For each regression we compute the t-statistic corresponding to each treatment coefficient of interest, t_{sample}. In regressions where treatment is interacted with other variables, we apply the same procedure to all treatment coefficients.

2. Second, we draw $R = 10,000$ hypothetical realizations of the treatment assignment, exactly following the original process (village-level treatment, stratified by ward), and compute the same t-statistics, $\{t_r\}_{r=1,\ldots,R}$, based on these realizations. The obtained set of hypothetical realizations of the t-statistics are independent draws from the distribution of t-statistics under the sharp null hypothesis of no treatment effect.

3. Lastly, we assess the significance of the true sample realization of the t-statistics by computing the share of alternative realizations of $\{t_r\}_{r\in\{1,\ldots,R\}}$ that lie further away from zero than the actual estimate, t_{sample}, to obtain a p-value for the null hypothesis of no treatment effect:
$$p^{\text{RI}} = \frac{1}{R}\sum_{r=1}^{R} \mathbb{1}(|t_{\text{sample}}| < |t_r|).$$

F Robustness of the Dyadic Average Treatment Effect Estimate

F.1 Disaggregation by Transaction Type

Table 2: Dyadic Regressions, Average Treatment Effect in Individual Networks

	(1) land	(2) labor	(3) inputs	(4) food	(5) gifts	(6) credit
treatment	-0.106	-0.179	0.037	-0.791	-0.387	-0.237
	(0.429)	(0.163)	(0.871)	(0.000)●●●	(0.001)●●●	(0.066)⊛
controls	✓	✓	✓	✓	✓	✓
dyads	151632	151632	151632	151632	151632	151632
households	2774	2774	2774	2774	2774	2774
control mean dep. var.	1.2	1.5	1.9	2.0	0.9	1.2

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. Regressions control for ward fixed effects and a set of control variables: the village-level variables in Appendix Table A1, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table A1, as well as ethnicity and enumerator dummies enter the regressions twice, for the sending and the receiving household of a dyad. Indicators for traditional leaders and marginal households are also included but not shown in this table.

F.2 Analysis at Village and Household Levels

Table 3 studies several measures capturing different aspects of network topology at the village level, namely: network density, average inverse path length, the network clustering coefficient, and the total number of closed triads. The results are consistent with the findings at the other levels of aggregation. Column 1 and 2 suggest that network density as well as the average inverse path length between households is reduced by treatment, indicating a reduction in the overall economic connectedness of households. Further, column 3 suggests that the network-level clustering coefficient decreased in treatment villages, though the corresponding treatment effect estimate is insignificant at conventional levels (p-value=0.104). Clustering measures the propensity of nodes in a network to close triangles of households that are already connected. Correspondingly, the total number of observed triangles is also reduced (column 4). A reduction in triangular transactions is consistent with a shift away from socially embedded transactions towards market-based exchanges. As discussed in Main Text Section 7 and Gagnon and Goyal (2017), socially embedded transactions can be characterized by positive local externalities, i.e., they increase the value of transactions for neighboring households, thus causing the emergence of clustering in the network of transactions. One-shot market transactions that do not require trust or social collateral do not have this effect.

Table 3: Village-Level Network Regressions

	(1) density in %	(2) avg. inv. path length	(3) clustering in %	(4) closed triads	(5) economic (union)
treatment	-1.026	-5.839	-2.518	-10.567	-3.795
	(0.048)●●	(0.054)●	(0.104)	(0.003)●●●	(0.042)●●
controls	✓	✓	✓	✓	✓
village networks	56	56	56	56	56
control mean dep. var.	6.9	66.4	21.5	19.1	45.1

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The units of observation are villages. Regressions control for ward fixed effects and a set of control variables: village-level variables in Appendix Table A1, Panel B, the shares of households belonging to each ethnicity and the shares of households interviewed by each enumerator.

Table 4 shows the result of the estimation of the household-level variant of Equation (4.4), using household degree centrality as the dependent variable. The coefficient of -0.827 in column 1 of Panel A indicates that households in treatment villages receive 20% fewer transactions from exchange partners than households in control villages.

Table 4: Degree in the Village-Internal Network

	(1) economic (union)	(2) land	(3) labor	(4) input	(5) food	(6) gifts	(7) credit
Panel A: in-degree							
treatment	−0.827	−0.067	−0.053	−0.082	−0.348	−0.146	−0.130
	(0.000)●●●	(0.191)	(0.357)	(0.325)	(0.000)●●●	(0.001)●●⊛	(0.025)●⊛
controls	✓	✓	✓	✓	✓	✓	✓
control mean dep. var.	4.1	0.6	0.7	0.9	0.9	0.4	0.6
households	2774	2774	2774	2774	2774	2774	2774
Panel B: out-degree							
treatment	−0.742	−0.093	−0.063	−0.097	−0.302	−0.132	−0.055
	(0.002)●●⊛	(0.077)⊛	(0.244)	(0.254)	(0.000)●●●	(0.005)●●⊛	(0.380)
controls	✓	✓	✓	✓	✓	✓	✓
control mean dep. var.	4.1	0.6	0.7	0.9	0.9	0.4	0.5
households	2774	2774	2774	2774	2774	2774	2774

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The units of observation are households. Total in- and out-degree measures vary within villages because some households report links but conceal the identity of their partner. This is most common for money lending and alms (*Zakat* in Islam), which—for religious reasons—tend to be reported by the recipient only. Regressions control for ward fixed effects and a set of control variables: household- and village-level variables in Panels B and C of Appendix Table A1 as well as ethnicity and enumerator dummies.

