# Development Projects and Economic Networks: Lessons From Rural Gambia*

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**Abstract**

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

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# 1 Introduction

Networks of informal relationships take on a central role in rural societies of less developed economies. Where markets are incomplete, intra-household networks help households to enforce informal contracts (Karlan, 2007; Karlan et al., 2009; Giné et al., 2010; Jackson et al., 2012; Chandrasekhar et al., 2018), pool risk to deal with adverse shocks (Rosenzweig, 1988; Fafchamps and Lund, 2003; De Weerdt and Dercon, 2006), and aggregate or diffuse information (Bandiera and Rasul, 2006; Conley and Udry, 2010; Beaman and Magruder, 2012; Banerjee et al., 2013; Alatas et al., 2016).

Although the role of networks in the lives of the poor is well established, much less is known about how their structure and their role in households' welfare evolve. In particular, despite prominent theoretical work on this question (Kranton, 1996; Gagnon and Goyal, 2017), not much is known empirically about how networks of interactions 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 and have a number of features that likely affect networks directly and indirectly. In this paper, we contribute towards filling this gap in the literature, providing empirical evidence from a large Community-Driven Development (CDD) program in The Gambia. Starting in 2008, this World Bank-financed program allocated funds for village-level development projects to about a third of all rural villages in The Gambia. Importantly, the almost 500 treated villages were chosen randomly from a set of over 900 eligible poor villages. The funding was significant, with a per-household allocation that is roughly equivalent to half the GDP per capita in The Gambia.

The typical CDD program is multidimensional with regard to goals and procedural features. A primary goal of the participatory approach is to affect economic well-being through improved targeting and project sustainability. Frequently, these programs also aim at building social capital and causing institutional change (Casey et al., 2012; Mansuri and Rao, 2012; White et al., 2018; Casey, 2018). Because of their multifaceted nature, CDD programs can affect the costs and benefits of informal economic interactions through a variety of channels, thus altering the network of economic transactions. In particular, existing theoretical and empirical work suggests the following three reasons why CDD programs may have a significant impact on networks of informal economic flows:

(i) Deciding on and implementing CDD projects requires many joint activities, leading to frequent social interactions. 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, and are supposed to form organizing committees that bring groups together that might otherwise have little contact with each other. Many implemented projects also require joint activities in administering and maintaining infrastructure or machinery provided through the program. Frequent interactions theoretically increase social capital, thus lowering the costs of interacting with others, and have empirically been shown to increase economic interactions beyond the initial program in a microfinance context (Feigenberg et al., 2013). However, empirical work is less clear on the effect of CDD programs on specific measures of social capital that are relevant for economic interactions such as group membership and trust (Avdeenko and Gilligan, 2015; Nguyen and Rieger, 2017). Therefore, the direction of the effect of this aspect of CDD programs on informal economic flows is ambiguous.

(ii) CDD projects may bring significant economic benefits. The main goal of CDD programs is typically to generate economic improvements at the individual and the village level. Theoretically, positive economic effects may result in a reduced need for insurance (e.g., Fafchamps and Lund, 2003) and thus a reduced benefit from having informal economic transactions. Income growth or shifts in the production technology may also induce a shift to market-based transactions, thus reducing reciprocated transactions (Kranton, 1996). Both of the above effects would reduce the number of informal transactions.

(iii) Benefits may be unequally distributed or CDD projects may fail to generate benefits at all. In particular, the participatory approach and the large size of the funds allocated to villages increase the incentives and opportunities for elite capture and favoritism (Platteau, 2004; Bardhan and Mookherjee, 2000; Bandiera et al., 2018). Both, unequally distributed benefits and failed projects, may lead to internal divisions (Barron et al., 2011), resulting in lower social capital, which implies higher costs of interacting with other villagers and thus fewer informal economic transactions.[1]

In sum, there are several features of the typical CDD program that may affect social interactions and economic transactions in both intended and unintended ways. The direction of the net effect of the program on interactions remains an empirical question.

To study the effects of a particular CDD program on networks of informal interactions, we collected post-treatment data in 2014 on the full set of interactions among the universe of households in 56 villages (half of them treatment villages). These data cover six different economic domains (land, labor, inputs, food, gifts, and credit). We also collected data for two social domains (namely an indicator of friendship, and kinship links). We interviewed all households in each village and designed the survey to record all relevant informal transactions among them. Thus, we started from the full set of all possible links among village households and collected data on whether informal transactions exist among households. In our main analysis, we combine all information for the six economic domains by taking the union to form one network of informal economic transactions. 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 allows for a straightforward estimation

[1]Qualitative interviews after data collection revealed, for example, that in one sample village the oldest family in the village broke apart into two quarreling factions over disputes involving a tractor that the village had bought from the project's money and which it struggled to maintain. In another village a passenger bus was bought. At the time of our interviews in 2014, a dispute had arisen about how the accruing revenues were to be used. By 2016, when we returned, the designated bus driver had sold off the bus for his own gain and had left the village.

of the effect of the CDD program on indicators of the network structure. Because we have a relatively small number of 56 treatment clusters, we take particular care of issues of inference by using randomization inference.[2]

Our main finding is that the CDD treatment significantly reduces informal economic interactions. In our main specification, where we use dyadic regressions, the estimated magnitude of the effect implies on average an economically significant loss of about one in six transactions between households. These findings are robust to using various alternative ways to aggregate the observed flows and also to moving from the dyadic to the household level or to the village level. Finally, the main results are robust to a number of approaches to deal with two limitations of our set-up, namely the lack of network data before the intervention, and the possibility that NGOs, the government, or other development actors target control villages more intensely in the post-treatment years.

We then investigate possible mechanisms that could lead to the overall negative effect of the program on informal economic flows. Overall, our findings are most consistent with channels (ii) and (iii) described above. First, we find that the program has positive, though modest average effects on indicators of economic well-being and outward (market) orientation. Reductions in reciprocated transactions and the reduced importance of socially supported relationships also point to an increased market orientation in treatment villages. However, the average effects on wealth, assets and animal ownership alone seem too small to fully explain the observed changes in networks. Yet, there is significant heterogeneity in these effects, which supports the other channel for which we find evidence: unequally distributed benefits and elite capture. We find significant heterogeneity in economic benefits and also evidence that households *perceive* the distribution of benefits as unequal. Unequal benefits, in turn, seem associated with the magnitude of reductions in transactions: Landowning households and members of the organizing committee report more benefits from CDD projects and marginalized households perceive that elites had more say in the implementation of the CDD projects. Initial wealth inequality (a correlate of elite capture in other work), pairwise differences in proxies for benefits (land holdings), and unequally perceived project outcomes, are all correlated with fewer economic transactions. Households in treatment villages also not only have fewer economic transactions with each other, they also report fewer social links and less participation in community-based organizations. Households from marginalized groups stand out by reporting the largest reduction in social links and attendance in village meetings. The latter finding strongly speaks against the first channel mentioned above, i.e., more frequent meetings do not lead to increases in social capital.

Although the research design limits our ability to pinpoint the exact mechanism, our interpretation of these findings is that unequally distributed benefits and de-facto or perceived elite capture lead to internal divisions, reducing social capital, which raises the cost of maintaining

[2]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, before data collection) is publicly available at http://gepris.dfg.de/gepris/projekt/250842093?language=en.

relationships, which in turn leads to reduced economic interactions.

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

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

Our paper contributes to five main literatures. First, networks are usually taken as exogenously given (e.g., 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 et al. 2014), but little is known about how networks evolve. Of particular relevance to policy is a better understanding of the effect that development interventions have on networks. Recently, a few papers have studied how networks react in response to changes in specific markets, namely the arrival of formal institutions for credit and insurance (Binzel et al., 2013; Cecchi et al., 2016; Comola and Prina, 2017; Banerjee et al., 2018). These papers report a reduction in informal arrangements between network members (Binzel et al., 2013), but they also find spillover effects into other realms, with treatments reducing interactions in networks not directly affected by the intervention and interactions with households who do not benefit from the new institution (Cecchi et al., 2016; Banerjee et al., 2018). This paper contributes to this emerging literature in that it studies an intervention that (a) constitutes a direct shock to the existing stock of (productive) capital in the village economies, (b) is not limited to one particular market, but had potential implications for various markets (e.g., markets for labor inputs, food outputs or credit), (c) has a much larger per household budget, and (d) raises significant concerns about distributional issues, including the possibility of elite capture, due to the participatory nature of the project and the size of the distributed funds.

Second, our data allow us to contribute to the recent literature on network formation that shows how cooperation—in situations lacking formal enforcement—can be achieved and sustained through social networks. Our analysis highlights the role of social proximity through "supported" relationships (Jackson et al., 2012; Karlan et al., 2009) and studies how this role changes in response to an intervention.[3]

Third, we add to the literature on the effects of CDD programs. Several meta-analyses reviewing a large body of evidence conclude that CDD programs have led to only modest improvements in infrastructure delivery and sustainability (Mansuri and Rao, 2012; Wong, 2012; White et al., 2018; Casey, 2018). Further, there is mixed evidence regarding institutions and social capital (Fearon et al., 2009; Fearon et al., 2015; Casey et al., 2012; Beath et al., 2013; King and Samii, 2014; Humphreys et al., forthcoming; Labonne and Chase, 2011; Nguyen and Rieger, 2017), including some findings that mirror our results of negative effects (Avdeenko and Gilligan, 2015). 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 economic networks. In addition, 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). We use networks analysis to contribute to this literature. Furthermore, elite capture is closely tied to the possibility of internal disputes, and our analysis also speaks to the fairly small literature relating development projects to internal disputes (Barakat, 2006; Barron et al., 2011).

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).

The remainder of this paper is structured as follows. In section 2 we lay out a conceptual framework. Section 3 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 4, while Section 5 contains additional results related to mechanisms. Finally, in Section 6 we study welfare implications of our results. Section 7 concludes.

[3] Further, we have detailed data on six different economic networks, which we collect for the universe of households within each village. Using these data, we contribute to a better understanding of network structures (independent of the random treatment), including the still scarce literature studying exchanges in the factors of production used in traditional economic activities (e.g., Udry and Conley, 2004; Krishnan and Sciubba, 2009). We also add to the still relatively small set of papers (e.g., Comola and Prina, 2017; Cruz et al., 2017) that are able to avoid the biases of network analyses based on samples of households (Chandrasekhar and Lewis, 2016).

## 2 Conceptual Framework

Our hypothesis is that informal economic transactions as well as social interactions within villages are affected by CDD programs. In this section we identify—based on existing theoretical and empirical results—three main channels and discuss empirical predictions.

Our data records the one-year history of economic transactions, which we regard as the outcome of a bilateral link formation process in which two households jointly assess whether having a transaction is beneficial to them or not (compare, e.g., Chandrasekhar, 2016; Comola and Fafchamps, 2014). To fix ideas, we group the determinants of link formation into two categories, broadly capturing benefits and costs of having an informal economic transaction. As benefits we consider for example economic gains from sharing input factors, the need to smooth income shocks, or the lack of viable market-based outside options. An example for costs is the effort required to build or maintain a social relationship (trust, social capital) necessary to have an informal economic transaction. Two households decide to have an informal economic interaction whenever benefits exceed costs.[4]

**(i) Frequent Social Interactions During Project Choice and Implementation**

This first channel is related to the costs of building and maintaining social relationships that facilitate informal economic transactions. Central features of CDD programs could imply changes to these costs by increasing social interaction among villagers. CDD programs generally require individuals to collaborate and interact frequently, in a series of meetings involving representatives of many different groups as well as in the implementation and management of the projects.[5]

Theory suggests that more interactions increase social capital. The picture painted by previous empirical work is more nuanced. On the one hand, more frequent interactions were indeed found to increase social capital in a microfinance context (Feigenberg et al., 2013). Thus, the mentioned aspects of CDD programs, which all increase interactions, plausibly imply that social capital increases. More social capital increases social network-based trust (Karlan et al., 2009), and should reduce the costs of maintaining relationships, i.e., we would expect to see an increase in informal economic flows if this mechanism prevails. On the other hand, previous studies that specifically focus on CDD programs suggest that the increase in interactions

[4]There may be other factors or general equilibrium effects—such as costs or benefits depending on the existing network of past interactions (see, e.g., Karlan et al., 2009). This is not the focus of our analysis and we group the determinants of link formation into these two categories for expositional purposes.

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

required to implement CDD projects is a substitute for other aspects of village interactions, therefore reducing aspects of social capital such as participation in community-based organizations (CBOs) and contribution to other village activities (Labonne and Chase, 2011; Avdeenko and Gilligan, 2015). Further, directly looking at the effect of a CDD program on interpersonal trust, Nguyen and Rieger (2017) find that trust is lower in Moroccan CDD villages than in a set of non-CDD villages. In line with the findings from the latter three studies, the possible reduction in trust as well as the possible substitution of different types of social capital implies that social interactions and economic transactions might also go down in treatment villages.

