Development Projects and Economic Networks: Lessons From Rural Gambia

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Abstract

This paper investigates the effects of development projects on economic networks. To this end, we study the impact that a randomly allocated Community-Driven Development program in The Gambia has on economic interactions within rural villages. The program provides an exogenous source of variation to village-level stocks of productive capital and to village-wide collective activities. Based on detailed data on economic and social networks, we find a significant reduction of transfers in these networks in treatment villages. Guided by a theoretical framework, we investigate several possible mechanisms and find evidence that is consistent with two channels. First, the evidence points to modest wealth effects and a village-level transformation process towards a more formal economy. Second, we also find evidence that is consistent with elite capture, favoritism, and unequally distributed benefits leading to reductions in social capital and thus economic transactions. Overall, our findings suggest changes in networks as an avenue through which development interventions may have unintended consequences.

1 Introduction

Networks of informal relationships take on a central role in rural societies of less developed economies. Where markets are incomplete, intra-village networks help households to enforce informal contracts (Karlan, 2007; Karlan et al., 2009; Giné et al., 2010; Jackson et al., 2012; Chandrasekhar et al., 2018), pool risk to deal with adverse shocks (Rosenzweig, 1988; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006), and aggregate or diffuse information (Bandiera and Rasul, 2006; Conley and Udry, 2010; Beaman and Magruder, 2012; Banerjee et al., 2013; Alatas et al., 2016). Although the role of networks in the lives of the poor is well established, much less is known about how their structure and their role in households' welfare evolve. In particular, not much is known empirically about how networks of interactions change during the process of development and as a consequence of development projects, despite prominent theoretical work on this question (Kranton, 1996; Gagnon and Goyal, 2017). Understanding the effects of development projects on networks is of particular policy relevance in light of the recent focus on participatory development programs that involve communities more directly in project choice and administration and have a number of features that likely affect networks directly and indirectly.

In this paper, we contribute towards filling this gap in the literature by providing empirical evidence from a large Community-Driven Development (CDD) program in The Gambia. Starting in 2008, this World Bank-financed program allocated funds for village-level development projects to about a third of all rural villages in The Gambia. Importantly, the almost 500 treated villages were chosen randomly from a set of over 900 eligible poor villages. The funding was significant, with a per-household allocation that is roughly equivalent to half the GDP per capita in The Gambia. The model framework that we develop below demonstrates theoretically that the predicted net effect of the CDD program on networks of interactions is ambiguous and remains an empirical question. To study the effect of the CDD program empirically, in 2014 we collected post-treatment data on a broad set of interactions among the universe of households in 56 villages (half of them treatment villages). These data cover six different economic domains: land, labor, inputs, food, gifts, and credit. We also collected data for two social domains: an indicator of friendship, and kinship. We interviewed all households in each village and designed the survey to record all relevant informal transactions among them. In our main analysis, we combine all information for the six economic domains by taking the union to form one network of informal economic transactions. The random assignment of the CDD program allows for a straightforward estimation of the effect of the CDD program on indicators of the network structure.

Our main finding is that the CDD treatment significantly reduces informal economic interactions. In our main specification, where we use dyadic regressions, the estimated magnitude of the average effect implies an economically significant loss of about one in six transactions between households. The main results are robust to a number of approaches to deal with two limitations of our setup, namely the lack of network data before the intervention, and the possibility that NGOs, the government, or other development actors target control villages more intensely in the post-treatment years.

To study how the CDD program has reduced transactions, we use a simple framework to model the effect of changes in the social and economic environment that the CDD program brings about. We focus on the following three channels.

(i) Deciding on and implementing CDD projects requires many meetings and other joint activities, leading to frequent social interactions. For example the Gambian CDD program mandates that communities organize up to 38 village-level activities (GoTG, 2006). During these activities, villagers make joint decisions, come together to contribute to the project, and form organizing committees, bringing groups together that might otherwise have little contact with each other. Many implemented projects also require joint activities in administering and maintaining project infrastructure or machinery. CDD programs generally also aim at building social capital (Casey et al., 2012; Mansuri and Rao, 2012; White et al., 2018; Casey, 2018). In line with common theoretical results on sustaining cooperation with frequently repeated interactions, the model predicts that a positive exogenous change in the frequency of meetings implies more informal links.

(ii) CDD programs aim at economic improvements at the individual and the village level. In addition, CDD programs can increase market integration. In our model framework, households benefit from exchanging gifts because it provides a way to share risk. Further, exchanging gifts is beneficial if the exchanged items are valued differently by different parties. In this setup increasing incomes combined with diminishing marginal utility as well as the emergence of universal prices for goods due to the development of markets can both reduce the net benefit of engaging in gift exchange and decrease the number of informal gift-exchange links.

(iii) Benefits of CDD projects may be unequally distributed. In particular, the participatory approach and the large size of the funds allocated to villages increase the incentives and opportunities for elite capture and favoritism (Platteau, 2004; Bardhan and Mookherjee, 2000; Bandiera et al., 2018). The conceptual framework shows how this can translate into fewer links on average and more central network positions for some individuals. Further, unequally distributed benefits may lead to internal divisions (Barron et al., 2011). If disputes lead to fewer social interactions, the model predicts fewer links.

Guided by this framework, we then investigate empirically the possible mechanisms. Overall, our findings are most consistent with channels (ii) and (iii) of the model framework. First, we find that the program has positive, though modest average effects on indicators of economic well-being and outward (market) orientation. Reductions in reciprocated transactions and the reduced importance of socially supported relationships also point to an increased market orientation in treatment villages. However, the *average* effects on wealth, assets, and animal ownership alone are modest and it remains questionable whether the program was able to increase wealth and income enough to reduce the need for informal insurance.

Yet, there is significant heterogeneity in these wealth effects, which supports the other channel for which we find evidence: unequally distributed benefits and elite capture. Beyond the significant heterogeneity in economic benefits, we also find evidence that households *perceive* the distribution of benefits as unequal. Unequal benefits, in turn, seem associated with reductions in transactions. We find that various proxies for unequally distributed benefits at the village-level and at the dyad-level are all correlated with fewer economic transactions. Households in treatment villages also not only have fewer economic transactions with each other, but also report fewer social links and less participation in community-based organizations and village meetings. This effect seems strongest for marginalized groups. The findings on decreased social interaction accompanying the reduction in economic networks support channel (iii) and strongly speak against channel (i), i.e., more frequent project-related meetings did not increase social capital.

Finally, we investigate what our network finding might imply for welfare. It is ex-ante unclear how a reduction in informal economic transactions should be interpreted from a normative standpoint. On the one hand, if significant economic benefits are the reason for observing fewer transactions (as suggested by channel (ii)), the lower activity in informal networks can be viewed as an indicator of a welfare improvement. However, we conjecture that the observed positive economic changes alone are too small and too heterogeneous to draw this positive conclusion. On the other hand, the reduction in the number of informal transactions can also be a reflection of a deterioration of social conditions (as suggested by the evidence related to channel (iii)). One important way in which reduced network interactions can affect welfare is through households' reduced ability to cope with shocks. An extensive literature has shown that in poor countries, networks and welfare are linked through social ties that insure against shocks, i.e., social ties define risk-sharing networks (e.g., Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006; Fafchamps and Gubert, 2007). To investigate this more directly, we collected data on shocks. We find a significant correlation of shocks and flows in the economic domains that we study, which confirms prior findings on risk-sharing networks. However, turning to the effect of the program on economic flows after shocks, we do not find a general difference in how flows respond to shocks between treatment and control villages. This suggests that welfare is not negatively impacted through a reduced ability to share risk. Yet, this also implies that treatment effects on wealth and savings were not large enough to reduce the reliance on intra-village risk sharing to deal with shocks.

Overall, our results caution that development projects, in particular those with participatory features in which roles and benefits may be unequally distributed, can have unintended consequences for the economic and social networks of villagers. These effects deserve more empirical work, and warrant awareness in future analyses of development projects.

Our paper contributes to five main literatures. First, networks are usually taken as exogenously given (e.g., Bramoullé et al. 2009; Calvó-Armengol et al. 2009; Banerjee et al. 2013; Ambrus et al. 2014), but little is known about how networks evolve. Of particular relevance to policy is a better understanding of the effect that development interventions have on networks. Recently, a few papers have studied how networks react in response to the arrival of formal institutions for credit and insurance (Binzel et al., 2013; Cecchi et al., 2016; Comola and Prina, 2017; Banerjee et al., 2019).[1] This paper contributes to this emerging literature in that it studies an intervention that (a) constitutes a direct shock to the existing stock of productive capital in the village economies, (b) has potential implications for various markets (e.g., markets for labor inputs, food outputs or credit), (c) has a much larger per household budget than the typically analyzed interventions, and (d) raises concerns about distributional issues, including the

[1] These papers report a reduction in informal arrangements between network members (Binzel et al., 2013). They also find that these effects spill over into other realms and reduce interactions in networks not directly affected by the intervention and interactions between households who do not benefit from the new institution (Cecchi et al., 2016; Banerjee et al., 2019).

possibility of elite capture, due to the participatory nature of the project and the size of the distributed funds.

Second, our data allow us to contribute to the recent literature on network formation that shows how cooperation, in situations lacking formal enforcement, can be achieved and sustained through social networks. Our analysis highlights the role of social proximity through “supported” relationships (Karlan et al., 2009; Jackson et al., 2012) and studies how this role changes in response to an intervention.[2]

Third, we contribute to the literature on the effects of CDD programs on economic changes (Mansuri and Rao, 2012; Wong, 2012; White et al., 2018; Casey, 2018) and on institutions and social capital (Labonne and Chase, 2011; Casey et al., 2012; Beath et al., 2013; King and Samii, 2014; Avdeenko and Gilligan, 2015; Fearon et al., 2015; Humphreys et al., 2019). We add a new perspective on the question about the CDD program’s effects on social capital and institutions as we study these through the lens of social and economic networks.

Fourth, the literature strongly points to elite capture as a serious concern in the context of development projects (Bardhan and Mookherjee, 2000), and in participatory programs at local levels in particular (Platteau, 2004; Olken, 2007; Alatas et al., 2019). We use networks analysis to contribute to this literature. Further, our analysis also speaks to the small literature relating development projects to internal disputes (Barakat, 2006; Barron et al., 2011).

Finally, we add to the literature on the role of networks to provide insurance against shocks (Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006; Fafchamps and Gubert, 2007).

The remainder of this paper is structured as follows. Section 2 introduces the CDD program. Section 3 introduces a simple model. Section 4 describes our data and empirical strategy and Section 5 presents our main results. Sections 6 and 7 contain results related to mechanisms and welfare implications. Finally, Section 8 concludes.

2 Background: The Gambian CDD Program

International donors, multilateral organizations, and national governments are increasingly favoring bottom-up approaches, such as CDD programs, that involve local communities in project design and implementation.[3] The participative process in CDD programs is expected to contribute to improvements in economic conditions through better targeting, reduced implementation costs, and improved maintenance, as well as to build capacity and improve local governance.

The Gambian CDD program was implemented between 2008 and 2010, was mainly financed by the World Bank, and targeted about 50 percent of the Gambian rural population (World Bank, 2006). The program was implemented in 495 villages belonging to 88 wards. Only communities with a population between 100 and 10,000 inhabitants (according to the census in 2003) were eligible for the program. For targeting purposes, village-level indicators of poverty were calculated using the Census 2003 data, and the two thirds of villages ranked the poorest in each ward were selected as eligible for the program. Within the group of 930 eligible villages, around half of the villages (495) were randomly assigned to treatment, i.e., received funding for one or several village-level projects of their choice. To distinguish the village-level projects from the CDD program at the country level, we refer to village-level projects as “sub-projects”. The random assignment of the treatment was stratified at the ward level (wards typically consist of around 6-14 eligible villages).

The program promoted community involvement at all stages of the process, from identification of the potential sub-projects to their maintenance after implementation. Specifically, a village-level institution called Village Development Committee (VDC), was tasked with identifying economic priorities, developing plans, and managing the financial resources, as well as the actual implementation of the sub-projects at the village level (World Bank, 2006). The VDCs are composed of normal villagers, and the establishment of VDCs was subject to a number of inclusiveness criteria to ensure the representation of different groups in the village (see, e.g., World Bank 2006, p. 4, or GoTG 2006, p. 9). More details are provided in Supplementary Material A.1.

The Gambian CDD program directly targeted poverty reduction, income growth and capacity building, in contrast to other CDD program designs that put a stronger emphasis on social outcomes (see

[2]Further, we have detailed data on six different economic networks, which we collect for the universe of households within each village. We also add to the still relatively small set of papers (e.g., Comola and Prina, 2017; Cruz et al., 2017) that are able to avoid the biases of network analyses based on samples of households (Chandrasekhar and Lewis, 2016).

