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International Interventions to Build Social Capital

Evidence from a Field Experiment in Sudan

Alexandra Avdeenko
Michael J. Gilligan

The World Bank
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&
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Abstract

Over the past decade the international community, especially the World Bank, has conducted programs to increase local public service delivery in developing countries by improving local governing institutions and creating social capital. This paper evaluates one such program in Sudan to answer the question: Can the international community change the grassroots civic culture of developing countries to increase social capital? The paper offers three contributions. First, it uses lab-in-the-field measures to focus on the effects of the program on pro-social preferences without the confounding influence of any program- induced changes on local governing institutions. Second, it tests whether the program led to denser social networks in recipient communities. Based on these two measures, the effect of the program was a precisely estimated zero. However, in a retrospective survey, respondents from program communities characterized their behavior as being more pro-social and their communities more socially cohesive. This leads to a third contribution of the paper: it provides evidence for the hypothesis, stated by several scholars in the literature, that retrospective survey measures of social capital over biased evidence of a positive effect of these programs. Regardless of one's faith in retrospective self-reported survey measures, the results clearly point to zero impact of the program on pro-social preferences and social network density. Therefore, if the increase in self-reported behaviors is accurate, it must be because of social sanctions that enforce compliance with pro-social norms through mechanisms other than the social networks that were measured.

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International Interventions to Build Social Capital: Evidence from a Field Experiment in Sudan*

Alexandra Avdeenko† and Michael J. Gilligan‡

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†Ph.D. candidate German Institute for Economic Research (DIW Berlin), aavdeenko@diw.de

‡Corresponding author, Associate Professor, Department of Politics, New York University, 19 West 4th St. 2nd Floor, New York, NY 10012, michael.gilligan@nyu.edu.

1 Introduction

Recently the international community, through some of its most important international and non-governmental organizations, has been engaged in a quiet campaign to bring about political change in the developing world from the bottom up. These interventions, known as *Community-Driven Development* (CDD) programs, are designed to improve public service delivery and livelihoods in poor areas. The method by which these programs seek to accomplish these goals includes the creation of more inclusive governing institutions coupled with a large dose of civic education. The new institutions are designed to foster greater citizen participation, instill a deeper appreciation for democratic values and equality (especially gender equality) and to increase the recipient communities' levels of generalized trust and capacities for collective action. While the main goal of these programs is to improve public service delivery at the local level, at their base they are attempting change the recipient societies' civic culture — a topic on which political scientists have made important contributions (Almond and Verba, 1963; Putnam, Leonardi and Nanetti, 1994; Putnam, 2000). In light of the number of these programs and the size of their budgets, their impact on the grassroots political processes of recipient communities is potentially huge.

These interventions have begun to draw the attention of political scientists (Beath, Christia and Eniolopov, 2012*a*,*b*, 2013; Humphreys, de la Sierra and van der Windt, 2012; Fearon, Humphreys and Weinstein, 2009; King, Samii and Snilstveit, 2010) and economists (Olken, 2010; Labonne and Chase, 2011; Casey, Glennerster and Miguel, 2012; Wong, 2012; Mansuri and Rao, 2013). We contribute to this literature by reporting findings from a field experiment of one such program, called the *Community Development Fund* (CDF), that was implemented in war-torn areas of Sudan between 2006 and 2011. The program was funded by a multi-donor trust fund managed by the World Bank and set up to foster post-war reconstruction and reconciliation following the (temporary) termination of Sudan's thirty-year civil war. Our goal was to determine if the program caused an increase in civic participation

and social capital as was intended.

The main contributions of the paper to the existing literature on CDD and grassroots political change are twofold. First we use lab-in-the field techniques that allow us to isolate one of the possible mechanisms by which CDD programs may (or may not) improve local political functioning. CDD programs attempt to improve communities' local political systems both by making citizens' preferences more pro-social and by making local governing institutions more inclusive and efficient. Our lab-in-the field measurement strategy allows us to isolate the effects of the program on pro-social preferences unmediated by any changes in local governance that may be caused by the program because local governance should have no effect on subjects' private decisions in the laboratory.

Our second contribution is a survey of social networks among the laboratory subjects. The networks survey provides a specific cataloging of the subjects' participation in civic associations, public service groups, savings groups and favor-exchange relationships (among others). This measurement of social network density is important because for some social capital theories (Putnam, Leonardi and Nanetti, 1994; Putnam, 2000), dense social networks are the *sine qua non* of social capital.[1] While the types of pro-social preferences we measure with the games are an important part of a well-functioning civil society they are not sufficient for adherents to this model of social capital, which requires not only that citizens possess pro-social preferences but also that they are in many relationships with each other.

As Putnam, 2000, pg. 19 puts it

> [S]ocial capital refers to networks among individuals—social networks and the norms of reciprocity and trustworthiness that arise from them. In that sense social

[1]Networks are particularly central in sociological theories of social capital. Bourdieu (1985) who can be credited with introducing the concept defined social capital as "the ... resources that are linked to ... a durable network of ... institutionalized relationships." Also see the review in Portes (1998)

> capital is closely related to what some have called "civic virtue." The difference is that "social capital" calls attention to the fact that civic virtue is more powerful when embedded in a dense network of reciprocal social relations. A society of many virtuous but isolated individuals is not necessarily rich in social capital.

Our networks data allow us to test if participation in the civic engagement activities mandated by the CDD program spilled over and encouraged participation in other areas of civic life. Combined with behavioral indicators of adherence to pro-social norms, these networks data allow us to test if these two components of social capital have improved as a result of the program we study.

Using these two measures we find no evidence that the program increased social capital. Subjects from treated communities did not act more pro-socially in the lab than did their counterparts from control communities. The estimated impacts of the program on these measures of social capital were precisely estimated zeros. Indeed in most cases the estimates of program effects were negative (and not significant) thus alleviating any concerns about low-powered tests. Furthermore members of our treated communities were no more involved in community networks than were members of the control communities; in fact they were significantly less involved on average.

In stark contrast, when answering retrospective survey questions, respondents from treated communities self-reported engaging in significantly more pro-social action than did members of the control communities and they characterized their communities as being much more socially cohesive than did members of control communities. This brings us to a third contribution of the paper: Our results corroborate suspicions voiced in the literature (Mansuri and Rao, 2013; Wong, 2012; Casey, Glennerster and Miguel, 2012; Fearon, Humphreys and Weinstein, 2009) about possible bias in self-reported levels of pro-sociality and social cohesion from retrospective surveys. Mansuri and Rao (2013) assert:

> Exposure to participatory messaging may [...] make members of program com-

> munities more likely to indicate more willingness to cooperate or to report higher levels of trust and support for democracy regardless of any substantive change in attitudes or practices. Local facilitators spend considerable time with community members elucidating the benefits of program participation, community collective action, self-help programs, community contributions to development projects and so forth. Isolating the impact of participation on preferences, trust, networks or cooperation is therefore likely to be difficult even in the best designed evaluation. Self-reported retrospective accounts of change are perhaps the least reliable source of information.

We were similarly concerned about such bias. Members of the treated communities in the program we studied were regularly coached on the importance of civic participation, voting, contributing to collective goods and so on. Members of the control communities received no such coaching because programmers did not operate in them. Thus we had strong reasons to be concerned that answers to survey questions might be biased by respondents who wanted to give the "right" answer. We adopted the measures mentioned above because of a concern at the outset that survey responses might be subject to social desirability bias.

Before proceeding to the main text we should clarify what we mean by social capital. We use social capital as an umbrella term for a set of individual preferences for social action that are believed to cause better political, social and economic outcomes *and* the social networks that purportedly support them. In particular, we include under this umbrella four such preferences: willingness to share with the needy, willingness to contribute to public goods, trust, and trustworthiness. We will discuss our measurement of these concepts and our measurement of social networks in greater detail later in the paper.

2 Sudan and the Community Development Fund

The Community Development Fund (CDF) was designed to address potential underlying economic causes of Sudan's civil war as well as any destruction of public infrastructure and social cohesion as a result of the violence. The 95 million USD project began operations in April of 2006. CDF has implemented projects in 616 communities. As of the end of 2012, 915 projects were completed, providing services to over two million people. These projects included extensive improvements and new construction of primary schools, health, water-supply and solar-electrification facilities and the training of midwives(Gossa, 2013).

A second component of the program aimed to develop capacity in areas of project management, community participation and empowerment. As mentioned in World Bank project documents from June 2008, while completing major infrastructure investments in primary schools, health facilities and water sources CDF staff provided "training and capacity building for better management of sub-projects to ensure sustainability and build social capital in socially fragmented communities." Thus building social capital is central to CDF's goals. The program sought to accomplish this goal with a community participatory approach to infrastructure building in which the villagers themselves selected the infrastructure program that would be built in their community.