F.3 Difference-in-Differences and Controlling for the Lagged Dependent Variable

Similar data to what we use for this study were collected in 2009. Because of the timing of the data collection, which occurred at a time when in some villages project implementation had already begun, and the villages had held first meetings and may have already received funds, these cannot be considered clean baseline data. In fact one of the goals of that data collection was to enable an analysis of short term effects on social dynamics.[3] There are also differences in how the data was collected, and matching households is problematic because of many changes in household composition (mergers and splits) in treatment and control villages.

Table 5 shows summary statistics based on these data. There is no evidence for a strong negative short-term treatment effect. At the same time, with this data it is also not possible to reject sizable negative treatment effects.

Table 5: Balance Tests for Kinship and Economic Network Degrees Measured in 2009

	Mean		Observations		Difference		*p*-value	
	(1) control	(2) treated	(3) control	(4) treated	(5) raw	(6) cond.	(7) CRSE	(8) RI
economic (union)	2.42	2.12	1307	1318	−0.299	−0.219	0.45	0.50
-land	0.54	0.52	1307	1318	−0.025	−0.019	0.76	0.78
-labor	0.68	0.64	1307	1318	−0.039	−0.008	0.94	0.94
-inputs	1.11	0.83	1307	1318	−0.280	−0.233	0.20	0.25
-credit	0.50	0.40	1307	1318	−0.093	−0.068	0.46	0.51
kinship	3.73	3.95	1307	1318	0.225	0.317	0.26	0.33

Notes: Columns 1 and 2 display the means of each variable in the respective treatment group. The respective sample sizes are shown in columns 3 and 4. Column 5 shows the raw difference in means, while column 6 shows the difference after controlling for ward fixed effects. Columns 7 and 8 show *p*-values of a test for no difference in means, controlling for ward fixed effects. The version of the test based on cluster-robust standard errors (CRSE) in column 7 is slightly more conservative on average than the test based on randomization inference (RI) in column 8. Number are based on the network data collected by Arcand et al. (2010) in 2009. Numbers represent the undirected, unweighted network degree of the households, i.e., the number of distinct transaction/link partners from within the village, irrespective of the number and direction of transactions with these households.

Nonetheless, our results are robust to using these data to estimate a Difference-in-Differences (DiD) regression using both waves as well as to controlling for the lagged dependent variable. The results are reported in Table 6.

The DiD estimate in Main Text Table 6a for economic transactions is negative and statistically significant, which is in line with our main average treatment effect estimates based only on our 2014 data. As an additional placebo test that would capture if changes between 2009 and 2014, that relate to village composition (migration, household splits) or reporting, were driving our results, we repeat the same DiD estimation using the kinship network and find no treatment effect there. In addition, there is no significant difference between treatment and control villages in 2009 (neither in the economic networks nor in the kinship network).

Thus, to explain our main findings with significant imbalances prior to the CDD project, it would need to be the case that there were significant short-term effects manifesting prior to the 2009 data collection that have roughly the same magnitude as the imbalances but have the opposite sign (in that case, the DiD estimate is the difference between long-term and short-term effects). Considering jointly the lack of significant differences in networks in the 2009 data (Table 5 and Main Text Table 6a) and the balance of social network proxies in 2003 (Appendix Table A1, Panel D), this seems highly unlikely.

The estimates in Main Text Table 6b, where we use the 2009 data to control for the lagged dependent variable, paint a similar picture. While there is clearly autocorrelation in the transactions, as indicated by the positive coefficient on the lagged dependent variable, the estimate on the treatment effect is almost unchanged. Some dyads in our sample contain households that have changed in some way between the two waves (roughly a fifth of all households experienced household head successions, migration of individual household members, [partial] mergers, or splits). Thus, we also control for an indicator variable for whether data for 2009 is missing due to such changes.

[3]"The villages were surveyed in two rounds. The second round focused on villages in which project implementation started in late 2008 [...] to get a sense of the preliminary project impacts on social dynamics." (Arcand et al., 2010)

Table 6: Estimation Usings the Data From 2009

(a) Difference-in-Differences

	economic (directed)		economic (undirected)		kinship/placebo (undirected)	
	(1) any transaction	(2) any transaction	(3) any transaction	(4) any transaction	(5) kinship	(6) kinship
treatment	0.042		0.260		−0.784	
	(0.933)		(0.733)		(0.399)	
post	2.518	2.314	4.630	4.302	−7.022	−7.042
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
treatment × post	−1.064	−1.166	−1.922	−2.064	0.004	0.042
	(0.081)●	(0.054)●	(0.034)●●	(0.024)●●	(0.997)	(0.972)
controls	✓	✓	✓	✓	✓	✓
village fixed effects		✓		✓		✓
observations (dyads)	292202	292202	146101	146101	146101	146101
observations (households)	3175	3175	3175	3175	3175	3175
villages	56	56	56	56	56	56
control mean dep. var.	6.332	6.332	10.572	10.572	8.852	8.852

(b) Lagged Transactions

	economic (directed)
	(1) any transaction
treatment	-1.004
	(0.010)●●⊛
any transaction in 2009	0.124
	(0.000)***
missing information 2009	0.749
	(0.047)**
controls	✓
village fixed effects	
observations (dyads)	151632
observations (households)	2774
villages	56
control mean dep. var.	6.927

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The first wave was collected in 2009, at the onset of the CDD program, and we can trace changes in household composition over the 5 year period between the two waves. Some network questions differ between the waves, in particular the 2009 data only include the networks of credit, inputs, labor, and land. To obtain comparable measures for 2009 and 2014 we form the union of all networks, combining them into a single 'economic' network, in the same way as described in Main Text Section 4.3. Units of observation are dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. In Main Text Table 6a, the 'post'-variable is a dummy indicating whether an observation stems from the 2014 wave. The treatment effect is thus captured by the interaction term of 'treatment' and 'post'. In Main Text Table 6b, the specification is identical to the one presented in Main Text Table 1, except for the additional controls shown in the table. In particular, we control for whether a transfer was also observed in 2009 and whether one or both households could not be matched to households that existed in 2009.