[3]These kinds of programs represent between 5% and 10% of the overall World Bank lending portfolio (Wong, 2012). In 2019 there were “219 active CDD projects in 79 countries totaling $21.6 billion [...]. An additional $12.1 billion was provided by borrowers and other donors.” (World Bank, 2019)

Supplementary Material A.1). Aside from inducing substantial amounts of social and intra-village political interaction, the program constituted a sizable positive economic shock to the productive capital in treatment villages. The budget allocated to treatment villages was a base of US$10,000, plus an extra budget determined by a formula based on population and poverty. The average disbursement for the 495 treatment villages was around US$11,500 (current values). Since in our sample the average treatment village has around 50 households, this translates into per-household allocations of around US$230, i.e., roughly equivalent to half of the annual per capita income in The Gambia.[4] Villages were free to choose a single large sub-project or multiple smaller ones. One third of the villages decided to invest the full grant into a single sub-project, while the remaining villages split funds into up to five sub-projects. Sub-projects that were financed through the CDD program were typically local public goods or club goods. In our sample of 28 treatment villages, the most common sub-projects are: farm implements and inputs (tractors, other machinery, seeds and fertilizer), milling machines, water pumps, seed stores and cereal banking, and draft animals. For a detailed discussion of sub-projects in all participating rural villages, see Heß et al. (2020).

3 Conceptual Framework

Our hypothesis is that informal economic transactions as well as social interactions within villages are affected by CDD programs. In this section we lay out a conceptual framework based on a simple model that focuses on three key channels through which CDD programs may affect interactions. We then discuss the empirical predictions of this model.

Consider the following model setup, based on Jackson et al. (2012). Two players interact in an infinitely repeated game where gift-exchange links can be maintained and gifts are made and consumed. The intertemporal discount factor is $\delta \in (0,1)$.

Initially, players are connected with a gift-exchange link—an informal agreement to give gifts when the opportunity arises. In each period, there is a chance that a player has something to give away. To this we refer as *gift opportunity*, which happens to each player $i \in \{1,2\}$ with probability p_i, independent of whether a link exists. Following Jackson et al. (2012), for simplicity we assume that a gift opportunity never arises for both players in the same period. Players meet in a given period with probability m. Thus, m can be thought of as a village-level component of social capital that is exogenous to the model, and for any given period mp_i denotes the probability that a gift opportunity for player i arises and players meet. If a gift opportunity arises and players meet and a link exists, the player with the gift opportunity can choose between making the gift to the other player at utility costs c and not making the gift. If the gift is made, the other player gains utility value ν. If the player refuses to make a gift, the link is dissolved. We follow Jackson et al. (2012) in assuming that links cannot be reestablished (see the discussion therein). Further, there is no alternative formal saving or insurance mechanism available to transfer utility between periods.

If $\nu > c$, an equilibrium path where all gifts are made is in expectation beneficial for both players. However, when a player i actually faces the decision whether to make a gift to the other player $-i$, at costs c, doing so is beneficial to i only if the net benefits from maintaining the link for all future periods exceed today's costs, i.e., if:

$$c < \frac{\delta}{1-\delta}\left(mp_{-i}\nu - mp_i c\right). \tag{1}$$

If $p = p_i$ for $i = 1, 2$:

$$c < \frac{\delta}{1-\delta} mp(\nu - c). \tag{2}$$

Denoting the net life-time utility of giving a gift with U^{gift}, Equation (2) implies that giving the gift is beneficial for i if and only if

$$U^{\text{gift}} = \frac{\delta}{1-\delta} mp\,(\nu - c) - c > 0 \tag{3}$$

Equipped with this basic framework, we turn to an analysis of three different channels through which the implementation of the CDD program may affect the incentives to maintain links. We explain how the CDD program may affect model parameters and study how changes in these parameters affect the households' utility from keeping links. For channel (iii) we extend the model to derive some properties of equilibrium networks in societies with $N > 2$ players.

[4] For a further illustration of the magnitude of the shock, see Supplementary Material A.2.

(i) Frequent Social Interactions During Project Choice and Implementation

The CDD program induces additional social interactions. In our model, this mechanism implies exogenous changes to m.

We expect a direct effect on social interactions, because CDD programs require individuals to collaborate and interact frequently, both during project choice and during the implementation and management of the projects.[5] Additionally, there could be an indirect effect on social interactions, because more frequent interactions due to the project may affect social capital and meeting frequency beyond the immediate project (Feigenberg et al., 2013).

Translated into our model, these two arguments imply an exogenous medium-run increase in the probability that two players meet, m. Equation (3) implies that maintaining links becomes more beneficial as m increases.

Result 1: Positive changes in meeting frequency increase incentives to keep a link.

On the other hand, the frequency of social meetings might also go down in treatment villages. First, previous studies suggest that CDDs reduce participation in community-based organizations (CBOs) and contribution to other village activities (Labonne and Chase, 2011; Avdeenko and Gilligan, 2015). Secondly, conflicts may arise. We will discuss this latter possibility further in the context of channel (iii) below.

(ii) Positive Economic Change

The CDD program may increase income and market integration. In our model, this mechanism implies exogenously induced changes in ν and c.

The village-level projects in the Gambian CDD program are mostly intended to promote income-generating activities and bring substantial resources to the villages. The program also induces interactions with village outsiders and provides means of transportation and means for the marketing of goods. Both types of transformations can lead to a reduction of the attractiveness of gift-exchange links for villagers.

Income

To conceptualize the interplay of income and gift opportunities we provide a foundation for the (per-period) utility values ν and c that is based on consumption utility. Assume that players have a steady income stream of per-period income y and that utility from consumption in each period is $u(y)$, with $u'(y) > 0$, $u''(y) < 0$, $u'''(y) > 0$. Gift opportunities arise as small amounts of additional resources, $\lambda > 0$, that households gain at random. Essentially, this introduces income uncertainty. In this setup, we model making the gift as transferring a fixed amount $\gamma \in (0, \lambda)$ to the other household while keeping the rest, $\lambda - \gamma$. Then, value and costs of receiving and giving gift γ, at a certain income y, are:[6]

$$\begin{aligned} &\nu(y) := u(y + \gamma) - u(y) \\ \text{and} \qquad &c(y) := u(y + \lambda) - u(y + \lambda - \gamma) \end{aligned}$$

Households have a concave utility function, which ensures that $\nu > c$ if $\gamma < \lambda$. Further, note that we can express c in terms of ν as follows: $c(y)$=$\nu(y + \lambda - \gamma)$.[7]

With this extension, the net life-time utility of maintaining links, U^{gift}, is a function of income y. Differentiating with respect to y yields:

$$\frac{\partial U^{\text{gift}}}{\partial y} = \frac{\delta}{1-\delta} mp \underbrace{\left(\nu'(y) - \nu'(y + \lambda - \gamma)\right)}_{<0} - \underbrace{\nu'(y + \lambda - \gamma)}_{<0},$$

[5] The participatory process implies meetings to which all community members are invited, in which possible projects are considered and where finally a joint decision is made about which projects to implement with the CDD grant. As indicated above, the Gambian CDD program mandates up to 38 village-level and sub-committee activities. Further, decisions and committees are expected to be inclusive, including typically marginalized groups of the village into decision making and implementing committees, thus bringing together people who usually do not interact much. In addition, community members are required to contribute to the project implementation, and most contributions are made in kind, which often means jointly working on a project site.

[6] For simplicity we omit the fixed arguments λ and γ in the definitions of c and ν.

[7] $c(y) = u(y + \lambda) - u(y + \lambda - \gamma) = u(y + \lambda - \gamma + \gamma) - u(y + \lambda - \gamma) = \nu(y + \lambda - \gamma)$

where the individual inequalities hold because $y + \lambda - \gamma > y$, and $\nu'(y)$ is negative and strictly increasing in y.[8] For sufficiently patient individuals, $\frac{\delta}{1-\delta}$ is large enough so that the sign of the derivative is determined by the first expression and thus $\frac{\partial U^{\text{gift}}}{\partial y} < 0$.

To understand the intuition behind this result, note that in the face of the uncertainty that is due to the random arrival of additional resources λ, gift-exchange provides risk-averse agents with a way to share risk. When deciding on making a gift, the household trades off today's costs against the present value of sharing risk in the future. If households are sufficiently patient (δ is close to 1) this trade off is dominated by the value of future risk sharing, which is decreasing in income, due to the concave utility function.

Result 2a: For patient individuals, an increase in income decreases incentives to keep a link.

Market integration

Above, gift opportunities are modeled as a "small amount of additional resources" that is transferable and has the same consumption value to both households. In our setting, gifts are often in-kind items, such as food, clothes, seeds, or fertilizer. If markets are not well developed, the two players' valuation of such items may be asymmetric. One household may not need particular items that are useful to the other, such as excess seeds, fertilizer or agricultural tools that are not used to capacity. With weakly developed goods markets, items are not easily monetized, and it is easy to see that asymmetric valuations can result in gift opportunities where $\nu > c$. However, if markets develop for these items to be bought and sold, the assumption that they have a comparable (monetary) value to both players is justified, because excess goods can be sold for cash. Then, the (monetary) opportunity costs of making a gift and the value of receiving it correspond to the market price of the item.

Thus, increased market integration is another way in which the CDD program may alter transactions. In terms of the model outlined above, goods market frictions imply the possibility of a larger difference between costs and benefits, $\nu - c$. Increased market integration reduces this net benefit of gift exchange. Consequently, a move to markets may decrease the frequency of gift-exchange links.[9]

Result 2b: Increased market integration decreases incentives to keep a link.

(iii) Unequally Distributed Benefits, Elite Capture, and Favoritism

The CDD leaves some villagers more powerful and provides opportunities for these households to capture benefits from the program. More generally, not all households may benefit equally from projects. In our model, this implies asymmetric changes in ν, c, and p. Further, disputes resulting from elite capture may reduce social interactions and affect meeting probability m.

Although decisions are supposed to be made jointly, certain groups could steer decisions to projects from which they reap larger benefits. In fact, the focus on grassroots decisions increases the potential for elite capture (Platteau, 2004; Olken, 2007; Alatas et al., 2019).[10] Local administration of projects also entails the risk of favoritism, i.e., agents who are responsible for delivering projects may favor people in their social network (Bandiera et al., 2018).

If, as a result, benefits are unequally distributed, this can reduce links through the mechanisms in our model for a number of reasons. First, the arguments put forward under channel (ii) also apply if only one household becomes wealthier or gains better market access. Thus, unequal benefits resulting in higher income or better market access for some households will reduce those households' incentive to participate in gift-exchange. Second, if unequal benefits increase the frequency of generating gift opportunities, p_i, relative to p_{-i}, the expected future costs of maintaining a gift-exchange relationship increase unilaterally for household i (see Equation (1)). In these cases, the household's net utility gain from maintaining a link decreases, implying that the link may not be sustained even if the other household would still gain as much or even more from gift exchange.

Third, unequally distributed benefits may lead to disputes. Especially if the unequal distribution of benefits is due to a project being captured by traditional elites or the members of the VDC, this

[8] $\nu'' = u''(y + \lambda - \gamma) - u''(y) > 0$, because $u'''(y) > 0$.

[9] Note further that the effect may be compounded by the effects of the externalities that exist if some individuals start switching to markets that are highlighted in the models by Kranton (1996); Ishiguro (2016); Gagnon and Goyal (2017).

[10] From other CDD programs there is evidence that "people who benefit tend to be the most literate, [...] and the most connected to wealthy and powerful people" (Mansuri and Rao, 2012, p. 6). Gugerty and Kremer (2008) show that the availability of additional resources through development programs changes the composition of community groups, attracting the better-off and weakening the role of the disadvantaged in these groups.

might lead to conflict and alienate other community members (see, for example, Barron et al., 2011).[11] Further, differences in benefits between groups and lower-than-expected benefits may induce conflict over the benefits' distribution (Grossman, 1992; Dube and Vargas, 2013; Crost et al., 2014; Nunn and Qian, 2014; Ray and Esteban, 2017).[12] Generally speaking, dissatisfaction with procedures and outcomes in participatory programs might result in internal disputes and social disruptions (Barron et al., 2011; Lund and Saito-Jensen, 2013). If disputes lead to fewer social interactions, thus reducing m, the opposite of the effect discussed in channel (i) may occur, resulting in fewer links.

> **Result 3a:** If benefits are unequally distributed, such that p_i or y_i increase for one household, but not the other, a gift-exchange link is less sustainable. If disputes arise as a consequence of unequal benefits, this may additionally decrease m, further reducing the incentive to keep a link.

In the above two-player scenario effects on links are always symmetric, i.e., both households end up with fewer links. To understand how unequally distributed benefits, favoritism, and elite capture can have heterogeneous impacts, it is necessary to consider more than two individuals. This is a modified version of the n-player model by Jackson et al. (2012). Deviating from the favor-exchange model by Jackson et al. (2012), we assume that the frequency of gift opportunities is independent from the network structure. This captures the idea that luck or excess goods occur at the household-level, so it is an appropriate modification for the context of gift-exchange.