"Social mobilizers" (essentially the same as "community organizers" in the US) were dispatched to each village. They established community-based organizations in all 616 program communities to assess the communities' development needs and assets and to oversee the construction of the CDF project. Social mobilizers helped the residents of the village complete a "community scorecard" through which villagers came to a collective understanding of their community's development and infrastructure needs and to identify their assets that could be used to help meet those needs. Through the community scorecard exercise the social mobilizers taught the community that they had capacity to solve some of their own problems through collective action, a lesson that was backed-up by requiring the community

to contribute at least 10 percent of the cost of its chosen projects either through monetary or in-kind contributions. In this way the program spurred some collective action in the treated communities. The social mobilizers set up executive committees and subcommittees for education, health and water for the planning and procurement of the infrastructure projects and they offered training in project management to these committees. They also organized frequent community meetings to ensure transparent project planning, procurement, monitoring and evaluation processes.

The four states chosen for CDF programming, South Kordofan, North Kordofan, Kassala, and Blue Nile were chosen with a peace-building goal in mind. Each of these states has been marred by violent conflict at some point over the last three decades. The *Comprehensive Peace Agreement* was signed in October 2005, ending 22 years of civil war, but the reemergence of tensions in Blue Nile and South Kordofan made it impossible for us to complete the study in those two states and therefore we are only able to report results from North Kordofan and Kassala.

While the main goal of the CDF was to increase public infrastructure, it has also undertaken capacity-building activities in communities to build social capital among fragmented populations and build ownership and collaboration to solve development problems. The programmers hoped that, by creating social capital, CDF could help communities overcome the collective action problems inherent in maintaining public goods infrastructure—problems that may have been exacerbated by the regions' recent experience with war—and thereby enhance the sustainability of its infrastructure investments.

3 Community-Driven Development's Theory of Change

For the last decade Community-Driven Development has been the instrument of choice among governmental and non-governmental aid agencies for fostering economic and social development in poor countries (Mansuri and Rao, 2013; Wong, 2012; Casey, Glennerster

and Miguel, 2012). The World Bank is particularly committed to the CDD approach. For example the International Development Association (IDA), the World Bank's fund for the world's poorest countries, has averaged over 1.3 billion USD in loans per year to CDD over the last decade. In 2008 alone they allocated almost two billion USD to CDD projects (IDA, 2009).

CDD programs are based on the hypothesis that they promote accountability, competence and inclusiveness of local institutions in developing countries and that they create social capital, which has well-known links to a variety of salubrious economic outcomes (Putnam, Leonardi and Nanetti, 1994; Knack and Keefer, 1997; La Porta et al., 1997). CDD is seen as a particularly effective approach in post-conflict countries because, by requiring community members to work cooperatively, CDD is argued to restore social cohesion.[2] The following types of claims are common:

> CDD has proved an effective way of rebuilding communities in post-conflict situations. By restoring trust at a local level and rebuilding social relationships, it has produced valuable peace dividends in places like, Afghanistan, Bosnia-Herzegovina, East Timor, and Rwanda. (IDA, 2009).

We can group CDD-programs into three different types.

Building public infrastructure. This first type of CDD program offers grants for local public projects. The concern, though, is whether the communities will maintain these infrastructure investments once the donor is no longer involved. Therefore these types of CDD programs attempt to increase citizen participation in local governance by establishing village development committees and requiring relatively frequent community-wide participatory meetings

[2]CDD programs also often seek to improve inclusion of marginalized groups, especially women, in community goverance. CDF shares this goal but we will not discuss this aspect of the program in this paper.

for selecting, planning and monitoring the local public infrastructure projects that are built with CDD funding. These efforts are combined with civics training and social mobilization designed to foster local collective action and create a sense of local ownership of the project. The programs studied by Beath, Christia and Eniolopov (2012*a*,*b*, 2013); Casey, Glennerster and Miguel (2012) and Fearon, Humphreys and Weinstein (2009) were of this type , as is the program we evaluate in this paper.

Community monitoring of public services. A second type of CDD program is designed to improve public service delivery (often health and education services) not by creating new infrastructure but by improving citizen monitoring of existing public services, thereby incentivizing better performance from public servants. As in the infrastructure programs, the social capital goal of the program is to increase citizens' capacity for collective action but in this case to monitor and report on public service delivery in existing public facilities so as to incentivize public servants to be less corrupt and more diligent in their duties. The programs studied by Olken (2007); Banerjee et al. (2010) and Bjorkman and Svensson (2009) fall into this category.

Self-help groups. A third type of CDD program is designed to foster the creation of local *self-help groups*, often savings groups. Savings-groups programs provide staff (and occasionally a small amount of seed money) to entice local residents to organize into small groups of friends, neighbors and relatives who make periodic payments into a common fund from which loans are made to members of the group. Once the groups have shown a capacity to remain solvent and function well, they are sometimes combined into village savings and loan associations so that members would make loans to people outside their original savings group. Another type of self-help group is the producers' group, which may share marketing costs or transportation costs to move products to more lucrative markets or to develop joint marketing plans to avoid gluts at harvest time. Whereas the first two types of CDD programs

appear to be designed to foster collective action, self-help groups, especially savings groups, also appear to be concerned with developing the trust and trustworthiness components of social capital.

These three types of projects are quite different in their conception and implementation but all share the goal of increasing villagers' participation in the local governance, encouraging them to take responsibility for the economic development of their villages and creating social capital.

The theory of change underlying these programs has at least three causal pathways. First, by improving local governing institutions the program should make local provision of public goods more efficient. Casey, Glennerster and Miguel (2012) offer a model of this hypothesized effect in which the program lowers costs of building public infrastructure through direct subsidies, lowers costs of participation to marginalized groups, like women, by explicitly including them in the decision-making process and lowers the costs of collective action by increasing the communities' organizing capacity. Second, by requiring more civic participation the program may lead to more social interaction among villagers creating denser social networks that help enforce pro-social norms. Social networks are hypothesized to increase pro-social action by providing informal enforcement mechanisms via a repeated-prisoners'-dilemma interaction where defection from group norms is met with counter-defection by members of the group toward the perpetrator or by "repaying" pro-social action with approval and status in the community (See Jackson (2010), Portes (1998) and the numerous other examples discussed therein). If CDD programs foster denser social networks they may create more pro-social behavior as villagers become bound by their enforcement mechanisms and incentivized by their social-status-granting powers.

Third, through these increased social interactions and via civic education and appeals to villagers' sense of civic virtue, the program may change villagers' primitive preferences for pro-social behavior. In the context of our laboratory activities *a preference for pro-*

social behavior means a desire to contribute one's monetary endowment to another person or persons even though doing so reduces the monetary award to oneself.[3] In understanding these pro-social preferences more deeply the work of Andreoni (1990) is quite helpful. He identifies three categories of such pro-social preferences: pure altruism, warm-glow giving, and impure altruism. *Pure altruism* is the case where the subject gains utility from the utility of the recipient. Pro-social preferences may also be motivated by *warm-glow giving* in which the donor gains utility not from the increased utility of the recipient but from the act of giving itself, the warm glow of having "done the right thing." Finally, the donor could, of course, be motivated by both impulses simultaneously, which Andreoni has termed *impure altruism*. We consider all of these motivations to be pro-social, and so, being mindful of our subjects' time, chose not to implement games that could distinguish between them.

The trust behavior we observe in the lab (described in greater detail below) may spring from a desire to share and as such may be due to pure altruism, warm-glow giving or impure altruism as described above. Trust, however, is also the belief that the average person in a given group will comply with a social norm even when that person has a dominant strategy not to do so. Thus, the trust behavior we observe in the lab may also be motivated by the desire to make as much money for oneself as possible combined with the belief that other members of the community are trustworthy (Ben Ner and Halldorsson, 2010; Glaeser et al.,

[3]We can observe these types of behavior in the donation to the needy, contribution to the public good and trustworthiness, which are discussed in more detail below. These games are similar in that they amount to giving some of one's resources to another person or persons while getting no monetary reward in return. The difference is that in the public goods and trust games there is no stipulation that the recipient is needy and in the case of contributions to public goods there is a society-wide positive externality from the contribution while this may not be the case in the donation to the needy or trust games. Furthermore there may be a greater sense of obligation in the trust game than in the other two games because the size of the trustee's pot is a function of the trust placed in him or her by the sender.

2000).

To summarize, when we hypothesize that the program created more pro-social preferences we mean that, in the donation to the needy, public goods game and amount returned in the trust game, the program may have: (1) increased the subjects' altruism, (2) increased the subjects' warm-glow effect, or (3) both. The program also may have increased subjects' beliefs that other members of the community were trustworthy.

The theory of how the increased community interaction via CDD generates more pro-social preferences is informal. It is based at least in part on a version of the contact hypothesis (Allport, 1954) where people learn that members of the out-group (in this case people from other families in the village, some of whom may have been on the "other side" in the civil war) do not possess the bad traits they had previously attributed to them. Preference change could also occur through a process of self-discovery where persons who are required to interact with others by the CDD program learn that they actually like social interaction more than they previously knew.