F.4 Robustness of the Main Findings: Compensation?

The data were collected 4-5 years after the end of the program, which increases the chances that some kind of "compensation" has taken place in which control villages received more development programs than treatment villages. This would complicate the interpretation of the results.

In this appendix we discuss that possibility and present evidence that compensation did not occur at a meaningful magnitude and is unlikely to be driving our results. We base this statement on three sets of findings: (i) Comparing numbers of projects and (rough estimates) of total project budgets, we do not find evidence for statistically significant or economically meaningful additional non-CDD activity in control villages; (ii) Our main results are robust to controlling for non-CDD activity; (iii) A planned scaling-up of the CDD program reduced incentives to compensate villages. Further, according to our data, non-CDD projects are implemented from a variety of actors, which speaks against a coordinated effort to compensate non-CDD villages.

(i) To address concerns that the government or other actors (e.g., NGOs) may have implemented more projects in control villages than in CDD villages, we first use data on other (non-CDD) projects that we collected in our surveys. In particular, during our in-depth interviews and village focus group discussion, which were held with village authorities to obtain a better understanding of recent developments in the villages, we asked respondents to list development projects from the past 10 years that they are aware of. A caveat here is that the survey instruments were not designed with a focus on ensuring comparability of all project reports between treatment and control villages, first, because CDD sub-projects had to sometimes be prompted in CDD villages and, second, because non-CDD and CDD projects were elicited jointly, which automatically implies a different interview flow in these segments. We tackle this concern by combining data across survey instruments. The focus group data allow us to assign a year of implementation to projects (year of implementation was not asked in the in-depth survey), which is needed to analyze the flow of projects over time. Merging those data with the in-depth survey and restricting the analysis to (CDD and non-CDD) projects that were listed by at least three households in the in-depth survey *before* the prompting ensures comparability of responses in treatment and control villages (a clear distinction between prompted and unprompted reports cannot be made in the focus group data).

In a further effort to investigate the role of non-CDD development programs, we also estimate the budgets of development programs. Since we do not have official data on non-CDD projects, we assign values based on project categories in the non-CDD development project descriptions and similar sub-projects that were implemented as part of the CDD program (e.g., if establishing a garden is mentioned as a non-CDD project, we use the median costs for gardens recorded in the CDD database).[4] While this is not a precise measure of the actual funds distributed through a development project, it is a rough proxy for the market value of the items provided by non-CDD development projects.

Using these data, we test whether treatment and control villages differ in the number of projects or funds they received in the relevant time frames.

Using the full data set on CDD and non-CDD projects reported by the focus group, we first confirm that there are indeed significantly more projects during the CDD-implementation period (2008-2010): in treatment villages there were on average 1.89 [sub-]projects, totaling US$12,130, compared to 0.46 projects, totaling US$3,070, in control villages. Both, project numbers and, even more, project budgets are likely measured with significant error. Thus, as discussed before, we repeat the analysis restricted to projects that were also mentioned (without prompting) by at least three independent respondents in the in-depth survey. This approach increases the comparability of the numbers by relying on the in-depth survey, which we consider less prone to reporting bias, at the expense of omitting projects that households are unaware of. These data reveal similar differences for 2008-2010: in treatment villages there were on average 1.43 [sub-]projects, totaling US$9,498, compared to 0.29 projects, totaling US$2,168, in control villages.

Considering all years from the CDD implementation onwards (2008-2014), we continue to find that CDD villages received more projects and were allocated higher budgets: in treatment villages there were on average 3.46 [sub-]projects, totaling US$19,466, compared to 2.36 projects, totaling US$10,856, in control villages. Thus, there is no evidence for a full compensation. Restricting the data to projects that are listed by three in-depth survey respondents reveals similar differences for 2008-2014: in treatment villages there were on average 2.29 [sub-]projects, totaling US$13,419, compared to 1.29 projects, totaling US$6,808, in control villages.

Finally, when we consider the post CDD-period separately, we also do not find strong evidence for compensation. Considering the years after the CDD program's implementation (2011-2014), we find that focus groups report overall slightly more projects in control villages (on average 1.89 projects totalling US$7,787) than in treatment villages (on average 1.57 projects totaling US$7,337). If we again consider only projects that were named by three or more households in the in-depth survey, this difference is even smaller: during 2011-2014 households report on average 1.0 projects (US$4,639) in control villages and 0.86 (US$3,921) in treatment villages.

[4]Since we have the full CDD database for all approximately 450 villages, we have a fairly large number of projects to estimate budgets.

