Development Projects and Economic Networks: Lessons From Rural Gambia

Simon H. Heß, Dany Jaimovich, Matthias Schündeln*

Goethe University Frankfurt†

This version: June 2018

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 (CDD) program in The Gambia has on economic interactions within rural villages. The CDD 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, our main finding is a significant reduction of links in these networks in treatment villages. We investigate several possible mechanisms, and find evidence that suggests two (compatible) explanations. First, the evidence points towards a modest village-level transformation process from a gift economy to a more formal economy. Second, we also find evidence that is consistent with elite capture, unequally distributed benefits, and failed projects leading to internal divisions that reduce economic interactions within villages. Overall, our findings suggest changes in networks as an avenue through which development interventions may have unintended consequences. These should be considered when a project's costs and benefits are evaluated.

*We gratefully acknowledge financial support from the DFG (Deutsche Forschungsgemeinschaft) through project 250842093 "The dynamics of economic community networks in the development process: An empirical study in rural Gambia, West Africa". We are thankful for many helpful comments from colleagues and seminar audiences. We particularly thank Jean-Louis Arcand, Erwin Bulte, Arun Chandrasekhar, Margharita Comola, Marcel Fafchamps, Matthew Jackson, Raymond Jatta, and Rachel Kranton for their comments, and Julien Labonne for insights about the implementation of the Gambian CDD program. We are grateful for the local support we received through the Gambia Bureau of Statistics and by the nine field workers without whom the data collection would not have been possible.

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

1 Introduction

Networks take on a central role in rural societies of less formal economies. Where markets are incomplete, intra-household networks help households pool risk and deal with adverse shocks (Rosenzweig, 1988; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006), and, more generally, enforce informal contracts (Karlan, 2007; Karlan et al., 2009; Giné et al., 2010; Jackson et al., 2012; Chandrasekhar et al., forthcoming), or aggregate and diffuse information (Bandiera and Rasul, 2006; Conley and Udry, 2010; Beaman and Magruder, 2012; Banerjee et al., 2013; Alatas et al., 2016). Networks are also at the core of discussions on social capital (Putnam, 2000; Durlauf and Fafchamps, 2005). Social capital, in turn, has been linked to economic returns on the aggregate level (Knack and Keefer, 1997; Guiso et al., 2004) and the micro level (Feigenberg et al., 2013).

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, despite prominent theoretical work on this question (Kranton, 1996; Gagnon and Goyal, 2017), not much is known empirically about how networks change during the process of development and as a consequence of development projects. 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. These have the goal of improving targeting and project sustainability, as well as to build social capital and cause institutional change (Casey et al., 2012; Mansuri and Rao, 2012; White et al., 2018; Casey, forthcoming), which has the strong potential to affect networks.

In this paper, we contribute towards filling this gap in the literature, providing empirical evidence from a large Community-Driven Development (CDD) program in The Gambia, which was allocated randomly to villages. In a participatory process, each local community could then decide on the specific project(s) for which the funds they received from the CDD program were to be used. By design, participatory programs almost always include features that directly aim at affecting interactions within villages. For example, according to the guidelines, the CDD program that we study mandates that communities organize up to 38 village-level activities (GoTG, 2006). They make joint decisions, come together to provide contributions to the project their village chooses, and form steering committees that include groups that are typically marginalized (young and female individuals) while purposefully excluding certain traditional authorities. The projects implemented by the villages in the Gambian CDD program are mostly related to public goods or club goods, e.g., agricultural machinery, whose day-to-day operation and administration also entail significant interactions across a large number of households. Further, the projects are mostly intended to promote income-generating activities and plausibly also affect market-based activities, and thus a project may also affect how households interact with people outside the village. In sum, there are several aspects of well-functioning CDD programs that likely have direct or indirect effects on links of individuals and households within

and across villages, and consequently on economic networks. The direction of this effect is ambiguous.[1]

On the other hand, CDD programs that do not function well may also have effects on social and economic interactions. One possible reason is that the focus on grassroots decisions increases the potential for elite capture (Bardhan and Mookherjee, 2000; Platteau, 2004; Olken, 2007). Indeed, 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). Further, Gugerty and Kremer (2008) show that the availability of additional resources that are coming in through development programs changes the composition of local community groups, attracting the better-off and weakening the role of the disadvantaged in these groups. At the same time, provisions against the involvement of certain groups, such as traditional elites, and the introduction of inclusive decision-making mechanisms have also been linked to social tensions in CDD programs (Barakat, 2006; Morel et al., 2009; King and Samii, 2014; White et al., 2018). Another concern is that projects may fail, for example, because of poor project choice or poor project management. Failure of a project that was decided upon by the community, or by some elite within the community, might result in internal disputes and social disruptions (Barron et al., 2011, p. 96). However, even in functioning projects, benefits may be unequally distributed and the availability of new resources may induce conflict over their distribution (Grossman, 1992; Dube and Vargas, 2013; Crost et al., 2014; Nunn and Qian, 2014; Ray and Esteban, 2017). Internal disputes in turn may reduce the size of social networks, which reduces social network-based trust and consequently economic interactions (Karlan et al., 2009). We thus hypothesize that the CDD program — partly by design, partly through unintended avenues — affects social activities and economic interactions beyond those that are immediate consequences of the project, and that the effects of CDD programs spill over into other realms of life. Our strategy is to study these changes in interactions through the lens of networks.

We study a World Bank-financed program that, starting in 2008, 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 about 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.[2] In 2014, to study the effects of this program on networks, we collected post-treatment data on full village networks in 56 villages (half of them treatment villages), i.e., we collected data for all households in these villages. Our survey was specifically designed for the purpose of collecting in-depth network data. These data cover six

[1]More frequent interactions were found to increase social capital in Feigenberg et al. (2013). On the other hand, previous studies suggest that some forms of social capital may be substitutes. For instance, Labonne and Chase (2011) and Avdeenko and Gilligan (2015) find that the increase in interactions required to implement the project implies a reduction in other aspects of the village community interactions.

[2]The country-wide average funding per household is approximately US$140. In the sample for this study, in which villages are smaller, it is US$230. By comparison, the per household funding in Casey et al. (2012), who study a CDD program that is in many ways comparable to the program in The Gambia, is US$100. As an alternative way to illustrate the magnitude, note that the funds received by an average village would be sufficient to provide each household with about eight goats (which is roughly the same order of magnitude that is reported, for example, in Banerjee et al. 2015).

different economic domains (land, labor, inputs, food, gifts, credit) — which we aggregate in two different ways to avoid over-interpretation of effects in individual domains — and two social domains, namely friendship and kinship.[3] Using the full network data allows us to calculate various network-related statistics at the dyad (household-pair) level, at the household level, and at the village level. The random assignment of the program then allows for a straightforward estimation of the effect of the CDD program on indicators of the structure of networks. Because we have a small number of clusters we take particular care of issues of inference, by using randomization inference.

Our main finding is that the CDD treatment significantly reduces interactions in economic networks. In our main specification, where we use dyadic regressions, the estimated magnitude of the effect implies on average an economically significant loss of about one in five transactions between households. This finding is robust to using various ways to aggregate the six different economic networks, and to moving from the dyadic to the household-level or to the village-level. We further investigate our main finding concerning a reduction in links, utilizing two related concepts, namely reciprocity[4] and support[5]. We first confirm prior results of empirical work and predictions of theoretical work by documenting that many links are reciprocated (e.g., Udry, 1990; Leider et al., 2009; Schechter and Yuskavage, 2011). New is our finding that reciprocity is reduced in treatment communities. Further, in settings where enforceability is a concern, cooperation — and thus network links — can be facilitated by common ties, who support the bilateral exchange (Jackson et al., 2012; Banerjee et al., 2018; Chandrasekhar et al., forthcoming). Indeed, we find empirical evidence for the theoretical prediction that the probability for a pair of households to link in an economic network increases with the number of supporting households (which we identify based on the exogenous characteristics of kinship and spatial proximity). However, in treatment villages this effect of support is reduced. The main finding of a reduction in network interactions is mirrored in other indicators of social capital that have been used in the literature. In particular, we find lower levels of participation in village-level groups and fewer contributions to those groups.

Having established the negative effect of the treatment on social networks, we then investigate various channels. We first confirm that village-level projects were actually implemented in all treatment villages and that they had procedural features that are central to the community-driven approach. We then study whether the projects had the intended consequences on social and economic development. To study economic changes, we analyze data from various sources, including household-level wealth indicators from the Gambian Census of 2013, asset indicators, and self-reported benefits that we collected in 2014. Summing up these analyses, we only find

[3]Even though we did not register a formal pre-analysis plan, note that a summary of the grant proposal for the project "The dynamics of economic community networks in the development process: An empirical study in rural Gambia, West Africa" (granted in 2014) is publicly available at http://gepris.dfg.de/gepris/projekt/250842093?language=en.

[4]"Reciprocal exchange is informally enforced agreements to give goods, services, information, or money in exchange for future compensation in kind" (Kranton, 1996, p. 830).

[5]*Support* captures the role shared ties with others play for the interaction between two individuals. "Two households may not be able to sustain cooperation amongst themselves, but if they were to have a link in common, then they could leverage this relationship to maintain cooperation" (Banerjee et al., 2018, p. 12).

at most evidence for modest economic change, which is in line with prior findings on economic gains due to CDD programs (Mansuri and Rao, 2012; White et al., 2018). The second area that the CDD program intends to affect is institutions. Here, we find some evidence that use of the CDD procedures (involving joint community meetings, voting for projects, and decisions made by the whole community or village development committees) also spilled over into joint decision making procedures in more recent, non-CDD projects. However, we do not find significant evidence of institutional change in how community meetings are run in general. In sum, the modest effects on the intended economic and social development alone are unlikely to explain changes in networks.

We also test a number of hypotheses related to secondary consequences and unintended consequences of the development program, namely a formalization of the village economy, elite capture, unequal benefits, and failed projects. Regarding the formalization of the village economy, we investigate a hypothesis related to work by Kranton (1996), Ishiguro (2016), and Gagnon and Goyal (2017) suggesting that, as market exposure increases, e.g., in the wake of positive income shocks or related economic changes, the probability that some actors switch from socially embedded activities to market-based activities increases. Socially embedded activities, such as reciprocal exchanges and relational contracting, are characterized by positive externalities. Hence, the more individuals switch to market-based activities, the lower the value of engaging in socially embedded activities, which makes it likely that even more actors switch to market-based activities. Indeed, our evidence shows that reciprocity is reduced and social proximity is a less important factor for interactions in treatment villages. We also find that in treatment villages there are more links to individuals outside the village. These findings suggest some movement from a local gift economy to a market economy as a contributing factor for the observed reduction in within-village network interactions.

Another channel that we investigate is related to unintended effects: elite capture, unequally distributed benefits, and failed projects. Despite provisions to the contrary, elites might capture projects such that benefits end up being unequally distributed (Bardhan and Mookherjee, 2000; Platteau, 2004; Alatas et al., 2013). Further, projects may function poorly or even fail.[6] Failed projects and projects in which benefits are unequally distributed, possibly because of elite capture, may cause village-internal divisions and quarrels (Barron et al., 2011). Moreover, the introduction of new institutions through CDD programs (Barakat, 2006; Morel et al., 2009; King and Samii, 2014) or the changes implied by the introduction of new technology (von Braun and Webb, 1989) may also lead to within-village conflicts, something for which we also found anecdotal evidence in our sample villages.[7] We hypothesize that one consequence of elite

[6]One CDD-specific reason for project failure might in fact be the exclusion of elites from the decision and management process. In cases where provisions against elite capture work, and elites are indeed largely excluded from the decision and management process, it might actually hurt the success of the project, if traditional leaders are better able to manage a project (as suggested by Voors et al., forthcoming) or because there is significant variation in project leader quality and some may not be sufficiently qualified (Khwaja, 2009).

[7]For the CDD program in Sierra Leone that is in many ways comparable to that in The Gambia, Casey et al. (2012) also test if the program reduced incidences of crime and conflict and find no such effect. One of the ten individual outcome variables used for this test can be considered an indicator of the kind of disputes that

capture and unequal or missing benefits is village-internal quarrels, and that these lead to an erosion of social capital that is reflected in reduced interactions in networks. Indeed, our survey evidence suggests that households view benefits as unequally distributed, with benefits more likely to be reported by richer and elite households. Further, many projects are considered failures by a large fraction of the households, which is in line with observations by our own enumerators. Also, especially in villages where project funds were used to acquire goods that are excludable in consumption, we observe more benefits being reported by elites than by non-elite households. Importantly, these findings can explain some of our network-related results: Combining information on network exchanges with village-level data regarding project characteristics and proxies for the distribution of benefits, we find that the reduction in network links is particularly large in villages with large inequality and in villages in which projects do not perform well. At the dyad level, we find that household pairs with a gap in self-reported benefits from the CDD program interact less with each other.

Finally, we also study whether the reduction in economic links is associated with changes in household welfare. Weakened intra-village networks can negatively affect welfare in multiple ways. In many contexts, utility derived from interacting on a network is increasing in the number of network nodes. Households that withdraw from the network, e.g., because other alternatives become available or because of quarrels, thus impose a negative externality on remaining households, potentially reducing overall welfare (Gagnon and Goyal, 2017). One such case, which is of particular relevance in poor countries, is that networks and welfare are linked through social ties that insure against shocks (e.g., Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006; Fafchamps and Gubert, 2007). We investigate if the households' ability to insure against shocks is reduced in treatment villages in two different ways. First, we elicit information on a variety of household idiosyncratic shocks, and relate shocks to economic interactions. We find a significant correlation of shocks and networks transactions, which confirms prior findings on the role of informal exchange networks for within-village insurance. However, we find that the reaction to shocks does not differ between households in treatment and control villages. Second, we study social connections between households via an analysis of "friendship" networks. We find that treatment reduces the number of friends a household mentions.[8]

Overall, our results caution that development projects, in particular those with participatory features in which roles and benefits may be unevenly distributed, can have unintended consequences for the economic and social networks of villagers.

Our paper contributes to five main literatures. First, networks are usually taken as exogenously given (e.g., in the peer-effects literature, Bramoullé et al. 2009; Calvó-Armengol et al. 2009; Lee et al. 2010, but also in other applications, e.g., by Banerjee et al. 2013 or Ambrus

we have in mind, namely whether households ever had a conflict with someone over a loan or other money business. Considering this outcome in isolation, the authors find that the Sierra Leone CDD program has increased conflicts over loans (the individual point estimate is statistically significant at 10%).

[8]Friendship is measured through a tangible social interaction that locally has a significant social meaning, namely, joining other households in drinking *Attaya*, a green tea.

et al. 2014). However, given their importance, it is of interest to understand 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 changes in specific markets, namely the arrival of formal institutions for credit and insurance. These papers demonstrate that the arrival of formal institutions leads to a reduction in informal arrangements between network members (Binzel et al., 2013), but also spill over into other realms, reducing interactions in networks not directly affected by the intervention (Banerjee et al., 2018) and interactions with households who do not benefit from the new institution (Cecchi et al., 2016). Comola and Prina (2015) show how benefits of development interventions can propagate through a network while the intervention simultaneously alters the network's configuration, and that failure to understand and account for this affects the effectiveness of development programs as well as the measurement thereof. This paper contributes to this emerging literature on the effect of economic shocks on social structure and interactions within networks by exploiting the exogenous CDD assignment. The funds allocated to villages were substantial and the villages were fairly unrestricted in how they could use the funds. Therefore, the intervention differed from those in the above-cited studies, in that it (a) constituted a direct shock to the existing stock of (productive) capital in the village economies; and (b) was not limited to one particular market, but had potential implications for various markets (e.g., markets for labor inputs, food outputs or credit).[9]

Second, this paper adds to the literature that studies the role of economic and social networks in the context of informal economies, the structure of networks, and the determinants of bilateral links.[10] One crucial question is how bilateral links are formed and sustained. Chandrasekhar et al. (forthcoming) show how cooperation — in situations lacking formal enforcement — can be achieved and sustained through social networks, highlighting the role of social proximity. An important recent methodological contribution is the concept of supported relations (i.e., a relation between agents i and j for which an agent k exists, who is linked to both i and j; Jackson et al., 2012). Our data allow us to contribute to that recent literature on how bilateral links are sustained through common ties. We confirm the importance of support, as a measure of social proximity, for economic interactions and find that this importance is reduced by the treatment. Further, we have detailed data on six different economic networks, which we collect for all households within each village. Using this data we can contribute to a better understanding of network structures (independent of the random treatment) and add to the still relatively small set of papers (e.g., Comola and Prina, 2015; Cruz et al., 2017) that are able to avoid the biases of network analyses based on samples of households (Chandrasekhar

[9]None of the projects implemented in Gambian CDD treatment villages are related to financial or insurance markets directly.

[10]While the literature has mainly focused on the analysis of networks created by social interactions and mutual help arrangements, recent empirical evidence has uncovered the importance of the networks created by informal exchanges of factors of production among community members and how informal exchanges of factors involved in the traditional production process overlap with other social and economic networks. Udry and Conley (2004) analyze detailed data on networks of land and labor, as well as networks of information and credit, collected in four villages in Ghana. Krishnan and Sciubba (2009) study data from Ethiopia and show that the architecture of labor-exchange networks is relevant for economic performance.

and Lewis, 2016). Moreover, we investigate reciprocity within and across networks, making use of the fact that our data spans multiple economic networks.