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

**(ii) Positive Economic Change**

This channel is related to both, the benefits and the costs of informal transactions. Because village-level projects in the Gambian CDD program are mostly intended to promote income-generating activities (as opposed to building general infrastructure or institutions, as in other CDD programs), successful projects should affect the economic situation of households. This has implications for informal economic flows as follows. Theoretically, a direct effect of an increase in individuals' wealth is that buffer stock savings go up. If wealth increases are sufficiently large, people might require less informal insurance, which suggests reduced benefits of informal transactions, in particular in networks that are related to consumption goods (e.g., Fafchamps and Lund, 2003).

Increases in economic welfare and in economic production due to income-generating activities may also lead to a shift from a gift economy to market-oriented transactions (Kranton, 1996; Ishiguro, 2016; Gagnon and Goyal, 2017). If market orientation increases, there may be fewer informal transactions in intra-village networks under the condition that these two transactions are substitutes (as suggested by Jaimovich, 2015). Instead, we expect more (possibly paid) transactions with outsiders. Further, when markets—as an outside option to informal economic transactions—develop and become more attractive, the relative benefits from socially embedded activities decrease, which would imply a reduction in informal exchanges, such as reciprocal exchanges and relational contracting. Socially embedded exchanges are characterized by positive externalities (Kranton, 1996; Ishiguro, 2016; Gagnon and Goyal, 2017). Consequently, the more individuals switch to market-based activities, the higher the costs of engaging in socially embedded activities for the remaining individuals, which makes it likely that even more switch to market-based activities. Thus, even small initial changes may increase the costs of engaging further in network-based transactions, eventually leading to larger shifts to market-based activities.

To investigate this channel, we first test for increases in proxies for household wealth and income in response to the program, using own data and census data. We also test whether consumption-related transactions are more affected than production-related transactions. Further, we test if transactions with households external to the village increase, and whether socially embedded transactions within the village decrease.

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

This channel is again mainly related to the cost of building and maintaining social relationships required for informal economic transactions. Not all households may benefit equally from projects. Although decisions are supposed to be made jointly, certain groups could steer decisions to projects from which they reap larger benefits. In fact, the focus on grassroots decisions increases the potential for elite capture (Platteau, 2004; Olken, 2007; Alatas et al., 2013). and elite capture may be more likely in more unequal villages (e.g., Bardhan and Mookherjee, 2000; Araujo et al., 2008).[6] Local administration of projects also entails the risk of favoritism, i.e., the possibility that agents who are responsible for delivering projects favor people in their social network, as was recently documented in Bandiera et al. (2018) in the context of an extension program. Projects also may fail, e.g., because of poor project choice or poor project management, reducing or entirely eliminating benefits for everyone in the village.[7]

Seeing projects and project benefits being captured by traditional elites or the members of the VDC, either for themselves or for people in their network, or seeing projects not generating benefits at all, might lead to conflict and alienate other community members (see, for example, Barron et al., 2011).[8] Further, and independent of the underlying reasons for unequal benefits, a large literature suggests that differences in income changes between groups, unequally distributed benefits, and lower-than-expected benefits induce conflict over the benefits' distribution (Grossman, 1992; Dube and Vargas, 2013; Crost et al., 2014; Mitra and Ray, 2014; Nunn and Qian, 2014; Ray and Esteban, 2017).[9] Generally speaking, dissatisfaction with procedures and outcomes in participatory programs might result in internal disputes and social disruptions (Barron et al., 2011; Lund and Saito-Jensen, 2013).

In sum, we hypothesize that as a consequence of elite capture, favoritism, and for other possible reasons, such as project failure, benefits are unequally distributed or in other ways not

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

[7] Exclusion of traditional leaders from the process might also contribute to the failure of a project if they are better at managing projects (Voors et al., 2017; Khwaja, 2009).

[8] On the flip-side, provisions against elite capture, such as the parallel institutions that are set up to run the programs, might be considered a threat by traditional authorities and lead to social tensions, as has been pointed out in the context of CDD programs by, e.g., Barakat (2006), Morel et al. (2009), King and Samii (2014), and White et al. (2018).

[9] 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 effect on their overall index of crime and conflict. One of the ten individual outcome variables used for this test can be considered an indicator of the kind of disputes that we have in mind, namely conflicts 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 (individual $p$-value=0.094).

in line with initial expectations. We conjecture that this leads to village-internal disputes, or more generally, to an erosion of social capital, thus increasing the costs of establishing informal economic transactions.

To investigate this channel, we first test for heterogeneity in benefits, both using our own data as well as census data, and identify characteristics that are associated with higher benefits. We then test whether these heterogeneous benefits translate into fewer transactions, using direct and indirect proxies for determinants of unequal benefits. At the village level, we test whether pre-program inequality (with respect to land and housing characteristics) is associated with a stronger decrease in economic transactions. At the dyadic level, we test whether pairwise differences in land holdings—important predictors of project-derived benefits—are associated with stronger decrease in economic transactions. Descriptively, we also test for a negative association between pairwise differences in self-declared benefits and having economic transactions.

**Discussion**

Note that all three channels suggest that the effects of the CDD program on interactions may well go beyond the immediately affected households. In fact, theoretical results from Banerjee et al. (2018) imply that a development intervention can have the largest effect on the networks of individuals that are themselves least affected by the intervention, by changing incentives for people to socialize in groups. In our case, a similar argument implies that even if the direct CDD effects reach only certain groups or pairs of households, relationships and transactions between individuals that are not directly affected by the program can end up being affected.

Further, it is important to highlight again that the channels described above mostly do not require that there are strong (measurable) effects on economic outcomes. Indeed, some of the channels work through small or absent economic improvements.

# 3 Background, Data, and Empirical Strategy

## 3.1 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.[10] The participative process in CDD programs is expected to contribute to improvements in economic conditions through better targeting, reduced implementation costs, improved maintenance, as well as to build capacity and improve local governance.

The Gambian CDD program was implemented between 2008 and 2010, was mainly financed by the World Bank, and targeted about 50 percent of the Gambian rural population (World

[10]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)

Bank, 2006). The program was implemented in 495 villages belonging to 88 wards. Only communities with a population between 100 and 10,000 inhabitants (according to the Census in 2003) were eligible for the program. For targeting purposes, village-level indicators of poverty were calculated using the Census 2003 data, and the two thirds of villages ranked the poorest in each ward were selected as eligible for the program.[11] 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.[12] The program promotes community involvement at all stages of the process from identification of the potential sub-projects to their maintenance after implementation. More details on this are provided in Online Supplement D.1.

The Gambian CDD program directly targeted poverty reduction and income growth as well as capacity building, in contrast to other CDD program designs that put a stronger emphasis on social outcomes (White et al., 2018).[13] Aside from inducing substantial amounts of social and intra-village political interaction, the program constituted a sizable positive economic shock to the productive capital in treatment villages. The budget allocated to treatment villages was a base of US$10,000, plus an extra budget determined by a formula based on population and poverty. The average disbursement for the 495 treatment villages was around US$11,500 (current values). Since in our sample the average treatment village has around 50 households, this translates into per-household allocations of around US$230, i.e., roughly equivalent to one-half of the annual per capita income in The Gambia.[14] Villages were free to choose a single large sub-project or multiple smaller ones. One third of the villages decided to invest the full grant into a single sub-project, while the remaining villages split funds into up to five sub-projects. Sub-projects that were financed through the CDD program were typically local public goods or club goods. The most common sub-projects are: farm implements and inputs, milling machines, water pumps, seed stores and cereal banking, and draft animals.[15]

## 3.2 Village Selection and Pre-Treatment Balance

For this study, we use a sample of 56 villages, drawn from the set of rural villages that were eligible for the CDD program and had a population between 300 and 1,000 inhabitants in 2003. The population restriction ensures a relatively homogenous sample of rural villages and made it

[11]The mean of the first four variables listed in Appendix Table 10, Panel B was used for the poverty ranking.

[12]Wards typically comprise around 6-14 eligible villages.

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

[14]To illustrate the magnitude of the shock, see Online Supplement D.2.

[15]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).

feasible to conduct full village censuses. Further details on the sampling are provided in Online Supplement E.

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

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

## 3.3 Data Collection

The principal data used in this paper were collected between April and June of 2014 at three 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 were held with village authorities to obtain a better understanding of the villages' main developments in the last years, including externally funded projects and village community groups. 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.[16] 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.[17]

[16] Details of this procedure are described in Jaimovich, 2015 and our working definition of households is discussed in Online Supplement E.

[17] We did not impose a strict limit on the number of reported transactions. However, in the case of the *Food*

For each transaction we recorded the direction, quantity, and further specifics of the exchange, such as whether there was payment involved, and treat the network of exchanges as a directed graph.[18] Given the directed nature of our survey questions, a household declaring a transaction can be either the source or the recipient of the transaction. To construct the dyadic data we aggregate the two potential reports each directed transactions, by both households in a dyad: We record a transactions if at least one of the two households declared a transaction in a given direction.[19]

The six economic domains have possible conceptual overlaps, which we were careful to clarify during the interviews. More details on the data collection are provided in Online Supplement E.3. However, for most of our analysis we consider the aggregation of the six domains into a single union network which discards the distinction between the six domains. Appendix Table 11 shows summary statistics for our network data. Across all economic networks, the average household in the control group lists 3.3 exchange partners. Exchanges of *Food* were the most common, with an average of 1.9 exchange partners per household.

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, to provide details about the implementation process, and to rate how they benefited from them.

### 3.4 Empirical Strategy

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

$$\ell_{ijvw} = D_{vw}\tau + X_{ijvw}\alpha + \beta_w + \varepsilon_{ijvw}, \tag{3.1}$$

where the dependent variable $\ell_{ijvw}$ indicates existence of a transaction from household $i$ to household $j$, in village $v$ of ward $w$ and takes on the value 100 whenever there is a transaction, 0 otherwise, so that coefficients can be interpreted as percentage points. The average treatment

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. The censoring was of negligible practical relevance as 97.6% of households reported three or fewer *Food*-exchange partners.

[18]While some transactions involved payments, our main specification uses all links as a measure for informal transactions. We argue that payments are often symbolic and transactions involving payment are not necessarily comparable to market transactions. This is supported by the fact that for the network where payment was most common, food, 60% of payments were *in-kind*. For labor and inputs, where less than a tenth of transactions involve any payment, still 12-15% were in-kind. Land and, by design, gift and credit transactions, involved virtually no payments.

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

effect (ATE) is captured by the coefficient, $\tau$, of the village treatment indicator, $D_{vw}$, which takes the value one if village $v$ was a CDD village. In all dyadic specifications we include ward-level fixed effects, $\beta_w$, as well as dyad-level control variables, $X_{ijvw}$. In addition to the control variables listed in Appendix Table 10, panels A and B, we control for the existence of kinship ties, shared ethnicity, interview group and enumerator fixed effects. The results are robust to alternative specifications of the control variables and to selecting control variables using the post double-LASSO (Belloni et al., 2014).

For ease of interpretation, Equation (3.1) is specified as a linear probability model and estimated using weighted OLS.[20] Our main results are unchanged if a probit model is estimated instead. Further, our results are equally confirmed through randomization inference, which does not rely on regression model assumptions, as explained in greater detail below. Further regressions, using other levels of aggregation (e.g., for household-level or village-level dependent variables) use analogous empirical setups and are always estimated using weighted OLS to account for the aggregation.

**Identifying village sub-groups** For our analysis of heterogeneous effects, we define groups of traditional leaders and marginalized households, and further obtain a proxy for households likely involved in the CDD projects' implementation. We can rely on exogenous positions to define traditional leaders in our data (as the chief's family and the religious leader) and use program-related documents to define marginalized households (as those headed by a woman or someone less than 35 years old).[21] In addition, the CDD increases the influence of another group of households, namely the members of the Village Development Committee (VDC). The VDC was tasked with identifying economic priorities, developing plans, and managing the financial resources as well as the actual implementation of the sub-projects at the village level (World Bank, 2006). Decisions were supposed to be made in consultation with the community, but members of the VDC—in principle—had the power to steer projects, to influence decision making, and control the CDD funds. To estimate heterogeneous impacts for this group, we need to identify comparable households in control villages. Although the Local Government Act (2002) required the creation of VDCs in all villages, the increased importance of the VDC, and the enforcement of inclusiveness criteria results in differential selection into the VDC across treatment and control villages.[22] Therefore, we cannot use actual VDC membership in our analysis. Instead, we use a random forest (Breiman, 2001) model to classify households consis-

[20]Regression weights are used to ensure comparability across 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.