In the original Putnam formulation (Putnam, Leonardi and Nanetti, 1994; Boix and Posner, 1998; Putnam, 2000) the components of social capital are created by groups that provide excludable goods (bowling leagues, choral societies) and social capital then spills over into groups that provide non-excludable goods, including, most importantly, civil society in general (Putnam, Leonardi and Nanetti, 1994; Boix and Posner, 1998; Putnam, 2000). The first two types of CDD programs (infrastructure and monitoring programs) do not appear to have adopted the model of creating social clubs in the hope that interactions in those clubs will create more pro-social preferences in the community at large. Instead these programs *begin* by coaxing citizens into greater contributions to public goods through civic education, encouraging participation in public-goods-providing village development committees and monitoring groups and training in providing public goods more efficiently. While such an approach may work, there is nothing in Putnam's original argument that suggests that it should. On the contrary, his argument seems to suggest that social capital begins in exclud-

able social clubs and carries over to public-good-providing civic participation later (perhaps much later) . The third type of program (self-help groups) is closer to the Putnam model. Siuch programs create groups that produce club goods, however even in these programs the benefits of membership are clearly more economic (better access to capital, better prices on produce) than social (making friends by singing or bowling together) and so even self-help groups do not strictly fit the Putnam mechanism.

In summary we identify three possible mechanisms by which CDD programs may affect outcomes. They may reduce the costs of collective action as Casey, Glennerster and Miguel (2012) model or they may create denser social networks or they may increase the benefits of pro-social behavior to individuals by making their preferences more pro-social. Our networks survey and laboratory measurements allow us to zero in on whether these latter two factors are changing. Thus, the theory of change we are testing is whether participation in CDD programming activities and civic education created denser social networks and increased participants' preferences to engage in pro-social action.

Due to the control we were able to exercise in the lab, our subjects' behavior should be unaffected by any increased efficiency or lower costs of collective action in the community. Any difference in the laboratory behavior of members of treated and control communities are, by the construction of our experiments, not attributable to different costs, which are constant across the treated and control communities. Similarly, while our networks survey allows us to test if CDD programs create denser social networks, such networks and the enforcement/incentivizing power they may possess, cannot explain subjects' behavior in the lab, where subjects' actions were unknown to any one else in the community. Thus what we measure in the lab is the subjects' willingness to behave pro-socially even when there were no punishments for not doing so. Our laboratory activities measure subjects' primitive preferences to engage in pro-social action—less prosaically, they measure subjects' civic

virtue.[4]

4 Existing Evidence on Community-Driven Development

A growing empirical literature has arisen to evaluate the impacts of CDD programs on social capital. The central finding has been that the impact of these programs on social capital is mixed at best. Wong (2012) offers an in-depth review of 14 such programs. She concludes that while these programs do improve local public service delivery, they seem to have no impact on local social capital development. Similarly Mansuri and Rao (2013), in their extensive book-length study of participatory development programs, point to the difficulty that these programs have had in overcoming local collective action problems to increase citizen participation.

The most rigorous study to date, (Casey, Glennerster and Miguel, 2012), evaluates a World Bank program in Sierra Leone. It is an important study for at least three reasons. First, like ours it is a randomized control study. Second, the researchers used innovative behavioral measures of social cohesion they called "structured community activities." Third, they archived a pre-analysis plan (PAP) that specified precisely which measures they would use to test their hypotheses, thereby making "cherry picking" of results impossible.[5] They

[4] Voors et al. (2012) also use lab-in-the-field techniques to show that preferences became more pro-social as a result of exposure to civil war violence.

[5] The nature of our involvement in the evaluation of the social impacts of the CDF did not allow us to develop and file a PAP. We were brought in quite late in the life cycle of the program and were charged only with devising and implementing better measures of the social capital (compared to standard survey measures) and using those measures to evaluate the social capital impacts of the program. Therefore while we did not have the opportunity to file a PAP, we are protected against the charge of cherry picking by the strict focus of our study on social capital as measured by our lab-in-the-field activities and networks survey.

find no impact of the project on village decision-making processes, inclusion of women or communities' abilities to raise money for public goods provision.

Fearon, Humphreys and Weinstein (2009) conducted a randomized study of a CDD program in Liberia. They found that the program significantly increased villagers' contributions to a development project, however this result was entirely due to the behavior in one treatment arm—mixed groups of men and women. All-female groups of subjects showed no difference between treated and control communities. Humphreys, de la Sierra and van der Windt (2012) completed an extensive randomized study of a community driven reconstruction project in eastern Congo. They examined many possible impacts of the program, including its potential effect on local governance and social cohesion. They found no noteworthy impact of the program along either of those dimensions. In their measures of quality of local governance which generally had to do with transparency, honesty and inclusivity they found that both the treated and control communities scored quite highly and there was no significant difference between them. Social cohesion, measured by survey responses to hypothetical questions about trust, sharing and other pro-social behaviors, also exhibited no significant differences between the treated and control communities. Labonne and Chase (2011) used difference in difference estimation and propensity score matching to identify the causal effects of the program they studied in the Philippines. Their results were also mixed. The program they evaluated appeared to increase participation in some community activities but it also appeared to reduce it in others, suggesting that perhaps the greater participatory demands of the program may have crowded-out other civic activities (a point we return to later).

We know of no studies that directly assess the impact of the second and third type of CDD program—those that are designed to improve monitoring of public service delivery and self-help groups—on social capital. Several studies of the second type of CDD program have focused on the downstream effects of the program (better health and education, less corruption) and as such do not really provide any direct evidence for the effect of these

programs on social capital.[6]

Taken together these studies indicate that the links in the causal chain between CDD and better local policy-making processes are at least weak if not broken. While more studies are necessary to increase certainty in that conclusion we argue that it is now time to begin to explore which of those links are the likely culprits. We need to begin to evaluate which of the several hypothesized causal mechanisms between CDD and better governance are failing so that we can determine where problems in the programming need to be fixed. For example, when the CDD program in Sierra Leone failed to produce better outcomes as reported by Casey, Glennerster and Miguel (2012) did they do so because the program failed to change preferences or, did the program cause more pro-social preferences among villagers, but the villagers could not translate them into outcomes because they were thwarted by local governing institutions and leaders? Similarly, were the overall positive but in the end mixed effects on villagers' contributions to the development project in Fearon, Humphreys and Weinstein (2009) due to differences in pro-social preferences between the two groups or due to the different abilities of the elites in the villages to file the proper paperwork and pick the right kind of project to elicit contributions?

Some recent studies have addressed one of these causal paths—the effect of more inclusive

[6]Olken (2007); Banerjee et al. (2010) and Bjorkman and Svensson (2009) study the second type of CDD program. Olken (2007) found no effect of increased community mobilization on reducing corruption in Indonesia and Banerjee et al. (2010) show that a program to increase monitoring of schools in India produced no improvements on education outcomes India. Bjorkman and Svensson (2009), by contrast, found a strong effect of a program to improve community monitoring of health facilities on health outcomes in Uganda. Bjorkman and Svensson (2009) credit their positive findings with a greater efforts on their part to avoid elite capture of the program and to disseminate the findings of the participatory monitoring groups to the broader community.

governance institutions. Olken (2010) and Beath, Christia and Eniolopov (2012*a*) studied the effects of the community choosing CDD projects by referendum compared to committees, which are susceptible to elite capture. Both studies found that villagers who were surveyed were more satisfied with the projects that were chosen by direct democracy, although there did not seem to be any difference in the types of projects chosen. Grossman (forthcoming) found that producers' groups run by elected managers performed better than groups run by mangers chosen by elites. He attributes the difference to the superior monitoring institutions and auditing practices adopted by elected managers but not by mangers chosen by elites. These studies indicate that some of the disconnect between CDD programming and better outcomes may be due to elite capture of the project selection process and subsequent monitoring and auditing of the project. Gugerty and Kremer (2008) have similar findings in their study of women's groups in Tanzania. The question remains whether CDD programs can increase participants' preferences for pro-social activity and enhance social networks that should help incentivize that activity.

5 Measurement

Social capital is a concept that raises special measurement difficulties. Often surveys are used in which respondents are effectively asked if they posses social capital ("Do you think people are generally trustworthy?" "Would ould you be willing to contribute to public good X?") Program staff typically stress the importance of social capital in their interactions with members of the treated communities and, of course, they do not operate in the control communities at all. Thus respondents in treated communities may feel more compelled to give the "right" answer to questions than do control-community members, who may not even know what the "right" answer is since they have not received the training component of the program.

Therefore behavioral measures are more appealing, which is why Casey, Glennerster and

Miguel (2012) and Fearon, Humphreys and Weinstein (2009) used them. For the same reason we also rely primarily on behavioral measures but we adopted a different measurement strategy than the two aforementioned studies did. We used behavioral games that permit us to measure these attributes through subjects' behavior in a controlled laboratory setting. Since the games were conducted in the laboratory where subjects interacted with each other anonymously, local governing institutions or informal social punishments could play no part in subjects' decisions. We can isolate the effects of the program on potential changes in subjects' preferences for pro-social behavior. Moving data collecting to the laboratory comes at a cost in terms of external validity, but we argue that the trade-off is worth it, particularly in light of the established results mentioned above.