Third, we add to the literature on the effects of CDD programs. Significant attention has been given to local participatory development programs within the last 15 years, and CDD programs are one major modality. Regarding the effects of CDD programs on immediate economic outcomes, several meta analyses reviewing a large body of evidence conclude that CDD programs have led to modest improvements in infrastructure delivery and sustainability (Mansuri and Rao, 2012; Wong, 2012; White et al., 2018; Casey, forthcoming). In addition to economic effects, it was also hypothesized that CDD programs, through their focus on inclusive decision making processes, would positively affect social capital and change power structures. These are supposed to lead to better village-level decisions, particularly regarding development projects, beyond the CDD program. Some CDD programs, especially those operating in post-conflict environments, focus specifically on these aspects, including designs to affect community-level decision making. Nevertheless, there is only very limited evidence for positive changes regarding institutions (Fearon et al., 2009, 2015; Casey et al., 2012; Beath et al., 2013; King and Samii, 2014; Humphreys et al., 2015). There is mixed evidence of CDD programs' effects on social capital, defined as membership and contributions to groups (e.g., Labonne and Chase, 2011; Nguyen and Rieger, 2017), including some that mirror our finding of negative effects (Avdeenko and Gilligan, 2015). Similarly, regarding within-community social cohesion and bridging social capital with local leadership, no or even weak negative effects have been documented (White et al., 2018). We contribute to the CDD-specific literature by studying direct economic effects as well as the social and institutional effects that CDD programs may have on decision-making processes within the village. 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 analysis of economic networks. This network perspective is especially useful since it is directly concerned with the interaction of social structure and economic activity. Further, we investigate effects around four years after the implementation of the CDD program, which adds a medium-run perspective to some existing studies that have looked at CDD programs' shorter-run effects.

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., 2013). Recognizing this, CDD programs have provisions to minimize elite capture. In the case of the Gambian CDD program, communities were required to have village development committees (VDCs) that include members of groups that are typically marginalized (young or female individuals).[11] Further, the traditional chiefs are not part of the VDCs and only function as advisers. One additional contribution of our paper is therefore to use networks analysis to confirm that those groups considered as central (marginal) by the CDD program funding organizations are indeed central

[11]The concept of VDCs was already introduced in The Gambia in 2002 (Local Government Act, 2002, §93.1) and most villages had some form of VDC by the onset of the CDD program. However, in many places the rules regarding composition and representativeness were only effectively enforced with the implementation of the CDD program (see GoTG, 2006)

(marginal), as evidenced by their position in the networks of exchanges. We also conduct an analysis of heterogeneous effects of the treatment to identify signs of elite capture. Furthermore, elite capture is closely tied to the possibility of internal disputes, and our analysis also contributes to the fairly small literature relating development projects to internal conflicts. On the one hand, elite capture may lead to unequally distributed benefits and poorly performing projects. On the other hand, provisions to avoid elite capture, such as the parallel institutions that are set up at the village level to run the programs, might be considered a threat by traditional authorities. Both scenarios have the potential to induce conflicts (Barakat, 2006; Morel et al., 2009; Barron et al., 2011; King and Samii, 2014).

Finally, we contribute to the literature on the role of networks to provide insurance against shocks (e.g., Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006; Fafchamps and Gubert, 2007). We add to this literature by investigating the reaction to shocks across a larger number of economic dimensions, by exploiting data on full village-networks in a larger number of villages, and by considering differences induced by the exogenous treatment.

The remainder of this paper is structured as follows. Section 2 provides background information on the context and the CDD program, and describes our data and the empirical strategy. Main results are presented and discussed in Section 3, while Section 4 contains additional results related to mechanisms. Finally, in Section 5 we study welfare implications of our results. Section 6 concludes.

2 Background, Data, and Empirical Strategy

2.1 The Gambian Community-Driven Development 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.[12] The participative process in CDD programs is expected to contribute to improvements in economic conditions through better targeting, reduced implementation costs, improved maintenance, and efficiency in allocation.[13]

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

[12]These kinds of programs represent between 5% and 10% of the overall World Bank lending portfolio (Wong, 2012). This corresponds to around US$85 billion in supporting close to 400 programs in 94 countries during the last decade (Mansuri and Rao, 2012). A major modality are the CDD programs, of which (in 2017) there are "187 active CDD projects in 77 countries totaling $19.1 billion [...]. An additional $13.4 billion has been provided by borrowers and other donors." (World Bank, 2018)

[13]We study a CDD program that allocates funds towards village-level income-generating activities, and public service delivery. There are other CDD-type programs that seek to improve public services indirectly, e.g., through monitoring (e.g., Björkman and Svensson, 2007) and audits (e.g., Olken, 2007).

in 2003) were eligible for the program.[14] 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.[15] 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 will refer to village-level projects as "sub-projects". The random assignment of the treatment was stratified at the ward level.[16] The remaining eligible villages (435) did not receive village-level projects at that time. Thus, unlike CDD programs in many other countries, the Gambian CDD program involves no competition over grants, and the communities that are part of the program are guaranteed a certain amount of funding.

The Gambian CDD program followed the design of a typical CDD program, which promotes community involvement at all stages of the process from identification of the potential sub-projects to their maintenance after implementation. In order to select the village-level sub-projects, each village had to follow a long decision-making process involving several local and external actors (GoTG, 2006). According to the guidelines, as a first step, a CDD facilitator organized a series of village meetings and consultations with community-based organizations, with the goal of identifying and preparing a Strategic Development Plan (SDP) that summarizes a three-year projection of the village's perspectives. The SDP had to be confirmed by the whole village in a special meeting supported by the CDD facilitator. Subsequently, the priorities contained in the SDP were translated into a list of sub-projects, whose feasibility was analyzed by the CDD regional team. As a last step of the decision-making process, feasible sub-projects were presented to all the villagers for the selection of sub-projects to be financed by the CDD program.[17] After project selection, community members were also involved in the implementation and maintenance of the investments. Villages were expected to contribute at least 10% of the project cost in cash and/or in kind, e.g., through labor or complementary infrastructure. The scope of the program-induced village-level activities is best illustrated by the fact that the implementation manuals used by the local facilitators mandated 38 village meetings in the course of project implementation, 20 of which were intended to involve the whole village, while the other 18 involved meetings of community-based organizations (CBOs), the VDC, or other subgroups of the village (GoTG, 2006).

[14] A sub-component of the program allocated resources to hamlets smaller than 100 inhabitants, as long as they cluster with other such small hamlets to create a cluster of at least 100 inhabitants. Another sub-component also considered ward-level projects.

[15] The mean of the first four variables listed in Table 1, Panel B was used for the poverty ranking.

[16] Wards typically comprise around 6-14 eligible villages. The year in which villages received the funding was also randomly selected. Half of the villages were randomly chosen to start the implementation in 2008/09, while the remaining half started in 2009/10 (GoTG, 2006).

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

The Gambian CDD program directly targeted poverty reduction and income growth as well as capacity building and social cohesion, in contrast to many other CDD program designs that put a stronger emphasis on social outcomes (White et al., 2018).[18] Aside from the substantial amount of social and intra-village political interaction induced by the program, it indeed 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 levels. 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 one-half of the annual per capita income in The Gambia.[19] 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 programs were typically local public goods or club goods. In the CDD program's disbursements data, for all beneficiary villages, the most common sub-projects are: farm implements and inputs, milling machines, water pumps, seed stores and cereal banking, and draft animals. These are also the kinds of sub-projects most commonly implemented in the treatment villages of our sample.[20]

2.2 Village Selection

For this study, we use a sample of 56 villages, drawn from the set of rural villages that were eligible for the CDD program (i.e., below a certain poverty threshold) and had a population between 300 and 1,000 inhabitants in 2003 (according to the Census). The population restriction ensures a relatively homogenous sample of rural villages and made it feasible to conduct full village censuses. A two-stage procedure was used. First, a set of wards was randomly chosen from all wards with eligible villages. Second, within wards an equal number of control and treatment villages was randomly selected from the set of all villages of the desired population

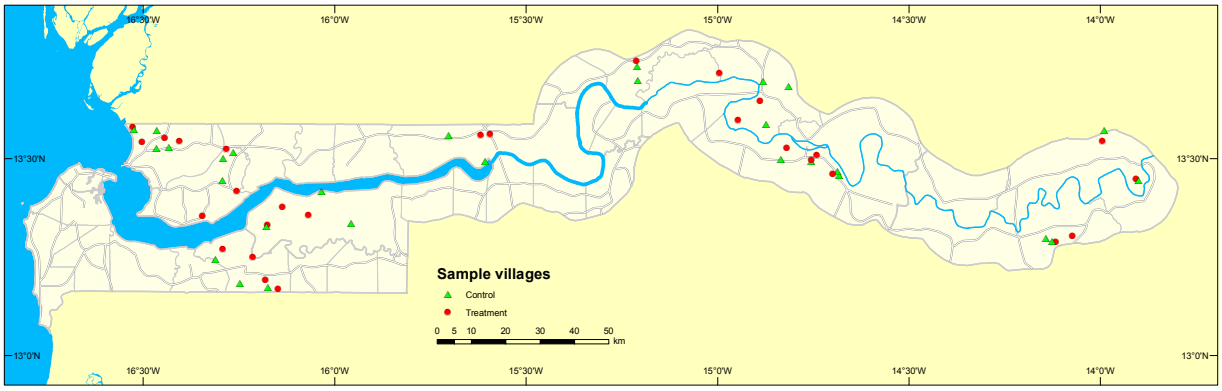
[18] According to official program documents, growth and poverty reduction constitutes the first of three focus pillars of the CDD program, alongside coverage of basic social services needs, and capacity building (World Bank, 2006). At the implementation level this is mirrored by the villages' sub-project proposals. Using administrative data, we find that 59% of all CDD villages indicate income generation and growth as a goal for their sub-project.

[19] To illustrate the magnitude of the shock, consider that four treatment villages in our sample chose to buy a tractor. In our control villages, no household owned a tractor, and only every second household (53%) owned a (simple) plough.

[20] In the 28 treatment villages of our sample, the most common sub-projects are: milling machines (14), hand pump wells (6), vegetable gardens (5), tractors (4), draft animals (4), and fertilizer (4). Recall that villages often implemented more than one sub-project. A more detailed discussion of different project types in all participating rural villages can be found in Heß et al. (2018).

size.[21] Figure 1 shows that sample villages are distributed across most of the different cultural and geographical zones of rural Gambia.

Figure 1: Sample Villages in The Gambia



Because our main specifications use only endline data, it is important to show that treatment and control group are comparable at baseline. Table 1 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). None of the pre-treatment characteristics in Panels A and B show statistically significant differences between the treatment and the control group. Out of the six household-level control variables, based on post-treatment data, one exhibits a statistically significant difference between the two groups (at the 10% level).[22]

2.3 Data Collection

The main data used in this paper were collected between April and June of 2014 at four different 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. Village focus group discussions (FGDs) were held with village authorities to obtain a better understanding of the villages'

[21] The sample was originally drawn in a first round of network data collection that took place in 2009, during the implementation of the Gambian CDD program (these data are described in detail by Arcand et al., 2010 and Jaimovich, 2015). In this original sample, 62 villages were selected. For the 2014 data collection for this study we excluded one urban ward (four communities) from the sample, in order to focus exclusively on rural villages. Aside from this, two villages had to be excluded from the data collection. In one village that was originally assigned to treatment, *Kerr Mod Ali*, all but a handful of households had followed a local religious leader, relocated right across the border to Senegal, and could not be interviewed. According to disbursement data this village did not receive funds. One control village was excluded because of incomplete data in the 2009 data collection. Our main results remain equally strong when we exclude either one or both wards where these two villages are located (see Appendix Tables A17, A18, and A19)

[22] The statistically significant difference occurs for the education status of the household head in 2014. Given all other results and our observations in the field, and the fact that the same imbalance is observed in the Census 2003 (4.4%, $p = 0.02$), we strongly believe that this is not an outcome of treatment but an accidental imbalance. This suggests a need to control for education, which we do in our main specifications, but our main results are also robust to not controlling for education. Note that we can not use the variables in Panel A of Table 1 as household-level controls, since we can not match our data to the data from the Census 2013 at a level below the village level.

Table 1: Summary Statistics and Balance

	Mean		Observations		Difference		p-value	
	(1) control	(2) treated	(3) control	(4) treated	(5) raw	(6) cond.	(7) CRSE	(8) RI
Panel A: individual-level household 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	0.17
mandinka	0.46	0.55	13820	13969	0.097	0.099	0.21	0.27
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	0.70
Panel B: village-level controls (2003)								
electrification rate (%)	3.22	2.37	28	28	−0.847	−0.725	0.44	0.45
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	0.23
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)								
elite	0.11	0.14	1358	1416	0.030	0.029	0.12	0.18
marginalized	0.23	0.24	1358	1416	0.006	0.013	0.54	0.59
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	0.72
household size	13.48	13.35	1358	1416	−0.125	−0.281	0.74	0.77
Panel D: networks links per household (2014)								
economic	3.32	2.87	1358	1416	−0.446	−0.440	0.03	0.07
-land	1.14	1.02	1358	1416	−0.119	−0.151	0.16	0.23
-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

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 and B stem from the Gambian Census 2003, with the exception of the household count, which is the de-facto count of interviews in 2014. Panels C and D are based on data collected by the authors. Numbers reported in Panel D represent the undirected, unweighted network degree of the households, i.e., the number of distinct exchange partners from within the village, irrespective of the number and direction of transactions with these households. Variables displayed in Panels B and C are the control variables used for all regressions below. Population and number of households enter the regressions logarithmically. The indicators for elites and marginalized households enter the regressions only where indicated.

main developments in the last years, including externally-funded projects, as well as information about village community groups. Finally, a village questionnaire was completed by our enumerators, who were instructed to rate various observable aspects of the social and economic village infrastructure. 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 (details of this procedure are described in Jaimovich, 2015).[23] Networks were elicited using a 'name generator' procedure (Campbell and Lee, 1991). Respondents were asked to name villagers with whom they had exchanges — 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 exchanges, information about connections external to the village were recorded as well. We did not impose a strict limit on the number of reported partners. 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 sizable transactions, approximately equivalent to the amount that would make up a meal for the household. This was of negligible practical relevance as 97.6% of households reported three or fewer *Food*-exchange partners.[24]

For each transaction we recorded the direction, quantity, and further specifics (such as whether there was payment involved) of the exchange and treat the network of exchanges as a directed graph. Given the directed nature of our survey questions, a household declaring a link can be either the source or the recipient of the transaction. To construct the networks we aggregate the two potential link reports from both households in each directed dyad, i.e., if at least one of the two households declared a link in a given direction, we consider this directed transaction to have occurred.[25] Figure 2 illustrates the resulting network.

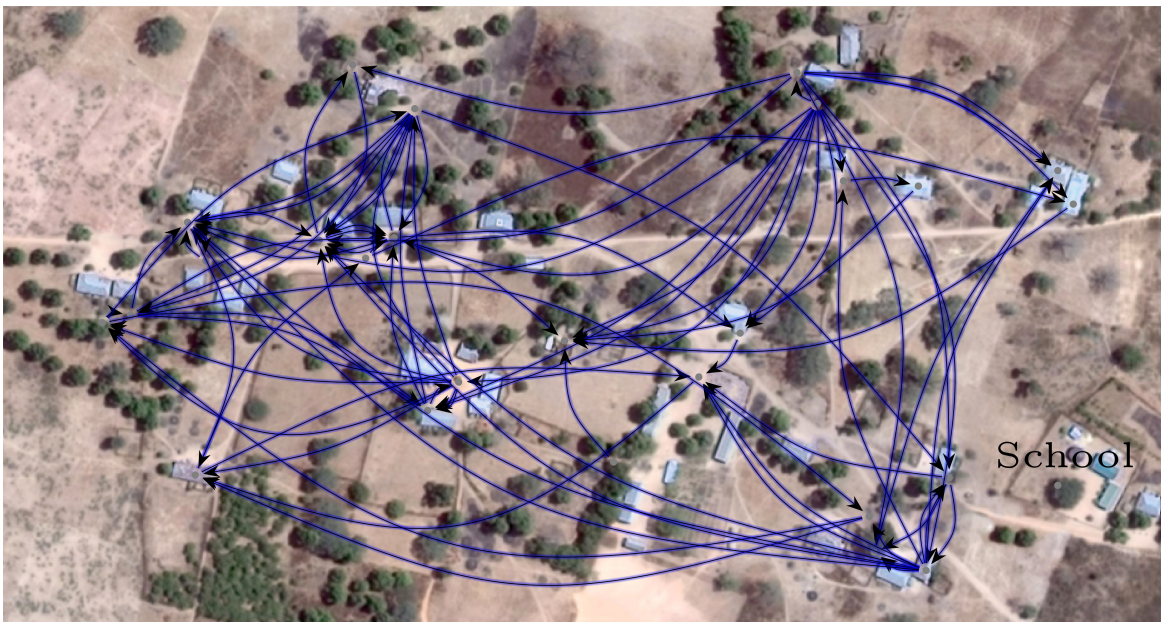
[23]Our network data uses households as the basic unit of analysis, but household definitions are not always straightforward. This is particularly the case in the West African context (Beaman and Dillon, 2012). In The Gambia, villages are typically organized into *compounds*, a group of huts usually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

[24]Enumerators were also given the discretion to record more than four links in cases where more transactions of substantial magnitude were reported. This indeed happened. Of the 2.4% of households that reported having more than three exchange partners for *Food*, over a quarter reported strictly more than four partners.

[25]This procedure makes the implicit assumption that each link 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. What is important for our

Aside from confirming information given during the main survey and 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, asked them to provide details about their implementation process, and to rate them with respect to functionality and benefit.

Figure 2: Example Village Network



Notes: In order to protect the anonymity of the respondents, the map's orientation was changed and important landmarks were altered. Background satellite imagery is based on Google Maps composites. Blue arrows represent economic links between pairs of households in the indicated direction.