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

[22]Supplement Table 14 shows that VDC members in CDD communities are significantly older, more educated, and more likely to be of an ethnic minority, or of a household who owns no land, than in control villages.

tently in treatment and control villages. In particular, for each household in both treatment and control villages we estimate—using a number of fixed household characteristics that capture social embeddedness in the village, education, ethnicity, age and wealth (using land, which can be considered exogenous to the program)—a probability of being a VDC member if the village were in fact a treatment village. This allows us to estimate treatment heterogeneity with respect to VDC membership. Our approach is inspired by Banerjee et al. (2018), who predict take-up of microfinance. More details are given in Online Supplement F. To facilitate an easier interpretation of the estimates, we rescale the measure from the random forest to have mean zero and variance one. Below, we refer to this measure as VDC-score. In dyadic specifications we also use a combined measure, where appropriate, capturing the probability that one or both households would belong to the VDC. Because VDC membership is not derived directly from the data, in some of the analyses of heterogeneity we keep this measure separate from the other two dimensions of interest, traditional leaders and marginalized 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., 2018). Additional issues potentially result from the relatively small number of treatment clusters (56) combined with heterogeneous village size (see MacKinnon and Webb, 2017). Thus the standard approach, using cluster-robust standard errors, is potentially problematic and we additionally rely on randomization inference (see Fisher, 1935; Rosenbaum, 2002) to test the significance of causal effects of the randomized treatment. All regressions where randomization inference is applicable indicate significance based on randomization inference as well as cluster-robust standard errors. Details are provided in Online Supplement G.

# 4 Effect of the Community-Driven Development Program on Economic Networks

## 4.1 Average Treatment Effect

Our main results concern the average treatment effect of the Gambian CDD program on the networks of economic transactions in rural Gambia. We use the dyadic specification described in Equation (3.1).

The estimates shown in Table 1, column 1, are based on the network of informal economic transactions that is the union of the six economic domains for which we have collected data. Column 1 shows that the effect of the program is a large and statistically significant reduction in the probability of the existence of an economic transaction. The point estimate for the average treatment effect implies a reduction of 1.133 percentage points in treatment villages. Considering the mean in the control group, this implies that the probability of two households having an economic transaction is 16.4% lower in treated villages than in control villages. Based

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

| | (1) any transaction | (2) consumption transaction | (3) production transaction | (4) any transaction | (5) any transaction |
|---|---|---|---|---|---|
| treatment | -1.133 | -1.191 | -0.140 | -1.304 | -1.300 |
| | (0.004)●●⊛ | (0.000)●●● | (0.676) | (0.010)●●⊛ | (0.008)●●⊛ |
| trad. leader$^{any}$ | 1.980 | 0.897 | 1.226 | 1.813 | 1.922 |
| | (0.000)$^{***}$ | (0.000)$^{***}$ | (0.000)$^{***}$ | (0.001)$^{***}$ | (0.000)$^{***}$ |
| marginalized$^{any}$ | -0.714 | -0.281 | -0.421 | -0.841 | -0.902 |
| | (0.004)$^{***}$ | (0.058)$^{*}$ | (0.038)$^{**}$ | (0.023)$^{**}$ | (0.016)$^{**}$ |
| VDC$^{\text{any}}$-score | | | | | -0.321 |
| | | | | | (0.134) |
| treatment $\times$ trad. leader$^{any}$ | | | | 0.317 | 0.094 |
| | | | | (0.695) | (0.902) |
| treatment $\times$ marginalized$^{any}$ | | | | 0.254 | 0.410 |
| | | | | (0.565) | (0.351) |
| treatment $\times$ VDC$^{\text{any}}$-score | | | | | 0.556 |
| | | | | | (0.056)● |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| dyads | 151632 | 151632 | 151632 | 151632 | 151632 |
| households | 2774 | 2774 | 2774 | 2774 | 2774 |
| control mean dep. var. | 6.9 | 3.5 | 4.0 | 6.9 | 6.9 |

*Notes:* ●/$^{*}$ $p < 0.1$, ●●/$^{**}$ $p < 0.05$, ●●●/$^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Where bullets are shown, randomization inference was used to compute $p$-values: filled bullets ● indicate significance levels with randomization inference; starred bullets ⊛ indicate significance levels that are only sustained by the cluster-robust standard errors. Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. "Consumption transactions" combine transfers of food, gifts and credits. "Production transactions" combine transfers of land, labor, and inputs. Regressions control for ward fixed effects and a set of control variables: The village-level variables in Appendix Table 10, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table 10, as well as ethnicity and enumerator dummies, enter the regressions twice, for the sending and the receiving household of a dyad. The variables *trad. leader*$^{any}$ and *marginalized*$^{any}$ indicate whether any of the two households in a dyad belongs to the traditional village leaders or the group of marginalized households. The variable VDC$^{\text{any}}$-score measures the likelihood that a dyad involves a household who would be in the VDC if the village was a CDD village and is computed as $1 - (1 - \Pr_{\text{RF}}[\text{VDC}_i])(1 - \Pr_{\text{RF}}[\text{VDC}_j])$, where $\Pr_{\text{RF}}[\text{VDC}]$denotes the probability for household to be in the VDC based on the random forest model. Values of VDC$^{\text{any}}$-score are normalized to have mean zero and variance one.

on cluster-robust standard errors, allowing for correlation of the model error within villages, this effect is significant at 1% with $p$-value=0.004, while when using randomization inference the $p$-values is 0.03, i.e., the significance level is 5% (the significance levels are jointly indicated by ●●⊛, see the table notes for further details). Splitting the data on transactions allows us to study separately a consumption network (which includes transactions in the food domain and gifts as well as credit transactions) and a production network (including transactions of land, labor and inputs). The results (in columns 2 and 3) show that the average treatment effect observed for the union of all domains is driven by consumption-related informal transactions.[23]

[23] Supplement Table 15 shows the results for each of the six economic networks individually. Five out of the six estimated coefficients are negative and in two cases this effect is significant at the 1% level, based on both cluster-robust standard errors and randomization inference. 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. 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).

## 4.2 Robustness of the Main Findings

The above results are robust to a battery of robustness tests. First, a number of alternative specifications are reported in the Appendix, namely: Using the post double-LASSO as a data-driven way to select control variables (Appendix Table 12, column 1); excluding either one or both wards with unequal numbers of treatment and control villages to ensure that results are not driven by imbalances in these wards (columns 2-4, see also Online Supplement E.1); using probit instead of a linear probability model to account for the binary nature of the dependent variable (column 5); using transaction intensity instead of a binary indicator, to test if the reduction of transactions is made up for by increased transaction volumes (column 6); using only transactions that did neither involve cash nor in-kind payments, as transactions involving pay could be regarded as market-based transactions (column 7); and treating the network as an undirected graph, to address concerns about the mismeasurement of the direction of flows (column 8).

The reduction in economic interactions as a result of the program is also confirmed when we aggregate the network information to analyze degree centrality at the household level as well as comparable network measures at the village levels (see Online Supplement H.2).

One limitation of our analysis is that we do not have economic network data from before the intervention. This, together with a relatively small sample size may raise concerns about spurious correlations. To address these concerns, we use a number of approaches. To begin, in light of a relatively small number of villages and wards, we take particular care of issues of inference, by using randomization inference. Second, although we do not have economic network data from before the intervention, we do have a fairly large number of other pre-intervention variables for our sample villages from different sources, which we use to provide evidence on pre-treatment balance (Appendix Tables 10 and 11). Besides standard household-level variables, these data include a number of proxies for social networks, such as compound size or the number of spouses that hail from the same village. We augment these data with our data on kinship networks and geography, which we consider static (based on evidence in Appendix Table 11 and the discussion in Appendix A). We find no evidence for differences between treatment and control village along any of these dimensions. Third, we have data on economic transactions collected through the early stages of the program implementation (in 2009). Because of the timing of the data collection, which occurred at a time when project implementation had already started in some villages (i.e., meetings were held and, possibly, funds disbursed) these can not be considered clean baseline data (see Online Supplement H.3 for a more detailed discussion). There are also differences in how the data was collected. Nevertheless, we explore these data to investigate robustness of our results. In particular, we use the 2009 networks data to implement a Difference-in-Differences (DiD) strategy (Supplement Table 19). The DiD estimate is negative and statistically significant, in line with our main average treatment effect estimates based only on our 2014 data. In addition, there is no significant difference between treatment and control villages in 2009 (neither in the economic networks nor in the kinship network).[24]

[24]Thus, to explain our main findings with significant imbalances at baseline, it would need to be the case that

Another possible concern is related to the fact that data were collected 4-5 years after the end of the program. On the one hand, this is a strength of our data, as it has the potential to show whether effects on economic interactions are not just short-lived. On the other hand, it increases the chances that some kind of "compensation" has taken place in which control villages received more development programs than treatment villages, which would complicate the interpretation of the results. In an appendix (Online Supplement H.4) we discuss this possibility. The evidence provided there, based on additional data that we have collected as well as based on information we have about other programs, does neither support the assertion that compensation may be responsible for the observed findings nor that it occurred at all at a meaningful magnitude.

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

Table 1 also provides information about the economic connectedness of different groups within the village. We first demonstrate that certain identifiable groups in the sample villages have very different levels of economic connectedness, highlighting through network analysis the existence of elite and marginalized groups, even in the absence of treatment. We then investigate heterogeneous effects.

We focus on three groups. Two of these groups typically receive special consideration in CDD programs, including in the Gambian CDD program, namely traditional village leaders and marginalized households.[25] A third group gains significance through the CDD program, namely the members of the VDC. For treatment villages, membership in the VDC entails a significant role in the selection and administration of the village's CDD sub-project(s). To obtain a proxy for VDC membership that is comparable between treatment and control villages, we compute a VDC-score through a random forest model, based on fixed household characteristics (introduced in Section 3.4 and explained in more detail in Online Supplement F).

We first focus on the two groups which we can identify in our data based on ex-ante criteria, namely traditional village leaders (the village chief and his first-degree relatives as well as the Imam), and the group of marginalized (young and female-headed) households.[26] We conjecture

there were significant short-term effects that have roughly the same magnitude as the imbalances but the opposite sign and that then dissipated until 2014 (in that case, the DiD estimate is the difference between long-term and short-term effects). Considering jointly the lack of significant differences in networks in the 2009 data (discussed in Online Supplement H.3) and the balance of social network proxies in 2003 (Appendix Table 11), this seems highly unlikely.

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

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

that traditional leaders are well connected because they head the most established families in a village, control a large share of the village's private agricultural landholdings as well as the village's commons, and are often of above-average wealth. Marginalized households are expected to be less connected because they tend to be poorer, less involved in the village's productive activities, and because age and gender are important factors for social interactions in The Gambia. Indeed, the findings (columns 1-3) confirm that traditional leader households are significantly more connected than other villagers. For marginalized households the opposite holds.

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

To study the effect of membership in the VDC, we rely on the variable $\text{VDC}^{\text{any}}$-score, which captures the probability that any household in a dyad belongs to the VDC in case the village was treated.[27] Column 5 suggests that, in control villages, the probability that any household in a dyad is a VDC member is not associated with informal economic transactions. However, the interaction of treatment and the variable $\text{VDC}^{\text{any}}$-score is positive and significant. This shows that likely VDC households are not necessarily very different in terms of connectedness ex-ante, but in CDD treatment villages they are more connected with other households in the village after the program. The results regarding traditional leaders and marginalized households are unaffected by the introduction of the VDC-related variables.

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

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

# 5 Mechanisms

We are guided by the conceptual framework described in Section 2. This framework suggests three main channels: (i) frequent social interactions during project choice and implementation, (ii) positive economic change, and (iii) unequally distributed benefits, possibly due to elite capture and favoritism.[28] In this section, we discuss relevant evidence.

## 5.1 Channel (i) – Frequent Social Interactions

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

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

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

[28]In the appendix we present evidence for the necessary condition for any of these channels to apply, namely that projects were physically implemented and involved standard CDD procedures, which mandate significant community interaction (see Online Supplements M.1 and M.2).

Table 2: Friendship and Help Links, and Heterogeneity Tests

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

*Notes:* $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). In columns 1-4, the units of observation are undirected (friendship) or directed (help) dyads. The dependent variable takes on the value 100 if a dyad had a link and 0 otherwise. Columns 5-8 show household-level regressions. The dependent variables *meetings* and *CBOs* count the number of village meetings the respondent household declares to have attended in the past year (2013/2014) and the number of community-based organizations household members participate in. For details on control variables and interacted variables see notes to Table 1. Columns 5-8 use the household-level analogue for these variables. Since help-links are elicited in the in-depth survey with a random subsample of households, sample sizes are smaller and regressions additionally control for a variable indicating whether one or both households were interviewed.

Overall, the observed reduction in friendship, the results regarding help-networks, meeting attendance and CBO membership do not support the hypothesis that more frequent interactions during the project spill over into more frequent social interactions outside the project. In fact, the evidence points towards a reduction in the number of friends and social involvement in the village.