Each period can be summarized as follows:

1. A gift opportunity arises for some i, or the next period starts.
2. Among i's links, a random j is met with probability m.
3. i decides whether or not to make the gift to j.
4. If the gift is given, i incurs costs c and j gains value ν.
5. All players have the opportunity to cut links, based on gifts that were made or refused.

We proceed by a characterization of the number of links households can maintain in networks that are enforceable as equilibria when all villagers play a grim-trigger strategy, i.e., everyone cuts any link to someone who refuses to provide a gift (ostracization). Following Jackson et al. (2012), we abstract from the process of network evolution and do not model how links are created or severed, but focus on characterizing properties of self-sustaining network structures.

We denote the set of households that i is linked to as N_i and the number of these households as $d_i = |N_i|$. With an ostracizing grim-trigger strategy, any network in which the costs of making a gift are less than the costs of complete ostracization can be sustained as equilibrium, i.e., any network such that for all i:

$$\text{staying connected} \succ \text{getting ostracized} \qquad \Leftrightarrow \qquad c < \frac{\delta}{1-\delta} m \left(\nu \sum_{j \in N_i} \frac{p_j}{d_j} - c p_i \right)$$

We observe a fundamental trade-off in this inequality. On one hand, for each i, avoiding ostracization becomes more attractive the more friends i has (through N_i). More friends implies receiving more gifts. On the other hand, staying connected becomes less attractive the more other links their friends have (through d_j in the denominator). This reflects the fact that only one of j's ties can receive the gift.

Further, the value of ties to other households is increasing in the probability of those other households to generate gift opportunities. Thus, i is willing to make gifts even when the other household's ties have a higher d_j, provided this household also has a sufficiently high p_j. That is, in equilibrium households with a high probability of generating gift opportunities are able to sustain more links. Lastly, getting ostracized becomes a less deterring threat as a household's own p_i increases, as costs c would have to be incurred more frequently if the links are kept.

> **Result 3b:** If benefits are unequally distributed, such that households are heterogeneous in their probability of generating gift opportunities, p, those with a higher p can sustain a larger number of gift-exchange links in equilibrium, but have lower incentives (higher costs) to stay connected at all.

Elite capture (e.g., through VDC members) can be thought of as a higher probability of generating gift opportunities, p_i. We thus expect a reduction in links especially among non-VDC members. A numeric

[11] On the flip-side, provisions against elite capture might be considered a threat by traditional authorities and lead to social tensions (Barakat, 2006; Morel et al., 2009; King and Samii, 2014; White et al., 2018).

[12] For the CDD program in Sierra Leone that is in many ways comparable to that in The Gambia, Casey et al. (2012) find that the program has increased conflicts over loans. The negative effect of a CDD in Morocco on trust found in Nguyen and Rieger (2017) also hints at conflicts in CDDs.

example of this effect is shown in Supplementary Material B. This example also illustrates that the effects of the CDD program on interactions in this model can go beyond the directly affected households, affecting links with and between households who did not benefit personally from the program. Thus, the findings complement a similar result in Banerjee et al. (2019).

Empirically Investigating the Channels

All three channels imply changes for the networks of transfer partners, which we consider a proxy for the gift-exchange links of our model. The sign of the net effect is ambiguous. Channel (i) predicts an overall increase in gift-exchange links, while the other channels imply a decrease. Thus, in our empirical analysis, we first study the net effect, i.e., the average treatment effect on the existence of links between households.[13] Beyond the average treatment effect we investigate the three channels as follows.

To investigate channel (i), we test whether social contacts, post-CDD meeting attendance, and membership in village groups are larger in treatment than in control villages. Further, we identify groups that by design are involved in the CDD-related activities, namely members of the VDC and marginalized households, and test whether these became more socially connected.

To investigate channel (ii), we first test for increases in proxies for household wealth and income in response to the program using our data and census data. We also test whether consumption-related transactions are more affected than production-related transactions. Since the CDD brings production-related items, effects on the consumption-related transfer networks are an indication that the treatment effects go beyond a crowding out of private transfers by the program provisions. Further, we test if transactions with households external to the village increase, and whether socially embedded transactions within the village decrease—which we consider proxies for increased market orientation and formality of the transfers.

Channel (iii) has two components: disputes that lower social capital overall, and heterogeneity in the attractiveness of link partners that affects the centrality of some households. Both components are based on unequal benefits. Thus, to investigate channel (iii), we first test for heterogeneity in benefits, using our own data as well as census data, and identify characteristics that are associated with higher benefits. We then test whether these heterogeneous benefits are related to changes in transfers in general, and specifically between groups of households who (likely) benefited from the project to different degrees.

4 Data and Empirical Strategy

4.1 Village Selection and Pre-Treatment Balance

For this study, we use a sample of 56 villages, drawn from the set of rural villages that were eligible for the CDD program and had a population between 300 and 1,000 inhabitants in 2003. The population restriction ensures a relatively homogenous sample of rural villages and made it feasible to conduct full village censuses. Supplementary Material C provides further details on the sampling.

Villages in the sample are very poor. Data from the Census 2003 (i.e., before the CDD program was implemented) show that households mostly did not have access to electricity or an improved water source (Appendix Table 10). Less than two thirds of all individuals were literate, of which many possessed only basic reading skills but could not write. In 2003, the average village population was roughly 500 inhabitants, but the size of most villages has increased in the years since. In our data the average village has roughly 670 inhabitants and villages have on average 50 households (Appendix Table 10).

Because our main specifications use only post-treatment data, it is important that treatment and control group are comparable prior to the CDD project. Appendix Table 10 provides evidence for the balance of village and household characteristics between treatment and control villages in our sample, based on data from the Gambian Census 2003 (Panels A and B) and our own data (Panel C). Additionally, we construct variables based on the Census that are proxies for social networks (Panel D). Also among those variables, there is no evidence for imbalances. Only one of the 25 variables in Appendix Table 10 exhibits a statistically significant difference between the treatment and the control group.[14]

[13] To study gift-exchange links, we use our data on transfers. Since we capture transfers in six separate networks over the course of a full year, and capture even small transfers, it is reasonable to assume that observed transfers are good proxies for gift-exchange links.

[14] This variable is an indicator for formal education, which we control for in all regressions. Appendix Table 12 shows that our treatment effect estimate remains qualitatively unchanged if we exclude this variable from the controls. All other estimation results in this paper are also qualitatively robust to excluding this control variable.

4.2 Data Collection

The principal data used in this paper were collected between April and June of 2014 at two levels. First, we collected data in the *main survey*—a full census of all households—covering basic demographics and the network data. Additionally, an *in-depth survey* was conducted with ten randomly sampled households per village. We complement these data with two national censuses collected by the Gambia Bureau of Statistics in 2003 and 2013 and with administrative data from the CDD program.

The full village censuses were carried out through household head gatherings co-organized with the village chief.[15] Networks were elicited using a name generator procedure (Campbell and Lee, 1991): Respondents were asked to name villagers with whom they had transactions—within the last year—of: (i) *Land*; (ii) *Labor*; (iii) other *Inputs* (such as tools, seeds, fertilizer, and others); (iv) agricultural outputs (*Food*); (v) *Gifts*; or (vi) *Credit*. On top of these economic networks, the interviews also collected information about social networks created by: (vii) *Kinship* (first-degree relatives and children's in-laws); and (viii) *Friendship* (which we measured with information about gatherings to drink green tea, *Attaya*). For all these domains information about interactions with village outsiders were recorded as well.[16]

For each transaction we recorded the direction (alongside further specifics, such as quantities, payments, type alongsideof good, etc.) and treat the network of transfers as a directed graph.[17] Given the directed nature of our survey questions, a household declaring a transaction can be either the source or the recipient of the transaction. To construct the dyadic data we aggregate the two potential reports of each directed transaction by both households in a dyad: We record a transaction if at least one of the two households declared a transaction in a given direction.[18]

The six economic domains have possible conceptual overlaps, which we were careful to clarify during the interviews. More details on the data collection are provided in Supplementary Material C.3. However, for most of our analysis we consider the aggregation of the six domains into a single union network, which discards the distinction between the six domains. Appendix Table 11 shows summary statistics for our network data. Across all economic networks, the average household in the control group lists 5.7 transfer partners. Transfers of *Food* were the most common, with an average of 1.9 transfer partners per household.

Aside from collecting wealth indicators, we used the *in-depth survey* to collect detailed data on development projects. We asked respondents to name village-level development projects (not only CDD) that they are aware of, to provide details about the implementation process, and to rate how they benefited from them.

4.3 Empirical Strategy

Empirical specification

In our main empirical specifications, we consider each household as a node i in the network of economic transactions. To study the effect of the CDD program on the existence of an economic transaction between two households, we follow the literature on dyadic regressions (Fafchamps and Gubert, 2007) using the following empirical specification:

$$\ell_{ijvw} = D_{vw}\tau + X_{ijvw}\alpha + \beta_w + \varepsilon_{ijvw}, \tag{4}$$

where the dependent variable ℓ_{ijvw} indicates existence of a transaction from household i to household j in village v of ward w and takes on the value 100 whenever there is a transaction, 0 otherwise, so that coefficients can be interpreted as percentage points. The average treatment effect (ATE) is captured by the coefficient, τ, of the village treatment indicator, D_{vw}, which takes the value one if village v was a

[15] Our working definition of households is discussed in Supplementary Material C.

[16] We did not impose a strict limit on the number of reported transactions. However, in the case of the *Food* network—where small transactions are very frequent—enumerators were instructed to list the four most important partners, focusing only on transactions approximately equivalent to the amount that would make up a meal for the household. The censoring was of negligible practical relevance as 97.6% of households reported three or fewer *Food*-exchange partners.

[17] While some involved payment, we treat all transfers as a measure for informal transactions. Payments are often symbolic and transactions involving payment are not necessarily market transactions. The fact that for the *Food* network, where payment is most common, 60% of payments were in kind suggests that paid transactions are not market purchases. For *Labor* and *Inputs*, where only few transactions involve any payment, still 12-15% are in kind. *Land* transfers and, by design, *Gifts* and *Credit*, involved virtually no payments.

[18] This procedure implicitly assumes that each transaction that is reported by only one household exists but was omitted by the other household. Non-overlapping responses to network questions are the norm rather than the exception, see, e.g., Comola and Fafchamps (2014) and the references therein.

CDD village. In all dyadic specifications we include ward-level fixed effects, β_w, as well as dyad-level control variables, X_{ijvw}. In addition to the control variables listed in Appendix Table 10, panels A and B, we control for the existence of kinship ties, shared ethnicity, interview group and enumerator fixed effects.

For ease of interpretation, Equation (4) is specified as a linear probability model and estimated using weighted OLS.[19] Our main results are unchanged if a probit model is estimated instead. Further, our results are equally confirmed through randomization inference, which does not rely on regression model assumptions, as explained in greater detail below. Further regressions at other levels of aggregation (e.g., household-level or village-level dependent variables) use analogous empirical setups and are always estimated using weighted OLS to account for the aggregation.

Identifying village sub-groups

For our analysis of heterogeneous effects, we define groups of traditional leaders and marginalized households, and further obtain a proxy for households likely involved in the CDD sub-projects' implementation. We can rely on exogenous positions to define traditional leaders in our data (as the chief's family and the religious leader) and use program-related documents to define marginalized households (as those headed by a woman or someone less than 35 years old).[20] In addition, the CDD increases the influence of another group of households, namely the members of the Village Development Committee (VDC). The VDC was tasked with identifying economic priorities, developing plans, and managing the financial resources, as well as the actual implementation of the sub-projects at the village level (World Bank, 2006). Decisions were supposed to be made in consultation with the community, but members of the VDC—at least in principle—had the power to steer projects, to influence decision making, and control the CDD funds. To estimate heterogeneous impacts for this group, we need to identify comparable households in control villages. Although the Local Government Act (2002) required the creation of VDCs in all villages, the increased importance of the VDC, and the enforcement of inclusiveness criteria resulted in differential selection into the VDC across treatment and control villages.[21] Therefore, we cannot use actual VDC membership in our analysis. Instead, we use a random forest model (Breiman, 2001) to classify households consistently in treatment and control villages. In particular, for each household in both treatment and control villages we estimate a probability of being a VDC member if the village were in fact a treatment village, using a number of fixed household characteristics that capture social embeddedness in the village, education, ethnicity, age and wealth (based on land, which can be considered exogenous to the program). This allows us to estimate treatment heterogeneity with respect to VDC membership. Our approach is inspired by Banerjee et al. (2019), who predict microfinance take-up. More details are given in Supplementary Material D. For an easier interpretation of the estimates, we rescale the measure from the random forest to have mean zero and variance one. Below, we refer to this measure as VDC−score. In dyadic specifications we also use a combined measure, where appropriate, capturing the probability that one or both households would belong to the VDC. Because VDC membership is not derived directly from the survey, in some of the analyses we keep this measure separate from the other two dimensions of interest, traditional leaders and marginalized households, to show that theses results are not affected by the inclusion of our measure for VDC involvement.