2.4 Data Description

Our sample consists of very poor rural communities. Data from the Census 2003 (before the CDD program was implemented), presented in Panel B of Table 1, show that households mostly did not have access to electricity or an improved water source. Less than two thirds of all individuals were literate, though 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. Accordingly, in our data the average village has roughly 670 inhabitants.[26] In Panel C of Table 1 we show some of the household-level variables collected in our main survey. The mean household size is 13 people, and villages have on average 50 households.

study is that the rates at which links are reported by both households of a dyad do not differ between treatment and control villages.

[26]The Census 2013 lists on average 565 inhabitants. These lower numbers are likely the consequence of three factors. First, the Census used village demarcations defined by a previously conducted country-wide mapping which sometimes broke disbursed settlements into smaller units. In our data collection we were careful to include all households that were considered to belong to same village chief. Second, the Census 2013 was a de-facto census of the census night, while our numbers are based on an aggregation of the household sizes reported by the household heads, which potentially include some absent residents, such as labor-migrating children of the head. Third, our data was collected 14 months after the Census 2013.

In our control group, 11% of household heads declared themselves to be a first-degree relative or the spouse of the village chief. We define this immediate family of the chief as well as the Imams' households as the elite. 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 vulnerable, and we categorize them as potentially marginalized. They constitute 23% of the households in our control group.

Panel D of Table 1 shows summary statistics for the network data. Across all economic networks, the average household in the control group lists 3.3 exchange partners during the year previous to the survey. Exchanges of *Food* were most common, with an average of 1.9 exchange partners per household.

2.5 Empirical Strategy

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

$$\ell_{ijmvw} = D_{vw}\tau + X_{ijvw}\alpha + \beta_w + \gamma_m + \varepsilon_{ijvw}, \tag{2.1}$$

where the dependent variable ℓ_{ijmvw} indicates existence of a directed link in network m from household i to household j, located in village v of ward w and takes on the value 100 whenever there is a link, 0 otherwise, so that coefficients can 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 a CDD program was implemented in village v. 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 Table 1, we control for the existence of kinship ties, shared ethnicity, interview group and enumerator fixed effects. The results are robust to alternative specifications of the control variables.[27]

Given that we have data on six different economic networks and no prior regarding in which network to find treatment effects, we need to take measures against over-interpreting individual significant treatment effect estimates. Thus, in our main specification, we pool observations from all economic networks together and control for network fixed effects, γ_m. We refer to this as the pooled network specification. Additionally, we create an index summing up dyadic

[27] To alleviate concerns about p-hacking, we investigate the robustness of our results to using alternative sets of control variables. Appendix Figure A3 visualizes the sensitivity of the point estimate to the omission of individual or combinations of control variables. While using fewer control variables tends to increase our standard error estimates, our ATE estimate remains qualitatively unchanged.

link indicators across networks, as described by Anderson (2008).[28] We refer to this as the VCV-index specification. While the pooled network specification also allows for an economic interpretation of coefficient estimates and for comparisons across specifications, this is not possible for the VCV-index specification. The VCV-index specification solely serves the purpose of addressing concerns regarding multiple hypothesis testing and only the sign and significance of the coefficient can be meaningfully interpreted.

For an easier interpretation of the point estimates, equation 2.1 is estimated using a linear probability model. Our main results are unchanged if a probit model is estimated instead (Table A20 in the Appendix). Further, our results are equally confirmed through randomization inference which does not rely on regression model assumptions, as explained in greater detail below.

We are not only interested in the effects of the CDD program on the probability of an economic link between villagers, but more generally on how the embeddedness of the households in the different economic networks in the village changes with the project. A basic metric of the embeddedness of a node i in a network is degree centrality, measuring the number of exchange partners a node is connected with. In the case of our data, a distinction based on the directionality of the link can be made. If the link goes from i to j, then, for household i, it is considered in the measure of the out-degree, defined as $d_{im}^{\text{out}} = \sum_j \ell_{ijm}$. When the link goes in the other direction, from j to i, it is counted as part of the in-degree of i in network m, defined as $d_{im}^{\text{in}} = \sum_j \ell_{jim}$.[29] To analyze the effect of the CDD program on household degree centrality, we estimate a linear specification of the following form at the household level:

$$d_{imvw} = D_{vw}\tau^{\text{h}} + X_{ivw}\alpha^{\text{h}} + \beta_w^{\text{h}} + \gamma_m^{\text{h}} + \varepsilon_{ivw}, \tag{2.2}$$

where the dependent variable is household-level degree centrality. Again, all networks are pooled and the regressions control for ward fixed effects, network fixed effects, and a vector of household variables (X_{ivw}). In Equation 2.2, the superscript 'h' is introduced to distinguish parameters from those in Equation 2.1. We use an equivalent specification to estimate average treatment effects for outcomes at the household level which are not based on network data.

Finally, we also study network measures at the village level. We use several measures capturing different aspects of the topology in a given network, namely: network density, average inverse path length, the network clustering coefficient, and the total number of closed triads.[30]

[28] Within each network, link indicators are standardized using the control group's mean and variance. A weighted average of these variables is computed, setting the weight on each variable to the corresponding row sum in the inverted variance covariance (VCV) matrix of these variables. The final index is obtained by standardizing the resulting variable again to have mean zero and variance one in the control group. This specific type of weighting is chosen to maximize the information content of the resulting index. For example, two highly correlated variables would receive weights that sum up to the weight of a single variable that is not correlated with other variables. For more details see Anderson (2008, p. 1485 and Appendix A therein).

[29] In the credit network, for example, the out-degree of i relates to its position as a lender and the in-degree is a characteristic of i as borrower.

[30] Network density is defined as the sum of all existing directed links in a network over the number of all directed dyads, i.e., the number of possible links. The density can be interpreted as the probability of observing a link between two randomly chosen households. The average inverse path length is the inverse of the average number

The effect of the CDD program on a village-level network characteristic, Y_{vw}, is analyzed using the following model:

$$Y_{vw} = D_{vw}\tau^{\mathrm{v}} + X_{vw}\alpha^{\mathrm{v}} + \beta_w^{\mathrm{v}} + \varepsilon_{vw}. \tag{2.3}$$

Village-level network characteristics capture the interplay of links between different dyads and networks, such as triangles formed by three transactions on three different networks. Thus, in order not to neglect such phenomena, we consider an aggregate 'economic' network that is the union of all six economic networks. In this aggregation a link between i and j exits whenever there is a link between i and j in *any* of the six individual economic networks. Network density, average inverse path length, the network clustering coefficient, and the total number of closed triads are computed based on this aggregation and analyzed using Equation 2.3. We refer to the specification using this aggregate network as the max-network specification.

In addition to the various aggregation levels for our network data, some of our data pertain to features of development projects, such as whether acquired machinery is still operational, or whether there was a vote to implement the project. These data were elicited by asking each respondent the same set of questions about each individual development project (including CDD sub-projects) that had been implemented in the village during the preceding five years. Thus, a project p is defined at the village level, and villagers reported up to five CDD sub-projects and up to six non-CDD projects. To estimate treatment effects on project-level outcomes or to compare characteristics of CDD and non-CDD projects we use the model:

$$Y_{pivw} = D_{vw}\tau^{\mathrm{p}} + \mathrm{CDD}_{pvw}\pi^{\mathrm{p}} + X_{ivw}\alpha^{\mathrm{p}} + \beta_w^{\mathrm{p}} + \varepsilon_{vw}, \tag{2.4}$$

where Y_{pivw} is an attribute of project p as reported by household i in village v. CDD_{pvw} is a binary variable indicating CDD sub-projects and X_{ivw} are the usual household-level control variables. τ^{p} captures differences between projects implemented in treatment villages and projects in control villages, while π^{p} captures differences between CDD and non-CDD projects.

Across our specifications the units of observation vary between dyads, households, villages, and project reports. To ensure comparability across specifications, we always weight observations to ensure proportionality to village size, i.e., observations in village-level regressions are weighted using the number of households, while in dyadic regressions the inverse of the number of households is used as weights.[31] Project report regressions are weighted by the inverse of the number of project reports elicited from the respective household, in order to ensure that households are weighted the same, irrespective of the number of projects.

of households on the shortest undirected path between any two nodes in the network. It is a measure for how compact a network is. Finally, the clustering — sometimes also referred to as transitivity — measures the propensity of nodes in a network to form triangles. Specifically, it is defined as the number of closed triads (where every node links to or from the two other nodes) over the number of connected triads (where there exists an undirected path from each node to the two other nodes). The number of (undirected) closed triads is also used as a separate dependent variable.

[31] Note that the number of dyads grows quadratically in the number of households.

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 thus always rely on cluster-robust standard errors, clustered at the village level, and ward fixed effects, to account for the stratification (see Bruhn and McKenzie, 2009; Bugni et al., forthcoming). Additional issues result from the small number of treatment clusters (56) combined with heterogeneous village size (see MacKinnon and Webb, 2017), and ward-level stratification, which induces substantial intra-ward correlation of treatment. Our sample contains 4 to 8 villages per ward with household numbers per village ranging from 21 to 106 and consequently dyad numbers ranging from 210 to 11,130. Thus, while within each ward exactly 50% of villages are treated, household treatment rates vary between 33% and 65% across wards and treatment rates of dyads vary between 19% and 80%. This within-ward correlation of treatment poses a threat to inference because most of our outcomes are likely to be correlated with ward-level unobservables and our sample comprises only 13 wards.

Thus the standard approach using cluster-robust standard errors is potentially problematic and we additionally rely on randomization inference (see Fisher, 1935; Rosenbaum, 2002) to test the causal effects of the randomized treatment. Randomization inference uses the logic that counterfactual outcomes and observed outcomes are identical under the *exact null hypothesis* of no treatment effect:

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

to derive the distribution of a test statistic under that null hypothesis. The observed sample realization of the test statistic is compared against the derived distribution to assess whether the results significantly differ from what would be observed under the null.[32] All regressions where randomization inference is applicable indicate significance based on randomization inference as well as cluster-robust standard errors.

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

3.1 Average Treatment Effect

We start by estimating the average treatment effect that the Gambian CDD program has on the networks of economic exchanges in rural Gambia. Our first empirical test shows how the

[32] We implement randomization inference as follows (for details see Heß, 2017). First, for each regression we compute the t-statistic corresponding to each treatment coefficient of interest, t_{sample}. In regressions where treatment is interacted with other variables, we apply the same procedure to all treatment coefficients. Second, we draw $R = 10,000$ hypothetical realizations of the treatment variable, exactly following the original treatment assignment (village-level treatment, stratified by ward) and compute the same t-statistics, $\{t_r\}_{r=1,\ldots,R}$, based on these realizations. The obtained set of hypothetical realizations of the t-statistics are independent draws from the distribution of t-statistics under the sharp null hypothesis of no treatment effect. Lastly, we assess the significance of the true sample realization of the t-statistics by computing the share of hypothetical realizations that lie further away from zero than the actual estimate, to obtain a p-value for the null hypothesis of no treatment effect: $p^{\text{RI}} = \frac{1}{R}\sum_{r=1}^{R} \mathbb{1}(|t_{\text{sample}}| < |t_r|)$.

probability of having an economic link is affected by treatment. In order to do this, we use the dyadic specification described in Section 2.5 (i.e., Equation 2.1 without the network fixed effects γ_m).

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

	(1) land	(2) labor	(3) inputs	(4) food	(5) gifts	(6) credit
treatment	-0.106	-0.179	0.037	-0.791	-0.387	-0.237
	(0.429)	(0.163)	(0.871)	(0.000)$^{\bullet\bullet\bullet}$	(0.001)$^{\bullet\bullet\bullet}$	(0.066)$^{\circledast}$
controls (see notes)	✓	✓	✓	✓	✓	✓
dyads	151632	151632	151632	151632	151632	151632
households	2774	2774	2774	2774	2774	2774
control mean dep. var.	1.168	1.536	1.886	1.952	0.910	1.171

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets $\bullet$ indicate significance levels preserved under randomization inference, while 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group. Indicators for elite and marginal households are also included but not shown in this table.

The results for each of the six economic networks are displayed in Table 2. Five out of the six estimated coefficients are negative and in two cases this effect is significant at the 1% level, based on both cluster-robust standard errors and randomization inference (indicated by $^{\bullet}$). The treatment effect estimate in the *Credit* network is statistically significant at the 10% level, based on cluster-robust standard errors, but not based on randomization inference (indicated by $^{\circledast}$). The one point estimate that is positive is small relative to the control-group mean and has a large p-value of 0.87 (based on clustered standard errors). The individual networks in which the program is estimated to have the largest effect are *Food* and *Gifts*, where the point estimate implies a reduction in the probability of a link of around 45% of the mean in control villages.

As we have data on six different networks, we are testing a large number of hypotheses. To deal with the problem of multiple hypotheses testing, we aggregate the data from the six individual networks as described in Section 2.5 and focus on the aggregated results presented in Table 3. The first two columns present aggregations of the individual results in Table 2. In the specification of column 1, the six economic networks are pooled together, and therefore the level of observation is at the household×network-level (thus we have six times more observations than in Table 2). Coefficient estimates in the pooled specification have to be interpreted as the effect of covariates on link existence, averaged across all six networks. The results in Table 3 show that the effect of the program is a large and statistically significant reduction in the probability of the existence of an economic link. The point estimate for the average treatment effect in column 1 implies a reduction of 0.277 percentage points in treatment villages. Considering the mean in the control group, this implies that the probability of two households forming

an economic link is 20% lower in treated villages than in control villages. Based on cluster-robust standard errors, allowing for correlation of the model error within villages, this effect is significant at the 1% level, while using randomization inference the significance level is 5%.

Table 3: Dyadic Regressions, Treatment Effects and Treatment Effect Heterogeneity

	average TE		by household types		reciprocal links		by support	
	(1) economic (pooled)	(2) economic (vcv)	(3) economic (pooled)	(4) economic (vcv)	(5) economic (pooled)	(6) economic (vcv)	(7) economic (pooled)	(8) economic (vcv)
treatment	-0.277	-0.048	-0.332	-0.057	-0.226	-0.042	-0.153	-0.029
	(0.002)$^{\bullet\bullet\circledast}$	(0.002)$^{\bullet\bullet\circledast}$	(0.004)$^{\bullet\bullet\circledast}$	(0.003)$^{\bullet\bullet\circledast}$	(0.016)$^{\bullet\bullet}$	(0.012)$^{\bullet\bullet}$	(0.121)	(0.083)$^{\circledast}$
elite^{any}	0.409	0.072	0.338	0.060	0.180	0.027	0.388	0.069
	(0.000)***	(0.000)***	(0.008)***	(0.006)***	(0.034)**	(0.077)*	(0.000)***	(0.000)***
marginal^{any}	-0.100	-0.018	-0.133	-0.023	0.008	0.003	-0.118	-0.020
	(0.075)*	(0.065)*	(0.119)	(0.114)	(0.878)	(0.793)	(0.037)**	(0.033)**
elite^{any} × treatment			0.134	0.022				
			(0.465)	(0.479)				
marginal^{any} × treatment			0.066	0.010				
			(0.485)	(0.519)				
support							0.113	0.017
							(0.003)***	(0.005)***
support × treatment							-0.085	-0.013
							(0.044)$^{\bullet\bullet}$	(0.054)$^{\bullet}$
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓		✓		✓		✓	
dyads	909792	151632	909792	151632	454896	75816	909792	151632
households	2774	2774	2774	2774	2774	2774	2774	2774
control mean dep. var.	1.437	0.000	1.437	0.000	0.677	0.000	1.437	0.000
mean support							1.209	1.209

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors.Units of observation are directed dyads, except in columns 5 and 6, where undirected dyads are used. The pooled specification stacks observations for each individual network and controls for network fixed effects. Thus the number of observations is sixfold in these columns. The dependent variable takes on the value 100 if a dyad had a link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. For the undirected dyads, each pair of households appears only once and thus the number of dyads is half of that in other columns. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group. The variables $elite^{any}$ and $marginal^{any}$ indicate whether any of the two households in a dyad belongs to the village elite or the group of marginal households.

The specification in column 2 confirms the results based on the pooled specification (as explained in section 2.5, the VCV-specification only serves the purpose of addressing concerns regarding multiple hypothesis testing and only the sign and p-value can be meaningfully interpreted). Our results are robust to a battery of robustness tests and alternative specifications that are reported in the Appendix, namely: using 210 alternative control variable specifications (Figure A3); excluding either one or both wards with unequal numbers of treatment and control villages (Tables A17, A18, A19, as discussed in footnote 21);using probit instead of a linear probability model (Table A20); using link intensity instead of a binary indicator (Table A21); treating the networks as an undirected graph (Table A22); linear regression without household-

and dyad-level controls (Table A23); and using similar data collected in 2009 by Jaimovich (2015) to obtain a difference-in-differences estimator (Table A24).[33]

Table 4: Degree in the Village-Internal Network

	(1) economic (pooled)	(2) economic (vcv)	(3) land	(4) labor	(5) input	(6) food	(7) gifts	(8) credit
Panel A: in-degree								
treatment	−0.138	−0.263	−0.067	−0.053	−0.082	−0.348	−0.146	−0.130
	(0.000)●●●	(0.000)●●●	(0.191)	(0.357)	(0.325)	(0.000)●●●	(0.001)●●⊛	(0.025)●⊛
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓							
control mean dep. var.	0.68	0.00	0.57	0.72	0.89	0.94	0.43	0.56
households	16644	2774	2774	2774	2774	2774	2774	2774
Panel B: out-degree								
treatment	−0.124	−0.160	−0.093	−0.063	−0.097	−0.302	−0.132	−0.055
	(0.002)●●⊛	(0.001)●●⊛	(0.077)⊛	(0.244)	(0.254)	(0.000)●●●	(0.005)●●⊛	(0.380)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓							
control mean dep. var.	0.68	0.00	0.56	0.71	0.88	0.93	0.43	0.54
households	16644	2774	2774	2774	2774	2774	2774	2774

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. The pooled specification stacks observations for each individual network and controls for network fixed effects. Thus the number of observations is sixfold in these columns. Total in- and out-degree measures vary within villages because some households report links but conceal the identity of their partner. This is most common for money lending and alms (*Zakat* in Islam), which — for religious reasons — tend to be reported by the recipient only. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies.