Based on the average treatment effect as well as the negative heterogeneous effects for marginalized households (columns 3 and 7), who by design were supposed to be more involved in joint activities, we can clearly reject the hypothesis that more frequent interactions due to the project lead to more social interactions of the type that we measure here. These negative effects warrant further attention in light of the hypothesis spelled out at the beginning of this section. We will come back to these findings in Section 5.3, where we study unequally distributed benefits and the role of elite capture and favoritism.

## 5.2 Channel (ii) – Positive Economic Change

### Wealth Effects

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

Table 3: Economic Change: Our data

| | respondents' subjective assessment | | | wealth indicators | |
|---|---|---|---|---|---|
| | (1) overall econ. condition | (2) own econ. condition | (3) benefited from any project | (4) $z$-score animals | (5) $z$-score wealth |
| *Panel A: average treatment effects* | | | | | |
| treatment | 0.151 | 0.017 | 0.131 | -0.027 | -0.138 |
| | (0.029)●⊛ | (0.818) | (0.008)●●⊛ | (0.638) | (0.127) |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Panel B: heterogeneity* | | | | | |
| treatment | 0.215 | -0.066 | 0.121 | -0.030 | -0.196 |
| | (0.003)●●⊛ | (0.427) | (0.016)●⊛ | (0.621) | (0.048)⊛⊛ |
| treatment × trad. leader | -0.323 | 0.235 | 0.069 | -0.104 | -0.129 |
| | (0.230) | (0.455) | (0.221) | (0.481) | (0.566) |
| treatment × marginalized | -0.213 | 0.308 | 0.019 | -0.001 | 0.259 |
| | (0.234) | (0.191) | (0.674) | (0.988) | (0.189) |
| treatment × VDC-score | 0.070 | 0.157 | 0.023 | 0.001 | 0.284 |
| | (0.364) | (0.028)●● | (0.352) | (0.992) | (0.002)●●● |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| households | 529 | 547 | 2767 | 2774 | 550 |
| villages | 56 | 56 | 56 | 56 | 56 |
| control mean dep. var. | 3.1 | 2.8 | 0.6 | 0.0 | 0.0 |
| dep. var. range | 1 - 5 | 1 - 5 | 0 - 1 | | |

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: Household- and village-level variables in Panels B and C of Appendix Table 10 as well as ethnicity and enumerator dummies. Columns 1, 2, and 5, are based on the in-depth survey with 10 random respondents per village. For columns 1 and 2, respondents were asked to rate changes of their own and the village's overall economic condition during the past 5 years on a 5-point Likert-scale where 1 is a deterioration, 3 is no change and 5 is an improvement. The dependent variable in column 3 is based on the question "Do you think that your household benefited from development projects implemented in the village in the last 5 years?" The $z$-score for animals in column 4 combines count variables for cattle and for other draught animals. The $z$-score in column 5 combines a large number of wealth indicators. Treatment effect estimates for individual indicators are found in Supplement Tables 22 to 24.

We first use measures of self-assessed economic conditions and benefits from programs to shed light on possible economic effects of the CDD.[29] Table 3 shows that respondents in the in-depth

[29] Self-assessed benefits from programs are subject to a number of caveats. In particular, McKenzie (2017) points out that business owners cannot predict their own counterfactuals very well. However, note that we ask each household to assess changes over the last five years or benefits from development programs. Thus, we do not ask for the counterfactual "what would have been if", but we ask for "what has been". Further, one may be concerned that subjective measures of benefits from development programs are driven by some kind of expectations about the effects of a program. However, one question we use here does not refer to development programs at all, while the other refers to development programs generally, without specific reference to the CDD. Finally, more related to our later analysis of elite capture, note that the "incorrect" subjective assessments of own and others' benefits might in fact be the relevant determinant of village-internal disputes and personal grievances.

survey in treatment villages are, on average, significantly more likely to state that the overall economic conditions in the village have improved in the last five years, i.e., since the beginning of the Gambian CDD program (column 1). However, they are not more likely to report an improvement in their own economic situation (column 2). In the larger sample, based on the main survey, households in CDD program villages are significantly more likely to report that they have benefited from a development project within the past five years (column 3).[30] When we consider animals and household assets, we find no evidence for a positive average treatment effect of the CDD program in our sample (Table 3, columns 4 and 5).

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

The divergence of results based on respondents' subjective assessment (which suggest positive effects) and survey-based wealth indicators (which does not show evidence of an effect) may seem somewhat puzzling. Of course, it is possible that perceptions and actual outcomes indeed differ. A second possibility is that wealth and assets are relatively imprecisely measured in our data and the economic effects might be too small to be picked up with our data. For this latter reason, we turn to Census data, which allow us to perform the analysis on a much larger sample. The Census 2013 contains a section on household assets and animals. We combine the available information and calculate standardized $z$-scores, similar to what we do with our own data. We work with the sample of all villages in The Gambia that were eligible for the CDD program and fall within the size range of our sample villages (300-1000 inhabitants in 2003). Table 4 shows the results. Indeed, based on this larger sample (using more than 20,000 households in 316 villages), treatment effects on the $z$-scores are positive. While the magnitudes of the effect are similar for animals and assets (about 0.08-0.1 standard deviations), the estimates are statistically highly significant for the average treatment effect on animals ($p$-value=0.008) and marginally significant for assets ($p$-value=0.1).[31]

We further investigate treatment heterogeneity with the Census data. We cannot identify the same subgroups in the Census data as in our data. However, the Census data contain variables that can proxy for elite and marginalized positions within the village. In particular, established families (those that have a high share of adult household members that were born in the village) and those with higher education are more likely to be part of the traditional elite and the VDC. On the other hand, recent migrants to the village, and individuals with low levels of education are more likely to be marginalized. Further, because of polygamy, household size could be seen as an indicator of wealth in the Gambian context. Based on these variables, we find treatment

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

[31] 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.

Table 4: Economic Change: Census Data

| | animals | | | assets | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | $z$-score | $z$-score | $z$-score | $z$-score | $z$-score | $z$-score |
| treatment | 0.080 | 0.078 | 0.080 | 0.108 | 0.107 | 0.109 |
| | (0.008)●●⊛ | (0.007)●●⊛ | (0.008)●●⊛ | (0.100)⊛ | (0.103) | (0.097)⊛ |
| treatment × education | | 0.049 | | | -0.017 | |
| | | (0.089)● | | | (0.786) | |
| treatment × established family | | 0.109 | | | 0.062 | |
| | | (0.028)●● | | | (0.550) | |
| treatment × household size | | 0.005 | | | -0.001 | |
| | | (0.289) | | | (0.610) | |
| treatment × VDC-score | | | 0.061 | | | -0.033 |
| | | | (0.004)●●● | | | (0.242) |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| households | 20504 | 20473 | 20504 | 20504 | 20473 | 20504 |
| villages | 316 | 316 | 316 | 316 | 316 | 316 |
| control mean dep. var. | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. The sample consists of households from all eligible rural villages with a population of 300-1000 inhabitants. Regressions control for ward fixed effects and a set of control variables: Village-level variables listed in Panel B of Appendix Table 10 as well as an ethnicity fixed effects, education, ethnic minority status, and household size. *education* is a binary indicator, indicating whether the household head ever attended school. *established family* is measured by the share of adult household members born in the village and *household size* is the number of adult household members. VDC-score measures the likelihood that a household would be in the VDC if the village is/would be a CDD village. Values of the score are normalized to have mean zero and variance one. All other interacted variables are centered. The $z$-score for assets is based on four variables indicating radio ownership, mobile ownership, TV ownership and bicycle ownership. The $z$-score for animals is based on the number of owned ruminants, poultry and cattle.

heterogeneity for effects on animal ownership. Established families and households with more educated members have significantly higher animal $z$-scores (column 2). Further, we use a random forest to identify likely VDC member households. The random forest model here is slightly different from the one referred to before, as we can only use variables that are both in our data as well as in the Census, to predict VDC member households in the Census (column 3). Likely VDC members have significantly higher $z$-scores for animals. We do not find significant treatment heterogeneity for the asset index.

On balance, the evidence leads us to conclude that there are some moderate positive economic effects. Interpreted jointly with the previous findings, suggesting that the reduction in informal economic transactions is largely due to consumption-related transactions, it appears plausible that positive economic change contributed to the reduction of economic transactions. We also find some heterogeneity of the effects, with households that have characteristics of elite households showing significantly larger treatment effects for the animal ownership outcome, which we will discuss more in Section 5.3, where we study unequally distributed benefits and the role of elite capture and favoritism. In addition to small sample sizes, another caveat should be mentioned: our focus was on the analysis of networks, not on identifying effects along wealth or income dimensions, which would have required more in-depth data collection related to income and/or consumption measures.[32]

[32] 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

### Increased Market Orientation of the Village Economy

Tangible economic benefits and availability of more advanced means of production and transportation (milling machines, tractors, etc.) make market participation more likely, which—according to Kranton (1996)—weakens the system of personalized exchange and leads to a reduction in overall interactions, and in reciprocal interactions in particular, and a move to market-based activities can be expected. Even with the limited evidence for increases in wealth or asset indicators, formalization might occur for other reasons: Many of the CDD sub-projects are related to income generating activities, such as producing goods for sale (e.g., in the vegetable garden) and bringing goods to the market (e.g., using the tractor to transport firewood to the weekly market). Also, the CDD sub-projects' day-to-day operation often introduced some form of payment (e.g., paying the tractor driver or renting out the milling machine to outsiders). As a result, the market orientation of the village economy, in the form of more formal transactions and a stronger orientation towards market activities, could increase.[33] A general increase in connections with village outsiders could further be related to increased exposure to neighboring villages during the CDD planning stage.

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

| | in-degree | | | out-degree | | |
|---|---|---|---|---|---|---|
| | (1) external | (2) external unpaid | (3) external paid | (4) external | (5) external unpaid | (6) external paid |
| treatment | 0.176 | 0.118 | 0.058 | −0.024 | −0.030 | 0.005 |
| | (0.006)●●⊛ | (0.026)●⊛ | (0.072)⊛ | (0.721) | (0.664) | (0.293) |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| households | 2774 | 2774 | 2774 | 2774 | 2774 | 2774 |
| control mean dep. var. | 1.1 | 0.9 | 0.1 | 0.7 | 0.7 | 0.0 |

*Notes:* •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. The dependent variable in columns 1-3 [4-6] counts incoming [outgoing] transactions. Transactions are considered "external" if a respondent replied "with a household from outside the village" to the questions used to elicit transactions. For details on control variables see notes to Table 3.

To test some aspects of the formalization hypothesis directly, we use our data on transactions with outsiders and whether these transactions involve a payment. Table 5, column 1 shows that households in treatment villages are indeed significantly more likely to have incoming transactions from outside the village. Further, while payment is not a sufficient condition to identify market transactions, a transformation to more market-based transactions would imply that more transactions involve payments. Our data allow us to distinguish transaction for which a payment was made and those for which no payment was made. Indeed, we find that paid incoming transactions from village outsiders are also increasing (column 3), accounting for roughly a third of the overall increase in external transactions, while in control villages paid transactions constitute little more than one eighth of all external transactions. Thus, there is

individual indicators (including a household asset score).

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

some evidence for more market-like transactions between buyers from CDD program villages and sellers from outside. On the other hand, there is no such evidence for the opposite direction, i.e., for more transactions with outsiders in which the CDD-village households are the sender (columns 4-6).

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

### Changes in Reciprocity and the Role of Social Proximity

As a second way of studying whether market orientation increases in treatment villages, we analyze the effects the program had on two specific types of transactions: reciprocal transactions and socially embedded transactions. We hypothesize, based on Kranton's (1996) work, that reciprocal interactions are reduced when market-economic transactions are available. In order to test the CDD program's effect on reciprocal transactions, we take advantage of the fact that our data record the direction of economic transactions. With this information, we can define a variable that indicates if a particular transaction was reciprocated, meaning that a transaction has a counterpart in the opposite direction in any of the six economic networks. More precisely, we define reciprocity as an undirected dyadic binary variable: $\text{recip}_{ij} = \ell_{ji} \cdot \ell_{ij}$, where $\ell_{ji}$ is an indicator for transactions from $i$ to $j$.[34]

In order to analyze if the CDD program had an effect on the reciprocity of the transactions, we estimate Equation (3.1) with $\text{recip}_{ij}$ as the dependent variable. Indeed, consistent with the above hypothesis, the results in column 1 of Table 6 suggest that the CDD program causes a strong reduction in reciprocal exchanges. The treatment coefficient is negative and statistically significant ($p$-value=0.005).[35]

A similar picture is drawn when analyzing the role that social proximity plays in network 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). Another way to think about this measure is as a variant of *trust flow*, as proposed by Karlan et al. (2009): the number of common social ties determines the social collateral that can facilitate informal

[34] 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. 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.