Inference Since treatment was assigned at the village level, stratified by ward, our statistical inference has to account for the intra-village correlation of regression model errors. Our main specifications rely on cluster-robust standard errors, allowing for clustering at the village level and ward fixed effects to account for the stratification (see Bruhn and McKenzie, 2009; Bugni et al., 2018). Additional issues potentially result from the relatively small number of treatment clusters (56) combined with heterogeneous village size (see MacKinnon and Webb, 2017). Thus the standard approach, using cluster-robust standard errors, is potentially problematic, and we additionally rely on randomization inference (see Fisher, 1935;

[19] Regression weights are used to ensure comparability across specifications. The units of observation vary between the dyad, household, village, and project report level. We weight observations to ensure proportionality to village size, i.e., in dyadic regressions the inverse of the number of households is used as weights.

[20] Note that the office of the chief is inherited, i.e., exogenous to the CDD implementation process. According to the World Bank, the Gambian CDD program was designed with a focus on "inclusion, particularly of women and youth, in decision making and access to resources [. . .] throughout all stages of [the] project" (World Bank, 2006, p. 14). Following this, we consider households headed by a female or young head (35 years or younger) as marginalized. These categories are not mutually exclusive. Some female or young household heads are also relatives of the chief. 14% of households classified as elite households are also classified as marginalized.

[21] Supplement Table 1 shows that VDC members in CDD communities are significantly older, more educated, and more likely to be of an ethnic minority, or of a household who owns no land, than in control villages.

Rosenbaum, 2002) to test the significance of causal effects of the randomized treatment. All regressions where randomization inference is applicable indicate significance based on randomization inference as well as cluster-robust standard errors. Details are provided in Supplementary Material E.

5 Effect of the Community-Driven Development Program on Economic Networks

5.1 Average Treatment Effect

Our main results concern the average treatment effect of the Gambian CDD program on the networks of economic transactions in rural Gambia. We use the dyadic specification described in Equation (4). The estimates shown in Table 1, column 1, are based on the network of informal economic transactions that is the union of the six economic domains for which we collected data. Column 1 shows that the effect of the program is a large and statistically significant reduction in the probability of the existence of an economic transaction. The point estimate for the average treatment effect implies a reduction of 1.133 percentage points in treatment villages. Considering the mean in the control group, this implies that the probability of two households having an economic transaction is 16.4% lower in treated villages. Based on cluster-robust standard errors, this effect is significant at 1% with p-value=0.004, while when using randomization inference the p-value is 0.03, i.e., the significance level is 5% (the significance levels are jointly indicated by ●●⊛, see the table notes for further details). Splitting the data on transactions allows us to study separately a consumption network (transactions in the food domain and gifts as well as credit transactions) and a production network (transactions of land, labor and inputs). The results in columns 2 and 3 show that the average treatment effect observed for the union of all domains is driven by consumption-related informal transactions.[22]

5.2 Robustness of the Main Findings

The above results are robust to a number of alternative specifications, which can be found in the Appendix, namely: (i) using the post double-LASSO (Belloni et al., 2014) as a data-driven way to select control variables, to address concerns about selectively included control variables (Appendix Table 12, column 1); (ii) excluding the indicator for formal education from the control variables, because education is unbalanced and a possible confounding factor (column 2); (iii) excluding either one or both wards with unequal numbers of treatment and control villages to ensure that results are not driven by imbalances in these wards (columns 3-5, see also Supplementary Material C.1); (iv) using probit instead of a linear probability model to account for the binary nature of the dependent variable (column 6); (v) using transaction intensity instead of a binary indicator, to test if the reduction of transactions is made up for by increased transaction volumes (column 7); (vi) using only transactions that did not involve cash or in-kind payments, since transactions involving payments could be regarded as market-based (column 8); and (vii) treating the network as an undirected graph, to address concerns about the mismeasurement of the direction of flows (column 9).

The reduction in economic interactions as a result of the program is also confirmed when we aggregate the network data to analyze degree centrality at the household level as well as related network measures at the village levels (see Supplementary Material F.2).

One limitation of our analysis is that we do not have economic network data from before the intervention. This, together with a relatively small sample size, may raise concerns about spurious correlations. We address these concerns in three ways. First, in light of a relatively small number of villages and wards, we take particular care of issues of inference by using randomization inference. Second, while we do not have pre-treatment data on economic networks, we have a large number of network proxies for our sample villages from different sources, which we use to provide evidence on balance (Appendix Tables 10 and 11). These network proxies include compound size, the number of spouses hailing from the same village, geography, and kinship networks, which we consider static (see Appendix A). We find no evidence for differences between treatment and control village along any of these dimensions. Third, we have data on economic transactions collected through the early stages of the program (in 2009). Because of the timing of the data collection, which occurred when project implementation had already started

[22] Supplement Table 2 shows the results for each of the six economic networks individually. Five out of the six estimated coefficients are negative and in two cases this effect is significant at the 1% level. The treatment effect estimate in the *Credit* network is statistically significant at the 10% level. The single positive point estimate is small relative to the control group mean and has a large p-value of 0.87.

Table 1: Main Results: Treatment Effects on Transactions and Treatment Effect Heterogeneity

	(1) any transaction	(2) consumption transaction	(3) production transaction	(4) any transaction	(5) any transaction
treatment	-1.133	-1.191	-0.140	-1.304	-1.300
	(0.004)$^{\bullet\bullet\circledast}$	(0.000)$^{\bullet\bullet\bullet}$	(0.676)	(0.010)$^{\bullet\bullet\circledast}$	(0.008)$^{\bullet\bullet\circledast}$
trad. leader^{any}	1.980	0.897	1.226	1.813	1.922
	(0.000)***	(0.000)***	(0.000)***	(0.001)***	(0.000)***
marginalized^{any}	-0.714	-0.281	-0.421	-0.841	-0.902
	(0.004)***	(0.058)*	(0.038)**	(0.023)**	(0.016)**
VDC$^{\text{any}}-$score					-0.321
					(0.134)
treatment × trad. leader^{any}				0.317	0.094
				(0.695)	(0.902)
treatment × marginalized^{any}				0.254	0.410
				(0.565)	(0.351)
treatment × VDC$^{\text{any}}-$score					0.556
					(0.056)$^{\bullet}$
controls	✓	✓	✓	✓	✓
dyads	151632	151632	151632	151632	151632
households	2774	2774	2774	2774	2774
control mean dep. var.	6.9	3.5	4.0	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. Where bullets are shown, randomization inference was used to compute p-values: filled bullets $\bullet$ indicate significance levels with randomization inference; starred bullets $\circledast$ indicate significance levels that are only sustained by the cluster-robust standard errors. Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. "Consumption transactions" combine transfers of food, gifts, and credits. "Production transactions" combine transfers of land, labor, and inputs. Regressions control for ward fixed effects and a set of control variables: the village-level variables in Appendix Table 10, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table 10, as well as ethnicity and enumerator dummies enter the regressions twice, for the sending and the receiving household of a dyad. The variables *trad. leader*^{any} and *marginalized*^{any} indicate whether any of the two households in a dyad belongs to the traditional village leaders or the group of marginalized households. The variable VDC$^{\text{any}}-$score measures the likelihood that a dyad involves a household who would be in the VDC if the village was a CDD village.

in some villages, these can not be considered clean baseline data (see Supplementary Material F.3 for a detailed discussion). There are also differences in how these data were collected. Nevertheless, we explore these data to investigate robustness of our results (Supplementary Material F.3) and find results that are comparable to our main result when using a Difference-in-Differences estimator or when controlling for the lagged dependent variable. In addition, there is no significant difference between treatment and control villages in 2009.

We discuss another possible concern, related to the fact that data were collected 4-5 years after the end of the program, in Supplementary Material F.4. While the potential to study medium-term effects of the CDD is a strength of our data, the long gap increases the chance that control villages received some kind of "compensation". We provide evidence based on additional data, including informationon about other programs, which supports neither the assertion that compensation may be responsible for the observed findings nor that it occurred at all at a meaningful magnitude.

Heterogeneous Treatment Effects: Traditional Elites, Marginalized Households, and the Village Development Committee

Table 1 also provides information about the economic connectedness of different groups of households. We first demonstrate heterogeneity in economic connectedness in the absence of treatment, highlighting through network analysis the existence of elite and marginalized groups. We then investigate heterogeneous treatment effects.

We focus on three groups. Two of these groups typically receive special consideration in CDD programs, including in the Gambian CDD program, namely traditional village leaders and marginalized households.[23] A third group gains significance through the CDD program, namely the members of the VDC. For treatment villages, membership in the VDC entails a significant role in the selection and administration of the village's CDD sub-project(s). To obtain a proxy for VDC membership that is comparable between treatment and control villages, we compute a VDC−score through a random forest model based on fixed household characteristics (introduced in Section 4.3 and explained in more detail in Supplementary Material D).

We first focus on the two groups we can directly identify based on ex-ante criteria, namely traditional village leaders (the village chief and his first-degree relatives as well as the Imam), and the group of marginalized (young and female-headed) households.[24] We conjecture that traditional leaders are well connected because they head the most established families in a village, control a large share of the village's private agricultural landholdings as well as the village's commons, and are often of above-average wealth. Marginalized households are expected to be less connected because they tend to be poorer, less involved in the village's productive activities, and because age and gender are important factors for social interactions in The Gambia. Indeed, columns 1-3 confirm that traditional leader households are significantly more connected than other households. For marginalized households, the opposite holds.

Regarding the program's heterogeneous effects on economic networks, there is no indication of heterogeneous effects with respect to traditional leaders and marginalized groups, and the average reduction in economic interactions is not driven by diminishing transactions with these groups (column 4 of Table 1). Under the premise that the economic interactions can be used more generally as a proxy for the position of households within the village economy, these results suggest that the CDD program has not achieved its goal of integrating marginalized groups such as female- and youth-headed households.

To study the effect of membership in the VDC, we rely on the variable VDC^{any}−score, which captures the probability that any household in a dyad belongs to the VDC in case the village was treated.[25] Column 5 suggests that, in control villages, the probability that any household in a dyad is a VDC member is not associated with informal economic transactions. However, the coefficient estimate for the interaction of treatment and the variable VDC^{any}−score is positive and significant. This shows that likely VDC households are not necessarily very different in terms of connectedness ex-ante, but in CDD treatment villages they are more connected with other households in the village after the program. The results regarding traditional leaders and marginalized households are unaffected by the introduction of the VDC-related variables.

Taken together, the results show that, independent of treatment, there is significant within-village heterogeneity in connectedness, and the role of traditional elites and marginalized groups is mirrored in differences in the number of informal economic transactions that take place. Further, in treatment villages likely VDC member households are significantly better connected than the average household. This hints at the possibility that the creation of new elites (VDC members) may have had effects that go beyond the intended role (i.e., helping to implement projects). This includes the possibility that VDC members may have captured the program with the goal of deriving disproportionate benefits for themselves or for households that they favor (e.g., their own kin). We investigate this further in later sections.

6 Mechanisms

In this section, we discuss evidence related to the three channels described in Section 3.

[23]The program documents highlight the specific attention paid to these groups. However, unlike in other programs (e.g., Alatas et al., 2012) the CDD program does not choose beneficiaries within villages, e.g., the poorest. In particular, the CDD does not involve community targeting.

[24]Traditional leaders and marginalized households are explicitly named in the administrative documents. The traditional chief was only given an advisory role in the VDC (Local Government Act, 2002, §93.1), while gender balance was specifically imposed for all its functions, and the inclusion of youth representatives was explicitly demanded. Additionally, the program implementation guidelines emphasize the importance of reducing the risk of elite capture and empowering women and youths through supporting and enforcing the establishment of VDCs following these rules (see, e.g., World Bank 2006, p. 4, or GoTG 2006, p. 9).

[25]The households' probabilities for VDC membership $\Pr_{\text{RF}}[\text{VDC}_i]$ are predicted at the household level, using our random forest predictor for both households i and j in a dyad. We then calculate VDC^{any}−score as $1 - (1 - \Pr_{\text{RF}}[\text{VDC}_i])(1 - \Pr_{\text{RF}}[\text{VDC}_j])$ and rescale the values to have mean zero and variance one, so that all other coefficients can be interpreted as treatment effects at the average.