We implemented adaptations of well-established games designed to measure risk preferences, willingness to share with the needy, trust and trustworthiness, and willingness to contribute to a public good. Games like these were used to measure social capital by Karlan (2005) and Henrich et al. (2004) and in the studies in developing countries as reviewed in Cardenas and Carpenter (2008). We conducted three games to measure subjects' preferences for pro-social behavior: (1) willingness to share with the needy, (2) trust and trustworthiness, (3) willingness to contribute to a collective good, (4) attitudes toward risk, and (5) discount rates.

The games are described in greater detail n the appendix. Here we provide only a short description We measured subjects' willingness to share with the needy with a simple alteration of the standard dictator game. Subjects were given three Sudanese pounds in six half-pound coins and asked to decide how much, if anything, of that amount to donate to an anonymous local needy family. We used the standard trust game (Berg, Dickhaut and McCabe, 1995) to measure trust and trustworthiness. We tripled the amounts sent by the truster to the trustee. We used a dichotomous public goods game similar to the one described in Barrett (2005). This game does not require supervision of the subjects to play. Our measures of the two possible confounders, risk nad time preferences, are described in

the appendix.

Total payouts from all five games were aggregated and made in one lump sum at the end of the session. The average payout was approximately 15 Sudanese pounds (roughly five US dollars), which corresponds to about one day's wage in the rural areas where we worked. We present summary statistics of these measures when we discuss the estimates of program impact in section 7.

Game instructions were given entirely verbally according to a specific script in the local language. Illiteracy rates are very high in rural Sudan, and our respondents found the use of paper and pens very challenging, so we were forced to have the subjects complete the game tasks for four of the five games under the supervision of a facilitator/record keeper. This is a common practice when conducting games in the field in developing countries with illiterate populations (Karlan, 2005; Henrich et al., 2004). Such observation was not required for the public goods game. While we were concerned about Hawthorne effects, having the subjects play under supervision was the only way we could ensure that the subjects understood the decisions that were making.

We also gathered network data from all of our laboratory subjects. We completed a matrix of relationships among the subjects for each of several different categories of social relationships. Table 1 provides an overview of the questions we asked and a summary of their responses.[7] For example, every person was asked whether he or she is a family relation to another subject. We aggregate each person's connectedness to the group we summed the number of relationships each person had in each category. If a person reported a relationship with four people in the group, that person would receive a score of four. We divided these scores by the total number of subjects in the village, which varied across villages due to attrition in subject recruitment. We instructed our enumerators to crosscheck

[7]We exclude networks in an irrigation group because it had a zero mean in both treatment and control communities.

each relationship with the other person in the reported relationship to make sure that both people agreed they were in such a relationship.[8]

We categorize these relationships into five types: (A) *basic social relations* (family, friends, neighbors and worshipers at the same mosque), (B) *favor exchange relationships* where there is some expectation of reciprocity but it is diffuse like babysitting and advice giving, (C) *standard economic relationships* (buying and selling, working with or for another subject), joint membership in community-wide service groups ((D) *voluntary groups*) like producers groups, parent-teacher associations (PTAs) or women's groups, as well as (E) *trust-based exchange relationships* where expectations of reciprocity are quite specific, such as a revolving credit groups and labor exchange relationships.

The data show the large degree of connectedness through basic social relationships but sparse relations otherwise. On average, in this randomly selected group of participants, a subject was family-related to about 22 percent of the other subjects in the village. Neighbor relationships were also quite high with the average subject being a neighbor of about 16 percent to the other subjects in the village. Subjects socialized (i.e., met for dinner, coffee or other social engagements) on average with about 13 percent of the other subjects in the village. Our laboratory subjects attended the same mosque with about 38 percent of the other subjects in this randomly-selected group.

The remainder of the relationships we examined show very little interaction. Of particular interest given our focus on social capital are the voluntary community-wide public service groups. Only four percent of our subjects were in the same producers group. Only three percent attended the same PTA meetings despite the large number of families with children

[8]Despite our instructions in few cases the enumerators did not obtain confirmation from the counterpart for idiosyncratic reasons. In these few cases we counted these relationships as existing even though they had not been corroborated by the counterpart. The results remain robust to other imputation methods.

among our subjects and communities that contained only one school. None of our subjects were in irrigation groups with each other despite the fact that these groups were flagged as important in our focus-group vetting of the questionnaire and the gravity of water problems in Sudan. The lack of civic associations is even more severe than these data indicate–in pilots for this study we included questions about youth groups, sports groups and cultural groups but, finding no participation in such groups, we dropped them from the questionnaire in the interest of saving the respondents' time.

6 Randomization and Sampling

For our sample the six neediest villages (according to CDF scoring) were chosen in each of ten representative localities in the four states. Four of these villages in each locality were randomly chosen to receive the program. The remaining two in each locality served as controls and received no programming. Programming began in 2006. We conducted field work in October and November 2011. As mentioned above we were unable to conduct our study in South Kordofan and Blue Nile due to the re-outbreak of the war and so we were left with twenty-four villages (sixteen treated and eight control) in four localities in North Kordofan and Kassala. The list of communities in which we worked is listed in the appendix (table A.1). The survey team randomly selected twenty-four households in each of these twenty-four communities. The survey gathered measures of pro-social behaviors and of villagers' perception of community cohesion from these 576 households in October of 2011. In each case we interviewed an adult member of the household capable of speaking for the household. At the conclusion of each survey enumeration we invited the survey respondent to participate in the laboratory activities at a later date.[9] On that date we would set-up our

[9]On average tThe lab activities were conducted within two weeks of the survey enumeration (all dates are reported in A.3).

mobile "lab" (which consisted of four stations where our game facilitators would explain the activities and record the subjects' actions) in the specified location in the village.[10] Of the 576 households sampled for the survey 475 sent a representative to the games session. We gathered data on observed behavior in the games mentioned below from these 475 subjects. We gathered information on these 475 subjects' relationships with each other for our social network data. The survey always preceded the games session and the survey respondents were not invited to the game session until the survey was completed.

A baseline survey was conducted before we were brought on the project. Balance statistics for a variety of pre-treatment indicators taken from that survey are presented in Table 2. We report the mean in the untreated communities and the OLS-estimated difference between the treated and the control communities with village-clustered standard errors. In all cases the differences between the treated and control groups are statistically insignificant and, with the possible exception of water consumption substantively very small, thus indicating that excellent balance was achieved.

Our game invitation was extended to the person interviewed in the survey. Often due to work or other commitments the respondent would send another adult member of the household in his or her place. Thus strictly speaking our laboratory respondents are not a random sample but are selected by the household. We have no reason to suspect that households in the treated communities sent more (or less) pro-social members to the laboratory than did households in the control communities and so we do not think this small violation of randomization affects our results. Descriptive statistics of games participants and, where available, survey respondents are provided in Table 3. Our games participants were a bit more likely to be younger, single and female than our survey respondents but not significantly so. The larger percentage of females in the lab sample helps account for the

[10]Usually we set up our lab in a community building such as a school or community center but on a few occassions we had to set up the lab in the open air.

larger percentage of "family workers" and the smaller percentage of "self-employed" in the lab sample than in the survey sample. The economic sectors of our games participants are statistically indistinguishable from those of the survey respondents. Descriptive statistics for "traders" are identical in both samples. There are slightly fewer agriculturalists in the game sample but the difference is small compared to the standard deviation. We included the category "housekeeping" as an economic sector in our survey of games participants but it was not included in the household survey, which, along with the slightly larger percentage of women among games participants, accounts for the slightly smaller number of agriculturalists in that group. The percentage of persons in the housekeeping sector is virtually identical to the percentage who reported being employed as family workers. In tables A.5 and A.6 we provide more information on the demographics of the treated and control villages.

7 Findings

The estimated effect of the program on pro-social behavior in the lab was zero and the effect on network membership may have actually been negative. The survey measures, by contrast, show a strong positive mean effect of the program on self-reported pro-social action and on respondents' characterizations of social cohesion in their communities. In the tables below we present OLS estimates of the mean of the dependent variable in the control community and the average treatment effect on the treated (ATT), that is the increment in the dependent variable in the treated group over or under the control-group mean.[11] We present these results for each individual measure and then combine the estimates of these individual effects into a single mean effect based on z-scores of the estimated treatment effects from each individual measure, the same method used by Kling, Liebman and Katz (2007) and Casey, Glennerster

[11]Due to a high degree of geographic isolation of the villages we are not worried about the control villages being affected by the program through the presence of spillover effects.

and Miguel (2012). In all cases we estimated ordinary least squares with standard errors clustered at the village level.