The reduction in economic interactions as a result of the program is confirmed when we aggregate the network information at the household and village levels. Table 4 shows the result of the estimation of equation 2.2, using household degree centrality as the dependent variable. The coefficient -0.138 in column 1 of Panel A indicates that households in treatment villages receive transactions from almost 20% fewer exchange partners than households in control villages. Interestingly, the effect is similar for both in- and out-degree (Panels A and B).

Table 5 suggests that the network density as well as the average inverse path length between households is reduced. These results indicate a reduction in the overall economic connectedness of households. Further, Table 5 suggests that the network-level clustering coefficient (columns 5 and 6) decreased for the average economic network in treatment villages (column 6), though the corresponding coefficient for the max-network specification is insignificant at conventional levels (p-value=0.104, column 5). Clustering measures the propensity of nodes in a network to close triangles of households that are already connected. Correspondingly, the total number

[33]We do not use the difference-in-differences specification as our main specification, since the first round of data collection happened during early stages of the roll-out of the program and we can not regard the data from 2009 as a clean baseline for our analysis. Nonetheless, for the sake of completeness, we conducted an analysis using these data and find results which are qualitatively comparable to those in our preferred specification.

of observed triangles is also reduced (columns 7 and 8).[34] One possible interpretation of the observed results is that, rather than socially embedded or reciprocal trades involving multiple households, economic transaction in treatment villages are the manifestation of one-shot market transactions. This finding is similar to the findings by Banerjee et al. (2018), who argue that links in closed triads are self-supporting and thus potentially break apart entirely once a single link is severed.

Table 5: Village-Level Network Regressions

	(1) density in % (max)	(2) density in % (pooled)	(3) avg. inv. path length (max)	(4) avg. inv. path length (pooled)	(5) clustering in % (max)	(6) clustering in % (pooled)	(7) closed triads (max)	(8) closed triads (pooled)
treatment	-1.026	-0.263	-3.795	-2.774	-2.518	-1.477	-10.567	-0.622
	(0.048)$^{\bullet\bullet}$	(0.003)$^{\bullet\bullet\circledast}$	(0.042)$^{\bullet\bullet}$	(0.001)$^{\bullet\bullet\circledast}$	(0.104)	(0.001)$^{\bullet\bullet\circledast}$	(0.003)$^{\bullet\bullet\bullet}$	(0.009)$^{\bullet\bullet\circledast}$
network fixed effects		✓		✓		✓		✓
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
village networks	56	336	56	336	56	336	56	336
control mean dep. var.	6.9	1.4	45.1	9.8	21.5	3.2	19.1	0.6

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are villages. The 'max' specification combines all economic networks into a single network. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B, as well as the shares of households belonging to each ethnicity and the shares of households interviewed by each enumerator.

3.2 The Role of Elites and Marginal Households

The above analysis shows an economically large and statistically significant negative average treatment effect. Table 3 also provides information with respect to the economic connectedness of different groups within the village. We focus on two groups that typically receive special consideration in CDD programs, including in the Gambian CDD program, namely elites and marginal households. The average difference in economic connectedness of these households relative to other households in the village is captured by the coefficient estimates for the two variables $elite^{any}$ and $marginal^{any}$, which indicate whether any of the two households in a pair belongs to the village elite (the village chief and his first-degree relatives as well as the Imam) or the group of marginal (young and female-headed) households. We expect that belonging to these groups, which are defined based on ex-ante criteria, also predicts network links.[35] We conjecture that elites are well connected because they are the most established families in a village, control a large share of the village's private agricultural landholdings as well as

[34] The discrepancy in mean dependent variables and effects observed between the max-network, which is the union of all economic networks, and the pooled-specification, which represents the average of all individual networks, suggests that most triangles are formed by a combination of links on multiple networks.

[35] When the concept of VDC — on which the implementation of the CDD program builds — was devised, the traditional chief was intentionally only given an advisory role (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 for example World Bank 2006, p. 4, or GoTG 2006, p. 9). Note that the composition of the VDC is endogenous and not used in our definition of elite.

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. And indeed, the findings (columns 1 and 2 of Table 3) confirm that elite households are significantly more connected than other villagers. For marginalized households the opposite holds.

Further, we explore if there is a differential effect of the program for these groups. Columns 3 and 4 of Table 3 show interactions of the CDD indicator variable with the variables that identify the group to which the households of a dyad belong. The coefficients for these interaction terms are not statistically significant at any conventional level and rather small in magnitude. Therefore, the project's effect on economic networks does not seem to be heterogeneous with respect to these different groups, and the average reduction in economic interactions is not driven by diminishing transactions between or within them. 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.

3.3 Reciprocity and the Role of Social Proximity

We hypothesize, based on Kranton's (1996) work, that reciprocal interactions are reduced when modern 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 links. With this information, we can define a variable that indicates if a particular transaction was reciprocated, meaning that a link has a counterpart in the opposite direction in any of the six economic networks.[36]

More precisely, we define reciprocity in a network m as a binary variable recip_{ijm}, such that:

$$\text{recip}_{ijm} = \mathbb{1}\left((\ell_{ijm}^{\text{unpaid}} \wedge \ell_{ji}^{\text{unpaid}}) \vee (\ell_{jim}^{\text{unpaid}} \wedge \ell_{ij}^{\text{unpaid}})\right),$$

where $\ell_{jim}^{\text{unpaid}}$ is an indicator for unpaid links from i to j in network m and $\ell_{ji}^{\text{unpaid}}$ for unpaid links in any economic network.[37]

In order to analyze if the CDD program had an effect on the reciprocity of the links, we estimate equation 2.1 with recip_{ijm} as the dependent variable. The results are shown in columns

[36] One technical concern could be that the directionality of good flows, on which the computation of this measure is based, is imprecisely measured via the survey. Whether good flows in both directions are recorded would then not capture reciprocation, but the rate at which both households state having an interaction at all, or the rate at which one household states the direction incorrectly. There is reason to believe this is not the case. Respondents made a clear distinction between reciprocal and non-reciprocal interactions. Many respondents unilaterally declared having had transactions involving bi-directional good flows with a specific exchange partner. For 80% of the dyads that we code as reciprocal, reciprocity is already implied in the reports made by a single households in this dyad.

[37] Paid transactions are generally rare in the sample villages. Only 13.5% of links in networks where payment information is recorded (i.e., excluding gifts and credit) actually do involve a payment. Only 25% [16%] of households have one or more incoming [outgoing] links involving payment.

5 and 6 of Table 3. Indeed, consistent with the above hypothesis, the results in Table 3 suggest that the CDD program causes a reduction in reciprocal exchanges. The coefficient is negative and statistically significant at the 5% level. It is not possible to distinguish whether or not this reduction in reciprocal exchanges is an independent effect from the overall reduction in links or a by-product. However, the magnitudes of the effect estimates suggest that reciprocal links are particularly prone to be severed in treatment communities: The average reduction in the probability of forming a reciprocal link is 0.226 percentage points, which corresponds to a drop of 33% compared to the control group.

A similar picture is drawn when analyzing the role social proximity plays in link formation. In settings where enforceability is a concern, bilateral exchanges are often facilitated by common ties, who support the bilateral exchange (Jackson et al., 2012). 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 through kinship 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.[38] Note that our support measure differs from that in Jackson et al. (2012) and Banerjee et al. (2018) in two important dimensions. First, our measure is not binary, but a count variable.[39] 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 Table 5, columns 7 and 8).

We consider this measure of support a proxy for social proximity of two households and thus expect it be an important factor for economic interactions. Indeed, in Table 3, columns 7 and 8, we document empirical evidence that support is a strong predictor of good flows in economic networks. In control villages, the probability for a pair of households to link in an economic network increases by 0.113 percentage points with each additional household supporting this relationship. This corresponds to a 0.23 percentage point increase in the probability of forming a link if support increases by one standard deviation (2.03), i.e., 16% of the mean dependent variable. However, in treatment villages this effect of support is reduced. The significant interaction of treatment with support suggests that the main reduction in links in treatment villages occurs between households with stronger support. Even more, in the pooled specifica-

[38] Both measures are unaffected by treatment (results not shown). Two households are considered neighbors if, for at least one household, the other is among the 10% closest in the village and not further away than the 25th percentile of pairwise within-village distances (on average 100m). Based on this definition, the average household has seven neighbors. Kinship is measured via the kinship network described in Section 2.3 and includes first-degree relatives and children's in-laws. In this network, the average household has kinship relations to three other households in the village (see Table 1, Panel D).

[39] We chose this because in our context almost all pairs of households have at least one common tie and meaningful variation exists only in the number of common ties.

tion, the treatment effect estimate for unsupported dyads is insignificant. This result is robust to controlling for the geographic distance of the dyad as well as kinship, and for their respective interactions with the treatment indicator (results not shown). 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 importance of common ties for link formation in treatment communities is also consistent with the reduction in the number of closed triads observed in treatment villages.

4 Mechanisms

In the previous section we have shown that there is a reduction of intra-village economic interaction caused by the CDD program. We have further documented that links between socially close households and reciprocal links were especially likely to be severed. In this section, we go through potential mechanisms that may explain our main findings. We will present evidence that projects were physically implemented and involved standard CDD procedures, which mandate significant community interaction. Nevertheless, our findings suggest that, in the medium-run, the projects lead to at most modest economic improvements. However, we still do find some evidence for a formalization of the economy. Finally, several pieces of evidence point to clearly unintended effects which bear the potential to cause village-internal divisions, namely elite capture, unequally distributed benefits, and failed projects.

4.1 Were Projects Implemented?

We first confirm a necessary condition for the CDD program to have any impact, namely that it was implemented, both physically as well as using the mandated procedures.[40] There is overwhelming evidence that CDD sub-projects were not only initiated but actually delivered and CDD procedures were followed.

First, sub-projects were physically implemented. Indeed, physical inspections by our enumerators during our fieldwork as well as survey evidence confirm implementation in all villages (evidence is summarized in Table A25).[41] Thus, all treatment villages received significant funds and implemented sub-projects, such as tractors, milling machines or seed stores, that resulted in substantial physical changes to the available economic infrastructure. Further, our surveys also allow us to confirm that households are generally very aware of those sub-projects (Table A26).[42]

Second, we investigate whether procedures were implemented as required by the CDD program's guidelines. Again, we confirm implementation. Using project-level reports from the

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

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

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

in-depth survey, we find that compared to non-CDD projects, CDD sub-projects are almost twice as likely to have involved the whole village or the VDC in decision making (a 17 or 23 percentage points increase respectively, as shown in Table 8). Also, evidence suggests that voting took place for at least some CDD sub-projects and voting is significantly more likely for CDD than for non-CDD development projects that were implemented in villages in the last 5 years.

4.2 Did Projects Lead to Intended Economic Change?

Although projects were delivered, they may not have had a meaningful economic impact in intended dimensions. If there is no or little impact, this may disappoint villagers. While any failed village development project would do so, in the case of externally driven projects there are other actors to blame. In the case of participatory projects, which are largely decided upon and managed by locals, internal actors might be blamed. Thus, the absence of economic changes may lead to quarrels among villagers (Barron et al., 2011), which may lead to social disruptions and reduce the willingness to cooperate, and might explain the reduction in economic interactions (Karlan et al., 2009).

Table 6: Economic Changes

	survey-based wealth and asset indicators					respondents' subjective assessment		
	(1) PCA wealth‡	(2) spending on food (USD)	(3) animals†	(4) cattle†	(5) total income†	(6) overall econ. condition	(7) own econ. condition	(8) benefited from any project
treatment	−0.348	18.463	−0.101	−0.072	−0.072	0.196	0.026	0.132
	(0.063)⊛	(0.523)	(0.069)⊛	(0.281)	(0.166)	(0.013)●●	(0.745)	(0.007)●●⊛
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
households	532	508	2772	2774	2527	531	549	2767
control mean dep. var.	0.2	646.2	0.9	0.6	3.8	3.1	2.8	0.6
dep. var. range						1 - 5	1 - 5	0 - 1

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies. ‡ Regressions for all individual wealth indicators are found in Appendix Tables A28, A29, and A30. † Dependent variables are transformed via the inverse hyperbolic sine function: $y^{\text{IHS}} = \log(y + \sqrt{y^2 + 1})$. The GMD USD conversion in column 2 is based on the exchange rate 0.048. Columns 1, 6 and 7 are based on the in-depth survey, where 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 8 is based on the question "Do you think that your household benefited from development projects implemented in the village in the last 5 years?" in the main survey.

When we consider various dimensions of welfare (households assets, animals and various income measures), we do not find a positive and statistically significant effect of the CDD program, as shown in Tables 6 and 7. In fact, for some measures there are significant negative impacts of treatment estimated, namely for the number of animals owned[43] and the wealth

[43]This, however, may be driven by the fact that many CDD sub-projects in the sample villages were geared towards bringing improvements related to growing crops (milling machines, tractors) — rather than benefiting animal husbandry — and villagers might have shifted into this area, at the expense of keeping livestock. Furthermore, draft animals are less valuable if the village has obtained a tractor from the CDD funds.

index. These are, however, only marginally significant, and insignificant under randomization inference. Table 6 shows that respondents to the in-depth survey in treatment villages are 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 6). However, they are not more likely to report an improvement in their own economic situation (column 7). In the larger sample, based on the main survey, households in CDD program villages are more likely to report that they have benefited from a development project within the past five years (column 8).[44]

Table 7: Census Assets

	assets				animals		
	(1) radio	(2) mobile	(3) TV	(4) bicycle	(5) sheep/goat	(6) poultry	(7) cattle
Panel A: only sample villages							
treatment	0.024	-0.037	-0.046	-0.024	0.090	0.017	0.884
	(0.70)	(0.48)	(0.42)	(0.76)	(0.83)	(0.99)	(0.30)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓
households	2964	2964	2964	2964	2967	2967	2967
villages	56	56	56	56	56	56	56
control mean dep. var.	0.771	0.882	0.196	0.531	3.961	6.948	2.688
Panel B: all eligible villages of comparable size							
treatment	0.073	0.023	0.006	0.001	0.139	0.921	0.388
	(0.00)$^{\bullet\bullet\circledast}$	(0.29)	(0.82)	(0.97)	(0.51)	(0.03)$^{\bullet\circledast}$	(0.31)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓
households	20495	20495	20495	20494	20504	20504	20504
villages	316	316	316	316	316	316	316
control mean dep. var.	0.771	0.882	0.161	0.537	4.090	6.751	3.653

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets $\bullet$ indicate significance levels preserved under randomization inference, while starred bullets $\circledast$ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Panel B of Table 1 and age, sex, and ethnicity.

We can also make use of Census data to investigate this question. The Census 2013 contains a section on household assets and animals. Table 7, Panel A shows that there are insignificant results in all dimensions. However, the effects might be too small to be picked up in our small sample. Table 7, Panel B shows the results of the same Census-based analysis, but using all villages in The Gambia that were eligible for the CDD program and fall within the size group of our sample villages (300-1000 inhabitants in 2003). Indeed, based on this larger sample (using

[44]Of course, the meaning of "have you benefited" and "has your situation improved" is somewhat different. A household can be the beneficiary, e.g., of health program, but the economic situation may not have improved.

more than 20,000 households in 316 villages), treatment effects on some minor categories turn significant, namely radio and poultry ownership.[45]

Overall, there is a fair mix of positive, negative and null results which leads us to conclude that there is at most moderate economic change: based on the villagers' own reports and the enumerators' quantitative assessments (not shown) there is some limited evidence for positive economic changes in the treated villages. However, we do not find evidence for changes in medium-run income or assets at the individual level using data for our sample villages. Using Census data we do find some moderate, statistically significant effects in the full sample of similarly sized villages. 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.[46]

4.3 Did Intended Institutional/Social Change Take Place?

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

However, we do not find evidence of institutional change in data on community meetings in general. Table A31 in the Appendix shows that there are no average treatment effects with respect to decision-making processes, or for households' feelings of empowerment. In fact, the results even provide suggestive evidence that marginalized households got further excluded from village activities and village-level decision making.

4.4 Formalization of the Village Economy

The previous section suggests that the CDD program brought at most modest economic and institutional change and the changes in networks appear not to be due to a big push in local economic growth or institutional change. We therefore turn now to the hypothesis that changes in networks are the consequence of secondary effects and unintended effects of the CDD

[45] In a related paper (Heß et al., 2018), 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 and the Gambian Integrated Household Survey 2015, we find complementary evidence consistent with a modest positive treatment effect on some wealth measures and employment.

[46] 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).

Table 8: CDD Projects Were Implemented in Line With CDD Requirements and Procedures Spilled Over Into Other Projects

	who decided on this project?				process
	(1) villagers	(2) VDC	(3) elites	(4) NGO/outsider	(5) was there a vote?
Panel A: Comparing all types of projects					
CDD project	0.170	0.234	0.028	-0.390	0.138
	(0.001)***	(0.000)***	(0.403)	(0.000)***	(0.000)***
marginalized	0.066	0.132	0.029	-0.174	0.017
	(0.239)	(0.068)*	(0.667)	(0.020)**	(0.468)
elite	-0.048	-0.020	0.094	0.001	0.018
	(0.527)	(0.723)	(0.146)	(0.992)	(0.636)
CDD project × marginalized	-0.162	-0.027	-0.020	0.187	-0.097
	(0.049)**	(0.794)	(0.799)	(0.051)*	(0.038)**
CDD project × elite	0.214	-0.021	-0.185	0.045	0.036
	(0.068)*	(0.800)	(0.010)**	(0.706)	(0.660)
control mean dep. var.	0.252	0.185	0.130	0.474	0.018
household×projects	1097	1097	1097	1097	1172
Panel B: Comparing non-CDD projects between treated and control villages					
treatment village	0.148	-0.043	-0.034	-0.059	0.040
	(0.029)**	(0.366)	(0.374)	(0.362)	(0.053)*
control mean dep. var.	0.252	0.185	0.130	0.474	0.018
household×projects	627	627	627	627	656

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

program. As secondary effects, we first consider a formalization of the economy in the form of increased outward/market orientation.