6.1 Channel (i) – Frequent Social Interactions

Frequent social interactions during project choice, implementation, and maintenance may affect social interactions beyond the project. Our model predicts that these would in turn translate into more gift-exchange links and thus more transfers. However, the results in the previous section showed that there are in fact less transfers in treatment villages. Thus, to investigate the program's effect on social interactions, we measure interactions by asking each household head to name other households with whom the respondent gets together to drink *Attaya* tea. Discussions with our enumerators and pre-tests suggested that the networks of individuals who drink *Attaya* together would constitute a fairly objectively measurable proxy for the network of friends and people who might be able to help in times of need. Table 2, columns 1 and 3 show results using the dyadic nature of this data. Column 1 shows a statistically significant and sizable negative treatment effect of about 21% of the control mean. The result in column 1 also mirrors results of our analysis of economic transfer networks for the two groups that we analyzed separately before, namely households belonging to the traditional elite and marginalized households. Traditional elites are more likely to be connected in this network as well, while marginalized households are again more isolated, confirming priors regarding social life in these villages. Column 3 investigates treatment heterogeneity. Results suggest that the reduction of friendship interactions is larger for dyads involving marginalized households.

In the in-depth interviews, we also asked households more directly to identify their network of individuals who could help them out in times of need. Results based on these data are in columns 2 and 4. We again find that traditional leaders are more connected, while marginalized households have fewer help links. However, on average, the number of people in these help networks does not differ significantly between treatment and control villages.

Table 2: Friendship and Help Links, and Heterogeneity Tests

	dyadic regressions				household-level regressions			
	(1) friendship	(2) help link	(3) friendship	(4) help link	(5) meetings	(6) CBOs	(7) meetings	(8) CBOs
treatment	-0.867	-0.261	-0.699	0.017	-0.222	-0.312	0.282	-0.351
	(0.021)$^{\bullet\circledast}$	(0.466)	(0.126)	(0.969)	(0.590)	(0.010)$^{\bullet\bullet\circledast}$	(0.529)	(0.008)$^{\bullet\bullet\circledast}$
trad. leader^{any}	0.468	1.001	0.135	1.190	1.557	0.218	2.891	0.149
	(0.049)**	(0.003)***	(0.730)	(0.016)**	(0.048)**	(0.259)	(0.009)***	(0.571)
marginalized^{any}	-0.668	-0.496	-0.284	-0.216	-0.937	-0.049	-0.107	-0.047
	(0.002)***	(0.025)**	(0.380)	(0.432)	(0.040)**	(0.778)	(0.859)	(0.849)
VDC$^{\text{any}}-$score			0.017	-0.207			-0.609	0.065
			(0.948)	(0.325)			(0.052)*	(0.463)
treatment × marginalized^{any}			-0.693	-0.524			-1.823	0.083
			(0.098)$^{\circledast}$	(0.201)			(0.037)$^{\bullet\circledast}$	(0.799)
treatment × trad. leader^{any}			0.545	-0.379			-1.982	-0.020
			(0.325)	(0.572)			(0.192)	(0.958)
treatment × VDC$^{\text{any}}-$score			-0.013	0.357			0.619	0.232
			(0.969)	(0.254)			(0.195)	(0.085)$^{\circledast}$
controls	✓	✓	✓	✓	✓	✓	✓	✓
dyads	75816	48642	75816	48642	.	.	.	.
households	2774	2774	2774	2774	545	550	545	550
control mean dep. var.	4.4	3.2	4.4	3.2	5.8	3.3	5.8	3.3

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 Table 1). In columns 1-4, the units of observation are undirected (friendship) or directed (help) dyads. The dependent variable takes on the value 100 if a dyad had a link and 0 otherwise. Columns 5-8 show household-level regressions. The dependent variables *meetings* and *CBOs* count the number of village meetings the respondent household declares to have attended in the past year (2013/2014) and the number of community-based organizations household members participate in. For details on control variables and interacted variables see notes to Table 1. Columns 5-8 use the household-level analogue for these variables. Since help links are elicited in the in-depth survey with a random subsample of households, sample sizes are smaller, and regressions additionally control for a variable indicating whether one or both households were interviewed.

A more organized form of social interactions are village meetings and community-based organizations (CBOs). In the in-depth interviews, we collected information on meeting attendance and CBO membership. Columns 5 to 8 of Table 2 use these data. Again, we see the familiar pattern for traditional leaders, who attend more meetings, and marginalized households, who attend fewer meetings. With regard to treatment, we do not see a significant average treatment effect on meeting attendance, which is possibly explained by the fact that any negative effect of treatment on social capital is balanced by the possibility that some of the CDD projects still entail certain joint village activities and meetings for the management of the projects. For CBO membership, we find a significant negative treatment effect.

Overall, the observed reduction in friendship, the results regarding help networks, meeting attendance, and CBO membership do not support the hypothesis that more frequent interactions during the project lead to more frequent social interactions outside the project. In fact, the evidence points towards a reduction in friendship ties and social involvement in the village.

Based on the average treatment effect as well as the negative heterogeneous effects for marginalized households (columns 3 and 7), who by design were supposed to be more involved in joint activities, we reject the hypothesis that more frequent interactions due to the project lead to more social interactions of the type that we measure here. These negative effects warrant further attention and we come back to these findings in Section 6.3, where we study unequally distributed benefits and the role of elite capture and favoritism.

6.2 Channel (ii) – Positive Economic Change

Wealth Effects

We investigate wealth effects using various approaches based on data we collected as well as data from the Census 2013. The average wealth effects are moderately positive and we find some important heterogeneity.

We first use measures of self-assessed economic conditions and benefits from programs to shed light on possible economic effects of the CDD.[26] Table 3 shows that respondents in the in-depth survey in treatment villages are on average significantly more likely to state that the overall economic conditions in the village have improved in the last five years, i.e., since the beginning of the Gambian CDD program (column 1). However, they are not more likely to report an improvement in their own economic situation (column 2). In the larger sample, based on the main survey, households in CDD program villages are significantly more likely to report that they have benefited from a development project within the past five years (column 3). When we consider animals and household assets, we find no evidence for a positive average treatment effect of the CDD program in our sample (columns 4 and 5). Note that most mechanisms described in our conceptual framework do not require that there are strong (measurable) effects on average economic outcomes in our data. Indeed, the third channel works through economic changes that are small or absent for some households.

Table 3 also shows some evidence for heterogeneous treatment effects. The most consistent finding of the heterogeneity analysis is the positive effect of treatment on households that are likely VDC members. In treatment villages, these rate their own economic condition significantly better than the non-traditional leader, non-marginalized households, they also have significantly higher wealth (as measured by the wealth z-score).[27]

The divergence of results based on respondents' subjective assessment (which suggest positive effects) and survey-based wealth indicators (which does not show evidence of an average effect) may seem somewhat puzzling. Of course, it is possible that perceptions and actual outcomes indeed differ. A second possibility is that wealth and assets are relatively imprecisely measured in our data and the economic effects might be too small to be picked up with our data. For this latter reason, we turn to Census data, which allow us to perform the analysis on a much larger sample. The Census 2013 contains a section on household assets and animals. We combine the available information and calculate standardized z-scores, similar to what we do with our own data. We work with the sample of all villages in The Gambia that were eligible for the CDD program and meet the same size restrictions that were applied when our sample villages were selected (300-1000 inhabitants in 2003). Table 4 shows the results. Indeed, based on this larger sample (using more than 20,000 households in 316 villages), treatment effects on the z-scores are

[26] Self-assessed benefits from programs are subject to a number of caveats. In particular, McKenzie (2018) points out that business owners cannot predict their own counterfactuals very well. However, here we do not ask for the counterfactual "what would have been if", but we ask for "what has been". Further, more related to our later analysis of elite capture, note that the "incorrect" subjective assessments of own and others' benefits might in fact be the relevant determinant of village-internal disputes and personal grievances.

[27] In the dimensions studied in Table 3, columns 2 and 5, the effect estimate for the average household (i.e., VDC-scores=0) is negative, while the coefficient on the interaction of treatment and VDC-score is positive and significant. Yet, the treatment effect for likely VDC members is not only significantly different from other households, but also their estimated net treatment effect is positive already for moderate VDC-scores. The estimates imply positive treatment effects for moderately high VDC-scores (0.42 in column 2 and 0.66 in column 5).

Table 3: Economic Change: Our Data

	respondents' subjective assessment			wealth indicators	
	(1)	(2)	(3)	(4)	(5)
	overall econ. condition	own econ. condition	benefited from any project	z-score animals	z-score wealth
Panel A: average treatment effects					
treatment	0.151	0.017	0.131	-0.030	-0.146
	(0.029)$^{\bullet\circledast}$	(0.818)	(0.008)$^{\bullet\bullet\circledast}$	(0.598)	(0.121)
controls	✓	✓	✓	✓	✓
Panel B: heterogeneity					
treatment	0.215	-0.066	0.121	-0.034	-0.192
	(0.003)$^{\bullet\bullet\circledast}$	(0.427)	(0.016)$^{\bullet\circledast}$	(0.575)	(0.062)$^{\circledast}$
treatment × trad. leader	-0.323	0.235	0.069	-0.105	-0.091
	(0.230)	(0.455)	(0.221)	(0.480)	(0.695)
treatment × marginalized	-0.213	0.308	0.019	0.004	0.167
	(0.234)	(0.191)	(0.674)	(0.952)	(0.304)
treatment × VDC−score	0.070	0.157	0.023	0.001	0.292
	(0.364)	(0.028)$^{\bullet\bullet}$	(0.352)	(0.985)	(0.001)$^{\bullet\bullet\bullet}$
controls	✓	✓	✓	✓	✓
households	529	547	2767	2760	532
villages	56	56	56	56	56
control mean dep. var.	3.1	2.8	0.6	0.0	0.0
dep. var. range	1 - 5	1 - 5	0 - 1		

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 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 10 as well as ethnicity and enumerator dummies. Columns 1, 2, and 5 are based on the in-depth survey with 10 random respondents per village. For columns 1 and 2, respondents were asked to rate changes of their own and the village's overall economic condition during the past 5 years on a 5-point Likert-scale, where 1 is a deterioration, 3 is no change and 5 is an improvement. The dependent variable in column 3 is based on the question "Do you think that your household benefited from development projects implemented in the village in the last 5 years?" The z-score for animals in column 4 combines count variables for cattle and for other draught animals. The z-score in column 5 combines a large number of wealth indicators. Treatment effect estimates for individual indicators are found in Supplement Tables 9 to 11.

positive. While the magnitudes of the effects are similar for animals and assets (about 0.08-0.1 standard deviations), the estimates are statistically highly significant for the average treatment effect on animals (p-value=0.008) and marginally significant for assets (p-value=0.1).[28]

We further investigate treatment heterogeneity with the Census data. We cannot identify the same subgroups in the Census data as in our data. However, the Census data contain variables that can proxy for elite and marginalized positions within the village. In particular, established families (those that have a high share of adult household members that were born in the village) and those with higher education are more likely to be part of the traditional elite and the VDC. On the other hand, recent migrants to the village and individuals with low levels of education are more likely to be marginalized. Further, because of polygamy, household size can be seen as an indicator of wealth in the Gambian context. Based on these variables, we find treatment heterogeneity for effects on animal ownership. Established families and households with more educated members have significantly higher animal z-scores (column 2). Further, we use a random forest to identify likely VDC member households. The random forest model here is slightly different from the one referred to before, as we can only use variables that are both in our data as well as in the Census, to predict VDC member households in the Census (column 3). Likely VDC members have significantly higher z-scores for animals. We do not find significant treatment

[28] In a related paper (Heß et al., 2020), we study the effect of the CDD program on deforestation. Based on a larger sample, including almost all rural villages that are part of the CDD program, and additional data from the Census 2013 and the Gambian Integrated Household Survey 2015, we find complementary evidence consistent with modest positive treatment effects on wealth and livestock.

Table 4: Economic Change: Census Data

	animals			assets		
	(1)	(2)	(3)	(4)	(5)	(6)
	z-score	z-score	z-score	z-score	z-score	z-score
treatment	0.080	0.078	0.080	0.108	0.107	0.109
	(0.008)●●⊛	(0.007)●●⊛	(0.008)●●⊛	(0.100)⊛	(0.103)	(0.097)⊛
treatment × education		0.049			-0.017	
		(0.089)●			(0.787)	
treatment × established family		0.109			0.062	
		(0.028)●●			(0.552)	
treatment × household size		0.005			-0.001	
		(0.289)			(0.610)	
treatment × VDC−score			0.061			-0.033
			(0.004)●●●			(0.243)
controls	✓	✓	✓	✓	✓	✓
households	20504	20473	20504	20504	20473	20504
villages	316	316	316	316	316	316
control mean dep. var.	0.0	0.0	0.0	0.0	0.0	0.0

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 Table 1). The units of observation are households. The sample consists of households from all eligible rural villages with a population of 300-1000 inhabitants. Regressions control for ward fixed effects and a set of control variables: Village-level variables listed in Panel B of Appendix Table 10 as well as an ethnicity fixed effects, education, ethnic minority status, and household size. *education* is a binary indicator, indicating whether the household head ever attended school. *established family* is measured by the share of adult household members born in the village, and *household size* is the number of adult household members. 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. All other interacted variables are centered. The z-score for assets is based on four variables indicating radio, mobile, TV, and bicycle ownership. The z-score for animals is based on the number of owned ruminants, poultry, and cattle.

heterogeneity for the asset index.