[35] It is impossible to ascertain whether this reduction in reciprocal exchanges is an independent effect from the overall reduction in transactions or a by-product. However, the magnitudes of the effect estimates suggest that reciprocal transactions are particularly prone to vanish in treatment communities: The average reduction in the probability of forming a reciprocal transaction is 0.585 percentage points, which corresponds to a drop of 29% of the control group mean. If these reductions in reciprocity were mostly incidental, i.e., a by-product of the overall reduction in transactions, there is no obvious reason for reciprocal transactions to be severed at a much higher rate than unidirectional transactions.

Table 6: Treatment effects on reciprocity, and the role of support

| | reciprocity | support |
|---|---|---|
| | (1) reciprocated econ. transaction | (2) any transaction |
| treatment | -0.585 | -0.613 |
| | (0.005)●●⊛ | (0.145) |
| support | | 0.520 |
| | | (0.001)*** |
| treatment × support | | -0.355 |
| | | (0.042)●● |
| controls | ✓ | ✓ |
| dyads | 75816 | 151632 |
| households | 2774 | 2774 |
| control mean dep. var. | 2.0 | 6.9 |
| mean support | | 1.2 |

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are undirected (reciprocity) and directed (support) dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. A dyad is considered to have a reciprocated economic transaction, if transactions in both directions occurred. *support* is a count variable capturing how many other households exist, to which both households of the dyads are linked, either through kinship or as neighbors. For details on control variables see notes to Table 1. The support regression additionally controls for geographic distance.

transactions. We use two measures for static social networks unaffected by treatment—kinship and geography—to formalize this concept. For each dyad we count how many supporting households exist, i.e., households that are connected to both households of a dyad as kin or as neighbors: $\text{support}_{ij} = \sum_k \mathbb{1}(\ell_{ik}^{\text{neighbor}} \vee \ell_{ik}^{\text{kin}}) \cdot \mathbb{1}(\ell_{jk}^{\text{neighbor}} \vee \ell_{jk}^{\text{kin}})$, where $\ell_{ij}^{\text{kin}}$ and $\ell_{ij}^{\text{neighbor}}$ indicate whether two households are related or are direct neighbors.[36] Note that our support measure differs from that in Jackson et al. (2012) in two important dimensions. First, our measure is not binary, but a count variable.[37] Second, our measure of support counts common ties in a static social network as opposed to common ties in the network being studied. An analysis of the latter, i.e., of the prevalence of closed triads in the outcome networks, is implicit in our village-level analysis (see Supplement Table 16, columns 3 and 4).

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

We take this as evidence that this particular measure for social proximity, support, indeed

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

[37] 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.

plays a crucial role in facilitating cooperation, but becomes less important in CDD communities. The reduction in the importance of common ties for network formation in treatment communities is also consistent with the reduction in the number of closed triads observed in treatment villages (see Supplement Table 16). In sum, the reduction of transactions in supported dyads in treatment villages is another indication for reduced informality and increased market orientation of the village economy.

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

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

#### Unequally Distributed Benefits: Additional Evidence

Unequally distributed benefits can occur for various reasons. Our primary hypothesis for unequally distributed benefits is related to elite capture. To illustrate how elite capture could lead to highly unequal benefits, recall that most sub-projects are related to agriculture, and land-holding households are likely to benefit more from this type of sub-project. This is particularly obvious in cases in which a tractor was purchased. Large landowners likely benefit from this mechanization of agriculture, while landless households might even lose: if agricultural machinery and manual labor are substitutes, labor is now less valuable.[38] Results in Supplement Table 25 are in line with this hypothesis. Here we complement our previous analysis of heterogeneous treatment effects with a descriptive analysis of the correlates of CDD-specific benefits as well as of subjective views on the implementation process and find significant within-village heterogeneity. Table 25a shows that landless households report significantly less benefits from

[38]We have observed that in villages with tractors, using the tractor required paying a fee to cover maintenance and to pay the driver. 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.

CDD projects.[39] Table 25a further shows that benefit from CDD projects is also associated with elite status (especially VDC membership). This contrasts with non-CDD projects in control villages, for which none of these correlations are significant. However, because CDD and non-CDD projects are inherently different, this piece of evidence has to be regarded as descriptive. Yet, the results regarding the VDC are consistent with the results on economic benefits being larger for (likely) VDC members (Tables 3 and 4). There is also evidence that respondents *perceive* benefits from the CDD projects as unequally distributed.[40]

Results reported in Table 25b provide further suggestive evidence that elite capture might play a role. These results show that households differ significantly in their views on procedures related to the CDD project choice.[41] The results in Table 25b suggest that traditional leaders and the VDC were more engaged in the decision making about projects. This would allow a purposeful steering of projects towards areas that would benefit them. Note that even if differences in benefits and influence in fact do not exist, perceived differences may cause grievances.

These results on benefits and decision making are of course only descriptive, but they are consistent with the hypothesis that elites and land-owning households benefit significantly more from CDD sub-projects than regular households, and that this inequality of benefits is associated with village-level measures of inequality. Further, the results highlight stark differences in how villagers perceive benefits from CDD and non-CDD development interventions.

**Unequally Distributed Benefits Translate Into Fewer Links**

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

Unequally distributed benefits can explain our main finding of reduced network interaction in two ways. First, households with fewer benefits might sever their social ties to households with more benefits 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

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

[40] In our in-depth survey, we asked households for each project whether they benefited more, the same or less, relative to other households. For the average non-CDD project in a control village, only about 4% of households report that they are "benefiting less" than other households. This number increases to about 13% for the average CDD sub-project in treatment villages (Supplement Table 26).

[41] Here we move the analysis to the level of each individual project report given by the households. In the in-depth survey, we asked households for each village-level development project (including non-CDD projects) who, in their opinion, decided on that project (more than one category could be named). These regressions allow for a descriptive study of how different households perceive different types of projects.

Marginalized households are significantly less likely than other households to report that all villagers were involved in the CDD project's decision making and significantly more likely to say that traditional leaders were important in making the decisions. The results reported in column 3 also clearly show that the VDC was very important in the decision making, especially for CDD projects. For non-CDD projects, 19% of households state that the VDC decided, while for CDD projects this number is 44%.

the whole community. If this promise is not fulfilled, the result could be internal quarrels among groups beyond individual "winners" and "losers" (Barron et al., 2011) that weaken social networks more broadly, not just among more and less benefiting households. To the extent that disputes imply lower levels of trust and social capital, this could increase the costs of maintaining relationships and therefore disputes have the potential to translate into fewer informal economic transactions.

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

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

*Notes:* $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. The Gini coefficients are standardized to have mean zero and variance one. Columns 3 and 4 use project assessments elicited in the in-depth interview and the village-level focus group as proxies for project failure. For details on control variables see notes to Table 1.

We start by testing the effect of village-level variables that plausibly proxy for unequally distributed benefits. Results are reported in Table 7. Columns 1 and 2 consider village-level inequality. Previous literature suggests that elite capture is positively related to village-level inequality (e.g., Bardhan and Mookherjee, 2000; Araujo et al., 2008). Because of the finding that land ownership is positively related to self-reported benefits, land inequality is our primary inequality measure. Yet, land ownership is difficult to measure in this context (and we cannot fully rule out that land ownership itself is affected by the program, see the discussion above and Footnote 39). Therefore, we also use an inequality measure based on housing wealth, as proxied by the number of rooms belonging to the household. The advantage of this measure is that it is fairly straightforward to measure rooms, and this variable was recorded in the Census 2003, i.e., before the program. Results in columns 1 and 2 show that, indeed, the treatment effect on networks is heterogeneous, and larger inequality implies a stronger reduction in economic transactions.

Focusing on CDD-villages, we have two measures of project failure that might plausibly be related to disputes, either because of unequal benefits (if some households identify a project as a failure, this might reflect unequal benefits) or because of completely absent benefits.[42] Column 3 uses data from the focus group discussions, while column 4 uses data from the in-depth survey of individual households. Both show a negative association with informal economic transactions. Because these two measures only exist for CDD villages, estimates cannot be regarded as causal and results should only be seen as suggestive.

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

| | all villages | | | CDD villages | |
|---|---|---|---|---|---|
| | (1) any transaction | (2) any transaction | (3) any transaction | (4) any transaction | (5) any transaction |
| *(likely) small benefit* | | | | | |
| treatment | -1.539 (0.000)●●● | | -1.686 (0.001)●●⊛ | | |
| treatment × neither has land | | -1.404 (0.006)●●⊛ | | | |
| neither benefited from projects | | | | -0.909 (0.088)* | |
| *different benefit / only one benefited* | | | | | |
| treatment × \| $\text{land}_\text{i} - \text{land}_\text{j}$ \| | -0.112 (0.137) | | | | |
| treatment × one has land | | -1.346 (0.002)●●⊛ | | | |
| treatment × \| $\text{VDC-score}_\text{i} - \text{VDC-score}_\text{j}$ \| | | | 0.621 (0.041)●● | | |
| one benefited from projects | | | | -0.345 (0.178) | |
| $i$ and $j$ benefited differently from CDD | | | | | -2.695 (0.087)* |
| *similar benefit / both benefited* | | | | | |
| treatment × ($\text{land}_i + \text{land}_j$) | 0.128 (0.066)● | | | | |
| treatment × both have land | | -0.749 (0.167) | | | |
| treatment × ($\text{VDC-score}_i + \text{VDC-score}_j$) | | | 0.236 (0.228) | | |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| dyads | 149472 | 149472 | 151632 | 81376 | 2304 |
| households | 2755 | 2755 | 2774 | 1415 | 267 |
| control mean dep. var. | 6.9 | 6.9 | 6.9 | | |

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. Column 4 uses statements about benefits from "any development projects" asked in the main survey. The omitted category are dyads where both households declared to have benefited. Column 5 uses project assessments elicited in the in-depth interview, specifically for CDD projects. These regressions are thus estimated on the subsample of dyads where both households were included in the in-depth survey and the omitted category are those dyads where either both households on average declared to have benefited more than others, the same as others or less than others. For details on control variables see notes to Table 1.

Next, in Table 8, we consider proxies for differences in benefits at the dyadic level. First, because of the above-mentioned arguments and our findings related to the importance of land for benefits, we look at dyadic differences in land holdings. Results shown in columns 1 and 2 show that the negative treatment effect is observed only for dyads in which both households

[42]Indeed, in the sample villages, more than one quarter of development projects are considered "not functioning" by the respondents (this number can be derived in various alternative ways, see Supplement Table 27)

likely have little benefits (column 1, where benefit is proxied by a continuous land variable) or where at most one household owns land (column 2). When both households own (similar amounts of) land, the coefficients are either positive (column 1) or insignificant (column 2).[43]

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

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

In sum, using dyadic proxies for the distribution of benefits, we find several pieces of evidence that unequal benefits or absent benefits are associated with fewer informal transactions. On the other hand, dyads in which both benefit, do not show treatment effects or they even show positive effects.

Overall, both the analysis using village-level proxies and the dyadic proxies for heterogeneous benefits suggest that heterogeneity in benefits or absence of benefits explain lower levels of informal transactions.

# 6 Implications for Household Welfare

The results above show that households in treatment villages are less connected in economic village networks. This can be expected to affect welfare. First, 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. Second, a large theoretical and empirical literature suggests that networks help households cope with shocks and that a household's ability to enforce informal contracts depends on social capital (e.g., Ligon et al., 2002; Fafchamps and Lund, 2003; De

[43]For an easier reading of the table, note that right-hand-side variables that indicate (likely) different benefit between the two households are listed in the middle of the table. At the top, variables are listed that indicate (likely) small benefits for both households, at the bottom variables that indicate (likely) higher benefits for both households.

Weerdt and Dercon, 2006; Karlan et al., 2009).[44]

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

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

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

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. For details on control variables see notes to Table 1. The shock variables indicate the occurrence of shocks of different types during the past two years. The production shocks sum up two indicators, "crop failure" and "animals died or got sick/agricultural tools broke", and can take on the values 0, 1, and 2. The housing shock indicates the destruction of a building belonging to the household, which is very common during rainy season. The health shock sums up two binary indicators for death and serious illness within the household, and can take on the values 0, 1, and 2. The total shock count is the sum of all three shock categories. Shocks are balanced between treatment and control communities, as shown in Supplement Table 28.

Turning to the effect of treatment, columns 2, 4, 6, and 8 report the coefficient of the interaction between treatment and shock. Throughout, we do not find any significant interaction

[44] 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.

effects. In addition, the magnitude of the parameter estimate is small relative to the control mean. Thus, there is no evidence for a difference between treatment and control villages in their ability to deal with these relatively serious types of shocks that we consider in this section. This finding is consistent with the finding from Section 5.1 that the number of potential "helpers" in times of need is not significantly different in treatment villages.[45]

One concern with the above analysis is that shocks themselves may be affected by treatment. Although we cannot fully rule out this possibility, to alleviate this concern we have further investigated the relation between treatment and shocks, and found that shocks are balanced between treatment and control communities (see Supplement Table 28).