Tangible economic benefits make market participation of some individuals more likely, which according to Kranton (1996) weakens the system of personalized exchange and leads to a reduction in overall interactions, and reciprocal interactions in particular — a move to market-based activities can be expected. But, even with the limited evidence for economic changes formalization might occur. 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). As a result, the level of formalization of the village economy, in the form of more formal transactions and an increased orientation towards market activities, could increase.[47] A general increase in connections with village outsiders could further be related to increased exposure to neighboring villages during the CDD planning stage.

Our data also contain information about links to outsiders and whether internal links involve a payment, which can be used to test some aspects of the formalization hypothesis directly. Table 9, Panel A, columns 1 and 2 show that households in treatment villages are indeed significantly more likely to interact with households outside the village. Further, a transformation to more market-based transactions would imply that more links represent paid transactions. Our data allow us to distinguish links that represent a transaction for which a payment was made and those for which no payment was made.[48] Indeed, we find that paid *in-links* from village outsiders are also increasing (Table 9, Panel A, columns 3 and 4), explaining a substantial share of the overall increase. Thus, there is some evidence for more market-like transactions between buyers from CDD program villages and sellers from outside. On the other hand, we only find a marginally significant reduction in paid village-internal transactions (Table 9, columns 5 and 6), which does not remain statistically significant once randomization inference is used. One important caveat to this analysis is that the number of paid transactions reported in the data is very small overall.

In sum, in addition to the findings regarding reciprocity and support, we find some further evidence consistent with a more formalized village economy in treatment villages, in the sense of increasing transactions involving pay and transactions with village outsiders.

4.5 Elite Capture and Unequally Distributed Benefits

We now turn to unintended consequences of the CDD program. To illustrate the possibilities for elite capture, recall that most sub-projects are related to agriculture, and land-holding

[47] Casey et al. (2012) find that in CDD program villages in Sierra Leone the number of traders and the number of locally available goods for sale increased.

[48] In our data, most transactions do not involve a payment. Typical exchanges that involve a monetary payment, on the other hand, are petty trade and payments for services such as hut construction or some agricultural tasks.

Table 9: External Links and Links Involving Pay

	external links		paid external links		paid internal links	
	(1) economic (pooled)	(2) economic (vcv)	(3) economic (pooled)	(4) economic (vcv)	(5) economic (pooled)	(6) economic (vcv)
Panel A: in-degree						
treatment	0.029	0.206	0.015	0.210	−0.025	−0.129
	(0.006)$^{\bullet\bullet\circledast}$	(0.002)$^{\bullet\bullet\circledast}$	(0.070)$^{\circledast}$	(0.026)$^{\bullet\circledast}$	(0.101)	(0.068)$^{\circledast}$
controls (see notes)	✓	✓	✓	✓	✓	✓
network fixed effects	✓		✓		✓	
control mean dep. var.	0.18	0.00	0.03	0.00	0.10	0.00
observations	16644	2774	11096	2774	11096	2774
Panel B: out-degree						
treatment	−0.004	−0.035	0.001	0.125	−0.009	−0.050
	(0.719)	(0.551)	(0.290)	(0.063)$^{\circledast}$	(0.538)	(0.166)
controls (see notes)	✓	✓	✓	✓	✓	✓
network fixed effects	✓		✓		✓	
control mean dep. var.	0.12	0.00	0.00	0.00	0.09	0.00
observations	16644	2774	11096	2774	11096	2774

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets $\bullet$ indicate significance levels preserved under randomization inference, while starred bullets $\circledast$ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. The pooled specification stacks observations for each individual network and controls for network fixed effects. Thus the number of observations is sixfold in these columns. The payment indicator is only defined for four networks (excluding the *Gifts* and *Credit* networks), thus the number of observations in pooled specifications for these dependent variables is fourfold. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies.

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 will likely benefit from this mechanization of agriculture, while landless households might even lose: if agricultural machinery and manual labor are substitutes, their labor is now less valuable. Indeed, our results indicate that land-holding is positively correlated with higher self-reported "benefiting from CDD projects" in treatment villages, while the same cannot be observed for non-CDD projects in control villages.[49]

Elite capture and unequally distributed benefits can explain our main finding of reduced network interaction in two ways. First, disadvantaged households might sever their social and economic ties to advantaged households directly, out of grievance over their status. Second, 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 that weaken economic networks more broadly, not just links between advantaged and disadvantaged households.

Evidence for Unequally Distributed Benefits

Our survey-based measures indicate that there is some concern among respondents that benefits are not evenly distributed in development projects. For the average non-CDD project in a control village, 4% of households report that they are "benefiting less" than other households. This number rises to about 12% for the average CDD sub-project in treatment villages. Table 10 reports results on how elite status, marginalization, land holdings, and the villages' land Gini coefficient predict the reported benefit, for CDD and non-CDD projects. Elite is again defined as either being the traditional chief, a close relative, or the Imam. Marginalized households are those headed by a young or female household head. The estimates in column 1 suggest that elite and land-owning households benefit significantly more from CDD sub-projects than regular households. Correspondingly, in villages where the land distribution is more unequal, the average household reports less benefit from the CDD program. On the other hand, there is no statistically significant difference between elites and regular households in their reported benefits for non-CDD projects, in treatment and in control communities (columns 2 and 3 respectively). While these results are descriptive, they suggest stark differences in how villagers perceive benefits from CDD and non-CDD development interventions to be distributed.

When the analysis is conducted at the level of each individual project report given by the households, it is possible to include household and (sub-)project fixed effects.[50] Based on this, the results in Appendix Tables A33 and A34 indicate that for CDD sub-projects, elites are more likely to report having benefited more than others, while marginalized households are less likely to report large benefits in absolute terms.

[49] In conversations with villagers during our fieldwork, poorer households sometimes stated that they could not benefit from the projects because they could not afford the fee for using the tractor. We have observed that in villages with tractors, the tractor could only be used after paying a small fee to cover maintenance and to pay the driver.

[50] Recall that each household in the in-depth survey reported on all current and recent development projects they could recall.

Table 10: Elite and Landowning Households Benefit More From CDD Projects

	CDD villages		control villages
	(1) benefited from CDD sub-projects	(2) benefited from non-CDD projects	(3) benefited from non-CDD projects
$\mathbb{1}$(land > 2ha)	0.246	0.178	0.011
	(0.020)**	(0.311)	(0.933)
land gini	−2.608	−1.521	0.309
	(0.012)**	(0.156)	(0.305)
elite	0.269	−0.018	−0.270
	(0.075)*	(0.915)	(0.133)
marginalized	0.148	0.370	0.122
	(0.224)	(0.017)**	(0.371)
controls (see notes)	✓	✓	✓
households	265	164	218
mean dep. var.	2.394	2.481	2.529

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, p-values in parentheses account for clustering at the village level. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies. The dependent variable is the mean response across all project reports by that household for projects in the respective category to the survey question "Does/did your household benefit from the project? 0=no; 1=very little; 2=moderately; 3=a lot". Numbers of observations vary because of non-response. In several villages (in particular treatment villages) some respondents did not recall any non-CDD development projects. The results remain qualitatively similar but become much stronger in terms of statistical significance when each village's lowest answer (indicating little benefit) is imputed for missing responses (see Table A32 in Appendix).

Are these findings indicative of elite capture or is the distribution of benefits a result of other features of the project? Table 8 shows that elite and marginal households differ in their view on procedures related to the CDD project choice. Marginal 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 than other households to say that elite households were important in making the decisions. If elites were indeed more engaged in the decision making about projects this would allow a purposeful steering of projects towards areas that would benefit them.

Aside from choosing sub-projects which only serve land-owning households, elites could also influence project choice away from the provision of public goods towards private and club goods, increasing their chances to appropriate gains. We investigate this by classifying all sub-projects into two categories, based on whether they provided goods which are excludable in consumption (such as machinery, inputs, or animals) or non-excludable public goods (such as public water or road infrastructure, erosion control systems, or other public facilities). Table 11 provides strong evidence to suggest that elites indeed benefit significantly more from sub-projects that brought excludable goods to the village.

Table 11: Heterogeneity in Self-Declared Benefit by Sub-Project Types

	absolute benefit		relative benefit	
	(1)	(2)	(3)	(4)
excludable	−0.125		−0.080	
	(0.687)		(0.309)	
excludable × marginalized	−0.173	−0.078	−0.084	0.025
	(0.616)	(0.601)	(0.312)	(0.659)
excludable × elite	0.687	0.778	0.205	0.185
	(0.069)*	(0.008)***	(0.067)*	(0.023)**
household fixed effects	✓	✓	✓	✓
project fixed effects		✓		✓
observations (household×project)	526	526	515	515
mean dep. var.	2.304	2.304	0.880	0.880
dep. var. range	0 - 3	0 - 3	0 - 2	0 - 2

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, p-values in parentheses account for clustering at the village level. Units of observation are reports about development projects elicited from 10 households per village. Observations are weighted by the inverse of the number of projects listed by each household, to give the same weight to each household. The sample is restricted to treatment villages and CDD sub-projects. The dependent variable records project benefit on a Likert scale. Columns 1 and 2 use the question "Does/did your household benefit from the project? 0=no; 1=very little; 2=moderately; 3=a lot". Columns 3 and 4 use the question "Relative to other households in the village, how does/did your household benefit from the project? 0=less; 1=the same; 2=more". Classified as 'excludable' are animals, boats, draft animals/carts, farm implements and inputs, milling machines, seeds, fertilizers, and tractors. Classified as 'non-excludable' are erosion control systems, boreholes, gardens, health facilities, latrines, salt plants, schools, mosques, forest, drainage systems, bridges, bantaba (village meeting place) renovations, roads, skill centers, and waiting sheds.

Unequally Distributed Benefits Translate Into Fewer Links

While the above results strongly point towards unequally distributed benefits, they do not directly relate to interactions in networks. This section now asks whether there is direct evidence that elite capture and unequally distributed benefits translate into fewer links.

First, and similar to our earlier analysis on heterogeneous dyadic treatment effects (Table 3, columns 3 and 4), we investigate whether people with less land or relatively lower (self-assessed) wealth are also less connected in treatment villages. Table A35 in the Appendix suggests that the latter is not the case. While dyads involving wealthier households are more likely to have a link, there seems to be no difference between treatment and control communities in this regard.

However there is evidence that fewer economic interactions occur between households that are 'winners' and households that are 'losers' in terms of perceived relative benefit from projects. Table 12 shows results from dyad-level regressions based on Equation 2.1, to which indicators of elite capture and unequally distributed benefits are added. In columns 1 and 2, the additional right-hand-side variable indicates whether the two households of a dyad have differing perceptions about whether, on average, they benefited less from development projects in their village than other households. If households in treatment villages and benefit from CDD sub-projects are considered (column 1), the estimate is negative, statistically significant, and comparable in magnitude to the effect of a household belonging to the group of marginal households. On the other hand, if control villages and the perceived benefit from non-CDD projects is considered

Table 12: The Dyadic Effect of Subjective Benefit and Inequality

	benefit inequality		wealth and land inequality		
	CDD	Non-CDD	all villages		
	(1) economic (pooled)	(2) economic (pooled)	(3) economic (pooled)	(4) economic (pooled)	(5) economic (pooled)
hhs benefit differently	-0.871	-0.105			
	(0.027)**	(0.869)			
treatment			-0.290	-0.292	-0.300
			(0.002)●●⊛	(0.000)●●●	(0.001)●●⊛
land gini			0.117		
			(0.111)		
treatment× land gini			-0.222		
			(0.020)●⊛		
asset/animal wealth gini				0.167	
				(0.018)**	
treatment× asset/animal wealth gini				-0.191	
				(0.031)⊛⊛	
assets/animal/land gini					0.143
					(0.044)**
treatment× assets/animal/land gini					-0.213
					(0.044)⊛⊛
elite^{any}	0.858	0.449	0.406	0.396	0.410
	(0.188)	(0.353)	(0.000)***	(0.000)***	(0.000)***
marginal^{any}	-0.706	-0.472	-0.101	-0.100	-0.099
	(0.006)***	(0.188)	(0.063)*	(0.076)*	(0.079)*
controls (see notes)	✓	✓	✓	✓	✓
network fixed effects	✓	✓	✓	✓	✓
dyads	13824	10104	909792	909792	909792
households	267	218	2774	2774	2774
control mean dep. var.		1.804	1.437	1.437	1.437

Notes: ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. The pooled specification stacks observations for each individual network and controls for network fixed effects. Thus the number of observations is sixfold in these columns. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies. In columns 1 and 2 the sample is restricted to dyads between households who were randomly selected for in-depth interviews and to the treatment and the control group respectively. The variable indicator variable 'hhs benefit differently' aggregates the two households' average response to how much they benefit from CDD (column 1) and non-CDD (column 2) projects relative to others. It takes on the value 0 whenever both households declare they benefit less than others, more than others, or the same. Whenever the two households give differing answers, e.g., because one declares benefit less than others on average, while the other declares to benefit as others it takes on the value 1. The land Gini coefficient is computed based on data from the main survey. The wealth Gini coefficient is computed using 19 asset indicators from the Census 2013, including animals, and detailed data on resale values from the Integrated Household Survey Gambia, 2014. Our data and the Census data can be matched at the compound level. In column 5, we thus use a Gini that measures inequality in total wealth across compounds.

(column 2), the point estimate is small and statistically insignificant. More indirect evidence is presented in Table 12, columns 3 to 5. The estimates suggest that inequality measures are related to the effect of the treatment on networks. In villages with larger inequality, treatment is estimated to lead to a larger reduction in links than in more equal villages. This holds for inequality measured through land and through asset wealth. One possible interpretation of these findings is as follows: Individual characteristics, such as being elite or having relatively large landholdings, determine benefits. The resulting unequally distributed benefits cause social disruptions, which result in fewer economic interactions. This includes a reduction of interactions between 'winners' and 'losers' but also seems to affect the social fabric more broadly, resulting in a disruption of the whole villages' economic exchange network.

4.6 Failing Projects

Failing projects are an extreme case of benefits that are not distributed in line with initial plans or expectations. Indeed, in the sample villages, more than one quarter of development projects fail, i.e., are considered "not functioning" by the respondents (this number can be derived in various alternative ways and equally applies to both CDD and non-CDD projects, see Table A27).

As in the discussion of unequally distributed benefits, the fact that the community decided on a project and is also responsible for running day-to-day operations can imply that failure of a project is attributed to certain individuals or groups, potentially leading to quarrels.[51] Again, these quarrels might result in a reduction of the willingness to engage with others, and consequently reduced interactions and trust.

We test this by introducing indicators of project failure into our dyadic regression framework. Here, the share of non-functional non-CDD projects serves as an indicator of a general village-level project implementation capacity. In villages with higher rates of project failure, treatment is estimated to lead to a larger reduction in economic network links than in other villages (see Table 13, columns 1 and 2). Accordingly, we also find that in the group of CDD villages, households have fewer economic links in communities where a larger share of CDD sub-projects or the main sub-project, based on the budget share, failed (see Table 13, columns 3 to 6). This pattern is consistent with project failure-related quarrels being a reason for the reduction of economic interactions between households.

[51] Qualitative interviews after data collection revealed, for example, that in one sample village the oldest family in the village broke apart into two quarreling fractions forming around two brothers — the Chief and the head of the newly formed VDC. The village had bought a tractor from the project's money and struggled to maintain it. According to the CDD guidelines, but to the dissatisfaction of many villagers, the Chief was excluded from the management of this tractor. The two brothers had not spoken to each other following the quarrels that arose due to the tractor's maintenance issues. In another village a passenger bus was bought from the project's money. At the time of our interviews in 2014, a dispute had arisen about how the accruing revenues were to be used for village-level activities. When we returned to the village again in 2016 for qualitative interviews, we were told that the designated bus driver, who claimed to have repeatedly used personal funds to pay for repairing the bus, sold off the bus for his own gain and had since left the village.

Table 13: Network Links and Village-Level Propensity for Project Failure

	all villages		CDD villages			
	(1) economic (pooled)	(2) economic (vcv)	(3) economic (pooled)	(4) economic (vcv)	(5) economic (pooled)	(6) economic (vcv)
treatment	-0.132	-0.029				
	(0.233)	(0.122)				
share of failed non-CDD projects	0.568	0.084				
	(0.031)**	(0.062)*				
treatment×	-0.676	-0.090				
share of failed non-CDD projects	(0.042)⊛⊛	(0.107)				
elite^{any}	0.405	0.071	0.485	0.085	0.478	0.083
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
marginal^{any}	-0.097	-0.017	-0.068	-0.012	-0.065	-0.011
	(0.091)*	(0.077)*	(0.266)	(0.268)	(0.288)	(0.291)
share of failed CDD sub-projects			-0.432	-0.089		
			(0.155)	(0.076)*		
main sub-project failed					-0.597	-0.115
					(0.060)*	(0.030)**
controls (see notes)	✓	✓	✓	✓	✓	✓
network fixed effects	✓		✓		✓	
dyads	909792	151632	488868	81478	488868	81478
households	2774	2774	1416	1416	1416	1416
control mean dep. var.	1.437	0.000				

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. The pooled specification stacks observations for each individual network and controls for network fixed effects. Thus the number of observations is sixfold in these columns. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group. The shares of failed projects are computed by averaging the failure dummy variables across all respondents of the in-depth interview. The main sub-project is coded as the project with the largest budget share, based on the administrative data.