7.1 Observed Behavior in Games

Estimates of the program effects on observed behavior in our laboratory games are shown in Table 4. The table is split into an upper and lower panel. In the upper panel we report the control-group means and average treatment effects on the treated (ATTs) from each of the four games-based measures of pro-sociality (donation to the needy, contribution to the public good, trust and trustworthiness), and measures of two possible confounders (risk attitudes and the discount rate). In the lower panel we report the mean effect of the program across all four of the social capital measures. There is a very consistent pattern across all four of these measures and their mean effect. In all cases the point estimate of the program is actually negative (showing a slight reduction in the pro-sociality in the treated communities) but the coefficients are very close to and statistically indistinguishable from zero. The following discussion offers more detail about these results.

Column (1) in the upper panel shows the ATT on the amount donated to the needy family. Subjects in both the treated and control communities contributed 1.55 pounds, on average, a little over half of their endowment.[12] The point estimate of the ATT suggests that persons in treated communities actually contributed slightly less than did those in the control communities but the coefficient is very small and statistically indistinguishable from zero. Column (2) shows the ATT of CDF programming on propensity to contribute to public goods in our laboratory game. The results show that on average about 76 percent of subjects

[12]This is a very high give rate compared to standard dictator games. In Engel's meta analysis the give rate was only about 28 percent of the available pot (Engel, 2010). We speculate that the reason for the large give rate we observed was our telling the subjects that the money would be given to a needy family.

in both the treatment and the control villages contributed to the public good. Again, the point estimate of the ATT is negative, very close to zero and far from statistically significant. Columns (3) and (4) in the upper panel show the estimates from the trust game measures.[13] The dependent variable in column (3) is the amount sent by the sender in the first round, which is a measure of generalized trust. On average subjects in both the treatment and control communities sent about 1.4 Sudanese pounds, about 47 percent their endowment.[14] The ATT is, again, negative, very close to zero and not at all statistically significant. The dependent variable in column (4) is the amount returned to the sender by the receiver as a percentage of the total amount available to the receiver, which is our laboratory measure of trustworthiness. On average, subjects in both the treatment and control communities returned about one-third of the amount available to them to their sender.[15] As in the other cases the point estimate ATT is negative, but statistically it is a precisely estimated zero. The lower panel of Table 4 presents the mean effect of the program across all four of the

[13]The number of senders and receivers is unequal because on a few occasions an odd number of subjects arrived for the games due to attrition. Rather than turn away a sure-to-be disappointed subject who had traveled through the dessert, often on foot, to attend our games session, we randomly matched two receivers to one sender in the trust game in these sessions. In those cases receivers received the payoff consistent with their actions and the relevant senders received the payoff decided by the first receiver with whom they were randomly paired.

[14]This amount is close to the average amount sent in the Johnson and Mislin (2011) meta analysis of trust games. The found that subjects sent about 50 percent of the endowment and that African subjects sent significantly less than did subjects from Western countries. Our results are consistent with their findings.

[15]Again these results mirror the general findings reported in the meta analysis of Johnson and Mislin (2011) who calculated that receivers sent about 37 pecent of their total pot back to their sender, and that African subjects tended to return less than subjects from western countries.

aforementioned laboratory measures of social capital. The mean effect is calculated from the average standardized effect (i.e. z-scores of the effect) from each individual measure. As before the point estimate is negative and statistically insignificant.

The final two columns in the upper panel of Table 4 present the ATTs of our measures of risk preferences and the discount rate (i.e., patience). We report estimates of the effect of the program on these variables because they may be important confounders. Persons with higher risk premiums or lower discount rates may appear to be less trusting when in fact their behavior is driven by their attitudes toward risk or their discount rate (or both). Thus, if there were significant differences between the treated and control communities in this regard, we would have cause for concern about confounding. The results in the last two columns of the upper panel in Table 4 show there is no such cause for concern. The attitudes toward risk and the discount rates are statistically identical in the treated and control communities. The average of the villagers' lottery choices is 2.8 in both the treated and the control communities and the average of villagers' time-preference choice was category 4 in both the treated and control communities.

Finally while not reported in teh main text we did check for the possibility that the program may have had impacts on sub-groups such as women or youth. We estimated the effects of the program on these various subsamples using interactive affects. In no case did those estimates indicate that people in the treated communities behaved more pro-socially than did people in the control communities. Thus the program did not cause greater pro-sociality even in subsamples of our subjects. The estimates from these specifications are exhibited in the appendix in Table A.2.

In summary the results using the behavioral measures from games conducted in the laboratory are quite clear in indicating that the program had no impact on individuals' pro-social behavior. The estimates of effect were very close to zero. Skeptics may still raise power concerns, however the fact that all of the point estimates were negative—the opposite of the hypothesized sign—assuages power concerns. Our results are not due to large standard

errors around large point estimates but rather the opposite, small standard errors around small (indeed negative) point estimates. We can be confident that the program truly had no effect on these measures.

7.2 Social and Economic Networks

We now turn to the results on the density of social and economic networks. Did the greater social interaction required by CDF prompt people to forge more social relationships with each other? We present our results in Table 5. The table is split into six sections: basic social relations (A), favor exchange relationships (B), basic economic relationships (C), voluntary groups (D), trust-based relations (E), and a final section that presents the mean effect across all five of these categories (F). In the case of basic social relations the estimated effect of the program is negative—it lead to a reduction in such social relationships. In the case of socializing the effect is actually significant and in the case of mosque attendance the effect is large although insignificant. The mean effect of the program on these basic relationships is also negative and significant.[16] The estimated effect of the program on favor-exchange relationship is close to zero in both cases—one positive, one negative. The mean effect is 13 percent of a standard error which is not statistically significant. This is the only category where the estimated mean effect is even positive. In the remaining three categories (basic economic relations, voluntary groups and trust-based relationships) the mean effects are in all cases negative and not significant. With the exception of advice giving and women's

[16]One might argue that basic social relations like family could not plausibly be affected by a CDD program over five years. We are not so sure. If the program caused improvements in livelihoods it could lead to earlier or (in a polygamous society like Sudan) more marriages and therefore denser family networks. Still, just to be sure we estimated the effects of the program on our measures of social capital and social networks controlling for these basic social relations. Doing so had no substantive impact on the results.

groups where the estimated effect was positive but not significant, the estimated effect of the program on 14 types of social relations was either negative or zero to two decimal places. As shown in the final panel of the table, averaging the effects of the program across all 14 of these types of relations produces a negative mean effect of 0.18 percent of a standard error and this estimate is highly significant statistically.

A clear picture emerges from these various network results: The program did not produce spillover effects for other sorts of social relationships. If anything the effect of the program was to reduce the number of these relationships among villagers. This estimated reduction in the number social relationship is interesting because it is consistent with the findings of Labonne and Chase (2011) who conjectured that that the CDD program they studied may have actually crowded out other social activities that would have naturally occurred. Perhaps the same phenomenon was occurring in this program.

7.3 Survey Measures

Finally we turn to measures of from the household survey. We included questions to get at pro-sociality and community cohesion in the treated and control communities. We present these results in two separate tables. Table 6 offers the results from a series of questions that asked respondents about their own pro-social action over the last three years. Table 7 presents results from a series of questions that asked respondents to characterize the cohesion of their communities.

Table 6 is divided into upper and lower panels. In the upper panel we offer estimates of the effect of the program on responses to each of the questions about the respondents' pro-social action over the last three years. The mean effect of the program calculated from z-scores of the estimates across all 11 indicators is shown in the lower panel (column 12). All of the questions in this table had the form "*In the last three years have you done*: X?" where X is listed at the heading of each of the 11 columns in the upper panel of Table 6. The actions about which the survey asked are: (1) voting in an election, (2) joining a civic association,

(3) contacting an influential person about a problem in the community, (4) contacting the media about a problem in the community, (5) participating in an information campaign about a problem in the community, (6) participating in an election campaign, (7) contacting an elected representative about a problem in the community, (8) discussing problems in the community with others, (9) contacting police or judicial officials about a problem in the community, (10) making a monetary or in-kind donation to a charitable organization, and (11) volunteering for a charitable organization. These are all *yes* or *no* questions that take on a value of one if the respondent did the listed action or zero if they did not. As such the coefficients in the upper panel of Table 6 amount to estimates of a linear probability model. According to these estimates the program produced significant increases in four of the 11 self-reported actions: joining an association, contacting an influential person, contacting the media, and discussing problems in the community with others.

We present the mean effect of the program on these self-reports of pro-social activity across all of these 11 measures in the lower panel of Table 6, column 12. This mean standardized coefficient implies that the program produced an increase of 21 percent of a standard error on average across all 11 indicators and that this effect is highly significant (better than the two percent level for a one-tailed test). Thus the program appears to have caused a significant increase in the respondents' self-reported pro-social action in the last three years. Of course the main concern with these measures is whether those self-reports are biased.