# 7 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 their importance 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 economically interact with other households and are less embedded in the village economy.

To understand the underlying mechanisms of this effect of the program, we identify three possible channels through which the CDD program could affect networks of informal economic exchange: (i) frequent social interactions during project choice and implementation, increasing social capital, thus reducing the costs of having informal economic transactions, (ii) positive economic change, reducing the need for informal insurance or raising the benefits from the market-based outside option, and (iii) unequally distributed benefits and elite capture, reducing social capital, thus increasing the costs of having informal economic transactions. Among these, the evidence is inconsistent with the first, but consistent with the last two channels.

First, there is no evidence for increased social capital resulting from the program-induced social interactions. On the contrary, on average households in CDD villages have fewer friendship links to other households and participate less in community-based organizations. Marginalized groups stand out particularly, exhibiting the largest reduction in friendship links and also a reduced attendance in village meetings.

[45] We also tested for heterogeneity with respect to elite and marginalized households in this effect, as our previous results suggest that marginalized households are particularly prone to lose friendship links, but found not evidence for heterogeneity in risk sharing (results not shown).

Concerning the second channel, we find some evidence for moderate positive effects on asset and animal ownership and there is evidence for a village-level transformation towards a more market-oriented economy, accompanying these changes: a reduced importance of social proximity, a reduction in reciprocal relationships among villagers, and an increase in (paid) interactions with individuals from outside the village.

In support of the third channel, we find evidence that benefits in CDD projects are more unequally distributed than in other projects. In particular, households with larger land holdings and members of the VDC (a key village-level institution for the CDD program) benefit significantly more than other households. We further document that proxies for unequal benefits at the village level or between households are associated with fewer transactions in economic networks. The observed reduction in economic transactions is accompanied by a reduction in friendship links, reduced participation in community-based organizations, and marginalized groups participating less in village meetings. This supports the hypothesis that within a CDD program unequally distributed benefits, elite capture and favoritism could cause a reduction in social capital (see also Barakat, 2006; Morel et al., 2009; Barron et al., 2011; King and Samii, 2014). Reduced social capital, in particular, smaller social networks, theoretically reduces social network-based trust (Karlan et al., 2009), and therefore increases the cost of maintaining relationships, and as a consequence reduces economic interactions.

Whether reduced links are a sign of welfare reductions partly depends on whether the treatment decreased the need for informal transactions and increased the benefits of the market-based outside option or whether it increased costs of link formation. The former would suggest that reductions in informal transactions are related to increases in utility, while the latter would suggest reduced transactions signal reductions in utility. We found indications for both. Yet, the positive effects on economic wealth seem too small and too unequally distributed to support the view that large decreases in the need for informal transactions caused the observed effect on networks. Thus, we conjecture that the utility loss caused by the increases in the cost of link formation outweigh potential positive utility gains implied by a reduced need for informal transactions. Additionally, using data on shocks, we investigate whether the reduced density of economic networks negatively impacts households' welfare through a reduction in their ability to insure informally and to cope with shocks through informal transactions. We do not find a difference in the effect of the CDD program on the probability that a shock triggers an economic flow, suggesting that at least the extent to which informal transactions are used to insure against the types of shocks that we measure, is not negatively impacted.

Thus, while a substantial amount of results indicating reduction in social capital within treatment villages suggests negative implications for welfare, the insignificant results related to the ability to deal with shocks do not provide additional evidence about negative implications of reduced network interactions for well-being. Additional work is needed to provide a more conclusive picture of these implications.

Further, while our experimental data allow us to cleanly identify the main effect of treatment on economic and social interactions, the analysis of possible channels is limited by available data. Therefore, future work should also consider research designs that allow researchers to test for the existence of specific channels that link the CDD projects' activities to (informal) economic within-village transactions.

Despite the need for further research, our paper has important implications for evaluations of the welfare benefits of interventions such as the Gambian CDD program. It highlights an important avenue through which a development program can have unintended consequences. In our case, informal networks were weakened as a result of the program and it may not be easy to fully rebuild them quickly.

The findings reported in this paper are first and foremost saying something about The Gambia. However, some recent concurrent work by Banerjee et al. (2018) and Binzel et al. (2013) suggests that our results may also generalize to other contexts. These authors study experimental variations in access to financial services. Both papers find, as we do, reductions in dimensions of village-interactions that are not directly related to the intervention. Banerjee et al. (2018) mainly document changes to networks without investigating the consequences. However, Binzel et al. (2013) explicitly highlight the reduction in the risk-sharing capacity of informal networks, in particular they find reduced ability to borrow informally, and fewer transfers in dictator games. One important difference between our paper and these related papers is that they study a change in the economic environment that has—through financial relationships with banks and microfinance organizations—direct effects at the individual-level or within small microfinance groups. This gives immediately rise to plausible channels through which the interventions may have an effect on network-related outcomes. Our paper, on the other hand, has at its root an intervention that affects villages as a whole. Further, the CDD intervention explicitly also aims at affecting social activities. Both features open up the possibility that aspects of political economy, such as elite capture, become important channels. However, more research is required to investigate the relationship between development projects and within-village conflict, as development projects may induce (distributional) conflict when a project is a success but also when benefits do not materialize or are unequally distributed.

Together, our findings suggest that development projects that intend to bring positive economic change and in the process intentionally affect social interactions, may influence social and economic networks negatively. We found evidence that is consistent with two possible explanations: a shift towards a more market-oriented economy and unequally distributed benefits, possibly due to elite capture. To the extent that these effects are due to market orientation, this implies that in environments where significant economic change occurs, measures to alleviate the loss of informal networks or measures to compensate for this loss, such as the introduction of formal insurance mechanisms, should be considered. To the extent that reductions in interactions reflect reductions in social capital due to unequally distributed benefits, elite capture, or favoritism, special care has to be taken, e.g., throughout the facilitation process, the choice of sub-projects, and when setting up hierarchical project structures, to avoid increasing existing tensions and to prevent potentially new sources of internal divisions.

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# Appendix

## A Baseline Balance and Summary Statistics

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

| | Mean | | Observations | | Difference | | $p$-value | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | control | treated | control | treated | raw | cond. | CRSE | RI |
| *Panel A*: household-level characteristics (2003) | | | | | | | | |
| female | 0.52 | 0.53 | 13820 | 13969 | 0.008 | 0.006 | 0.48 | 0.56 |
| 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 | 0.50 |
| 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 | 0.26 |
| migrant | 0.08 | 0.09 | 11730 | 11792 | 0.010 | 0.018 | 0.47 | 0.57 |
| education | 0.57 | 0.55 | 12641 | 12776 | −0.024 | −0.016 | 0.65 | 0.68 |
| *Panel B*: village-level controls (2003) | | | | | | | | |
| electrification rate (%) | 3.22 | 2.37 | 28 | 28 | −0.847 | −0.725 | 0.44 | 0.44 |
| private WC (%) | 63.16 | 59.72 | 28 | 28 | −3.443 | −3.307 | 0.66 | 0.66 |
| access to clean water (%) | 9.96 | 8.25 | 28 | 28 | −1.712 | −1.804 | 0.59 | 0.60 |
| literate rate (%) | 64.69 | 59.67 | 28 | 28 | −5.022 | −4.962 | 0.22 | 0.22 |
| population | 493.57 | 498.89 | 28 | 28 | 5.321 | 2.145 | 0.96 | 0.96 |
| households (2014) | 48.50 | 50.57 | 28 | 28 | 2.071 | 1.566 | 0.75 | 0.76 |
| *Panel C*: household-level controls (2014) | | | | | | | | |
| trad. leader | 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 | 0.60 |
| formal education (head) | 0.15 | 0.19 | 1358 | 1416 | 0.045 | 0.043 | 0.03 | 0.05 |
| 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*: pre-treatment network proxies (2003) | | | | | | | | |
| born in this village | 0.79 | 0.79 | 1275 | 1325 | −0.001 | −0.012 | 0.76 | 0.79 |
| # households in compound | 2.13 | 2.25 | 1275 | 1325 | 0.117 | 0.215 | 0.53 | 0.63 |
| # coethnics in village | 344.28 | 363.69 | 1275 | 1325 | 19.408 | 15.182 | 0.68 | 0.73 |
| # spouses born in village | 0.83 | 0.78 | 1275 | 1325 | −0.045 | −0.082 | 0.22 | 0.28 |

*Notes:* Columns 1 and 2 display the means of each variable in the respective treatment group. The respective sample sizes are shown in columns 3 and 4. Column 5 shows the raw difference in means, while column 6 shows the difference after controlling for ward fixed effects. Columns 7 and 8 show $p$-values of a test for no difference in means, controlling for ward fixed effects. The version of the test based on cluster-robust standard errors (CRSE) in column 7 is slightly more conservative on average than the test based on randomization inference (RI) in column 8. The data underlying Panels A, B and D stem from the Gambian Census 2003, with the exception of the household count, which is taken from the network data from 2014. Panel C is based on data collected by the authors. Variables displayed in Panels B and C are the control variables used for all regressions unless indicated otherwise. Population and number of households enter the regressions logarithmically. Variables in Panel D are variables that we consider close proxies of network degree in various social networks.

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

| | Mean | | Observations | | Difference | | $p$-value | |
|---|---|---|---|---|---|---|---|---|
| | (1) control | (2) treated | (3) control | (4) treated | (5) raw | (6) cond. | (7) CRSE | (8) RI |
| economic (union) | 3.32 | 2.87 | 1358 | 1416 | −0.446 | −0.440 | 0.03 | 0.06 |
| -land | 1.14 | 1.02 | 1358 | 1416 | −0.119 | −0.151 | 0.16 | 0.22 |
| -labor | 1.44 | 1.44 | 1358 | 1416 | 0.000 | −0.045 | 0.74 | 0.77 |
| -inputs | 1.79 | 1.54 | 1358 | 1416 | −0.252 | −0.190 | 0.32 | 0.38 |
| -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 | 0.11 |
| -credit | 1.09 | 0.95 | 1358 | 1416 | −0.148 | −0.104 | 0.46 | 0.52 |
| friendship | 2.66 | 2.42 | 1358 | 1416 | −0.239 | −0.276 | 0.31 | 0.37 |
| kinship | 3.06 | 3.19 | 1358 | 1416 | 0.132 | 0.005 | 0.99 | 0.99 |
| mean distance to other villagers | 184.87 | 204.68 | 1302 | 1349 | 19.810 | 15.116 | 0.32 | 0.40 |
| missing exact GPS location | 0.04 | 0.05 | 1358 | 1416 | 0.006 | −0.002 | 0.84 | 0.87 |
| number of “neighbors” | 6.65 | 7.04 | 1358 | 1416 | 0.383 | 0.322 | 0.56 | 0.64 |

*Notes:* Columns 1 and 2 display the means of each variable in the respective treatment group. The respective sample sizes are shown in columns 3 and 4. Column 5 shows the raw difference in means, while column 6 shows the difference after controlling for ward fixed effects. Columns 7 and 8 show $p$-values of a test for no difference in means, controlling for ward fixed effects. The version of the test based on cluster-robust standard errors (CRSE) in column 7 is slightly more conservative on average than the test based on randomization inference (RI) in column 8. Number are based on the network data collected by the authors. Numbers represent the undirected, unweighted network degree of the households, i.e., the number of distinct transaction/link partners from within the village, irrespective of the number and direction of transactions with these households. Neighbors are defined based on geographic closeness (see Footnote 36).

Our analysis considers both, geographic distance and kinship, as invariant to treatment and Table 11 suggests that there are no systematic difference between treatment and control villages. Further, we have no evidence for geographic movement within villages. The spatial extent of villages is usually small (the 90% of all pairwise distances are less than 389 m, half are less than 166 m) and thus incentives to relocate strategically are limited. Kinship is measured via the kinship network described in Section 3.3 and includes first-degree relatives and children's in-laws. Thus, changes in response to treatment would require marriages in response to treatment or systematically different reporting. We consider both unlikely given the anecdotal evidence and two econometric tests: in-law networks, where strategic marriages should matter most, as well as the rate at which kinship ties are confirmed by both sides, which would indicate omissions due to differential reporting, are statistically indistinguishable in treatment and control communities.