4.7 Substitution of Other Forms of Social Capital

Previous studies suggest that CDD programs may favor particular forms of social capital, which can substitute for other forms that were relevant before the program (Labonne and Chase, 2011; Avdeenko and Gilligan, 2015; Arcand and Wagner, 2016). For instance, if the CDD program increases the membership and participation in community-based organizations (CBOs), then the reduction in economic interactions may be the result of a change in allocation of time and resources from traditional economic exchanges to CBO activities.

The results in Table 14 show that this does not seem to be case. In fact, the opposite is true. Based on the in-depth survey, villagers in CDD communities are significantly less likely to be members of CBOs, attend CBO meetings, and contribute resources. This result is mirrored in lower levels of engagement by treatment households in village activities such as improvement of village infrastructure and work on the communal farm. Therefore, the reduction in traditional economic interactions was accompanied by a reduction in other forms of social capital.[52]

Table 14: Village-Group Membership and Participation in Village Activities

	village groups			village activities		
	(1) memberships	(2) meeting attendance	(3) contributions	(4) participation	(5) contribution (labor)	(6) contribution (kind/money)
treatment	−0.312	−0.219	−0.153	−0.220	−0.216	−0.169
	(0.010)$^{\bullet\bullet}$	(0.061)$^{\circledast}$	(0.159)	(0.003)$^{\bullet\bullet\circledast}$	(0.005)$^{\bullet\bullet\circledast}$	(0.098)$^{\circledast}$
elite	0.218	0.217	0.177	0.055	0.056	0.176
	(0.259)	(0.257)	(0.383)	(0.612)	(0.580)	(0.119)
marginalized	−0.049	0.002	0.047	−0.012	−0.057	−0.004
	(0.778)	(0.989)	(0.780)	(0.896)	(0.547)	(0.964)
controls (see notes)	✓	✓	✓	✓	✓	✓
households	550	523	522	550	550	550
control mean dep. var.	3.266	3.245	2.989	2.383	2.354	1.139

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies.

5 Implications for Household Welfare

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

[52] Appendix Table A36 shows results disaggregated by types of CBOs and an accompanying discussion of how results relate to and strengthen our findings regarding the economic network links.

enforce informal contracts depends positively on social capital (e.g., Ligon et al., 2002; Karlan et al., 2009; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006).[53]

In this section, we investigate two possible implications of our findings for household welfare. First, we test whether the households' ability to cope with shocks is affected by treatment, i.e., we directly test the relationship between shocks and activity in economic networks, and study whether shocks are less likely to lead to economic transactions in treatment villages. In a second, less direct approach, we test whether friendship networks in treatment villages are different than in control villages and document a treatment effect estimate similar to the one described for economic networks above.

5.1 Effects on the Ability to Cope With Shocks

As households in treatment villages are less embedded in economic networks, this can translate into lower welfare through a reduced ability to cope with shocks. We have collected data on different types of shocks that we aggregate into three categories (production, housing, and health shocks), as well as one overall shock-count that sums up all three 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 15, which uses our dyadic data, shows that shocks experienced by the receiving household of a given directed dyad are statistically significant predictors of a flow towards this household. In simple terms this means 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 on the interaction 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 shocks.

5.2 Effects on the Network of Friends

We measure "friendship" by asking each household head to name other households with whom the respondent gets together to drink *Attaya* tea. As previously indicated, this involves a longer ritual. 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. Still, this data is subject to the usual concerns regarding the measurement of help networks. Table 16 uses the dyadic nature of this data on the friendship networks. The results in column 1 show that households belonging to the elite are more likely to be connected in this network as well, while marginal households are again more isolated.

[53]Smaller networks might also translate into lower welfare through other channels (e.g., lower levels of information or trust), but one can also think of scenarios in which smaller networks are welfare increasing, e.g., if only weak links are cut, for which the cost of maintaining them is larger than the benefit.

Table 15: Treatment Effect on Village-Internal Links in Response to Shocks

Shock variable:	Total shock count		Production shock		Housing shock		Health shock	
	(1) economic (pooled)	(2) economic (pooled)	(3) economic (pooled)	(4) economic (pooled)	(5) economic (pooled)	(6) economic (pooled)	(7) economic (pooled)	(8) economic (pooled)
treatment	-0.285	-0.314	-0.283	-0.246	-0.276	-0.299	-0.281	-0.302
	(0.001)$^{\bullet\bullet\circledast}$	(0.004)$^{\bullet\bullet\circledast}$	(0.002)$^{\bullet\bullet\circledast}$	(0.006)$^{\bullet\bullet\circledast}$	(0.002)$^{\bullet\bullet\circledast}$	(0.002)$^{\bullet\bullet\circledast}$	(0.002)$^{\bullet\bullet\circledast}$	(0.003)$^{\bullet\bullet\circledast}$
shocks$_j$	0.105	0.097	0.151	0.174	0.064	0.034	0.090	0.075
	(0.000)***	(0.006)***	(0.000)***	(0.001)***	(0.080)*	(0.577)	(0.014)**	(0.236)
treatment × shocks$_j$		0.015		-0.044		0.060		0.029
		(0.728)		(0.511)		(0.446)		(0.705)
elite^{any}	0.410	0.410	0.403	0.403	0.412	0.412	0.410	0.410
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
marginal^{any}	-0.099	-0.099	-0.096	-0.097	-0.101	-0.102	-0.101	-0.101
	(0.078)*	(0.077)*	(0.085)*	(0.083)*	(0.073)*	(0.071)*	(0.074)*	(0.071)*
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
dyads	909792	909792	909792	909792	909792	909792	909792	909792
households	2774	2774	2774	2774	2774	2774	2774	2774
control mean dep. var.	1.437	1.437	1.437	1.437	1.437	1.437	1.437	1.437
mean shock var.	1.90	1.90	0.79	0.79	0.40	0.40	0.72	0.72

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. Units of observation are dyads. The dependent variable takes on the value 100 if a dyad had a link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. The pooled specification stacks observations for each individual network and controls for network fixed effects. Thus the number of observations is sixfold in these columns. All regressions control for ward fixed effects, network fixed effects, and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group. The shock variables indicate the occurrence of shocks of different types during the past two years. The *production shock* sums up two indicators, "crop failed" 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.

Table 16: Treatment Effect on Friendship

	(1) friendship	(2) friendship (days/week)	(3) friendship	(4) help link	(5) help link
treatment	-1.040	-0.022	-0.876	-0.435	-0.159
	(0.013)$^{\bullet\circledast}$	(0.017)$^{\bullet\circledast}$	(0.095)$^{\circledast}$	(0.288)	(0.740)
elite^{any}	0.530	0.001	0.048	1.142	1.175
	(0.071)*	(0.791)	(0.911)	(0.009)***	(0.033)**
marginal^{any}	-0.564	-0.012	-0.109	-0.447	-0.090
	(0.029)**	(0.040)**	(0.747)	(0.075)*	(0.776)
elite^{any} $\times$ treatment			0.851		-0.084
			(0.193)		(0.922)
marginal^{any} $\times$ treatment			-0.856		-0.715
			(0.064)$^{\bullet}$		(0.115)
controls (see notes)	✓	✓	✓	✓	✓
dyads	75816	75812	75816	48642	48642
households	2774	2774	2774	2774	2774
control mean dep. var.	4.934	0.094	4.934	3.560	3.560

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets $\bullet$ indicate significance levels preserved under randomization inference, while starred bullets $\circledast$ indicate significance levels that are only sustained by the cluster-robust standard errors. Units of observation are dyads. The dependent variable takes on the value 100 if a dyad had a link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. Columns 4 and 5 use directed dyads but restrict the sample to those dyads in which at least one household was interviewed in the in-depth interviews, because the help-link question was not part of the main survey. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group.

Turning to the effect of treatment, we observe a statistically significant and sizable negative treatment effect of about 20% of the control mean. Column 2 shows that the same holds true when the frequency of meetings is considered instead of a binary indicator. Column 3 presents estimates allowing for heterogeneity of the treatment effect for marginal and elite households. These results suggest that the reduction of friendship interactions is larger for dyads involving marginalized households. Overall, the observed reduction in friendship is consistent with the hypothesis of internal divisions emerging during the implementation of the CDD program, as a result of elite capture, unequally distributed benefits, failed projects or for other reasons.

In the in-depth interviews, we also asked households more directly to identify their network of potential helpers. Specifically, we asked them to name individuals who could help them out in times of need. The number of people in these help networks does not differ significantly between treatment and control villages (Table 16, columns 4 and 5). This finding is consistent with the finding from the previous subsection that the reaction to shocks is not significantly different in treatment villages.

6 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. In the literature on the role of networks in rural economies, which stresses the importance of networks for, e.g., risk sharing and information acquisition, larger and denser networks are generally seen as an indicator of social capital and considered a positive feature of rural societies. 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.

We collect detailed data on the networks of social and economic exchanges between households, after the CDD program brought significant resources and community-level activities to randomly selected villages. Our main finding is that, more than four years after the program began operations, households in treatment villages are significantly less likely to be connected with other households and are less embedded in the village economy.

To understand the underlying mechanisms of this effect of the program, we go through a number of possible explanations, and among these the evidence is most consistent with two explanations. First, there is some evidence for a village-level transformation: a reduced importance of social proximity, a reduction in reciprocal relationships among villagers, and an increase in interactions with individuals from outside the village, including an increase in interactions involving pay. The CDD program resources brought about a short-run economic shock of significant magnitude and projects that changed the nature of economic transactions. And, despite modest effects on medium-run economic development, the program-induced economic changes had the potential to contribute to the observed changes. Secondly, we find strong evidence for unequally distributed benefits in CDD sub-projects and some evidence that these

are due to more benefits accruing to elite households. We also show that unequally distributed benefits are associated with fewer links in economic networks. This is true both at the dyadic level (household pairs who exhibit a large gap in reported benefits from CDD sub-projects are less connected) and the aggregate level (the treatment effect is stronger in villages with characteristics that proxy for unequally distributed benefits, such as a highly unequal distribution of land holdings). Our findings also suggest a connection between project failure and reduced economic interactions. Based on anecdotal evidence as well as findings in Barakat (2006), Morel et al. (2009), Barron et al. (2011), and King and Samii (2014), we speculate that unequally distributed benefits and project failure are linked to lower interaction in networks because they induce disputes, which in turn affects the size of social (friendship) networks (which are significantly smaller in treatment communities). Smaller social networks reduce social network-based trust, the willingness to cooperate, and subsequently economic interactions (Karlan et al., 2009).

We investigate whether the reduced density of economic networks negatively impacts households' welfare through a reduction in their ability to cope with shocks. As a side product of this analysis, we first document significant effects of shocks on the existence of incoming economic links — i.e., for risk-sharing through economic exchanges — within these villages. However, we do not find a difference in the effect of the CDD program on the probability that a shock triggers an economic flow. But we do find evidence suggesting reduced friendship between households in treatment communities.

For policy purposes, our analysis points to an important dimension that should be considered when evaluating the welfare benefits of interventions such as the Gambian CDD program. In addition to any positive short- to medium-run effects on immediate outcomes, such as the benefits due to improved village infrastructure and productive asset availability, our findings suggest the importance of taking into account unintended consequences of such programs. In our case, informal networks were weakened in the process and it may not be easy to fully rebuild them quickly. Since our analysis of the ability to deal with shocks does not suggest any negative impact while our analysis of friendship networks suggests a negative effect, our evidence regarding the implications of reduced interactions is inconclusive. Nonetheless, several related findings, such as the effect of the CDD program on group membership, suggest lower levels of social capital in a number of dimensions beyond economic networks. Still, further work is needed to study the consequences of the reduced interactions in economic networks.

Further related to policy, our findings also relate to issues of elite capture and village-level disputes. We reconfirm prior findings about the dangers of elite capture and find unequally distributed benefits that are related to intra-village wealth inequality. However, more research is required to investigate the relationship between development projects and within-village conflict, as development projects may induce conflict independent of a project's success. When development projects create the intended economic benefits, they may induce distributional conflict at local levels. At the same time, even if benefits do not materialize or are unequally distributed, disputes might also arise.

Together, our findings suggest that development projects that intend to affect economic development and social interactions may influence social and economic networks negatively. We found evidence that is consistent with two possible explanations, formalization and internal divisions. To the extent that these effects are due to formalization, 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 social conflicts that arose in the course of the program, 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.

References

Alatas, V., Banerjee, A., Chandrasekhar, A. G., Hanna, R. and Olken, B. A. (2016), 'Network structure and the aggregation of information: Theory and evidence from Indonesia', *American Economic Review* **106**(7), 1663–1704.

Alatas, V., Banerjee, A., Hanna, R., Olken, B. A., Purnamasari, R. and Wai-Poi, M. (2013), Does elite capture matter? Local elites and targeted welfare programs in Indonesia, Working Paper 18798, National Bureau of Economic Research.

Ambrus, A., Mobius, M. and Szeidl, A. (2014), 'Consumption risk-sharing in social networks', *American Economic Review* **104**(1), 149–182.

Anderson, M. L. (2008), 'Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and early training projects', *Journal of the American Statistical Association* **103**(484), 1481–1495.

Arcand, J.-L., Chen, Y.-P., He, Y., Diop, C. I. F., Wouabe, E. D., Garbouj, M., Jaimovich, D. and Zec, S. (2010), The Gambia CDDP baseline: Rural household survey, qualitative survey, village network survey, Working Paper, Geneva: The Graduate Institute.

Arcand, J.-L. and Wagner, N. (2016), 'Does community-driven development improve inclusiveness in peasant organizations? Evidence from Senegal', *World Development* **78**, 105–124.

Avdeenko, A. and Gilligan, M. J. (2015), 'International interventions to build social capital: Evidence from a field experiment in Sudan', *American Political Science Review* **109**(3), 427–449.

Bandiera, O. and Rasul, I. (2006), 'Social networks and technology adoption in Northern Mozambique', *Economic Journal* **116**(514), 869–902.

Banerjee, A., Chandrasekhar, A. G., Duflo, E. and Jackson, M. O. (2013), 'The diffusion of microfinance', *Science* **341**(6144), 363–371.

Banerjee, A., Chandrasekhar, A. G., Duflo, E. and Jackson, M. O. (2018), Changes in social network structure in response to exposure to formal credit markets, Working Paper, Stanford University.

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

Barakat, S. (2006), Mid-term evaluation report of the national solidarity programme (NSP), Afghanistan, Working Paper, Reconstruction and Development Unit, University of York and Ministry of Rural Development, Islamic Republic of Afghanistan.

Bardhan, P. and Mookherjee, D. (2000), 'Capture and governance at local and national levels', *American Economic Review* **90**(2), 135–139.

Barron, P., Diprose, R. and Woolcock, M. J. (2011), *Contesting development: Participatory projects and local conflict dynamics in Indonesia*, Yale University Press.

Beaman, L. and Dillon, A. (2012), 'Do household definitions matter in survey design? Results from a randomized survey experiment in Mali', *Journal of Development Economics* **98**(1), 124–135.

Beaman, L. and Magruder, J. (2012), 'Who gets the job referral? Evidence from a social networks experiment', *American Economic Review* **102**(7), 3574–3593.

Beath, A., Christia, F. and Enikolopov, R. (2013), 'Empowering women through development aid: Evidence from a field experiment in Afghanistan', *American Political Science Review* **107**(3), 540–557.

Binzel, C., Field, E. and Pande, R. (2013), Does the arrival of a formal financial institution alter informal sharing arrangements? Experimental evidence from village India, Unpublished manuscript, Harvard University.

Björkman, M. and Svensson, J. (2007), 'Power to the people: Evidence from a randomized field experiment of a community-based monitoring project in Uganda', *Quarterly Journal of Economics* **124**(2), 735–769.

Bramoullé, Y., Djebbari, H. and Fortin, B. (2009), 'Identification of peer effects through social networks', *Journal of Econometrics* **150**(1), 41–55.

Bruhn, M. and McKenzie, D. (2009), 'In pursuit of balance: Randomization in practice in development field experiments', *American Economic Journal: Applied Economics* **1**(4), 200–232.

Bugni, F. A., Canay, I. A. and Shaikh, A. M. (forthcoming), 'Inference under covariate-adaptive randomization', *Journal of the American Statistical Association* .

Calvó-Armengol, A., Patacchini, E. and Zenou, Y. (2009), 'Peer effects and social networks in education', *Review of Economic Studies* **76**(4), 1239–1267.

Campbell, K. E. and Lee, B. A. (1991), 'Name generators in surveys of personal networks', *Social Networks* **13**(3), 203–221.

Casey, K. (forthcoming), 'Radical decentralization: Does community-driven development work?', *Annual Review of Economics* .

Casey, K., Glennerster, R. and Miguel, E. (2012), 'Reshaping institutions: Evidence on aid impacts using a preanalysis plan', *Quarterly Journal of Economics* **127**(4), 1755–1812.

Cecchi, F., Duchoslav, J. and Bulte, E. (2016), 'Formal insurance and the dynamics of social capital: Experimental evidence from uganda', *Journal of African Economies* **25**(3), 418–438.

Chandrasekhar, A. G., Kinnan, C. and Larreguy, H. (forthcoming), 'Social networks as contract enforcement: Evidence from a lab experiment in the field', *American Economic Journal: Applied Economics* .

Chandrasekhar, A. G. and Lewis, R. (2016), Econometrics of sampled networks, Working Paper, Stanford University.

Comola, M. and Fafchamps, M. (2014), 'Testing unilateral and bilateral link formation', *Economic Journal* **124**(579), 954–976.

Comola, M. and Prina, S. (2015), Treatment effect accounting for network changes: Evidence from a randomized intervention, Working Paper, Paris School of Economics.