The survey also asked ten questions about the respondents' perceptions of the cohesiveness of their community. The questions presented the respondents with a statement that characterized their community. Most of these statements indicated that their community was socially cohesive. The respondents were then asked if they *agree*, *somewhat agree*, *somewhat disagree* or *disagree* with the statement. The answers were placed on a scale from one to four with *agree* coded one and *disagree* coded four. Thus lower scores indicate greater perceived cohesiveness (with one exception as described below) and if the program had the hypothesized effect we should find negative ATTs. These questions were of two types. The

first type asked how things are in the village now and the second type asked if cohesiveness had improved over the last year. The results for the first type of question are presented in columns 1 through 5 of Table 7 (upper panel A) and the results for the second type of question are shown in columns 6 through 10 (lower panel B).

The wording for each question in columns 1 through 5 is as follows:

Coop. Likely "*Community members, outside your family, are likely to cooperate with each other to solve a community problem like water supply, roads, and security.*"

Coop. Pers. Likely "*Community members, outside your family, are likely to cooperate with each other to solve a private problem like harvest loss, money need.*"

Dir. Ben. "*Community members are likely to participate and contribute for a development project that directly benefits them.*"

Not Dir. Ben. "*Community members are likely to participate/contribute for a development project that does not directly benefit them but benefits majority of the members.*"

Diff. Agree "*It is difficult to get the whole community to agree on any decision.*"

Notice that the statement in last question characterizes the community as non-cohesive and all the other questions characterize it as cohesive, so by hypothesis the sign on the estimate of the program on that question should be positive, the opposite of the other questions. For the statements in columns 1 through 3 respondents in the control communities answered somewhere between *agree* and *somewhat agree* (1 and 2) on average. The estimated mean in the treated communities is, as hypothesized, negative, that is closer to the *agree* and *somewhat agree* responses than it was in the control communities. The ATT is significant in only one of these three cases ("*cooperation on a personal matter is likely*"). For the statement in column 4, which was about a community project that did not "*directly benefit*" the participants agreement was lower in both the treated and control communities. The

average response in the control communities was between the *somewhat disagree* and *disagree* on average. The point estimate of the response in the treated communities was almost precisely at the *somewhat disagree* response and this difference between treated and control communities was statistically significant. There is no discernible difference between treated and control villages in responses to the statement in column 5; both *somewhat disagreed* on average with the statement that "*It is difficult to get the whole community to agree on any decision.*." The mean effect estimated is shown to the right of the estimates from these five indicators (column 6). This mean standardized coefficient implies that the program produced a decrease of 18 percent of a standard error on average across all 5 indicators and that this effect is highly significant (a p-value of about one-half of one percent).

We now turn to the lower panel B of table 7 capturing the perceptions on whether things have improved. The wording for each question in columns 6 through 10 is:

Coop. Dev. "*Cooperation with community members outside family to solve a development problem has improved in the last year.*"

Coop. Pers. "*Cooperation with community members outside of the family to solve personal problems like harvest or money loss has improved in the past year.*"

Dir. Ben. "*Community members are more likely than a year ago to participate and contribute for a development project that directly benefits them.*"

Not Dir. Ben. "*Community members are more likely than a year ago to participate and contribute for a development project that does not directly benefit them.*"

Agree Easier Now "*Getting the whole community to agree on a decision is easier today than a year ago.*"

All of these change related statements characterize the community as becoming more cohesive in the last year, so, given our scaling, with *agree* lower than *disagree*, all coefficients

should be negative if the program caused greater reported cohesiveness. In all cases the coefficients are in the hypothesized direction and highly significant. For the questions in columns 6 though 8 the respondents in the control communities answered somewhere between the *agree* and *somewhat agree* responses, closer to the later, though, than the former. Respondents in the treated communities answered roughly half a degree lower so that their answers were closer to *agree* than *somewhat agree*. For the question in column 9, which concerned improvement in participation in community activities that did not offer direct benefits to the participants, responses in the control communities were between the *somewhat agree* and *somewhat disagree* categories on average while respondents in the treated community answered about one-half a category lower, at the *somewhat agree* category on average. The strongest effect in the table is on responses to the final indicator, whether it is easier to get agreement among the community now than it was a year ago. Responses from the treated communities indicate that the respondents answered this question almost a whole category lower on average than did respondents in the control communities. Respondents in the control communities answered near the *somewhat agree* category while respondents in the treated community answered near the *agree* category. In the panel directly to the right of these five estimates is the mean effect estimated from these five indicators. This mean standardized coefficient in column 12 implies that the program produced a decrease of about 46 percent of a standard error on average across all 5 indicators and that that effect is very highly significant (the p-value that is zero to four decimal places for a one-tailed test).

Clearly a higher percentage of respondents in the treated communities agreed with statements that characterized their communities as cohesive than did respondents in control communities. To an even larger degree respondents in treated communities felt that social cohesion was improving compared to respondents in control communities. The respondents' characterizations of their communities was not on average matched by the subjects behavior in the lab. The divergence between behavior in the lab and responses in the survey raises the question of whether the respondents' characterizations of their communities are based on

actual behavior or on perceptions biased by the civics training provided by program itself.

Our own view is that there is sufficient reason to be skeptical about self-reported behavior in a retrospective survey. Still it must be noted that there is no necessary inconsistency between the results using the behavioral measures and the self-reported survey measures because the laboratory measures should be unaffected by social monitoring and sanctions while the activities reported in the survey are not. Thus it is possible that the respondents in the treated communities actually did participate more in civic life in teh treated communities because they would have been socially punished if they did not, however in the lab, where social sanctions did not exist, subjects from control communities behaved no more pro-socially. Our results would then point to the clear conclusion that, since the program did not create more pro-social preferences, the greater civic engagement by members of treated communities must be due to lower costs of such engagement or higher punishments for failing to participate. Furthermore we know from our networks survey that these lower costs and greater punishments, if they exist, did not change as a result of changes to social networks in the program communities. Thus if the self-reported survey results are to be trusted they must be attributed to changes elsewhere in the communities, presumably to local governing institutions or to the direct mobilization efforts of the program itself. Alternatively one could simply attribute the results from the self-reported survey measures to social desirability bias.

8 Conclusion

Community-driven development programs have become a common means of delivering development aid to poor countries. CDD programs (depending on the type) are designed to build local infrastructure, improve citizen monitoring of government services and encourage self-help among villagers through the creation of savings and producer groups. CDD is based on the premise that it can achieve these benefits by improving local governance, encouraging more civic participation, opening up local policy-making processes to citizens and increasing

social capital. While there is some evidence that CDD programs have been able to achieve local public service delivery, several well-executed recent studies have been unable to find much of a causal impact of CDD programs on villages' capacities for local collective action and public good provision. What has not been clear from these previous studies is whether the disconnect between CDD and improved capacity to provide public goods was occurring at the level of local governing institutions (local leaders were thwarting the greater pro-sociality of citizens), pro-social preferences (local leaders were open to change and greater participation from villagers but villagers free rode and shunned opportunities for more involvement) or both. We study villagers' behavior in a controlled laboratory setting allowing us to isolate the effects that the program had on villagers' pro-social behavior, stripping away any causal impact that local governing institutions could have on results. We also study the effect of the program on forging new social relationships among community members—a key feature of social capital.

Using our most trusted measures, our findings are consistent with earlier studies that have found little or no effect of these programs on social capital. Our estimates of the impact of the program on pro-social preferences were very close to zero using behavioral measures from the lab and the mean estimated effect of the program on the density of social networks was actually negative, suggesting the possibility of some crowding out of naturally occurring social relationships. We measured pro-social behavior in a laboratory setting where the effects of the communities' governing institutions and any possible informal enforcement of social norms have been carefully excluded. We have thereby isolated at least one broken link in the causal pathway between CDD programming and better local governance—the program is not making citizens preferences more pro-social. While our research design does not allow us to comment on whether the CDD program we study had an impact on improving local governing institutions, it is quite clear from our results that it did not have the desired impact on the villages' stocks of social capital as measured by our laboratory activities or the network survey.

In stark contrast to the laboratory results and the networks survey, traditional survey measures of self-reported behavior and beliefs about the community did indicate a significant impact of the program on both the retrospectively self-reported social action and the respondents' characterizations of the pro-sociality of their communities. There was no evidence from the behavioral laboratory measures that villagers in treated communities were more pro-social, but villagers in those communities believe that they are. While we have no smoking-gun evidence that the observed behaviors in the lab are the better measures and the self-reported retrospective behaviors are biased, that was the hypothesis with which we began this study based on concerns in the literature about potential bias in retrospective surveys of self-reported behavior. That hypothesis is certainly supported by our results. If, notwithstanding, one chooses to believe the self-reported survey results, then our laboratory results and networks survey clearly indicate that the greater self-reported civic participation in program communities was not due to an increase in pro-social preferences or to denser social networks and must have been due to other changes in the communities such as local governing institutions or the mobilization efforts of the program itself.