# B Other Specifications for the Average Treatment Effect Estimate

Table 12: Main ATE Specification, Robustness

| | post double-LASSO | w/o ward 319 | w/o ward 607 | w/o ward 319 + 607 | probit | intensity | unpaid | undirected |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | any transaction | any transaction | any transaction | any transaction | any transaction | any trans. (intensity) | unpaid econ. transaction | economic transaction |
| treatment | -0.884 | -1.109 | -1.219 | -1.187 | -1.201 | -0.033 | -1.087 | -1.751 |
| | (0.046)●● | (0.006)●●⊛ | (0.003)●●⊛ | (0.004)●●⊛ | (0.000)●●⊛ | (0.031)●⊛ | (0.005)●●⊛ | (0.007)●●⊛ |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| dyads | 151632 | 140368 | 145472 | 134208 | 151632 | 151495 | 151632 | 75816 |
| households | 2774 | 2596 | 2640 | 2462 | 2774 | 2774 | 2774 | 2774 |
| control mean dep. var. | 6.9 | 6.8 | 6.8 | 6.7 | 6.9 | 0.2 | 6.5 | 11.9 |

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. In column 8 undirected dyads are used. Regressions control for ward fixed effects and a set of control variables: The village-level variables in Table 10, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Table 10, as well as ethnicity and enumerator dummies, enter the regressions twice, for the sending and the receiving household of a dyad. In column 1 we use the post double-LASSO to selectively exclude control variables from the regression, as suggested by Belloni et al. (2014) and implemented by Ahrens et al. (2018). Due to factors unrelated to treatment, two villages were excluded from the sample and consequently two ward-strata have unequal numbers of treatment and control villages. Columns 2-4 document that the results are robust to excluding those wards entirely. Column 5 estimates a probit instead of a linear probability model and reports average marginal effects. Results in column 6 document that an ATE of comparable magnitude (relative to the control group mean) is found when instead of a binary indicator, the intensity of the transaction is used. Land transactions are measured in hectares. For labor, the volume/intensity of a transaction is measured as the product of people and days one household provided to the other. For inputs, intensity is measured as the number of distinct inputs categories provided (e.g., seeds, tools, fertilizers). In the food network, transactions are converted into units roughly equivalent to the nutritional value of one kilogram of fruit or beans, by applying the factor 0.5 to rice, maize, millet, and groundnut, and the factor 4 to milk, meat, and fish. Across the six domains, measures are aggregated to a $z$-score that is normalized to have variance 1 and take on the value 0 if there is no transaction. Weights for this $z$-score are obtained following the variance-covariance weighting suggested by Anderson (2008) and also normalized to have variance 1. Column 7 excludes transactions that involved some form of payment. Column 8 treats the network of transactions as an undirected network.

# C Favoritism

To shed light on the possible role of favoritism, we study whether dyads between decision makers and closely connected households show disproportionately large treatment effects. Indeed, recently Bandiera et al. (2018) provides evidence for an agricultural extension program in Uganda, that social ties determine program delivery. They argue that households that are central to the program channel resources to individuals that they are socially connected to, possibly in exchange for other favors. Translated into the context of networks of economic transactions, this would imply increased informal economic interaction between pairs that are socially tied and where one belongs to the VDC and the other does not. Consistent with this, in Table 13 we find that the heterogeneous effect with respect to likely VDC membership that we document in Table 8 is to a large extent explained by transactions in dyads that are connected through kinship and where the two households have a very different VDC-score.

Table 13: Heterogeneous Treatment Effects for Contacts Between Likely VDC members and Their Social Links

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

*Notes:* $^{\bullet}$/$^{*}$ $p < 0.1$, $^{\bullet\bullet}$/$^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}$/$^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. Regressions control for ward fixed effects and a set of control variables: The village-level variables in Table 10, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Table 10, as well as ethnicity and enumerator dummies, enter the regressions twice, for the sending and the receiving household of a dyad. VDC-score measures the likelihood that a household would be in the VDC if the village is/would be a CDD village. Values of the score are normalized to have mean zero and variance one. The absolute difference of the two scores used in these regressions is centered.

# Online Supplement

## D The Gambian CDD

### D.1 Institutional Background

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

The Gambian CDD program followed the design of a typical CDD program, which promotes community involvement at all stages of the process from identification of the potential sub-projects to their maintenance after implementation.[46] 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.[47] 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).

### D.2 Magnitude of the Disbursement

The country-wide average funding per household is approximately US$140. In the sub-sample for this study, because villages have fewer households, it is US$230 (in 2009). Per-household funding in Casey et al. (2012), who study a CDD program that is in many ways comparable

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

[47] 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.

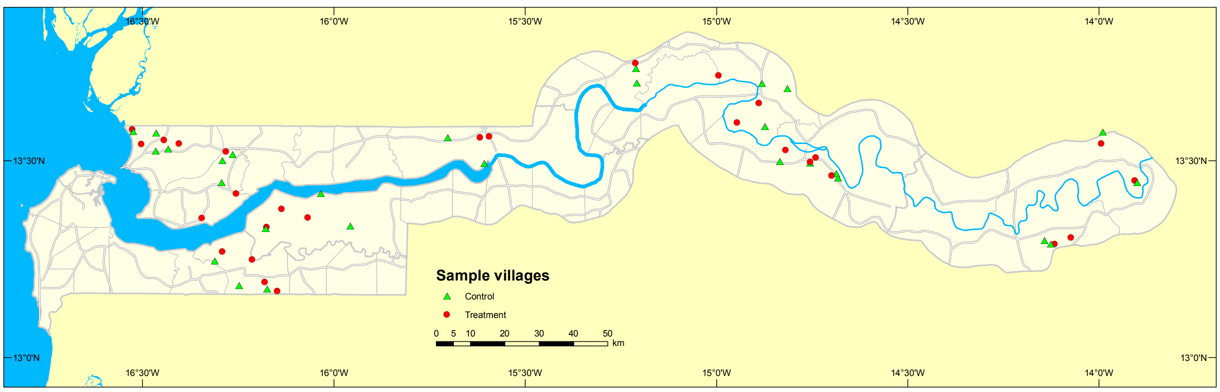
to the program in The Gambia, is US$100. A summary provided by Wong (2012) shows that per-household spending in the Gambia CDD is also large relative to CDD programs in other parts of the world (Wong 2012, Table 4). For comparison, for microfinance programs discussed in Banerjee et al. (2015b) the loan size as a proportion of income is between 6 and 43% of annual household income in 5 of the 6 countries covered. To translate this into a subsidy, consider, for example, Mexico, where the difference in interest rates charged by the program and market interest rates is 35% APR, which translates into subsidy levels of less than 3% of annual household income. As an alternative way to illustrate the magnitude, note that the funds in the Gambian CDD would suffice to provide each household with about eight goats (roughly the same magnitude that is reported, for example, in Banerjee et al. 2015a).

# E Sampling and Data Collection

## E.1 Village Sample

The sample was originally drawn in a first round of network data collection that took place in 2009, during the implementation of the Gambian CDD program (these data are described in detail by Arcand et al., 2010 and Jaimovich, 2015). In this original sample, 62 villages were selected in a two-stage procedure. First, a set of wards was randomly chosen from all wards with eligible villages. Second, within wards an equal number of control and treatment villages was randomly selected from the set of all villages of the desired population size. For the 2014 data collection for this study we excluded one urban ward (four communities) from the sample, in order to focus exclusively on rural villages. Aside from this, two villages had to be excluded from the data collection. In one village that was originally assigned to treatment, *Kerr Mod Ali*, all but a handful of households had followed a local religious leader, relocated right across the border to Senegal, and could not be interviewed. According to disbursement data this village did not receive funds. One control village was excluded because of incomplete data in the 2009 data collection. Our main results remain equally strong when we exclude either one or both wards where these two villages are located (see Appendix Table 12, columns 2-4).

Figure 1: Sample Villages in The Gambia



## E.2 Household Structure and Sampling of Households

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

## E.3 Elicitation of Networks in the Main Survey

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

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

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

# F VDC Membership Determinants and Random Forest Classifications

The Village Development Committee (VDC) is a group that is central for the CDD program's implementation and for our analysis. While the concepts of VDCs existed before the implementation of the CDD program (Local Government Act, 2002), they were often not fully established and the roll-out of the CDD program was used to enforce the inclusiveness criteria and ensure the establishment of VDCs in all communities (GoTG, 2006). As a result, the members of the VDC in treatment and control communities are not comparable. For the purpose of a treatment effect heterogeneity analysis, VDC membership is thus endogenous. As Table 14, columns 1 and 2 show, VDC members in treatment villages are on average significantly older, more educated, more likely to belong to an ethnic minority and owning less land than their counterparts in control villages. In order to be able to study treatment effect heterogeneity, we compute a measure for the household's likelihood of being in the VDC if a village becomes a CDD village. This likelihood is estimated using several variables which are not affected by the program and thus is not endogenous. We treat the predicted likelihood as a household-level characteristic and study heterogeneity of treatment effects along this dimension. Our approach is inspired by Banerjee et al. (2018), but relies on household characteristics to predict VDC membership, instead of take-up of microfinance.

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

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

- Survey-based household characteristics: being the village chief's household, being the household of a first degree relative of the chief, being the imam's household, age*, sex*, formal education*, Koranic education, household size*, owned land, number of wives*, be-

Table 14: VDC Composition Differs Between Treatment and Control Communities

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

*Notes:* $^{\bullet}$/$^{*}$ $p < 0.1$, $^{\bullet\bullet}$/$^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}$/$^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The dependent variable is an indicator for VDC membership (being a VDC executive or the head of the committee). For observations where information on land holdings, age or gender of the household head were missing, we imputed the village-level mean (applies to less than 1% of observations). The units of observation are households. Control variables are the village-level variables listed in Appendix Table 10, Panel B, and enumerator fixed effects, ethnicity fixed effects and, in column 1, ward-fixed effects. In column 3 the sample is restricted to only treatment villages, to assess the accuracy of the random forest result for a sub-sample where the outcome is observable.

ing the compound head, ethnicity* and a how long a household has been living in that village*.

- Network data-based household characteristics: inverse distance to the chief in the family network and in the joint family and neighborhood network, average (inverse) distance to all households in the family network, the neighborhood network, and in the joint family and neighborhood network, the number of other households in the village a household is connected to through kinship, and the number of households outside the village household is connected to through kinship.

- Other household characteristics: the ward a village/household belongs to*, the share a household's ethnicity makes up in a village*, and whether or not a household belongs to the village's ethnic majority*.

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

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

The predicted probabilities resulting from this process have a mean of 0.188 and a standard deviation of 0.096. Column 3 of Table 14 shows that households who are classified by the out-of-sample prediction from the random forest to have a high probability of being in the VDC actually are significantly more likely to report being in the VDC.

# G Details for Randomization Inference

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

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

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. We implement randomization inference as follows (for details see Heß, 2017):

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

2. Second, we draw $R = 10,000$ hypothetical realizations of the treatment assignment, exactly following the original process (village-level treatment, stratified by ward) and compute the

same $t$-statistics, $\{t_r\}_{r=1,\ldots,R}$, based on these realizations. The obtained set of hypothetical realizations of the $t$-statistics are independent draws from the distribution of $t$-statistics under the sharp null hypothesis of no treatment effect.

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

# H Robustness of the Dyadic Average Treatment Effect Estimate

## H.1 Disaggregation by Transaction Type

Table 15: Dyadic Regressions, Average Treatment Effect in Individual Networks

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

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. Regressions control for ward fixed effects and a set of control variables: The village-level variables in Appendix Table 10, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table 10, as well as ethnicity and enumerator dummies, enter the regressions twice, for the sending and the receiving household of a dyad. Indicators for traditional leaders and marginal households are also included but not shown in this table.

## H.2 Analysis at Village and Household Levels

Table 16 studies several measures capturing different aspects of network topology at the village level, namely: network density, average inverse path length, the network clustering coefficient, and the total number of closed triads. The results are consistent with the findings at the other levels of aggregation. Column 1 and 2 suggest that network density as well as the average inverse path length between households is reduced by treatment, indicating a reduction in the overall economic connectedness of households. Further, column 3 suggests that the network-level clustering coefficient decreased in treatment villages, though the corresponding coefficient for the insignificant at conventional levels ($p$-value=0.104). Clustering measures the propensity of nodes in a network to close triangles of households that are already connected.

Correspondingly, the total number of observed triangles is also reduced (column 4). A reduction in triangular transactions is consistent with a shift away from socially embedded transactions towards market-based exchanges. As discussed in Section 6 and Gagnon and Goyal (2017), socially embedded transactions can be characterized by positive local externalities, i.e. they increase the value of transactions for neighboring households, thus causing the emergence of clustering in the network of transactions. One-shot market transactions that do not require trust or a social collateral do not have this effect.

Table 16: Village-Level Network Regressions

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

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are villages. Regressions control for ward fixed effects and a set of control variables: Village-level variables in Appendix Table 10, Panel B, as well as the shares of households belonging to each ethnicity and the shares of households interviewed by each enumerator.

Table 17 shows the result of the estimation of the household-level variant of Equation (3.1), using household degree centrality as the dependent variable. The coefficient of -0.827 in column 1 of Panel A indicates that households in treatment villages receive transactions from 20% fewer exchange partners than households in control villages.