In sum, the evidence leads us to conclude that there are some moderate positive economic effects. Interpreted jointly with the previous findings, suggesting that the reduction in informal economic transactions is largely due to consumption-related transactions, it appears plausible that positive economic change contributed to the reduction of economic transactions. This is in line with the model. We also find some heterogeneity of the effects. Households that have characteristics of elite households show significantly larger treatment effects for the animal ownership outcome, which we discuss more in Section 6.3 where we study unequally distributed benefits and the role of elite capture and favoritism. In addition to small sample sizes, another caveat should be mentioned: our focus was on the analysis of networks, not on identifying effects along wealth or income dimensions, which would have required more in-depth data collection related to income and/or consumption measures.[29]

Increased Market Orientation of the Village Economy

Tangible economic benefits and availability of more advanced means of production and transportation (milling machines, tractors, etc.) make market participation more likely, which—according to Kranton (1996)—weakens the system of personalized exchange and leads to a reduction in overall interactions, and in reciprocal interactions in particular, and a move to market-based activities can be expected. Even with the limited evidence for increases in wealth or asset indicators, formalization might occur for other reasons. Many of the CDD sub-projects are related to income generating activities, such as producing goods for sale (e.g., in the vegetable garden) and bringing goods to the market (e.g., using the tractor to transport firewood to the weekly market). Also, the CDD sub-projects' day-to-day operation often introduced some form of payment (e.g., paying the tractor driver or renting out the milling machine to outsiders) and the CDD program increased exposure to neighboring villages during joint meetings in the

[29]Casey et al. (2012) investigate effects of the CDD program in Sierra Leone on economic welfare based on 15 different outcomes and find statistically significant effects of treatment on the aggregate index as well as individual indicators (including a household asset score).

planning stage. As a result, the market orientation of the village economy, in the form of more formal transactions and a stronger orientation towards market activities, could increase.[30]

To test some aspects of the hypothesis of increased market orientation directly, we use our data on transactions with outsiders and whether these transactions involve a payment. Table 5, column 1 shows that households in treatment villages are indeed significantly more likely to have incoming transactions from outside the village. Further, while payment is not a sufficient condition to identify market transactions, a transformation to more market-based transactions would imply that more transactions involve payments. Our data allow us to identify transaction for which a payment was made. Indeed, we find that paid incoming transactions from village outsiders are also increasing (column 3), accounting for roughly a third of the overall increase in external transactions, while in control villages paid transactions constitute little more than one eighth of all external transactions. Thus, there is some evidence for more market-like transactions between buyers from CDD program villages and sellers from outside. On the other hand, there is no such evidence for the opposite direction, i.e., for more transactions with outsiders in which the CDD-village households are the sender (columns 4-6).

Table 5: Indications for Increased Market Orientation: Transactions with Outsiders

	in-degree			out-degree		
	(1) external	(2) external unpaid	(3) external paid	(4) external	(5) external unpaid	(6) external paid
treatment	0.176	0.118	0.058	−0.024	−0.030	0.005
	(0.006)$^{\bullet\bullet\circledast}$	(0.026)$^{\bullet\circledast}$	(0.072)$^{\circledast}$	(0.721)	(0.664)	(0.293)
controls	✓	✓	✓	✓	✓	✓
households	2774	2774	2774	2774	2774	2774
control mean dep. var.	1.1	0.9	0.1	0.7	0.7	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 Table 1). The units of observation are households. The dependent variable in columns 1-3 [4-6] counts incoming [outgoing] transactions. Transactions are considered "external" if a respondent replied "with a household from outside the village" to the questions used to elicit transactions. For details on control variables see notes to Table 3.

As village outsiders are not uniquely identified in our data, comparisons between the number of outside transactions to the village-internal degree have to be made with caution. Yet, comparing the magnitude of the effect here with the decrease in village internal in-degree (0.827), lets it appear unlikely that outside connections compensate for the reduction of internal transactions.

As a second way of studying whether market orientation increases in treatment villages, we analyze the effects the program had on two specific types of transactions: reciprocal transactions and socially embedded transactions. We hypothesize, based on Kranton's (1996) work, that reciprocal interactions are reduced when market-economic transactions are available. In order to test the CDD program's effect on reciprocal transactions, we take advantage of the fact that our data record the direction of economic transactions. With this information, we can define a variable that indicates if a particular transaction was reciprocated, meaning that a transaction has a counterpart in the opposite direction in any of the six economic networks. More precisely, we define reciprocity as an undirected dyadic binary variable: $\text{recip}_{ij} = \ell_{ji} \cdot \ell_{ij}$, where ℓ_{ji} is an indicator for transactions from i to j.[31]

In order to analyze if the CDD program had an effect on the reciprocity of the transactions, we estimate Equation (4) with recip_{ij} as the dependent variable. Indeed, consistent with the above hypothesis, the results in column 1 of Table 6 suggest that the CDD program causes a strong reduction in reciprocal transactions. The treatment coefficient is negative and statistically significant (p-value=0.005).[32]

A similar picture is drawn when analyzing the role that social proximity plays in network formation. In settings where enforceability is a concern, bilateral transactions are often facilitated by common ties, who

[30] In the CDD program in Sierra Leone the number of traders and the number of locally available goods for sale increased (Casey et al., 2012).

[31] A concern might be missmeasurement of flow directions. Supplementary Material C.3 discusses this.

[32] It is impossible to ascertain whether this reduction in reciprocal transactions is an independent effect from the overall reduction in transactions or a by-product. However, the magnitudes of the effect estimates suggest that reciprocal transactions are particularly prone to vanish in treatment communities: The average reduction in the probability of forming a reciprocal transaction corresponds to 29% of the control group mean. This suggests that reciprocal transfers are reduced at a much higher rate than other transfers.

Table 6: Treatment Effects on Reciprocity, and the Role of Support

	reciprocity	support
	(1) reciprocated econ. transaction	(2) any transaction
treatment	-0.585	-1.114
	(0.005)●●●	(0.004)●●●
support		0.520
		(0.001)***
treatment × support		-0.355
		(0.042)●●
controls	✓	✓
dyads	75816	151632
households	2774	2774
control mean dep. var.	2.0	6.9
mean support		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 Table 1). Units of observation are undirected (reciprocity) and directed (support) dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. A dyad is considered to have a reciprocated economic transaction if transactions in both directions occurred. *support* is a count variable capturing how many other households both households of the dyads are linked to, either through kinship or as neighbors. *support* is centered, and the coefficient on treatment can be interpreted as ATE. For details on control variables see notes to Table 1. The support regression additionally controls for geographic distance.

support the bilateral transaction (Jackson et al., 2012). Another way to think about this measure is as a variant of *trust flow*, as proposed by Karlan et al. (2009): the number of common social ties determines the social collateral that can facilitate informal transactions. We use two measures for static social networks unaffected by treatment—kinship and geography—to formalize this concept. For each dyad we count how many supporting households exist, i.e., households that are connected to both households of a dyad as kin or as neighbors: $\text{support}_{ij} = \sum_k \mathbb{1}(\ell_{ik}^{\text{neighbor}} \vee \ell_{ik}^{\text{kin}}) \cdot \mathbb{1}(\ell_{jk}^{\text{neighbor}} \vee \ell_{jk}^{\text{kin}})$, where ℓ_{ij}^{kin} and $\ell_{ij}^{\text{neighbor}}$ indicate whether two households are related or are direct neighbors.[33] Note that our support measure differs from that in Jackson et al. (2012) in two important dimensions. First, our measure is not binary, but a count variable.[34] Second, our measure of support counts common ties in a static social network as opposed to common ties in the network being studied. An analysis of the latter, i.e., of the prevalence of closed triads in the outcome networks, is implicit in our village-level analysis (see Supplement Table 3, columns 3 and 4).

We consider this measure of support a proxy for social proximity of two households and thus expect it to be an important factor for economic interactions. Indeed, in Table 6, column 2, we provide empirical evidence that support is a strong predictor of transactions in economic networks. In control villages, each additional household "supporting" a dyad is associated with a 0.52 percentage points larger probability of observing a transaction. The significant interaction of treatment with support suggests that the main reduction in transactions in treatment villages occurred between households with stronger support.

We take this as evidence that this particular measure for social proximity, support, indeed plays a crucial role in facilitating cooperation, but becomes less important in CDD communities. The reduction in the importance of common ties for network formation in treatment communities is also consistent with the reduction in the number of closed triads observed in treatment villages (see Supplement Table 3). In sum, the reduction of transactions in supported dyads in treatment villages is another indication for reduced informality and increased market orientation of the village economy.

[33] Two households are considered neighbors if, for either of them, the other is among the 10% closest in the village and not further away than the 25^{th} percentile of pairwise within-village distances (on average 100 m). We consider both, geographic distance and kinship, to be static and Appendix Table 11 suggests that there are no systematic difference between treatment and control (see also the discussion in Appendix A).

[34] Almost all pairs of households have at least one common tie and meaningful variation exists only in the number of common ties.

6.3 Channel (iii) – Unequally Distributed Benefits, Elite Capture, and Favoritism

Several of the previous results showed significant treatment effect heterogeneity. First, the main result for informal economic transactions suggested a significantly smaller magnitude of the treatment effect for likely VDC members. Second, likely VDC members in treatment villages also reported larger economic benefits than in control villages and had larger wealth scores in our data and significantly larger animals scores based on Census data. At the same time, we saw some indication for marginalized and non-established households benefiting less when we considered reported income or animals in the Census data. Third, likely VDC members appear to be more involved in CBOs in treatment villages, while marginalized households are socially and politically less engaged, when considering the friendship network or village meetings. In addition, in treatment villages there are fewer friendship links. This pattern of heterogeneous benefits and overall reductions in social capital is consistent with an erosion of social capital resulting from unequally distributed benefits from the CDD project. In this sub-section we examine more direct evidence for this hypothesis.

Unequally Distributed Benefits: Additional Evidence

Unequally distributed benefits can occur for various reasons. Our primary hypothesis for unequally distributed benefits is related to elite capture. To illustrate how elite capture could lead to highly unequal benefits, recall that most sub-projects are related to agriculture and land-holding households are likely to benefit more from this type of sub-project. This is particularly obvious in cases in which a tractor was purchased. Large landowners likely benefit from this mechanization of agriculture, while poor households may not be able to afford the fees charged for using tractors, and landless households might even lose: if agricultural machinery and manual labor are substitutes, labor is now less valuable. Results in Supplement Table 13 are in line with this hypothesis. Here we complement our previous analysis of heterogeneous treatment effects with a descriptive analysis of the correlates of CDD-specific benefits as well as of subjective views on the implementation process and find significant within-village heterogeneity. Table 13a shows that landless households report significantly less benefits from CDD projects.[35] Table 13a further shows that benefit from CDD projects is also associated with elite status (especially VDC membership). This contrasts with non-CDD projects in control villages, for which none of these correlations are significant. However, because CDD and non-CDD projects are inherently different, this piece of evidence has to be regarded as descriptive. Yet, the results regarding the VDC are consistent with the results on economic benefits being larger for (likely) VDC members (Tables 3 and 4). There is also evidence that respondents *perceive* benefits from the CDD projects as unequally distributed.[36]

Results reported in Table 13b provide further suggestive evidence that elite capture might play a role. These results show that households differ significantly in their views on procedures related to the CDD project choice.[37] The results suggest that traditional leaders and the VDC were more engaged in the decision making about projects. This would allow a purposeful steering of projects towards areas that would benefit them. Note that even if differences in benefits and influence in fact do not exist, perceived differences may cause grievances.

These results on benefits and decision making are of course only descriptive, but they are consistent with the hypothesis that elites and land-owning households benefit significantly more from CDD sub-projects than regular households and that this inequality of benefits is associated with village-level

[35] We use land ownership, as self-reported by respondents, rather than land under cultivation. Land ownership is measured in 2014, i.e., post-treatment. Land under cultivation would obviously be endogenous to the project. However, neither observations in the field nor balance tests using land and/or land Gini coefficients suggest that the distribution of land ownership is affected by treatment.

[36] In our in-depth survey, we asked households for each project whether they benefited more, the same, or less, relative to other households. For the average non-CDD project in a control village, only about 4% of households report that they are "benefiting less" than other households. This number increases to about 13% for the average CDD sub-project in treatment villages (Supplement Table 14).