Our laboratory measures of pro-social behavior and the networks survey have allowed us to pinpoint one (although certainly not the only) answer to the question of *how* the CDF program failed to create social capital. The question of *why* the program failed would be speculative, but we have already alluded to one possible reason: There is really nothing in the Putnam model of social capital formation that suggests that a program like this *should* create social capital. The program was very assiduous in dispatching social mobilizers to program villages to lecture them on the importance of social capital, collective action, inclusiveness, citizen participation, social responsibility, trust and trustworthiness. But in the Putnam model social capital is not created by people being told it is important; it is created organically when people associate with each other in enjoyable social interactions. In that model, social capital is not created in civics class but in the pleasant day-to-day activities that people undertake collectively. CDF and, indeed no CDD program of which we

are aware, has taken this fairly obvious feature of the original Putnam argument seriously. It did nothing to foster these types of interactions and indeed, as our network survey suggests, may have even crowded some of them out.

These results will undoubtedly come as a disappointment to those who had hoped for increases in the program communities' stocks of social capital but there are several points to keep in mind. First, CDF had several goals besides increases in social capital and this study has not assessed the impact of the program on those other goals. Second, perhaps five years is too short a time to expect such fundamental changes in people's attitudes. Third, regardless of the impacts of the program on social capital or other outcomes, CDF has already made a contribution to our understanding of development programming by agreeing to a rigorous randomized impact evaluation. The findings from this impact evaluation combined with findings from other programs in other countries will together make development planners smarter so that scarce development funds can be allocated more efficiently. In that way CDF has already had a positive impact on development programming not only in Sudan but also around the world.

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Tables

Table 1: **Summary Statistics of Key Network Variables**

	(1) Relative to Group Size		(2) Absolute		
	mean	*sd*	*mean*	*sd*	*max*
Basic Social Relationships					
Are you family members with ...?	0.22	0.18	4.46	3.53	16
Are you neighbors with ...?	0.16	0.10	3.24	2.08	10
Do you get together socially with ...?	0.13	0.11	2.53	2.10	11
Do you attend the same mosque with ?	0.38	0.38	7.25	7.26	19
Economic Relationships					
Do you buy or sell products or services with ...?	0.09	0.19	1.90	4.00	23
Are you employed at the same farm or shop with ...?	0.01	0.03	0.18	0.66	4
Do you work for ...?	0.00	0.01	0.02	0.16	2
Voluntary Groups					
Are you members of the same producers group with ...?	0.04	0.15	0.70	2.78	15
Do you attend PTA meetings with ... ?	0.03	0.08	0.52	1.42	7
Are you members of the same women's group with ...?	0.03	0.11	0.56	2.32	13
Favor Exchange Relationships					
In the last year have you sought advice about an important personal matter from ... ?	0.06	0.09	1.10	1.95	22
In the last year has ... watched your children for a short period of time?	0.01	0.04	0.28	0.74	4
Trust-based Groups					
Are you members of the same revolving credit group with ...?	0.07	0.18	1.36	3.63	14
Do you exchange labor with ...?	0.09	0.25	1.73	4.54	17
N	477		477		

Table 2: **Pre-Treatment Balance Statistics**

	(1) **Female HH head**	(2) **Married**	(3) **Nr. wives**	(4) **Perm. job**
Difference in treated	0.00	-0.02	0.03	0.02
	(0.03)	(0.02)	(0.10)	(0.05)
Control mean	0.10	0.93	1.40	0.19
	(0.02)	(0.01)	(0.07)	(0.04)
N	576	576	576	576
	(5) **Farmer**	(6) **Herder**	(7) **Trader**	(8) **Sufficient income**
Difference in treated	-0.13	-0.01	-0.01	0.00
	(0.13)	(0.07)	(0.06)	(0.05)
Control mean	0.64	0.11	0.14	0.18
	(0.11)	(0.06)	(0.04)	(0.04)
N	115	115	115	576
	(9) **Comm. soc.**[1]	(10) **Personal soc.**[2]	(11) **Disagreement**[3]	(12) **Water consump.**
Difference in treated	-0.34	-0.08	-0.13	12.22
	(0.36)	(0.15)	(0.45)	(16.17)
Control mean	7.02	4.98	5.30	24.71
	(0.28)	(0.10)	(0.39)	(9.04)
N	575	575	576	528

Notes: Standard errors in parentheses. Village clustered standard errors. (1) Mokken scale of responses to questions about participation community decision making; (2) Mokken scale of responses to questions about household's participation in community; (3) Mokken scale of responses to questions about difficulty of getting community agreement.

Table 3: **Game Participant and Survey Respondent Background Variables**

	(1) **Games Participants**		(2) **Survey Participants**	
	mean	*sd*	*mean*	*sd*
Sex	0.55	0.50	0.69	0.46
Age	40.38	15.55	45.32	14.80
Single (never married)	0.10	0.30	0.05	0.22
Married monogamously	0.73	0.44	0.76	0.43
Married polygamously	0.12	0.33	0.07	0.25
Divorced/ separated	0.02	0.15	0.03	0.17
Widowed	0.02	0.15	0.09	0.28
Number of people in household*	7.55	3.69	6.04	2.83
No basic education**	0.60	0.49	0.66	0.47
Self-employed	0.54	0.50	0.88	0.32
Family worker	0.31	0.46	0.03	0.16
Employee	0.04	0.21	0.09	0.29
Agriculture	0.45	0.50	0.62	0.49
Commerce, trading	0.12	0.33	0.12	0.33
Housekeeping***	0.32	0.47		
Other economic sector	0.05	0.22	0.13	0.34
Party-member***	0.29	0.45		
Distance to game venue on foot (in min.)***	14.29	15.36		
N	475		576	

Notes: *Self-reported by games subjects and actually counted in household survey. **Games subjects who reported zero years of education and household survey respondents who were illiterate. *** Information not collected in the survey. Survey respondents are same people who also responded to the social capital questions.

Table 4: **Observed Game Behavior: Social Capital**

	(1) **Donation to Needy**	(2) **Public Goods**	(3) **Trust**	(4) **Trust-worthiness**	(5) **Risk Choice**	(6) **Patience**
ATT	-0.10	-0.05	-0.03	-0.02	-0.02	0.01
	(0.07)	(0.07)	(0.10)	(0.03)	(0.19)	(0.39)
Control Mean	1.55***	0.76***	1.43***	0.34***	2.82***	4.04***
	(0.04)	(0.05)	(0.08)	(0.03)	(0.14)	(0.30)
N	474	475	235	240	475	474
Mean effect			-0.12			
			(0.08)			

Notes: Standard errors in parentheses. Village clustered standard errors. * $p < 0.1$** $p < 0.05$, *** $p < 0.01$.

Table 5: **Effects of Treatment on Networks**

	(A) Basic Social Relations				(B) Favor Exchange Relations	
	(1)	(2)	(3)	(4)	(5)	(6)
	Family	**Neighbors**	**Socialize**	**Mosque**	**Advice**	**Babysat**
ATT	-0.14	-0.05	-0.06***	-0.23	0.02	-0.01
	(0.09)	(0.03)	(0.02)	(0.15)	(0.02)	(0.01)
Control mean	0.32***	0.20***	0.17***	0.55***	0.04**	0.02
	(0.08)	(0.03)	(0.02)	0.3	(0.02)	(0.01)
N	476	477	477	477	477	476
Mean Effect		-0.55***			0.13	
		(0.18)			(0.22)	

	(C) Basic Econ. Relations			(D) Voluntary Groups		
	(7)	(8)	(9)	(10)	(11)	(12)
	Buy/Sell	**Coworkers**	**Employed by**	**Producers'**	**PTA**	**Women's**
ATT	-0.02	-0.00	0.00	-0.10	-0.03	0.02
	(0.03)	(0.01)	(0.00)	(0.09)	(0.02)	(0.03)
Control mean	0.10***	0.01**	0.00	0.11	0.04**	0.01
	(0.02)	(0.01)	(0.00)	(0.09)	(0.02)	(0.01)
N	477	476	476	476	476	476
Mean Effect		-0.05			-0.05	
		(0.10)			(0.19)	

	(E) Trust-based Relations		(F) All Relations
	(13)	(14)	
	Revolving Credit	**Labor Exchange**	
ATT	0.00	-0.07	
	(0.06)	(0.13)	
Control Mean	0.07	0.15	
	(0.05)	(0.11)	
N	476	476	
Mean Effect	-0.11		-0.18***
	(0.23)		(0.00)

Notes: Standard errors in parentheses. Village clustered standard errors. * $p < 0.1$** $p < 0.05$, *** $p < 0.01$.

Table 6: **Survey Responses: Self-Reported Behavior**

	(1) **Voted**	(2) **Assn.**	(3) **Contact Infl.**	(4) **Media**	(5) **Info Camp.**	(6) **Elect. Camp.**
ATT	0.03	0.08*	0.06***	0.07***	0.06	0.04
	(0.06)	(0.05)	(0.02)	(0.02)	(0.04)	(0.06)
Control Mean	0.80***	0.15***	0.06***	0.03**	0.10***	0.17***
	(0.05)	(0.03)	(0.01)	(0.01)	(0.03)	(0.05)
N	575	561	567	566	570	570
	(7) **Contact Rep.**	(8) **Discuss**	(9) **Police**	(10) **Donate**	(11) **Volunteer**	(12) **Mean Effect**
ATT	0.06	0.09*	0.03	0.09	0.05	0.21**
	(0.04)	(0.05)	(0.02)	(0.08)	(0.04)	(0.10)
Control Mean	0.11***	0.13***	0.03***	0.24***	0.08**	
	(0.03)	(0.03)	(0.01)	(0.05)	(0.04)	
N	568	572	536	566	546	576

Notes: Standard errors in parentheses. Village clustered standard errors. * $p < 0.1$** $p < 0.05$, *** $p < 0.01$.