Table 7: Number of Projects and Estimated Budget by Year

	projects listed by village focus group		projects listed by at least 3 respondents	
	(1)	(2)	(3)	(4)
	projects	project budget	projects	project budget
treatment	-0.0232	101.9	0.0366	290.7
	(0.637)	(0.709)	(0.329)	(0.200)
CDD years (2008-2010)	0.0696	734.8*	0.0829*	679.9**
	(0.307)	(0.069)	(0.059)	(0.028)
treatment × CDD years (2008-2010)	0.502***	2913.2***	0.346***	2151.1***
	(0.000)	(0.000)	(0.002)	(0.003)
post CDD years (2011-2014)	0.388***	1658.3***	0.238***	1117.0***
	(0.000)	(0.000)	(0.000)	(0.000)
treatment × post CDD years (2011-2014)	-0.0547	-219.2	-0.0710	-471.6
	(0.613)	(0.702)	(0.374)	(0.255)
ward fixed effects	Yes	Yes	Yes	Yes
N	560	560	560	560
annual average dep. var.	0.268	1230.9	0.132	702.5

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, p-values in parentheses allow for village-level clustering. The units of observations are villages×years. The regressions control for ward fixed effects.

To further investigate this, Table 7 uses those proxies for the number of projects and for project budgets in a Difference-in-Differences framework, where each observation is the number of projects or the budget size for a village-year. Based on data from village focus groups (columns 1-2), this table shows that there were 0.5 more projects reported in treatment villages than in control villages, i.e., about 1.5 more overall during the years 2008-2010. The total budget in treatment villages is estimated to exceeded that of the control group by an annual US$2,913 during the CDD years (a total of roughly US$9,000 over the three CDD years). On the other hand, there is no significant difference for the years before or after the CDD. Columns 3 and 4 again restrict the data to those projects reported by focus groups that were named by three or more households in the in-depth survey. The results using these reports, which are likely less prone to reporting biases, remain comparable.

In sum, in terms of project counts and total budget, there is no evidence for a focus on control villages in the period after the CDD. The differences in numbers of projects and budgets between treatment and control villages are statistically insignificant and small in comparison to the benefits that CDD-villages received during the CDD years.

(ii) As a further robustness check, we use the measurements on non-CDD projects as controls in our main dyadic regressions. We add a variable measuring the number of non-CDD projects implemented between 2011 and 2014 and one that controls for a binary indicator for having implemented any non-CDD projects in this time frame. The results are shown in Table 8, columns 2-3. In these regressions, the main CDD-treatment effect is still negative, statistically significant, and of comparable magnitude as in the main specification in column 1. Of course, adding data on the number of other programs to our main specification makes the estimates harder to interpret. Because of obvious concerns about endogeneity, we are reluctant to interpret the coefficient on the non-CDD projects variables. Yet, if the negative treatment effects we observe in our main regressions would indeed be a *positive* effect of compensatory non-CDD projects, we would expect that controlling for non-CDD projects affects the ATE estimate in those regressions. The fact that this is not the case suggests that non-CDD projects are not fully responsible for the correlation between CDD projects and economic flows that we find.

Table 8: Robustness of the Main Result to Controlling for Compensatory Projects

	standard specification	controlling for non-CDD projects		
	(1) any transaction	(2) any transaction	(3) any transaction	(4) any transaction
treatment	-1.133	-1.114	-1.127	-1.307
	(0.004)●●⊛	(0.004)●●⊛	(0.005)●●⊛	(0.012)●⊛
projects 2011-14		0.086		
		(0.523)		
any projects 2011-14			0.066	
			(0.895)	
CILIP				-0.439
				(0.481)
controls	✓	✓	✓	✓
dyads	151632	151632	151632	151632
households	2774	2774	2774	2774
control mean dep. var.	6.9	6.9	6.9	6.9

Notes: $^{\bullet}$/* $p < 0.1$, $^{\bullet\bullet}$/** $p < 0.05$, $^{\bullet\bullet\bullet}$/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. Regressions control for ward fixed effects and a set of control variables: the village-level variables in Appendix Table A1, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table A1, as well as ethnicity and enumerator dummies enter the regressions twice, for the sending and the receiving household of a dyad. Indicators for traditional leaders and marginal households are also included but not shown in this table.

(iii) At the time of the implementation of the CDD, a later scaling-up was envisioned in which all eligible villages would eventually receive program benefits similar to the villages that were treated in 2008-2010. However, the scaling-up was conditional on a positive evaluation of the program by the implementing agency.[5] Therefore, the intentions to scale up were not announced to control villages at the time, and control villages could not expect a treatment in the future. By the time of our data collection in 2014 this scaling-up had not yet occurred. After our data collection—towards the end of 2014—activities within a program called the Community-Based Infrastructure and Livelihood Improvement Project (CILIP), which was modeled after a CDD (and funded by the Islamic Development Bank), started in some of the control communities. We have the full list of CILIP recipient villages and some of the control villages received funds as part of CILIP. In our sample, less than half of the control villages (11 out of 28) were selected to receive CILIP funds.[6] Our results remain virtually unchanged if we control for CILIP recipient villages (see Table 8, column 4), which is reassuring but not surprising, given that the actual disbursement of the CILIP funds had not begun at the time of our data collection.

In addition, given that a scale-up was always planned, we believe that it is unlikely that the government would implement other official compensatory-type projects in control villages and that this also reduced incentives for other development actors to focus their activities on non-CDD villages.