Table 17: Degree in the Village-Internal Network

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

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. Total in- and out-degree measures vary within villages because some households report links but conceal the identity of their partner. This is most common for money lending and alms (*Zakat* in Islam), which—for religious reasons—tend to be reported by the recipient only. Regressions control for ward fixed effects and a set of control variables: Household- and village-level variables in Panels B and C of Appendix Table 10 as well as ethnicity and enumerator dummies.

## H.3 Difference-in-Differences

Similar data to what we use for this study was collected in 2009. Because of the timing of the data collection, which occurred at a time when in some villages project implementation had already begun, and the villages have had meetings and may have already received funds, these cannot be considered clean baseline data. In fact one of the goals of that data collection was to enable an analysis of short term effects on social dynamics.[48] There are also differences in how the data was collected, and matching households is problematic because of many changes in household composition (mergers and splits) in treatment and control villages.

Table 18 shows summary statistics based on these data. There is no evidence for a strong negative short-term treatment effect. At the same time, with this data it is also not possible to reject sizable negative treatment effects.

Nonetheless, our results are robust to using these data to estimate a Difference-in-Differences (DiD) regression using both waves. The results are reported in Table 19. The DiD estimate for economic transactions is negative and statistically significant, in line with our main average treatment effect estimates based only on our 2014 data. As an additional placebo test that would capture if changes between 2009 and 2014, that relate to village composition (migration, household splits) or reporting were driving our results, we repeat the same DiD estimation using the kinship network and find no treatment effect there. In addition, there is no significant difference between treatment and control villages in 2009 (neither in the economic networks

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

Table 18: Balance Tests for Network Degrees Kinship Measured in 2009

| | Mean | | Observations | | Difference | | $p$-value | |
|---|---|---|---|---|---|---|---|---|
| | (1) control | (2) treated | (3) control | (4) treated | (5) raw | (6) cond. | (7) CRSE | (8) RI |
| economic (union) | 2.42 | 2.12 | 1307 | 1318 | −0.299 | −0.219 | 0.45 | 0.51 |
| -land | 0.54 | 0.52 | 1307 | 1318 | −0.025 | −0.019 | 0.76 | 0.78 |
| -labor | 0.68 | 0.64 | 1307 | 1318 | −0.039 | −0.008 | 0.94 | 0.95 |
| -inputs | 1.11 | 0.83 | 1307 | 1318 | −0.280 | −0.233 | 0.20 | 0.26 |
| -credit | 0.50 | 0.40 | 1307 | 1318 | −0.093 | −0.068 | 0.46 | 0.50 |
| kinship | 3.73 | 3.95 | 1307 | 1318 | 0.225 | 0.317 | 0.26 | 0.34 |

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

nor in the kinship network).

Thus, to explain our main findings with significant imbalances prior to the CDD project, it would need to be the case that there were significant short-term effects manifesting prior to the 2009 data collection that have roughly the same magnitude as the imbalances but the opposite sign (in that case, the DiD estimate is the difference between long-term and short-term effects). Considering jointly the lack of significant differences in networks in the 2009 data (Tables 18 and 19) and the balance of social network proxies in 2003 (Appendix Table 10, Panel D), this seems highly unlikely

Table 19: Difference-in-Differences With the Data From 2009

| | economic (directed) | | economic (undirected) | | kinship/placebo (undirected) | |
|---|---|---|---|---|---|---|
| | (1) any transaction | (2) any transaction | (3) any transaction | (4) any transaction | (5) kinship | (6) kinship |
| treatment | 0.042 | | 0.260 | | −0.784 | |
| | (0.933) | | (0.733) | | (0.399) | |
| post | 2.518 | 2.314 | 4.630 | 4.302 | −7.022 | −7.042 |
| | (0.000)*** | (0.000)*** | (0.000)*** | (0.000)*** | (0.000)*** | (0.000)*** |
| treatment × post | −1.064 | −1.166 | −1.922 | −2.064 | 0.004 | 0.042 |
| | (0.081)• | (0.054)• | (0.034)•• | (0.024)•• | (0.997) | (0.972) |
| 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. var. | 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 and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). 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. Some network questions differ between the waves, in particular 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 3.4. Units of observation are dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. 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'.

## H.4 Robustness of the Main Findings: Compensation?

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

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

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

In a further effort to investigate the role of non-CDD development programs, we also estimate the budgets of development programs. Since we do not have official data on non-CDD project, we assign values based on project categories in the non-CDD development project descriptions and similar sub-projects that were implemented as part of the CDD program (e.g., if establishing a garden is mentioned as a non-CDD project, we use the median costs for gardens recorded in the CDD database).[49] While this is not a precise measure of the actual funds distributed through a development project, it is a rough proxy for the market value of the items provided by non-CDD development projects.

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

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

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

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

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

To further investigate this, Table 20 uses those proxies for the number of projects and for project budgets in a difference-in-difference framework, where each observation is the number of projects or the budget size for a village-year. Based on data from village focus groups (columns 1-2), this table shows that there were 0.5 more projects reported in treatment villages than in control villages, i.e., about 1.5 more overall during the years 2008-2010. The total budget in treatment villages is estimated to exceeded that of the control group by an annual 2,913 USD during the CDD years (a total of roughly 9,000 USD over the three CDD years). On the other hand, there is no significant difference for the years before or after the CDD. Columns 3 and 4 again restrict the data to those projects reported by focus groups that were named by three or more households in the in-depth survey. The results using these reports, that are likely less prone to reporting biases, remain comparable.

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

Table 20: Number of Projects and Estimated Budget by Year

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

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

to the benefits that CDD-villages received during the CDD years.

(ii) As a further robustness check, we use the measurements on non-CDD projects as controls in our main dyadic regressions. We add a variable measuring the number of non-CDD projects implemented between 2011 and 2014 and one that controls for a binary indicator for having implemented any non-CDD projects in this time frame. The results are shown in Table 21, columns 2-3. In these regressions, the main CDD-treatment effect is still negative, statistically significant and of comparable magnitude as in the main specification in column 1. Of course, adding data on the number of other programs to our main specification makes the estimates harder to interpret. Because of obvious concerns about endogeneity, we are reluctant to interpret the coefficient on the non-CDD projects variables. Yet, if the negative treatment effects we observe in our main regressions would indeed be a *positive* effect of compensatory non-CDD projects, we would expect that controlling for non-CDD projects affects the ATE estimate in those regressions. The fact that this is not the case suggests that non-CDD projects are not fully responsible for the correlation between CDD projects and economic flows that we find.

(iii) At the time of the implementation of the CDD, a later scaling-up was envisioned in which all eligible villages would eventually receive program benefits similar to the villages that were treated in 2008-2010. However, the scaling-up was conditional on a positive evaluation of the program by the implementing agency.[50] Therefore, the intentions to scale up were not announced to control villages at the time, and control villages could not expect a treatment in the future. By the time of our data collection in 2014 this scaling-up had not yet occurred. After our data collection—towards the end of 2014—activities within a program called the

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

Table 21: Robustness of the Main Result to Controlling for Compensatory Projects

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

*Notes:* •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). Units of observation are directed dyads. The dependent variable takes on the value 100 if a dyad had a transaction and 0 otherwise. Regressions control for ward fixed effects and a set of control variables: The village-level variables in Appendix Table 10, Panel B, dyadic indicators for kinship, shared ethnicity, and interview group. Further, household-level variables in Panel C of Appendix Table 10, as well as ethnicity and enumerator dummies, enter the regressions twice, for the sending and the receiving household of a dyad. Indicators for traditional leaders and marginal households are also included but not shown in this table.

Community-Based Infrastructure and Livelihood Improvement Project (CILIP), which was modeled after a CDD (and funded by the Islamic Development Bank), started in some of the control communities. We have the full list of CILIP recipient villages and some of the control villages received funds as part of CILIP. In our sample, less than half of the control villages (11 out of 28) were selected to receive CILIP funds.[51] Our results remain virtually unchanged if we control for CILIP recipient villages (see Table 21, column 4), which is reassuring but not surprising, given that the actual disbursement of the CILIP funds had not begun at the time of our data collection.

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

There is also a large number of different "project partners" mentioned during the focus group discussion with the village authorities for the non-CDD projects. While there is clearly measurement error in these data, this is evidence that there was no single organization (international, government or NGO) behind the projects that were implemented in non-CDD villages. Thus, the data suggest that the projects implemented in control villages were not part of a coordinated effort by one or a small number of organizations to compensate control villages for being left out from the CDD project.

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

# I Other Treatment Effect Regressions

## I.1 Wealth

Table 22: In-Depth Survey: Asset/Wealth Variables (1)

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

*Notes:* $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: Household- and village-level variables in Panels B and C of Appendix Table 10 as well as ethnicity and enumerator dummies.

Table 23: In-Depth Survey: Asset/Wealth Variables (2)

| | (1)<br>bicycle | (2)<br>mobile | (3)<br>smart phone | (4)<br>radio | (5)<br>generator | (6)<br>motorbike | (7)<br>car | (8)<br>tv | (9)<br>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)$^{\bullet\bullet\circledast}$ | (0.209) | (0.330) | (0.019)$^{\bullet\circledast}$ | (0.904) |
| controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| households | 550 | 548 | 549 | 548 | 550 | 550 | 550 | 550 | 550 |
| control mean dep. var. | 0.6 | 1.0 | 0.1 | 0.7 | 0.2 | 0.1 | 0.0 | 0.1 | 0.0 |

*Notes:* $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: Household- and village-level variables in Panels B and C of Appendix Table 10 as well as ethnicity and enumerator dummies.

Table 24: In-Depth Survey: Asset/Wealth Variables (3)

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

*Notes:* $^{\bullet}/^{*}$ $p < 0.1$, $^{\bullet\bullet}/^{**}$ $p < 0.05$, $^{\bullet\bullet\bullet}/^{***}$ $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: Household- and village-level variables in Panels B and C of Appendix Table 10 as well as ethnicity and enumerator dummies.

# J Project Benefits and Project Decision Making

Table 25: Who Reaps Benefits from Projects? Who Decided on Projects?

(a) Household-Level Regressions

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

(b) Project-Level Regressions

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

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

# K Project Summary Statistics

Table 26: Perceived Relative Benefit by Project Type

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

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

Table 27: Project Failure Rates

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

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

# L Shocks are Balanced

Table 28: Shock Incidence Is Not Affected by Treatment

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

*Notes:* •/* $p < 0.1$, ••/** $p < 0.05$, •••/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: Household- and village-level variables in Panels B and C of Appendix Table 10 as well as ethnicity and enumerator dummies.

# M More on Mechanisms

## M.1 Were Projects Implemented?

In this appendix we confirm a necessary condition for the CDD program to have any impact, namely that it was implemented, both physically as well as using the mandated procedures.[52] 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 29).[53] Thus, all treatment villages received significant funds and implemented sub-projects, such as tractors, milling machines or seed stores, that resulted in

[52]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.

[53]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.

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 30).[54]

Second, we investigate whether procedures were implemented as required by the CDD program's guidelines. Again, we confirm implementation. Using project-level reports from the in-depth survey, we find that compared to non-CDD projects, CDD sub-projects are almost twice as likely to have involved the whole village or the VDC in decision making (a 17 or 23 percentage points increase respectively, as shown in Table 31). Also, evidence suggests that voting took place for at least some CDD sub-projects and voting is significantly more likely for CDD than for non-CDD development projects that were implemented in villages in the last 5 years.

Table 29: 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 30: 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.

[54]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.

## M.2 Did Intended Institutional/Social Change Take Place?

The second area that CDD aims to affect is institutions. Table 31 investigates data at the project level. Columns 1 to 4 shows that non-CDD projects implemented in these villages also seem to be more inclusive, in the sense that villagers report participating in the decision making and they apparently have done so at least in part through voting. Results in columns 5 to 8 are evidence that relative to other development projects, CDD sub-project implementation was accompanied by substantially more voting and that decisions were mostly made by the whole community or delegated to village development committees, as opposed to being taken by the traditional elites. Moreover, these data can be used to test whether these procedural changes also affected joint decision-making procedures in other, non-CDD projects. Indeed, we find some evidence of such spillovers.

Table 31: CDD Projects Were Implemented in Line With CDD Requirements and Procedures Spilled Over Into Other Projects

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

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

However, we do not find evidence of institutional change in data on community meetings in general. Table 32 shows that there are no average treatment effects with respect to decision-making processes, or for households' feelings of empowerment. In fact, the results even provide suggestive evidence that marginalized households got further excluded from village activities and village-level decision making.

Table 32: Institutional Change in Village-Level Decision Making

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

*Notes:* ●/* $p < 0.1$, ●●/** $p < 0.05$, ●●●/*** $p < 0.01$. $p$-values in parentheses and asterisks allow for village-level clustering. Bullets indicate significance under randomization inference (see notes to Table 1). The units of observation are households. Regressions control for ward fixed effects and a set of control variables: Household- and village-level variables in Panels B and C of Appendix Table 10 as well as ethnicity and enumerator dummies. 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".

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