Conley, T. G. and Udry, C. R. (2010), 'Learning about a new technology: Pineapple in Ghana', *American Economic Review* **100**(1), 35–69.

Crost, B., Felter, J. and Johnston, P. (2014), 'Aid under fire: Development projects and civil conflict', *American Economic Review* **104**(6), 1833–56.

Cruz, C., Labonne, J. and Querubin, P. (2017), 'Politician family networks and electoral outcomes: Evidence from the Philippines', *American Economic Review* **107**(10), 3006–3037.

De Weerdt, J. and Dercon, S. (2006), 'Risk-sharing networks and insurance against illness', *Journal of Development Economics* **81**(2), 337–356.

Dube, O. and Vargas, J. F. (2013), 'Commodity price shocks and civil conflict: Evidence from Colombia', *The Review of Economic Studies* **80**(4), 1384–1421.

Durlauf, S. N. and Fafchamps, M. (2005), Social capital, *in* P. Aghion and S. N. Durlauf, eds, 'Handbook of Economic Growth, Volume 1B', Elsevier B.V., Amsterdam, The Netherlands, pp. 1639–1699.

Fafchamps, M. and Gubert, F. (2007), 'The formation of risk sharing networks', *Journal of Development Economics* **83**(2), 326–350.

Fafchamps, M. and Lund, S. (2003), 'Risk-sharing networks in rural Philippines', *Journal of Development Economics* **71**(2), 261–287.

Fearon, J. D., Humphreys, M. and Weinstein, J. M. (2009), 'Can development aid contribute to social cohesion after civil war? Evidence from a field experiment in post-conflict Liberia', *American Economic Review* **99**(2), 287–291.

Fearon, J. D., Humphreys, M. and Weinstein, J. M. (2015), 'How does development assistance affect collective action capacity? Results from a field experiment in post-conflict Liberia', *American Political Science Review* **109**(3), 450–469.

Feigenberg, B., Field, E. and Pande, R. (2013), 'The economic returns to social interaction: Experimental evidence from microfinance', *Review of Economic Studies* **80**(4), 1459–1483.

Fisher, S. R. A. (1935), *The Design of Experiments*, Edinburgh and London: Oliver and Boyd.

Gagnon, J. and Goyal, S. (2017), 'Networks, markets, and inequality', *American Economic Review* **107**(1), 1–30.

Giné, X., Jakiela, P., Karlan, D. and Morduch, J. (2010), 'Microfinance games', *American Economic Journal: Applied Economics* **2**(3), 60–95.

GoTG (2006), Gambia – community-driven development project, Project Implementation Manual, Government of The Gambia.

Grossman, H. I. (1992), 'Foreign aid and insurrection', *Defence and Peace Economics* **3**(4), 275–288.

Gugerty, M. K. and Kremer, M. (2008), 'Outside funding and the dynamics of participation in community associations', *American Journal of Political Science* **52**(3), 585–602.

Guiso, L., Sapienza, P. and Zingales, L. (2004), 'The role of social capital in financial development', *American Economic Review* **94**(3), 526–556.

Heß, S. H. (2017), 'Randomization inference with Stata: A guide and software', *Stata Journal* **17**(3), 630–651.

Heß, S. H., Jaimovich, D. and Schündeln, M. (2018), Community-driven deforestation? Experimental evidence from a rural development program in West African drylands, Working Paper, Goethe University Frankfurt.

Humphreys, M., de la Sierra, R. S. and Van der Windt, P. (2015), Social engineering in the tropics: A grassroots democratization experiment in the Congo, Working Paper, Columbia University.

Ishiguro, S. (2016), 'Relationships and growth: On the dynamic interplay between relational contracts and competitive markets in economic development', *Review of Economic Studies* **83**(2), 629–657.

Jackson, M. O., Rodriguez-Barraquer, T. and Tan, X. (2012), 'Social capital and social quilts: Network patterns of favor exchange', *American Economic Review* **102**(5), 1857–1897.

Jaimovich, D. (2015), 'Missing links, missing markets: Evidence of the transformation process in the economic networks of Gambian villages', *World Development* **66**, 645–664.

Karlan, D. (2007), 'Social connections and group banking.', *Economic Journal* **117**(517), F52–F84.

Karlan, D., Mobius, M., Rosenblat, T. and Szeidl, A. (2009), 'Trust and social collateral', *Quarterly Journal of Economics* **124**(3), 1307–1361.

Khwaja, A. I. (2009), 'Can good projects succeed in bad communities?', *Journal of Public Economics* **93**(7), 899–916.

King, E. and Samii, C. (2014), 'Fast-track institution building in conflict-affected countries? Insights from recent field experiments', *World Development* **64**, 740–754.

Knack, S. and Keefer, P. (1997), 'Does social capital have an economic payoff? A cross-country investigation', *Quarterly Journal of Economics* **112**(4), 1251–1288.

Kranton, R. E. (1996), 'Reciprocal exchange: A self-sustaining system', *American Economic Review* **86**(4), 830–851.

Krishnan, P. and Sciubba, E. (2009), 'Links and architecture in village networks', *Economic Journal* **119**(537), 917–949.

Labonne, J. and Chase, R. S. (2011), 'Do community-driven development projects enhance social capital? Evidence from the Philippines', *Journal of Development Economics* **96**(2), 348–358.

Lee, L.-f., Liu, X. and Lin, X. (2010), 'Specification and estimation of social interaction models with network structures', *Econometrics Journal* **13**(2), 145–176.

Leider, S., Möbius, M. M., Rosenblat, T. and Do, Q.-A. (2009), 'Directed altruism and enforced reciprocity in social networks', *Quarterly Journal of Economics* **124**(4), 1815–1851.

Ligon, E., Thomas, J. P. and Worrall, T. (2002), 'Informal insurance arrangements with limited commitment: Theory and evidence from village economies', *Review of Economic Studies* **69**(1), 209–244.

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

MacKinnon, J. G. and Webb, M. D. (2017), 'Wild bootstrap inference for wildly different cluster sizes', *Journal of Applied Econometrics* **32**(2), 233–254.

Mansuri, G. and Rao, V. (2012), *Localizing development: Does participation work?*, Washington, DC: World Bank.

Morel, A., Watanabe, M. and Wrobel, R. (2009), Delivering assistance to conflict-affected communities: The BRA-KDP program in Aceh, Working Paper 53715, Washington, DC: World Bank.

Nguyen, T. C. and Rieger, M. (2017), 'Community-driven development and social capital: Evidence from Morocco', *World Development* **91**, 28–52.

Nunn, N. and Qian, N. (2014), 'US food aid and civil conflict', *American Economic Review* **104**(6), 1630–66.

Olken, B. A. (2007), 'Monitoring corruption: Evidence from a field experiment in Indonesia', *Journal of Political Economy* **115**(2), 200–249.

Platteau, J.-P. (2004), 'Monitoring elite capture in community-driven development', *Development and Change* **35**(2), 223–246.

Putnam, R. D. (2000), *Bowling alone: The collapse and revival of American community*, New York: Simon and Schuster.

Ray, D. and Esteban, J. (2017), 'Conflict and development', *Annual Review of Economics* **9**, 263–293.

Rosenbaum, P. (2002), *Observational Studies*, New York: Springer.

Rosenzweig, M. R. (1988), 'Risk, implicit contracts and the family in rural areas of low-income countries', *Economic Journal* **98**(393), 1148–1170.

Schechter, L. and Yuskavage, A. (2011), 'Inequality, reciprocity, and credit in social networks', *American Journal of Agricultural Economics* **94**(2), 402–410.

Udry, C. (1990), 'Credit markets in Northern Nigeria: Credit as insurance in a rural economy', *World Bank Economic Review* **4**(3), 251–269.

Udry, C. and Conley, T. (2004), Social networks in Ghana, Discussion Paper 888, Yale University Economic Growth Center.

von Braun, J. and Webb, P. (1989), 'The impact of new crop technology on the agricultural division of labor in a West African setting', *Economic Development and Cultural Change* **37**(3), 513–534.

Voors, M., Turley, T., Bulte, E., Kontoleon, A. and List, J. A. (forthcoming), 'Chief for a day: Elite capture and management performance in a field experiment in Sierra Leone', *Management Science* .

White, H., Menon, R. and Waddington, H. (2018), Community-driven development: Does it build social cohesion or infrastructure?, Working Paper 30, International Initiative for Impact Evaluation.

Wong, S. (2012), What have been the impacts of World Bank community-driven development programs? CDD impact evaluation review and operational and research implications, Working Paper 69541, Washington, DC: World Bank.

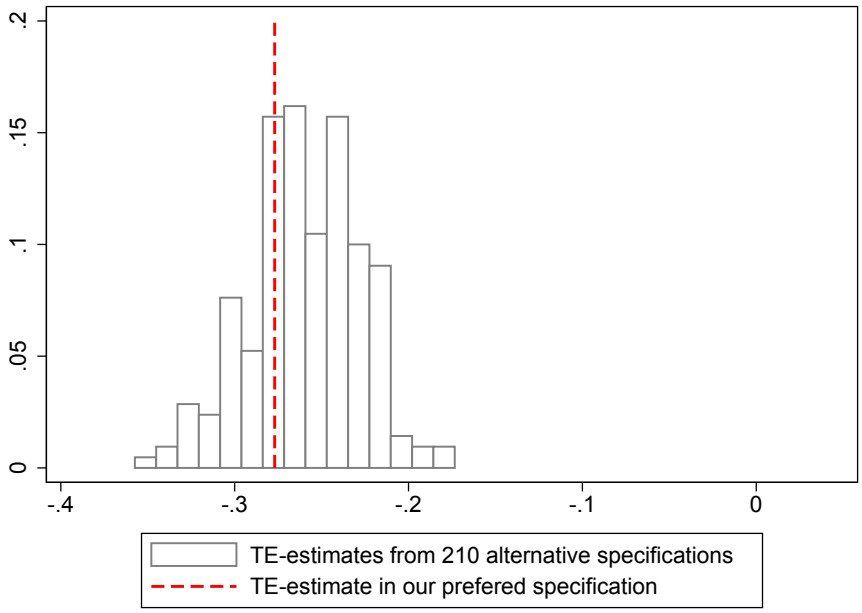
World Bank (2006), Gambia – Community-driven development project, Project Appraisal Document 36786-GM, Washington, DC: World Bank.

World Bank (2018), 'Community-driven development'. www.worldbank.org/en/topic/communitydrivendevelopment#2 [accessed April 13, 2018].

Appendix

7 Robustness of the Dyadic Average Treatment Effect Estimate

Figure A3: Robustness: Distribution of ATE-Estimates Across Alternative Specifications



Underlying this figure is the specification presented in Table 3, column 1. Alternative specifications are obtained by dropping any single or any possible combination of two control variable sets from that specification. Control variable sets can be single variables (e.g., the village-level controls, or the dyadic controls), groups of fixed effects (e.g., ward fixed effects, network fixed effects, or ethnicity fixed effects), or pairs of variables (household characteristics enter the regression twice, once for the receiving and once for the giving end of a dyad). Our main specifications contains 20 control variable sets (elite^{any}, marginal^{any}, 3 dyadic control variables, 2 ethnicity fixed effects, ward fixed effects as well as enumerator fixed effects, 4 household-level controls, and 6 village-level control variables), hence there are $\binom{20}{2} + \binom{20}{1} = 210$ alternative specifications in which one or two sets are dropped. The median ATE estimate of the 210 alternative specifications is -0.26.

Table A17: Excluding Both Wards With Unequal Numbers of Treatment and Control Villages

	(1) economic (pooled)	(2) economic (vcv)	(3) land	(4) labor	(5) inputs	(6) food	(7) gifts	(8) credit
treatment	-0.290	-0.051	-0.202	-0.200	0.111	-0.840	-0.378	-0.232
	(0.002)$^{\bullet\bullet\circledast}$	(0.001)$^{\bullet\bullet\circledast}$	(0.102)	(0.102)	(0.644)	(0.000)$^{\bullet\bullet\bullet}$	(0.002)$^{\bullet\bullet\bullet}$	(0.070)$^{\circledast}$
elite^{any}	0.446	0.079	0.549	0.664	0.340	0.547	0.256	0.319
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.063)*	(0.002)***	(0.017)**	(0.017)**
marginal^{any}	-0.138	-0.023	-0.190	0.020	-0.296	-0.225	-0.025	-0.111
	(0.020)**	(0.024)**	(0.024)**	(0.865)	(0.019)**	(0.029)**	(0.712)	(0.210)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓							
dyads	805248	134208	134208	134208	134208	134208	134208	134208
households	2462	2462	2462	2462	2462	2462	2462	2462
control mean dep. var.	1.376	-0.010	1.177	1.485	1.731	1.936	0.845	1.079

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group.

Table A18: Excluding the Ward Where no Data for One Control Village Could Be Collected Because Villagers Had Relocated to Senegal

	(1) economic (pooled)	(2) economic (vcv)	(3) land	(4) labor	(5) inputs	(6) food	(7) gifts	(8) credit
treatment	-0.274	-0.049	-0.148	-0.204	0.095	-0.744	-0.397	-0.249
	(0.003)$^{\bullet\bullet\circledast}$	(0.001)$^{\bullet\bullet\circledast}$	(0.252)	(0.100)	(0.686)	(0.000)$^{\bullet\bullet\bullet}$	(0.001)$^{\bullet\bullet\bullet}$	(0.047)$^{\circledast\circledast}$
elite^{any}	0.405	0.071	0.523	0.595	0.326	0.511	0.217	0.256
	(0.000)***	(0.000)***	(0.000)***	(0.001)***	(0.063)*	(0.002)***	(0.037)**	(0.058)*
marginal^{any}	-0.112	-0.019	-0.194	0.051	-0.237	-0.188	-0.056	-0.047
	(0.053)*	(0.050)*	(0.022)**	(0.663)	(0.057)*	(0.065)*	(0.396)	(0.613)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓							
dyads	842208	140368	140368	140368	140368	140368	140368	140368
households	2596	2596	2596	2596	2596	2596	2596	2596
control mean dep. var.	1.410	-0.004	1.175	1.511	1.837	1.896	0.898	1.141

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group.

Table A19: Excluding the Ward Where no Data for One Control Village Could Be Collected Due to Incomplete Data in 2009

	(1) economic (pooled)	(2) economic (vcv)	(3) land	(4) labor	(5) inputs	(6) food	(7) gifts	(8) credit
treatment	-0.294	-0.051	-0.151	-0.175	0.046	-0.889	-0.368	-0.229
	(0.001)$^{\bullet\bullet\circledast}$	(0.001)$^{\bullet\bullet\circledast}$	(0.246)	(0.170)	(0.844)	(0.000)$^{\bullet\bullet\bullet}$	(0.002)$^{\bullet\bullet\circledast}$	(0.081)$^{\circledast}$
elite^{any}	0.449	0.079	0.538	0.640	0.353	0.577	0.253	0.334
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.046)**	(0.001)***	(0.013)**	(0.010)**
marginal^{any}	-0.124	-0.021	-0.211	-0.007	-0.265	-0.187	-0.009	-0.067
	(0.031)**	(0.033)**	(0.012)**	(0.949)	(0.033)**	(0.070)*	(0.891)	(0.469)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓							
dyads	872832	145472	145472	145472	145472	145472	145472	145472
households	2640	2640	2640	2640	2640	2640	2640	2640
control mean dep. var.	1.406	-0.006	1.170	1.513	1.786	1.994	0.859	1.113

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group.

Table A20: Directed Dyadic ATE Regressions – Probit Specification

	(1) economic (pooled)	(2) land	(3) labor	(4) inputs	(5) food	(6) gifts	(7) credit
treatment	−0.283	−0.147	−0.142	−0.145	−0.693	−0.420	−0.258
	(0.000)$^{\bullet\bullet\circledast}$	(0.185)	(0.147)	(0.415)	(0.000)$^{\bullet\bullet\bullet}$	(0.000)$^{\bullet\bullet\bullet}$	(0.016)$^{\circledast\circledast}$
elite^{any}	0.365	0.467	0.498	0.335	0.474	0.196	0.213
	(0.000)***	(0.000)***	(0.000)***	(0.032)**	(0.000)***	(0.006)***	(0.037)**
marginal^{any}	−0.158	−0.258	−0.065	−0.256	−0.228	−0.084	−0.054
	(0.004)***	(0.002)***	(0.532)	(0.042)**	(0.024)**	(0.130)	(0.541)
dyads	909792	151632	151632	151632	151632	151632	151632
households	2774	2774	2774	2774	2774	2774	2774

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group.

Table A21: Dyadic Regressions, Treatment Effect, Link Intensity

	(1) economic (vcv)	(2) land (ha)	(3) labor (p×d)	(4) input (#categ.)	(5) food (kgs)	(6) gifts (GMD)	(7) credit (GMD)
treatment	-0.033	-0.000	-0.123	0.000	-0.052	-0.497	-1.183
	(0.031)$^{\bullet\circledast}$	(0.964)	(0.245)	(0.862)	(0.000)$^{\bullet\bullet\bullet}$	(0.001)$^{\bullet\bullet\circledast}$	(0.088)$^{\circledast}$
elite^{any}	0.054	0.010	0.257	0.003	0.023	0.223	1.290
	(0.000)***	(0.002)***	(0.051)*	(0.077)*	(0.086)*	(0.173)	(0.118)
marginal^{any}	-0.019	-0.005	0.068	-0.002	-0.015	-0.146	0.294
	(0.051)*	(0.007)***	(0.473)	(0.070)*	(0.114)	(0.096)*	(0.594)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓
dyads	151495	151631	151620	151632	151508	151632	151632
households	2774	2774	2774	2774	2774	2774	2774
control mean dep. var.	0.000	0.018	0.482	0.020	0.116	1.086	5.058

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets $\bullet$ indicate significance levels preserved under randomization inference, while 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group. 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.