There is also a large number of different “project partners” mentioned during the focus group discussion with the village authorities for the non-CDD projects. While there is clearly

[5] Both the evaluation and the roll-out were slowed down. The evaluation survey of the program only took place in 2012, with a scaled-down version of the original survey, which in our view cannot be used to make statements about the program’s effects with much confidence. However, the evaluation is fairly in-line with our results and reports some effects on VDC participation and livestock ownership, but finds no significant treatment effects on “the remaining measures (wealth, health, volunteering, and cohesion)”. (Fanneh and Jallow, 2013)

[6] These statements are based on administrative data available to us and conversations with government officials, during which we were also told that the assignment for CILIP was random. We do not have further details on the assignment process however.

measurement error in these data, this is evidence that there was no single organization (international, government or NGO) behind the projects that were implemented in non-CDD villages. Thus, the data suggest that the projects implemented in control villages were not part of a coordinated effort by one or a small number of organizations to compensate control villages for being left out from the CDD project.

G Other Treatment Effect Regressions

G.1 Wealth

Table 9: In-Depth Survey: Asset/Wealth Variables (1)

	(1) plough	(2) seeder	(3) cart	(4) draught animals
treatment	-0.075	-0.040	-0.073	-0.116
	(0.065)$^{\circledast}$	(0.463)	(0.076)$^{\circledast}$	(0.166)
controls	✓	✓	✓	✓
households	550	550	550	550
control mean dep. var.	0.6	0.6	0.4	1.0

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: household- and village-level variables in Panels B and C of Appendix Table A1 as well as ethnicity and enumerator dummies.

Table 10: In-Depth Survey: Asset/Wealth Variables (2)

	(1) bicycle	(2) mobile	(3) smart phone	(4) radio	(5) generator	(6) motorbike	(7) car	(8) tv	(9) AC
treatment	0.015	-0.031	-0.016	0.005	-0.072	-0.029	0.009	-0.075	0.001
	(0.715)	(0.041)$^{\bullet\bullet\circledast}$	(0.340)	(0.892)	(0.010)$^{\bullet\bullet\circledast}$	(0.209)	(0.330)	(0.019)$^{\bullet\bullet\circledast}$	(0.904)
controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
households	550	548	549	548	550	550	550	550	550
control mean dep. var.	0.6	1.0	0.1	0.7	0.2	0.1	0.0	0.1	0.0

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: household- and village-level variables in Panels B and C of Appendix Table A1 as well as ethnicity and enumerator dummies.

Table 11: In-Depth Survey: Asset/Wealth Variables (3)

	(1) grid electr.	(2) tap water	(3) toilet	(4) bank account	(5) floor quality	(6) roof quality	(7) wall quality
treatment	0.011	0.012	0.025	0.016	0.040	-0.033	-0.050
	(0.521)	(0.150)	(0.523)	(0.544)	(0.755)	(0.603)	(0.697)
controls	✓	✓	✓	✓	✓	✓	✓
households	550	550	548	543	550	550	550
control mean dep. var.	0.0	0.0	0.9	0.1	2.4	2.6	1.8

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: household- and village-level variables in Panels B and C of Appendix Table A1 as well as ethnicity and enumerator dummies.

H Favoritism

To shed light on the possible role of favoritism, we study whether dyads between decision makers and closely connected households show disproportionately large treatment effects. Indeed, recently Bandiera et al. (2018) provide evidence that social ties determine program delivery in an agricultural extension program in Uganda. They argue that households that are central to the program channel resources to individuals that they are socially connected to, possibly in exchange for other favors. Translated into the context of networks of economic transactions, this would imply increased informal economic interaction between pairs that are socially tied and where one belongs to the VDC and the other does not. Consistent with this, in Table 12 we find that the heterogeneous effect with respect to likely VDC membership (documented in Main Text Table 8) is to a large extent explained by transactions in dyads between households that are connected through kinship and have a very different VDC-score.

Table 12: Heterogeneous Treatment Effects for Contacts Between Likely VDC members and Their Social Links

	(1) any transaction
treatment	-1.165
	(0.002)$^{\bullet\bullet\circledast}$
treatment × kinship	1.115
	(0.605)
$\mid$ VDC-score$_\mathrm{i}$ − VDC-score$_\mathrm{j}$ $\mid$	-0.239
	(0.314)
treatment × $\mid$ VDC-score$_\mathrm{i}$ − VDC-score$_\mathrm{j}$ $\mid$	0.570
	(0.058)$^{\bullet}$
kinship × $\mid$ VDC-score$_\mathrm{i}$ − VDC-score$_\mathrm{j}$ $\mid$	-2.051
	(0.110)
treatment × kinship × $\mid$ VDC-score$_\mathrm{i}$ − VDC-score$_\mathrm{j}$ $\mid$	4.598
	(0.068)$^{\bullet}$
controls	✓
dyads	151632
households	2774
control mean dep. var.	6.9

Notes: $^{\bullet}$/* $p < 0.1$, $^{\bullet\bullet}$/** $p < 0.05$, $^{\bullet\bullet\bullet}$/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. Regressions control for ward fixed effects and a set of control variables: the village-level variables in Appendix Table A1, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table A1, as well as ethnicity and enumerator dummies enter the regressions twice, for the sending and the receiving household of a dyad. VDC-score measures the likelihood that a household would be in the VDC if the village is/would be a CDD village. Values of the score are normalized to have mean zero and variance one. The absolute difference of the two scores used in these regressions is centered.

I Project Benefits and Project Decision Making

Table 13: Who Reaps Benefits from Projects? Who Decided on Projects?