[37] This analysis is at the level of individual project reports given by the households. In the in-depth survey, we asked households for each village-level development project (including non-CDD projects) about their view on who decided on that project. These regressions allow for a descriptive study of how different households perceive different types of projects. We find that marginalized households are significantly less likely than other households to report that all villagers were involved in the CDD project's decision making and significantly more likely to say that traditional leaders were important in making the decisions. The results clearly show that the VDC was very important in the decision making for CDD projects. For non-CDD projects, 19% of households state that the VDC decided, while for CDD projects this number is 44%.

measures of inequality. Further, the results highlight stark differences in how villagers perceive benefits from CDD and non-CDD development interventions.

Unequally Distributed Benefits Translate Into Fewer Links

While the above points towards unequally distributed benefits, it does not directly relate to interactions in networks. This section examines direct evidence on whether unequally distributed benefits translate into fewer transactions.

Based on our model framework, unequally distributed benefits can explain our main finding of reduced network interaction in two ways. First, there may be disputes and as a consequence less social capital (fewer meetings in the language of the model). Households with fewer benefits might sever their social ties to households with more benefits directly, out of grievance over their status. Further, unlike other projects in which benefits may also be unequally distributed, CDD comes with the promise of benefiting the whole community. If this promise is not fulfilled, the result could be internal quarrels among groups beyond individual "winners" and "losers" (Barron et al., 2011) that weaken social networks more broadly, not just among more and less benefiting households. To the extent that disputes imply lower levels of trust and social capital (i.e., in terms of the model, fewer meetings), disputes have the potential to translate into fewer informal economic transactions. Second, as laid out in our model for channel (iii), even without conflicts arising, households who benefit more or gain better market access become less interested in maintaining gift-exchange links with others, or are only willing to stay connected in relatively central network positions.

Table 7: Village-Level Inequality: Do Unequal Benefits Translate Into Fewer Transactions?

	all villages		CDD villages	
	(1) any transaction	(2) any transaction	(3) any transaction	(4) any transaction
treatment	-1.147	-1.151		
	(0.004)●●⊛	(0.004)●●⊛		
land Gini	0.416			
	(0.183)			
treatment × land Gini	-0.640			
	(0.086)⊛			
room Gini		0.073		
		(0.735)		
treatment × room Gini		-0.768		
		(0.086)⊛		
multiple respondents said the main project failed (in-depth survey)			-1.687	
			(0.019)**	
focus group said the main project failed				-1.906
				(0.053)*
controls	✓	✓	✓	✓
dyads	151632	151632	81478	81478
households	2774	2774	1416	1416
control mean dep. var.	6.9	6.9		

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 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. The Gini coefficients are standardized to have mean zero and variance one. Columns 3 and 4 use project assessments elicited in the in-depth interview and the village-level focus group as proxies for project failure. For details on control variables see notes to Table 1.

We start by testing the effect of village-level variables that plausibly proxy for unequally distributed benefits. Columns 1 and 2 in Table 7 consider village-level inequality. Previous literature suggests that elite capture is positively related to village-level inequality (e.g., Bardhan and Mookherjee, 2000; Araujo et al., 2008). Because of the finding that land ownership is positively related to self-reported benefits, land inequality is our primary inequality measure. Yet, land ownership is difficult to measure in this context and we cannot fully rule out that this measure is affected by treatment (see the discussion above and Footnote 35). Therefore, we also use an inequality measure based on housing wealth, as proxied by the number of rooms belonging to the household.[38] The advantage of this is that it is straightforward to measure rooms, and this variable was recorded in the Census 2003, i.e., before the program. Results in columns 1 and 2 suggests that, indeed, the treatment effect on networks is heterogeneous with respect to our measures for inequality. More inequality, as measured through inequality of land holdings and the number of rooms, is associated with a stronger reduction in economic transactions.

Focusing on CDD-villages, we have two measures of project failure that might plausibly be related to disputes, either because of unequal benefits (if some households identify a project as a failure, this might reflect unequal benefits) or because of completely absent benefits.[39] Column 3 uses data from the in-depth survey of individual households, while column 4 uses data from focus group discussions, which we conducted with village authorities to obtain a better understanding of recent developments in the villages. Both show a negative association with informal economic transactions. Because these two measures only exist for CDD villages, we cannot compare treatment and control villages along those dimensions. Further, since project failure is the result of possibly unobserved village characteristics that also determine informal interactions, the failure variables are endogenous. Therefore, these results cannot be regarded as causal and should only be seen as indicating a positive correlation of project failure and the existence of economic flows between households within treatment villages.[40]

Next, in Table 8, we consider proxies for differences in benefits at the dyadic level. First, because of the above-mentioned arguments and our findings related to the importance of land for benefits, we look at dyadic differences in land holdings. Results in columns 1 and 2 show that the negative treatment effect is observed only for dyads in which both households likely have little benefits (column 1, where benefit is proxied by a continuous land variable) or where at most one household owns land (column 2). When both households own (similar amounts of) land, the coefficients are either positive (column 1) or insignificant (column 2).

Second, because of our previous findings related to VDC members, we also study heterogeneity related to the probability of being a VDC member (column 3). On the one hand, we find that the negative treatment effect is concentrated among dyads where both households are similar and have an average (or lower) VDC−score, i.e., a low probability of being member of the VDC (this is the omitted category shown by the treatment dummy). On the other hand, where both households have a relatively large VDC−score, there is no significant treatment effect (indicated by the coefficient in the last row). Finally, if only one end of the dyad has a high probability of being a VDC member there is a positive treatment effect. This might reflect favoritism, i.e., goods flowing between households that benefit from the project due to their position and friends or family of those households. Supplementary Material H provides evidence for favoritism.

Third, looking only at CDD villages, we use self-reported benefits from CDD projects (columns 4 and 5). Again, these results are not experimentally identified and should be interpreted as suggestive. The omitted category in column 4 is "both benefited from project" and "both reported the same benefit from project" in column 5. Thus, the results show that dyads in which neither household benefited (column 4) or in which benefits are unequally distributed between i and j (column 5) have fewer informal transactions among themselves.

In sum, using dyadic proxies for the distribution of benefits, we find several pieces of evidence that unequal benefits or absent benefits are associated with fewer informal transactions. On the other hand, dyads in which both benefit do not show treatment effects or even show positive effects. Overall, both the analysis using village-level proxies and the dyadic proxies for heterogeneous benefits suggest that heterogeneity in benefits or absence of benefits explain lower levels of informal transactions.

7 Implications for Household Welfare

The results above show that households in treatment villages are less connected in economic village networks. This can be expected to affect welfare for a number of reasons. For example, Gagnon and Goyal (2017) show theoretically that when some individuals choose to substitute market interactions for socially embedded network transactions, and thereby impose a negative externality on other households,

[38] Richer household heads in The Gambia tend to have more wives and more children (49% of the married household heads in our sample are polygamous). Due to polygamy, household size itself is likely positively affected by wealth. We thus consider the total number of rooms a better indicator for wealth than rooms per capita.

[39] Indeed, in the sample villages, more than one quarter of development projects are considered "not functioning" by the respondents (see Supplement Table 15)

[40] The point estimates in columns 3 and 4 are robust to controlling for the existence of a transfer in the respective dyad in 2009 (results not shown).

Table 8: Dyad-Level Inequality: Do Unequal Benefits Translate Into Fewer Transactions?

	all villages			CDD villages	
	(1) any transaction	(2) any transaction	(3) any transaction	(4) any transaction	(5) any transaction
(likely) small or no benefit					
treatment	-1.539 (0.000)●●●		-1.686 (0.001)●●●		
treatment × neither has land		-1.404 (0.006)●●⊛			
neither benefited from projects				-0.909 (0.088)*	
different benefit / only one benefited					
treatment × | $\text{land}_\text{i} - \text{land}_\text{j}$ |	-0.112 (0.137)				
treatment × one has land		-1.346 (0.002)●●●			
treatment × | $\text{VDC}-\text{score}_\text{i} - \text{VDC}-\text{score}_\text{j}$ |			0.621 (0.041)●●		
one benefited from projects				-0.345 (0.178)	
i and j benefited differently from CDD					-2.695 (0.087)*
similar benefit / both benefited					
treatment × ($\text{land}_i + \text{land}_j$)	0.128 (0.066)●				
treatment × both have land		-0.749 (0.167)			
treatment × ($\text{VDC}-\text{score}_i + \text{VDC}-\text{score}_j$)			0.236 (0.228)		
controls	✓	✓	✓	✓	✓
dyads	149472	149472	151632	81376	2304
households	2755	2755	2774	1415	267
control mean dep. var.	6.9	6.9	6.9		

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 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. Column 4 uses statements about benefits from "any development projects" asked in the main survey. We omit the category of dyads where both households declared to have benefited. Column 5 uses project assessments elicited in the in-depth interview, specifically for CDD projects. These regressions are estimated on the subsample of dyads where both households were included in the in-depth survey. We omit the category of dyads where both households concordantly declared either to have benefited more than others, the same as others, or less than others on average (across all reported sub-projects). For details on control variables see notes to Table 1.

overall welfare can be reduced. A large theoretical and empirical literature suggests that networks help households cope with shocks and that a household's ability to enforce informal contracts depends on social capital (e.g., Ligon et al., 2002; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006; Karlan et al., 2009).

In this section, we test directly the relationship between shocks and activity in economic networks, and study whether idiosyncratic shocks are less likely to result in economic transactions towards the affected household in treatment villages. Above, we have already seen that households in treatment villages have fewer friends, which is particularly true for marginal households (Table 2). On the other hand, there is no difference in potential "helpers" in times of need. Thus, whether our main results are indicative of a reduced ability to deal with shocks is unclear. We have collected data on different types of shocks that are relevant in our setting (production, housing, and health shocks) that we aggregate into one overall shock-count that sums up all categories. If households are able to deal (at least partly) with shocks through their social networks, the flow of goods that constitute the actual act of helping out each other should be observed in our data. Indeed, Table 9, which uses our dyadic data, shows that shocks experienced by the receiving household of a given directed dyad are statistically significant predictors of flows towards this household (columns 1, 3, 5, and 7). These results show that people in need are more likely to receive goods and services from other households. This is true for all three shock categories that we consider. This finding strongly suggests the existence of some form of intra-village risk-sharing.

Turning to the effect of treatment, columns 2, 4, 6, and 8 report the coefficient of the interaction

Table 9: Treatment Effect on Village-Internal Links in Response to Shocks

Shock type:	count		production		housing		health	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	any transaction	any transaction	any transaction	any transaction	any transaction	any transaction	any transaction	any transaction
treatment	-1.165	-1.175	-1.163	-0.986	-1.129	-1.157	-1.147	-1.228
	(0.003)●●⊛	(0.013)●●	(0.003)●●⊛	(0.011)●⊛	(0.004)●●⊛	(0.010)●●⊛	(0.004)●●⊛	(0.005)●●⊛
shock$_j$	0.444	0.442	0.678	0.787	0.285	0.247	0.325	0.270
	(0.000)***	(0.003)***	(0.000)***	(0.000)***	(0.091)*	(0.363)	(0.027)**	(0.274)
treatment × shock$_j$		0.005		-0.210		0.074		0.108
		(0.976)		(0.453)		(0.839)		(0.727)
controls	✓	✓	✓	✓	✓	✓	✓	✓
dyads	151632	151632	151632	151632	151632	151632	151632	151632
households	2774	2774	2774	2774	2774	2774	2774	2774
control mean dep. var.	6.9	6.9	6.9	6.9	6.9	6.9	6.9	6.9
mean shock var.	1.9	1.9	0.8	0.8	0.4	0.4	0.7	0.7

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 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. For details on control variables see notes to Table 1. The shock variables indicate the occurrence of shocks of different types during the past two years. The production shocks sum up two indicators, "crop failure" and "animals died or got sick/agricultural tools broke", and can take on the values 0, 1, and 2. The housing shock indicates the destruction of a building belonging to the household, which is very common during rainy season. The health shock sums up two binary indicators for death and serious illness within the household, and can take on the values 0, 1, and 2. The total shock count is the sum of all three shock categories. Shocks are balanced between treatment and control communities, as shown in Supplement Table 16.

between treatment and shock. Throughout, we do not find any significant interaction effects. In addition, the magnitude of the parameter estimate is small relative to the control mean. Thus, there is no evidence for a difference between treatment and control villages in their ability to deal with these relatively serious types of shocks that we consider in this section. This finding is consistent with the finding from Section 6.1 that the number of potential "helpers" in times of need is not significantly different in treatment villages.

One concern with the above analysis is that shocks themselves may be affected by treatment. Although we cannot fully rule out this possibility, to alleviate this concern we have further investigated the relation between treatment and shocks, and found that shocks are balanced between treatment and control communities (see Supplement Table 16).

8 Conclusion

We study the effects of development projects on economic and social interactions in small, rural villages of The Gambia through the lens of networks. Participatory development projects, such as the Community-Driven Development (CDD) program that we study, are supposed to bring economic and social change and we hypothesize that these changes may also affect economic networks in largely unintended ways.