Table A22: Undirected Dyadic ATE Regressions

	(1) economic (pooled)	(2) economic (vcv)	(3) land	(4) labor	(5) inputs	(6) food	(7) gifts	(8) credit
treatment	-0.520	-0.063	-0.209	-0.264	0.098	-1.483	-0.729	-0.532
	(0.002)$^{\bullet\bullet\circledast}$	(0.001)$^{\bullet\bullet\circledast}$	(0.434)	(0.188)	(0.823)	(0.000)$^{\bullet\bullet\bullet}$	(0.001)$^{\bullet\bullet\bullet}$	(0.023)$^{\bullet\circledast}$
elite^{any}	0.758	0.091	1.006	0.965	0.647	1.003	0.438	0.486
	(0.000)***	(0.000)***	(0.000)***	(0.001)***	(0.050)*	(0.002)***	(0.020)**	(0.052)*
marginal^{any}	-0.208	-0.024	-0.395	-0.137	-0.391	-0.285	-0.077	0.034
	(0.028)**	(0.028)**	(0.017)**	(0.403)	(0.088)*	(0.116)	(0.528)	(0.844)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓							
dyads	454896	75816	75816	75816	75816	75816	75816	75816
households	2774	2774	2774	2774	2774	2774	2774	2774
control mean dep. var.	2.709	0.000	2.326	2.642	3.628	3.638	1.752	2.267

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets $\bullet$ indicate significance levels preserved under randomization inference, while starred bullets $\circledast$ indicate significance levels that are only sustained by the cluster-robust standard errors. Units of observation are undirected dyads. The dependent variable takes on the value 100 if a dyad had a link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group.

Table A23: Dyadic Regressions, Average Treatment Effect, only using village-level controls and enumerator fixed effects

	(1) economic (pooled)	(2) economic (vcv)	(3) land	(4) labor	(5) inputs	(6) food	(7) gifts	(8) credit
treatment	-0.183	-0.032	-0.026	0.004	0.175	-0.747	-0.310	-0.196
	(0.039)$^{\bullet\circledast}$	(0.034)$^{\bullet\circledast}$	(0.840)	(0.981)	(0.437)	(0.000)$^{\bullet\bullet\bullet}$	(0.005)$^{\bullet\bullet\circledast}$	(0.087)$^{\circledast}$
village controls	✓	✓	✓	✓	✓	✓	✓	✓
Enumerator fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
network fixed effects	✓							
dyads	909792	151632	151632	151632	151632	151632	151632	151632
households	2774	2774	2774	2774	2774	2774	2774	2774
control mean dep. var.	1.437	0.000	1.168	1.536	1.886	1.952	0.910	1.171

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group.

7.1 Difference-in-Differences

Table A24: Difference-in-Differences With the Data From 2009

	economic (directed)		economic (undirected)		kinship/placebo (undirected)	
	(1)	(2)	(3)	(4)	(5)	(6)
	any network	any network	any network	any network	kinship	kinship
treatment	0.007		0.260		−0.784	
	(0.989)		(0.733)		(0.399)	
post	2.387	2.179	4.630	4.302	−7.022	−7.042
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
treatment × post	−1.067	−1.171	−1.922	−2.064	0.004	0.042
	(0.081)•	(0.056)•	(0.034)••	(0.024)••	(0.997)	(0.972)
dyadic/indiv. controls	✓	✓	✓	✓	✓	✓
village controls	✓		✓		✓	
ward FEs	✓		✓		✓	
village FE		✓		✓		✓
observations (dyads)	292202	292202	146101	146101	146101	146101
observations (households)	3175	3175	3175	3175	3175	3175
villages	56	56	56	56	56	56
control mean dep. variable	6.332	6.332	10.572	10.572	8.852	8.852

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The first wave was collected in 2009, at the onset of the CDD program and the two waves can be matched at the household level. Several network questions were substantially different (the 2009 data only includes the networks of credit, inputs, labor, and land). To obtain comparable measures for 2009 and 2014 we form the union of all networks, combining them into a single 'economic' network, in the same way as described in Section 2.5. Thus, the interpretation of coefficients deviates slightly from that in our main tables, but is qualitatively comparable: the control mean of the dependent variable in columns 1 and 2 indicates that 6.33% of dyads have had an economic link on *any* of the recorded networks. The 'post'-variable is a dummy indicating whether an observation stems from the 2014 wave. The treatment effect is thus captured by the interaction term of 'treatment' and 'post'.

8 Project-Related Descriptive Statistics

Table A25: Evidence for Project Implementation

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

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

Table A26: Evidence for Project Awareness and Functionality

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

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

Table A27: Project Failure Rates

Projects	failure rate	distinct projects
reported by households:		
CDD	30%	60
non-CDD in treatment villages	23%	92
non-CDD in control villages	25%	122
reported by village authorities:		
CDD	25%	64
non-CDD in treatment villages	29%	45
non-CDD in control villages	29%	82

Notes: The project reports by households are based on the in-depth survey and hence aggregated from responses by ten households per village. The number of projects reported by the households is a lot larger than what is listed by the village authorities for two reasons. First, households often did not remember the funding agency/NGO of a project correctly or mentioned only part of the project's deliverable. Thus matching projects across household was in several cases not possible. This does not apply to the CDD, where we had administrative data available to verify the reports. Also CDD projects were prompted if the household did not list them. Second, some individual households listed very small projects by outsiders, such as small money grants by private groups for school books, which were omitted by the village authorities as they did not consider them development projects.

9 Other Treatment Effect Regressions

9.1 Wealth

Table A28: In-Depth Survey: Asset/Wealth Variables (1)

	(1) plough	(2) seeder	(3) cart	(4) draught animals
treatment	−0.075	−0.040	−0.073	−0.116
	(0.065)$^{\circledast}$	(0.463)	(0.076)$^{\circledast}$	(0.166)
controls (see notes)	✓	✓	✓	✓
households	550	550	550	550
control mean dep. var.	0.591	0.555	0.434	1.004

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies.

Table A29: In-Depth Survey: Asset/Wealth Variables (2)

	(1) bicycle	(2) mobile	(3) smart phone	(4) radio	(5) generator	(6) motorbike	(7) car	(8) tv	(9) AC
treatment	0.015	−0.031	−0.016	0.005	−0.072	−0.029	0.009	−0.075	0.001
	(0.715)	(0.041)$^{\bullet\circledast}$	(0.340)	(0.892)	(0.010)$^{\bullet\bullet}$	(0.209)	(0.330)	(0.019)$^{\bullet\circledast}$	(0.904)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓	✓
households	550	548	549	548	550	550	550	550	550
control mean dep. var.	0.569	0.960	0.055	0.737	0.179	0.128	0.015	0.124	0.007

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies.

Table A30: In-Depth Survey: Asset/Wealth Variables (3)

	(1) grid electr.	(2) tap water	(3) toilet	(4) bank account	(5) floor quality	(6) roof quality	(7) wall quality
treatment	0.011	0.012	0.025	0.016	0.040	−0.033	−0.050
	(0.521)	(0.150)	(0.523)	(0.544)	(0.755)	(0.603)	(0.697)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓
households	550	550	548	543	550	550	550
control mean dep. var.	0.015	0.000	0.860	0.123	2.401	2.584	1.818

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies.

9.2 Social Changes

Table A31: Institutional Change in Village-Level Decision Making

	vill. meetings (past year)		vill. decision-making		unjust decisions at the village		
	(1) attend and speak	(2) #meetings	(3) favors voting	(4) actual voting	(5) speak up against	(6) can change	(7) talks to chief
Panel A: without interactions							
treatment	−0.043	−0.119	0.009	0.010	0.039	0.071	−0.035
	(0.233)	(0.771)	(0.725)	(0.676)	(0.538)	(0.124)	(0.706)
Panel B: with interactions							
treatment	−0.066	0.192	0.062	−0.009	0.039	0.063	−0.041
	(0.123)	(0.656)	(0.023)•⊛	(0.742)	(0.583)	(0.250)	(0.709)
marginalized	−0.272	−0.064	0.143	−0.050	−0.316	−0.369	−0.500
	(0.001)***	(0.915)	(0.009)***	(0.278)	(0.014)**	(0.001)***	(0.003)***
treatment × marginalized	0.094	−1.818	−0.291	0.107	−0.048	−0.009	−0.033
	(0.399)	(0.036)•⊛	(0.000)•••	(0.232)	(0.750)	(0.953)	(0.883)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓
households	540	545	546	505	547	547	548
control mean dep. var.	0.641	5.826	0.375	0.075	1.250	1.007	1.577

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies. Dep. var. in columns 5 and 6 are measured on a three point scale; in column 7 on a five point scale. In column 5 the answers range from "Would not speak up against an unjust decision" to "Would definitely speak up". In column 6 the answers range from "No chance my household could change an unjust decision within the village" to "It is very likely my household could change an unjust decision". In column 7 the answers range from "Never spoke to the Alkalo or the VDC about village issues in the last year" to "Almost every day".

10 Project Benefits

Table A32: Inequality in Project Benefits?

	CDD villages		control villages
	(1) benefited from CDD sub-projects	(2) benefited from non-CDD projects	(3) benefited from non-CDD projects
$\mathbb{1}$(land > 2ha)	0.305	0.191	−0.040
	(0.003)***	(0.091)*	(0.703)
land gini	−2.513	1.084	−0.153
	(0.019)**	(0.268)	(0.739)
elite	0.275	0.150	−0.201
	(0.065)*	(0.190)	(0.175)
marginalized	0.157	−0.031	−0.015
	(0.203)	(0.809)	(0.909)
controls (see notes)	✓	✓	✓
households	274	274	274
mean dep. var.	2.349	2.222	2.418

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies. The units of observation are households. The dependent variable is the response across all project reports by that household for projects in the respective category to the survey question "Does/did your household benefit from the project? 0=no; 1=very little; 2=moderately; 3=a lot". Missing responses are imputed by taking each village's lowest response.

Table A33: Self-Declared Benefit From Projects

	absolute benefit		relative benefit	
	(1)	(2)	(3)	(4)
CDD project	−0.009		−0.070	
	(0.953)		(0.155)	
CDD project × marginalized	−0.466	−0.462	0.082	−0.027
	(0.053)*	(0.022)**	(0.111)	(0.578)
CDD project × elite	0.171	0.198	0.068	0.179
	(0.293)	(0.276)	(0.236)	(0.010)**
household fixed effects	✓	✓	✓	✓
project fixed effects		✓		✓
observations (household×project)	1188	1188	1178	1178
mean dep. var. (non-CDD)	2.540	2.540	0.961	0.961
dep. var. range	0 - 3	0 - 3	0 - 2	0 - 2

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. Units of observation are reports about development projects elicited from 10 households per village. Observations are weighted by the inverse of the number of projects listed by each household, to give the same weight to each household. The dependent variable records project benefit on a Likert scale. Columns 1 and 2 use the question "Does/did your household benefit from the project? 0=no; 1=very little; 2=moderately; 3=a lot". Columns 3 and 4 use the question "Relative to other households in the village, how does/did your household benefit from the project? 0=less; 1=the same; 2=more".

Table A34: Inequality in Project Benefits?

	absolute benefit		relative benefit	
	(1) benefited my hh	(2) benefited my hh	(3) benefited my hh more	(4) benefited my hh more
normal proj × treatment village	−0.064	−0.123	−0.040	−0.053
	(0.527)	(0.321)	(0.244)	(0.173)
cdd proj × treatment village	−0.300	−0.380	−0.102	−0.115
	(0.031)**	(0.013)**	(0.005)***	(0.005)***
marginalized	0.045	−0.039	−0.018	−0.025
	(0.635)	(0.795)	(0.521)	(0.576)
elite	0.167	−0.141	0.033	−0.035
	(0.147)	(0.383)	(0.491)	(0.484)
normal proj × treatment village × marginalized		0.236		−0.018
		(0.292)		(0.783)
cdd proj × treatment village × marginalized		0.129		0.023
		(0.511)		(0.684)
normal proj × treatment village × elite		0.277		0.131
		(0.240)		(0.321)
cdd proj × treatment village × elite		0.610		0.100
		(0.010)**		(0.181)
hh controls	✓	✓	✓	✓
observations (household×project)	1185	1185	1175	1175
mean dep. var. (non-CDD)	2.551	2.551	0.964	0.964

Notes: •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. Units of observation are reports about development projects elicited from 10 households per village. Observations are weighted by the inverse of the number of projects listed by each household, to give the same weight to each household. This table includes prompted reports for CDD projects. If the sample is restricted to reports that were given without prompting, the results in columns 3 and 4 remain qualitatively unchanged, i.e., for CDD projects more people perceive to benefit less than others. In column 1 and 2, the coefficients for 'CDD project' turn insignificant and decrease by a third. While the two interaction effect coefficients for treatment×elite increase in size and significance.

Table A35: Elite Capture: Links and Wealth

	(1) economic (pooled)	(2) economic (vcv)	(3) economic (pooled)	(4) economic (vcv)
treatment	-0.326	-0.057	-0.221	-0.036
	(0.002)$^{\bullet\bullet\circledast}$	(0.002)$^{\bullet\bullet\circledast}$	(0.020)$^{\bullet\circledast}$	(0.025)$^{\bullet\circledast}$
elite^{any}	0.390	0.068	0.395	0.069
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
marginal^{any}	-0.082	-0.014	-0.098	-0.017
	(0.138)	(0.135)	(0.083)*	(0.069)*
$\mathbb{1}$(land > 2ha)^{any}	0.171	0.035		
	(0.045)**	(0.020)**		
treatment × $\mathbb{1}$(land > 2ha)^{any}	0.074	0.013		
	(0.511)	(0.505)		
top 2 wealth tiers^{any}			0.195	0.035
			(0.012)**	(0.007)***
treatment × top 2 wealth tiers^{any}			-0.104	-0.022
			(0.365)	(0.267)
controls (see notes)	✓	✓	✓	✓
network fixed effects	✓		✓	
dyads	909600	151600	894216	149036
households	2774	2774	2756	2756
control mean dep. var.	1.438	0.000	1.456	0.003

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ 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 link and 0 otherwise. Coefficient estimates thus should be interpreted as percentage points. All regressions control for ward fixed effects and the same set of control variables. Control variables are the village-level variables listed in Table 1, Panel B. Household-level control variables as listed in Panel C of Table 1, as well as ethnicity and enumerator dummies, enter the regressions once for the sending and once for the receiving household of the dyad. Additional dyadic controls are indicators for kinship ties, shared ethnicity, and interview group. The dummy variable 'top 2 wealth tiers' indicates whether respondents considered their household to belong to one of the two richest categories on four-step wealth ranking of households within their village.

11 CBO Membership, Participation, and Contribution

A closer look at the type of CBOs in which participation is reduced may provide some clues for the channels through which the CDD project may reduce some forms of social capital. The disaggregated results are presented in Table A36. Participation in youth associations is reduced by 13% in CDD villages with respect to control villages. This could be an effect of the failure of the CDD project to empower youths as a marginalized group. The other statistically significant results are for groups directly related to production, namely producers' cooperatives and *kafos*. The latter are particularly relevant groups. A *kafo* is an organized workforce of villagers who can be hired for a fixed wage by a household, and who

sometimes participate in the provision of public goods as well. In control villages 59% of households declare having members belonging to a *kafo*, and the effect of the CDD project is a reduction of eight percentage points in membership. Given that *kafos* are a CBO closely linked to production, the exchanges of labor, and thus to our economic network measures, we interpret these results as a corroboration of our finding of reduced economic network links.

Table A36: Participation in Village CBOs

	(1) producers	(2) water	(3) women	(4) youth	(5) kafo	(6) credit	(7) forest	(8) seed bank	(9) osusu
Panel A: membership									
treatment	−0.053	0.008	−0.015	−0.081	−0.078	−0.012	−0.027	−0.010	−0.031
	(0.058)$^{\circledast}$	(0.726)	(0.479)	(0.024)$^{\bullet\circledast}$	(0.016)$^{\bullet\bullet\circledast}$	(0.564)	(0.282)	(0.311)	(0.579)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓	✓
households	546	544	550	550	550	548	549	548	546
control mean	0.085	0.117	0.905	0.646	0.588	0.048	0.099	0.029	0.594
Panel B: attendance									
treatment	−0.053	0.016	−0.013	−0.074	−0.082	−0.008	−0.026	−0.007	−0.023
	(0.069)$^{\circledast}$	(0.500)	(0.574)	(0.027)$^{\bullet\circledast}$	(0.008)$^{\bullet\bullet\circledast}$	(0.694)	(0.258)	(0.364)	(0.673)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓	✓
households	544	543	538	541	544	548	548	548	543
control mean	0.078	0.110	0.892	0.629	0.580	0.040	0.095	0.018	0.587
Panel C: contribution									
treatment	−0.048	0.025	−0.015	−0.063	−0.086	−0.005	−0.014	−0.006	−0.017
	(0.095)$^{\circledast}$	(0.179)	(0.678)	(0.069)$^{\circledast}$	(0.006)$^{\bullet\bullet\circledast}$	(0.760)	(0.552)	(0.449)	(0.751)
controls (see notes)	✓	✓	✓	✓	✓	✓	✓	✓	✓
households	544	543	531	534	545	547	548	547	538
control mean	0.070	0.096	0.815	0.589	0.578	0.037	0.077	0.022	0.538

Notes: $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$, p-values in parentheses account for clustering at the village level. Where bullets are used, randomization inference was conducted to obtain alternative p-values: filled bullets ● indicate significance levels preserved under randomization inference, while starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. The units of observation are households. All regressions control for ward fixed effects and the same set of control variables. Control variables are the household- and village-level variables listed in Panels B and C of Table 1 as well as ethnicity and enumerator fixed dummies.