(a) Household-Level Regressions

	Benefit Averaged over...		
	(1) CDD projects	(2) CDD projects	(3) non-CDD projects
marginalized	0.186	0.193	0.121
	(0.129)	(0.125)	(0.381)
trad. leader	0.325	0.293	-0.256
	(0.064)*	(0.067)*	(0.175)
VDC member	0.157	0.240	0.027
	(0.070)*	(0.023)**	(0.850)
$\mathbb{1}$(land $\leq$ 2ha)		-0.317	-0.003
		(0.004)***	(0.981)
controls	✓	✓	✓
households	268	265	218
sample	Treatment	Treatment	Control
mean dep. var.	2.384	2.394	2.529
dep. var. range	0 - 3	0 - 3	0 - 3

(b) Project-Level Regressions

	Who decided on this project?		
	(1) villagers	(2) trad. leaders	(3) VDC
CDD	0.153	-0.042	0.225
	(0.024)**	(0.322)	(0.002)***
CDD × marginalized	-0.212	0.155	0.067
	(0.002)***	(0.009)***	(0.511)
CDD × trad. leader	0.025	0.005	0.049
	(0.820)	(0.908)	(0.602)
CDD × VDC member	0.105	-0.003	0.155
	(0.257)	(0.947)	(0.209)
CDD × $\mathbb{1}$(land $\leq$ 2ha)	-0.032	-0.009	-0.096
	(0.610)	(0.856)	(0.302)
household fixed effects	✓	✓	✓
project reports	706	706	706
sample	Treatment	Treatment	Treatment
mean d. v. (non-CDD)	0.355	0.120	0.186
dep. var. range	0 - 1	0 - 1	0 - 1

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, p-values in parentheses allow for village-level clustering. Units of observation are households in the left table and reports about development projects elicited from 10 households per village in the right table. Household-level regressions in Table (a) control for ward fixed effects. household- and village-level variables in Panels B and C of Appendix Table A1 as well as ethnicity and enumerator dummies. Project-level regressions in Table (b) control for household fixed effects. CDD projects were prompted when the responded did not report them. When prompted reports are excluded to ensure comparability between CDD and non-CDD projects in Table (b), results are similar but slightly weaker: The main effect in column 1 (reports on CDD projects by non-marginalized, non-leader, non-VDC member, and landless households) is not significant anymore (p-value=0.188). But the heterogeneity for marginalized households remains significant. The heterogeneity in column 2 (CDD × marginalized) is not significant anymore (p-value=0.132). The main effect in column 3, suggesting that households perceived the VDC to be decisive, remains. Additionally, it appears significant that VDC members are even more likely to report that they were influential (p-value=0.033).

J Project Summary Statistics

Table 14: Perceived Relative Benefit by Project Type

villlages:	control	treatment	
project:	non-CDD	non-CDD	CDD
Benefit from this project compared to other villagers:			
less	4.4%	7.1%	13.0%
same	94.9%	88.1%	86.0%
more	0.7%	4.7%	1.0%

Notes: The project reports by households are based on the in-depth survey and hence aggregated from responses by ten households per village. Percentages are unweighted shares among all reports by all households. CDD projects were prompted when the responded did not report them. When prompted reports are excluded, the results remain quantitatively unchanged.

Table 15: Project Failure Rates

Projects	failure rate	matched project groups
reports by households	29%	60
reports by village authorities	25%	64
reports by enumerators	29%	52

Notes: The project reports by households are based on the in-depth survey and hence aggregated from responses by ten households per village. CDD projects were prompted if the household did not list them. Reports by village authorities are based on the village focus group. Reports by enumerators are based on visits to project sites and thus exclude projects without visitable sites (e.g., if only bags of fertilizers were bought that were distributed to households).

K Shocks are Balanced

Table 16: Shock Incidence Is Not Affected by Treatment

	(1) shock count	(2) production shock	(3) housing shock	(4) health shock
treatment	0.061	0.039	-0.017	0.039
	(0.224)	(0.227)	(0.472)	(0.201)
households	2769	2774	2774	2769
control mean dep. var.	1.9	0.8	0.4	0.7
95% confidence band	[-0.04, 0.16]	[-0.02, 0.10]	[-0.06, 0.03]	[-0.02, 0.10]

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: household- and village-level variables in Panels B and C of Appendix Table A1 as well as ethnicity and enumerator dummies.

L More on Mechanisms

L.1 Were Projects Implemented?

In this appendix we confirm a necessary condition for the CDD program to have any impact, namely that it was implemented, both physically as well as using the mandated procedures.[7] There is overwhelming evidence that CDD sub-projects were not only initiated but actually delivered, and CDD procedures were followed.

First, sub-projects were physically implemented. Indeed, physical inspections by our enumerators during our fieldwork as well as survey evidence confirm implementation in all villages (evidence is summarized in Table 17).[8] Thus, all treatment villages received significant funds and implemented sub-projects, such as tractors, milling machines, or seed stores, that resulted in substantial physical changes to the available economic infrastructure. Further, our surveys also allow us to confirm that households are generally very aware of those sub-projects (Table 18).[9]

[7] However, it is only necessary that the project was locally initiated and households were made aware. If eventually a planned CDD sub-project was not delivered, it may still be possible that this "failed promise" would lead to changes in networks. For example, failure to deliver a project may lead to quarrels in the village.

[8] Administrative disbursement data confirms disbursement for 26 out of 28 villages. Two of our sample villages (for which we confirmed disbursements on the ground) could not be matched to the disbursement data base.