Our main finding is that, more than four years after the Gambian CDD program began operations, households in treatment villages are significantly less likely to economically interact with each other. To understand the underlying mechanisms of this effect of the program, we provide a simple model that highlights three possible channels through which the program may affect networks of transactions.

We find no evidence supporting the first channel. The program did not increase social interactions. On the contrary, on average households in CDD villages have fewer friendship links to other households and participate less in community-based organizations. Marginalized groups stand out particularly, exhibiting the largest reduction in friendship links and also a reduced attendance in village meetings.

Second, we find some evidence for positive economic change. In particular we find effects on asset and animal ownership, and there is evidence for a village-level transformation towards a more market-oriented economy. We find a reduced importance of social proximity, a reduction in reciprocal relationships among villagers, and an increase in (paid) interactions with individuals from outside the village. Our theoretical framework illustrates how these increases in incomes and market orientation can explain fewer informal transactions.

Third, the model lays out how unequally distributed benefits can explain reductions in transactions and more central positions of some individuals. In support of this channel, we find that benefits in CDD projects are more unequally distributed than in other projects. In addition, proxies for unequal benefits at the village level or between households are associated with fewer transactions in economic networks. Further, we find that observed reductions in economic transactions are accompanied by a reduction in social interactions. This supports the hypothesis that unequally distributed benefits, elite capture, and favoritism could have caused a reduction in social capital in CDD villages (see also Barakat, 2006; Morel et al., 2009; Barron et al., 2011; King and Samii, 2014). In line with the model, we also find that people with a high likelihood of being involved in the program (who may derive additional incomes or opportunities to favor their friends) are less affected by the reduction in economic transactions.

Whether reduced links are a sign of welfare reductions partly depends on the responsible mechanism. On the one hand, the CDD may have decreased the relative benefit of informal transactions by increasing income and the value of market-based outside options. On the other hand, the CDD may have decreased the ability to sustain beneficial cooperation by reducing social capital or through changes in the relative attractiveness of some households as linking partners. The former would suggest that reductions in informal transactions are related to increases in welfare, while the latter would suggest reduced transactions signal reductions in welfare. We found some indications for both. Yet, the positive effects on economic wealth seem small and unequally distributed, casting doubt on whether decreases in the need for informal transactions caused the observed effect on networks. Additionally, using data on shocks, we investigate whether the reduced density of economic networks negatively impacts households' welfare through a reduction in their ability to insure informally and to cope with shocks through informal transactions. We find no indication that CDD program affected the probability that an economic shock triggers an economic flow, which suggests that at least the role of informal transactions as insurance against the shocks that we measure, is not negatively impacted.

In sum, the evidence on welfare is mixed. A substantial amount of results indicates a reduction in social capital within treatment villages thus suggests negative implications for welfare. Yet, the insignificant results related to the ability to deal with shocks do not provide additional evidence about negative implications of reduced network interactions for well-being. Additional work is needed to provide a more conclusive picture of these implications. Further, while our experimental data allow us to cleanly identify the main effect of treatment on economic and social interactions, our analysis of possible channels is limited by available data. Future work should consider research designs that allow testing for the existence of specific channels.

The findings reported in this paper are first and foremost saying something about The Gambia. However, some recent concurrent work by Banerjee et al. (2019) and Binzel et al. (2013) suggests that our results may also generalize to other contexts. Both papers find, as we do, reductions in dimensions of village-interactions that are not directly related to the intervention. An important difference to our paper is that these papers study a change in the economic environment that has direct effects at the individual-level or within small microfinance groups, while we study an intervention that affects villages as a whole and explicitly also aims at affecting social activities. These features of our setting open up the possibility that aspects of political economy, such as elite capture, become important channels. More research is required to investigate the relationship between development projects and within-village conflict, as development projects may induce (distributional) conflict when a project is a success but also when benefits do not materialize or are unequally distributed.

In sum, our findings suggest that development projects that intend to bring positive economic change and in the process intentionally affect social interactions, may have unintended consequences and could influence social and economic networks negatively. To the extent that these effects are due to increased income and market orientation, this implies that in environments where significant economic change occurs, measures to alleviate the loss of informal networks or measures to compensate for this loss, such as the introduction of formal insurance mechanisms, should be considered. To the extent that reductions in interactions reflect reductions in social capital due to unequally distributed benefits, elite capture, or favoritism, special care has to be taken, e.g., throughout the facilitation process, the choice of sub-projects, and when setting up hierarchical project structures, to avoid increasing existing tensions and to prevent potentially new sources of internal divisions.

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A Baseline Balance and Summary Statistics

Table 10: Summary and Balance of Pre-Treatment Demographics, Network Proxies and Control Variables

	Mean		Observations		Difference		p-value	
	(1) control	(2) treated	(3) control	(4) treated	(5) raw	(6) cond.	(7) CRSE	(8) RI
Panel A: household-level characteristics (2003)								
female	0.52	0.53	13820	13969	0.008	0.006	0.48	0.57
age	21.85	21.80	13737	13909	−0.051	−0.034	0.94	0.95
access to electricity	0.03	0.02	13795	13889	−0.005	−0.008	0.37	0.43
access to water	0.02	0.01	13779	13913	−0.008	−0.006	0.41	0.49
access to private WC	0.69	0.66	13729	13813	−0.022	−0.009	0.89	0.90
literacy	0.66	0.60	10657	10672	−0.064	−0.056	0.12	0.18
mandinka	0.46	0.55	13820	13969	0.097	0.099	0.21	0.28
migrant	0.08	0.09	11730	11792	0.010	0.018	0.47	0.56
education	0.57	0.55	12641	12776	−0.024	−0.016	0.65	0.69
Panel B: village-level controls (2003)								
electrification rate (%)	3.22	2.37	28	28	−0.847	−0.725	0.44	0.44
private WC (%)	63.16	59.72	28	28	−3.443	−3.307	0.66	0.66
access to clean water (%)	9.96	8.25	28	28	−1.712	−1.804	0.59	0.60
literate rate (%)	64.69	59.67	28	28	−5.022	−4.962	0.22	0.22
population	493.57	498.89	28	28	5.321	2.145	0.96	0.96
households (2014)	48.50	50.57	28	28	2.071	1.566	0.75	0.75
Panel C: household-level controls (2014)								
trad. leader	0.11	0.14	1358	1416	0.030	0.029	0.12	0.19
marginalized	0.23	0.24	1358	1416	0.006	0.013	0.54	0.60
formal education (head)	0.15	0.19	1358	1416	0.045	0.043	0.03	0.06
ethn. minority ($< 30\%$)	0.18	0.15	1358	1416	−0.021	−0.033	0.30	0.35
proxy respondent	0.27	0.25	1358	1416	−0.014	−0.009	0.68	0.71
household size	13.48	13.35	1358	1416	−0.125	−0.281	0.74	0.78
Panel D: pre-treatment network proxies (2003)								
born in this village	0.79	0.79	1275	1325	−0.001	−0.012	0.76	0.80
# households in compound	2.13	2.25	1275	1325	0.117	0.215	0.53	0.64
# coethnics in village	344.28	363.69	1275	1325	19.408	15.182	0.68	0.73
# spouses born in village	0.83	0.78	1275	1325	−0.045	−0.082	0.22	0.28

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. The data underlying Panels A, B, and D stem from the Gambian Census 2003, with the exception of the household count, which is taken from the network data from 2014. Panel C is based on data collected by the authors. Variables displayed in Panels B and C are the control variables used for all regressions unless indicated otherwise. Population and number of households enter the regressions logarithmically. Variables in Panel D are variables that we consider close proxies of network degree in various social networks.

Our analysis considers both, geographic distance and kinship, as invariant to treatment and Appendix Table 11 suggests that there are no systematic difference between treatment and control villages. Further, we have no evidence for geographic movement within villages. The spatial extent of villages is usually small (90% of all pairwise distances are less than 389 m, half are less than 166 m) and thus incentives to relocate strategically are limited. Kinship is measured via the kinship network described in Section 4.2 and includes first-degree relatives and children's in-laws. Thus, changes in response to treatment would require marriages in response to treatment or systematically different reporting. We consider both unlikely given the anecdotal evidence and two econometric tests: in-law networks, where strategic marriages

Table 11: Summary of Network Degrees and Balance of Kinship and Geographic Variables

	Mean		Observations		Difference		p-value	
	(1) control	(2) treated	(3) control	(4) treated	(5) raw	(6) cond.	(7) CRSE	(8) RI
economic (union)	5.68	4.99	1358	1416	-0.697	-0.679	0.03	0.06
-land	1.14	1.02	1358	1416	-0.119	-0.151	0.16	0.22
-labor	1.44	1.44	1358	1416	0.000	-0.045	0.74	0.77
-inputs	1.79	1.54	1358	1416	-0.252	-0.190	0.32	0.37
-food	1.91	1.38	1358	1416	-0.534	-0.525	0.01	0.02
-gifts	0.86	0.64	1358	1416	-0.219	-0.175	0.08	0.12
-credit	1.09	0.95	1358	1416	-0.148	-0.104	0.46	0.51
friendship	2.66	2.42	1358	1416	-0.239	-0.276	0.31	0.38
kinship	3.06	3.19	1358	1416	0.132	0.005	0.99	0.99
mean distance to other villagers	184.87	204.68	1302	1349	19.810	15.116	0.32	0.40
missing exact GPS location	0.04	0.05	1358	1416	0.006	-0.002	0.84	0.87
number of "neighbors"	6.65	7.04	1358	1416	0.383	0.322	0.56	0.64

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. Numbers are based on the network data collected by the authors. 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. Neighbors are defined based on geographic closeness (see Footnote 33).

should matter most and the rate at which kinship ties are confirmed by both sides, which would indicate omissions due to differential reporting, are statistically indistinguishable in treatment and control communities.

B Other Specifications for the Average Treatment Effect Estimate

Table 12: Main ATE Specification, Robustness

	post double-LASSO	not controlling for education	w/o ward 319	w/o ward 607	w/o wards 319 and 607	probit	intensity	unpaid	undirected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	any transaction	any transaction	any transaction	any transaction	any transaction	any transaction	any trans. (intensity)	unpaid econ. transaction	economic transaction
treatment	-0.884	-1.138	-1.109	-1.219	-1.187	-1.201	-0.033	-1.087	-1.751
	(0.046)$^{\bullet\bullet}$	(0.004)$^{\bullet\bullet\circledast}$	(0.006)$^{\bullet\bullet\circledast}$	(0.003)$^{\bullet\bullet\circledast}$	(0.004)$^{\bullet\bullet\circledast}$	(0.000)$^{\bullet\bullet\circledast}$	(0.031)$^{\bullet\circledast}$	(0.005)$^{\bullet\bullet\circledast}$	(0.007)$^{\bullet\bullet\circledast}$
controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
dyads	151632	151632	140368	145472	134208	151632	151495	151632	75816
households	2774	2774	2596	2640	2462	2774	2774	2774	2774
control mean dep. var.	6.9	6.9	6.8	6.8	6.7	6.9	0.2	6.5	11.9

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 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. In column 8 undirected dyads are used. Regressions control for ward fixed effects and a set of control variables: the village-level variables in Appendix Table 10, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table 10, as well as ethnicity and enumerator dummies enter the regressions twice, for the sending and the receiving household of a dyad. In column 1 we use the post double-LASSO to selectively exclude control variables from the regression, as suggested by Belloni et al. (2014) and implemented by Ahrens et al. (2018). Columns 2 shows that the ATE estimate remains almost unchanged if we exclude the indicator for the sender's and the recipient's formal education from the set of controls. This is the only control variable that showed a significant imbalance between treatment and control in Appendix Table 10. Due to factors unrelated to treatment, two villages were excluded from the sample and consequently two ward-strata have unequal numbers of treatment and control villages. Columns 3-7 document that the results are robust to excluding those wards entirely. Column 6 estimates a probit instead of a linear probability model and reports average marginal effects. Results in column 7 document that an ATE of comparable magnitude (relative to the control group mean) is found when instead of a binary indicator, the intensity of the transaction is used. Land transactions are measured in hectares. For labor, the volume/intensity of a transaction is measured as the product of people and days one household provided to the other. For inputs, intensity is measured as the number of distinct inputs categories provided (e.g., seeds, tools, fertilizers). In the food network, transactions are converted into units roughly equivalent to the nutritional value of one kilogram of fruit or beans, by applying the factor 0.5 to rice, maize, millet, and groundnut, and the factor 4 to milk, meat, and fish. Across the six domains, measures are aggregated to a z-score that is normalized to have variance 1 and take on the value 0 if there is no transaction. Weights for this z-score are obtained following the variance-covariance weighting suggested by Anderson (2008) and also normalized to have variance 1. Column 8 excludes transactions that involved some form of payment. Column 9 treats the network of transactions as an undirected network.

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