[9] In open survey questions about recent or ongoing development projects, two thirds of all households in treatment villages discussed CDD sub-projects without prompting. 94% could provide details about CDD sub-projects after being prompted.

Second, we investigate whether procedures were implemented as required by the CDD program's guidelines. Again, we confirm implementation. Using project-level reports from the in-depth survey, we find that compared to non-CDD projects, CDD sub-projects are almost twice as likely to have involved the whole village or the VDC in decision making (the probabilities are larger by 17.6 and 20.1 percentage points respectively, as shown in Table 19). Also, evidence suggests that voting took place for at least some CDD sub-projects and voting is significantly more likely for CDD than for non-CDD development projects that were implemented in villages in the last 5 years.

Table 17: Evidence for Project Implementation

	Administrative disbursement data	Enumerators in the village	Focus groups with local authorities	Median villager in interviews
CDD-villages identified (of 28)	26	28	28	28
average number of subprojects	2.0	1.8	2.1	2
average grant amount (USD)	14389.1		13752.3	
at least one subproject is functional		24	25	24

Notes: The GMD USD conversion is based on the exchange rate 0.048. Enumerators only visited sites of existing projects. In cases were the project money was used to buy, e.g., fertilizer to be distributed to households, enumerators could not visit the project sites, hence the lower number of sub-projects.

Table 18: Evidence for Project Awareness and Functionality

	percentage	households
respondents...		
...know CDD projects without prompting	66%	277
...know details after prompting	94%	277
...list CDD as the most beneficial project	59%	1413
...list at least one functioning CDD sub-project	88%	268
enumerators...		
...list at least one functioning CDD project	86%	28

Notes: This table summarizes CDD projects in treatment villages. 277 households in 28 treatment villages were interviewed about the projects in detail. 1416 households were asked broad questions about development projects. Missing values are due to non-response/refusal to the specific sub-questions regarding benefit. In each village, one enumerator confirmed the existence and assessed the functionality of project sites.

L.2 Did Intended Institutional/Social Change Take Place?

The second area that CDD aims to affect is institutions. Table 19 investigates data at the project level. Columns 1 to 4 shows that non-CDD projects implemented in these villages also seem to be more inclusive, in the sense that villagers report participating in the decision making and they apparently have done so at least in part through voting. Results in columns 5 to 8 are evidence that relative to other development projects, CDD sub-project implementation was accompanied by substantially more voting and that decisions were mostly made by the whole community or delegated to village development committees, as opposed to being taken by the traditional elites. Moreover, these data can be used to test whether these procedural changes also affected joint decision-making procedures in other, non-CDD projects. Indeed, we find some evidence of such spillovers.

Table 19: CDD Projects Were Implemented in Line With CDD Requirements and Procedures Spilled Over Into Other Projects

	Comparing non-CDD projects across villages				Comparing CDD to non-CDD			
	(1) villagers decided	(2) VDC decided	(3) trad. leaders decided	(4) was there a vote?	(5) villagers decided	(6) VDC decided	(7) trad. leaders decided	(8) was there a vote?
treatment	0.036	-0.062	-0.035	0.032				
	(0.281)	(0.226)	(0.262)	(0.075)*				
CDD					0.176	0.201	0.030	0.132
					(0.000)***	(0.000)***	(0.269)	(0.001)***
marginalized					0.059	0.145	0.013	-0.003
					(0.206)	(0.033)**	(0.839)	(0.925)
trad. leader					0.025	-0.109	0.068	0.016
					(0.739)	(0.066)*	(0.297)	(0.729)
CDD × marginalized					-0.163	-0.032	-0.017	-0.086
					(0.010)**	(0.692)	(0.818)	(0.117)
CDD × trad. leader					0.098	0.064	-0.160	0.028
					(0.358)	(0.372)	(0.021)**	(0.747)
controls	✓	✓	✓	✓	✓	✓	✓	✓
project reports	626	626	626	610	1096	1096	1096	1064
sample	non-CDD	non-CDD	non-CDD	non-CDD	all projects	all projects	all projects	all projects
mean dep. var.	0.3	0.2	0.1	0.0	0.3	0.3	0.1	0.1
dep. var. range	0 - 1	0 - 1	0 - 1	0 - 1	0 - 1	0 - 1	0 - 1	0 - 1

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, p-values in parentheses allow for village-level clustering. Units of observation are reports about development projects elicited from 10 households per village. Observations are weighted by the inverse of the number of projects listed by a household, to give the same weight to each household. All regressions control for ward fixed effects. The sample includes projects from all 56 villages. Villagers could give multiple answers regarding who participated in the decision-making process. Possible answer categories omitted from this table are: *Kabilo* (neighborhood) heads, compound heads and 'other', which were only rarely mentioned. If respondents did not recall a CDD sub-project, it was prompted by the enumerators, for non-CDD projects that was not possible. Thus, comparisons between non-CDD projects and potentially prompted CDD sub-projects as in Panel A could be problematic. However, the results in Panel A are qualitatively unchanged when prompted project reports are excluded from the estimation sample.

However, we do not find evidence of institutional change in data on community meetings in general. Table 20 shows that there are no average treatment effects with respect to decision-making processes or for households' feelings of empowerment. In fact, the results even provide suggestive evidence that marginalized households got further excluded from village activities and village-level decision making.

Table 20: Institutional Change in Village-Level Decision Making

	vill. meetings (past year)		vill. decision-making		unjust decisions in the village		
	(1) attend and speak	(2) meetings	(3) favors voting	(4) actual voting	(5) speak up against	(6) can change	(7) talks to chief
Panel A: without interactions							
treatment	-0.043	-0.119	0.009	0.010	0.039	0.071	-0.035
	(0.233)	(0.771)	(0.725)	(0.676)	(0.538)	(0.124)	(0.706)
Panel B: with interactions							
treatment	-0.066	0.192	0.062	-0.009	0.039	0.063	-0.041
	(0.123)	(0.656)	(0.023)●⊛	(0.742)	(0.583)	(0.250)	(0.709)
marginalized	-0.272	-0.064	0.143	-0.050	-0.316	-0.369	-0.500
	(0.001)***	(0.915)	(0.009)***	(0.278)	(0.014)**	(0.001)***	(0.003)***
treatment × marginalized	0.094	-1.818	-0.291	0.107	-0.048	-0.009	-0.033
	(0.399)	(0.036)●⊛	(0.000)●●●	(0.232)	(0.750)	(0.953)	(0.883)
controls	✓	✓	✓	✓	✓	✓	✓
households	540	545	546	505	547	547	548
control mean dep. var.	0.6	5.8	0.4	0.1	1.3	1.0	1.6

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. p-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Main Text Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: household- and village-level variables in Panels B and C of Appendix Table A1 as well as ethnicity and enumerator dummies. Dep. var. in columns 5 and 6 are measured on a three point scale and in column 7 on a five point scale. In column 5 the answers range from "Would not speak up against an unjust decision" to "Would definitely speak up". In column 6 the answers range from "No chance my household could change an unjust decision within the village" to "It is very likely my household could change an unjust decision". In column 7 the answers range from "Never spoke to the Alkalo or the VDC about village issues in the last year" to "Almost every day".

References (Online Supplement)

Arcand, J.-L., Chen, Y.-P., He, Y., Diop, C. I. F., Wouabe, E. D., Garbouj, M., Jaimovich, D., and Zec, S. (2010). *The Gambia CDDP Baseline: Rural Household Survey, Qualitative Survey, Village Network Survey.* Working Paper. Geneva: The Graduate Institute.

Bandiera, O., Burgess, R., Deserranno, E., Morel, R., Rasul, I., and Sulaiman, M. (2018). *Social Ties and the Delivery of Development Programs.* Working Paper. mimeo.

Banerjee, A., Chandrasekhar, A., Duflo, E., and Jackson, M. O. (2019). *Changes in Social Network Structure in Response to Exposure to Formal Credit Markets.* Working Paper. Stanford University.

Banerjee, A., Duflo, E., Goldberg, N., Karlan, D., Osei, R., Parienté, W., Shapiro, J., Thuysbaert, B., and Udry, C. (2015a). "A multifaceted program causes lasting progress for the very poor: Evidence from six countries". *Science* 348 (6236), pp. 772–788.

Banerjee, A., Karlan, D., and Zinman, J. (2015b). "Six Randomized Evaluations of Microcredit: Introduction and Further Steps." *American Economic Journal: Applied Economics* 7 (1), pp. 1–21.

Beaman, L. and Dillon, A. (2012). "Do Household Definitions Matter in Survey Design? Results from a Randomized Survey Experiment in Mali". *Journal of Development Economics* 98 (1), pp. 124–135.

Breiman, L. (2001). "Random Forests". *Machine Learning* 45 (1), pp. 5–32.

Campbell, K. E. and Lee, B. A. (1991). "Name Generators in Surveys of Personal Networks". *Social Networks* 13 (3), pp. 203–221.

Casey, K., Glennerster, R., and Miguel, E. (2012). "Reshaping Institutions: Evidence on Aid Impacts Using a Preanalysis Plan". *Quarterly Journal of Economics* 127 (4), pp. 1755–1812.

Fanneh, M. M. and Jallow, Y. S. (2013). *The Gambia Community-Driven Development Project (CDDP) Report - 2013.* Report. University of The Gambia.

Friedman, J., Hastie, T., and Tibshirani, R. (2001). *The Elements of Statistical Learning.* Springer series in statistics New York.

Gagnon, J. and Goyal, S. (2017). "Networks, Markets, and Inequality". *American Economic Review* 107 (1), pp. 1–30.

GoTG (2006). *Gambia – Community-Driven Development Project.* Project Implementation Manual. Government of The Gambia.

Heß, S. (2017). "Randomization Inference with Stata: A Guide and Software". *Stata Journal* 17 (3), pp. 630–651.

Jaimovich, D. (2015). "Missing Links, Missing Markets: Evidence of the Transformation Process in the Economic Networks of Gambian Villages". *World Development* 66, pp. 645–664.

Local Government Act (2002). *LGA 2002.* Legal Document. National Council for Civic Education, Government of The Gambia.

Mullainathan, S. and Spiess, J. (2017). "Machine Learning: An Applied Econometric Approach". *Journal of Economic Perspectives* 31 (2), pp. 87–106.

White, H., Menon, R., and Waddington, H. (2018). *Community-Driven Development: Does It Build Social Cohesion or Infrastructure?* Working Paper 30. International Initiative for Impact Evaluation.

Wong, S. (2012). *What Have Been the Impacts of World Bank Community-Driven Development Programs? CDD Impact Evaluation Review and Operational and Research Implications.* Working Paper 69541. Washington, DC: World Bank.

World Bank (2006). *Gambia – Community-Driven Development Project.* Project Appraisal Document 36786-GM. Washington, DC: World Bank.