Table 7: **Survey Responses: Perceptions of Community Cohesion**

	(A) **How are things now?**					
	(1) **Coop. Likely**	(2) **Coop. Pers. Likely**	(3) **Dir. Ben.**	(4) **Not Dir. Ben.**	(5) **Diff. Agree**	(6) **Mean Effect**
ATT	-0.16	-0.18*	-0.26	-0.34**	0.01	-0.18**
	(0.10)	(0.10)	(0.18)	(0.16)	(0.18)	(0.07)
Control M.	1.37***	1.56***	1.51***	2.34***	3.01***	
	(0.09)	(0.09)	(0.16)	(0.12)	(0.14)	
N	563	556	565	521	576	
	(B) **Have things improved?**					
	(7) **Coop. Dev.**	(8) **Coop. Pers.**	(9) **Dir. Ben.**	(10) **Not Dir. Ben.**	(11) **Agree Easier Now**	(12) **Mean Effect**
ATT	-0.48***	-0.48**	-0.45**	-0.45***	-0.91***	-0.46***
	(0.14)	(0.18)	(0.18)	(0.13)	(0.26)	(0.11)
Control M.	1.73***	1.84***	1.74***	2.36***	2.28***	
	(0.13)	(0.17)	(0.18)	(0.09)	(0.25)	
N	532	558	549	535	513	

Notes: Standard errors in parentheses. Village clustered standard errors. * $p < 0.1$** $p < 0.05$, *** $p < 0.01$.

Table A.1: **List of Communities**

(1) **Locality**	(2) **Name**	(3) **Type**
State: **Kassala**		
Aroma	Al Azargawe	Control
Aroma	Amadam	Control
Aroma	Al Sasraib	Treated
Aroma	Al Sidaira	Treated
Aroma	Tamantty	Treated
Aroma	Ariyab	Treated
Seteit	Magareef	Control
Seteit	Al Sewail	Control
Seteit	Taboseib	Treated
Seteit	Al Amara K	Treated
Seteit	Arab 26	Treated
Seteit	Al Rimailla	Treated
State: **North Kordofan**		
Gubeish	Dira	Control
Gubeish	Sibiel	Control
Gubeish	Um Zameel	Treated
Gubeish	Al Shohait	Treated
Gubeish	Al Sabagh	Treated
Gubeish	Abo Raie	Treated
Um Ruaba	Abar Shawal	Control
Um Ruaba	Umm Daiwan	Control
Um Ruaba	Umm Sayala	Treated
Um Ruaba	Al Beraissa	Treated
Um Ruaba	Haggam	Treated
Um Ruaba	Umm Tilaih	Treated

Table A.2: **Heterogeneous Effects of Treatment on Social Capital (Mean Effect from Behavioral Outcomes)**

	(1) **Male**	(2) **Age**	(3) **Married**	(4) **Education in years**	(5) **People in household**	(6) **Party membership**	(7) **In Kassala**
ATT	-0.21**	0.16	-0.40***	-0.16	0.09	-0.19**	-0.18***
	(0.09)	(0.19)	(0.12)	(0.10)	(0.15)	(0.09)	(0.05)
Sex	0.15						
	(0.10)						
Sex x treatment	0.17						
	(0.13)						
Age		0.01**					
		(0.00)					
Age x treatment		-0.01*					
		(0.00)					
Married			-0.22**				
			(0.10)				
Married x treatment			0.33**				
			(0.14)				
Education				0.00			
				(0.02)			
Education x treatment				0.02			
				(0.02)			
In HH					0.03***		
					(0.01)		
In HH x treatment					-0.03*		
					(0.01)		
Party-member						-0.01	
						(0.10)	
Party-member x treatment						0.24	
						(0.15)	
Kassala							-0.05
							(0.11)
Kassala x treatment							0.13
							(0.16)
Control Mean	-0.08	-0.23	0.20**	-0.00	-0.21**	0.00	0.03
	(0.07)	(0.15)	(0.08)	(0.08)	(0.09)	(0.07)	(0.03)
N	470	470	475	470	470	469	475

Notes: Standard errors in parentheses. Village clustered standard errors. * $p < 0.1$** $p < 0.05$, *** $p < 0.01$. Separate regressions including treatment dummy, variable of interst, and interaction with treatment.

Table A.3: **Timing of Interviews and Games**

(1) **Community**	(2) **Date of Interview** (at community level)	(3) **Date of Games**	(4) **Difference** (in days)
Al Azargawe	25.10	27.10	2
Amadam	24.10	28.10	4
Al Sasraib	26.10	29.10	3
Al Sidaira	24.10	27.10	3
Tamantty	16.10	24.10	8
Ariyab	20.10	28.10	8
Magareef	18.10	22.10	4
Al Sewail	23.10	27.10	4
Newseib	20.10	22.10	2
Al Amara K	21.10	26.10	5
Arab 26	25.10	27.10	2
Al Rimailla	16.10	25.10	9
Dira	20.10	16.11	27
Sibiel	25.10	15.11	21
Umm Zarafat	17.10	16.11	30
Al Shohait	21.10	19.11	29
Al Sabagh	23.10	17.11	25
Abo Raie	24.10	17.11	24
Abar Shawal	25.10	1.11	7
Umm Daiwan	24.10	1.11	8
Umm Sayala	19.10	2.11	14
Al Beraissa	21.10	2.11	12
Haggam	22.10	2.11	11
Umm Tilaih	17.10	1.11	15
Mean			11.54

Table A.4: Additional Information on the CDF Project

	Mean	*SD*	**N**
Do you know where a CDF Project in your community was constructed?	0.989	(0.102)	380
How far away (in kilometers) is the CDF Project from your house?	0.989	(2.207)	360
Who is the CDF Project primarily intended to be used by? (MA)			
The entire community	0.914	(0.281)	384
Those who live closest to it	0.331	(0.471)	384
Poorer community members	0.143	(0.351)	384
New migrants	0.089	(0.284)	384
The elderly	0.010	(0.102)	384
Community leaders and their families	0.096	(0.295)	384
Other	0.036	(0.188)	384
How often do you use the CDF Project?			
(1) Frequently	0.920	(0.271)	364
Whose idea do you think it was to construct the CDF Project (incl. location)?			
People in the community	0.492	(0.501)	368
Community leaders	0.332	(0.471)	368
Local government	0.057	(0.232)	368
National government	0.005	(0.074)	368
An organized group/party [SPLM]	0.054	(0.227)	368
An international organization	0.016	(0.127)	368
Other	0.043	(0.204)	368
In general, whose responsibility is it to construct this type of CDF Project? (MA)			
People in the community	0.492	(0.501)	384
Community leaders	0.297	(0.457)	384
Richer members of the community	0.052	(0.222)	384
Local government	0.073	(0.260)	384
State government	0.599	(0.491)	384
National government	0.276	(0.448)	384
An organized group/party [SPLM]	0.005	(0.072)	384
An international organization	0.055	(0.228)	384
Other	0.008	(0.088)	384
Overall, how satisfied are you with the CDF Project?			
(1) Very satisfied	0.852	(0.355)	366

Source: Household Questionnaire (2012). Questions only asked in treatment communities. "MA" refers to "multiple answers allowed" .

Table A.5: Household Demographics

	Control	*N*	**Treated**	*N*	**Diff.**/*SE*
Numner of household members	7.08	1134	7.47	2338	-0.39*** (0.118)
Sex	0.52	1134	0.51	2338	0.00 (0.018)
Age	21.73	1130	21.73	2332	-0.01 (0.671)
Marital status:					
Single and never married	0.47	704	0.46	1402	0.02 (0.023)
Married monogamously	0.43	704	0.47	1402	-0.04 (0.023)
Married polygamously	0.02	704	0.02	1402	-0.00 (0.007)
Divorced	0.02	704	0.01	1402	0.00 (0.006)
Separated	0.00	704	0.00	1402	0.00 (0.002)
Widowed	0.05	704	0.03	1402	0.02* (0.009)
Within the last six months did (NAME) live in the same place as now?					
Yes.	0.96	1129	0.96	2323	0.00 (0.007)
Please specify the reason why (NAME) has changed location:					
Change of marital status	0.00	43	0.05	102	-0.05 (0.033)
Disease	0.09	43	0.16	102	-0.06 (0.063)
Work	0.58	43	0.45	102	0.13 (0.091)
Education	0.30	43	0.23	102	0.08 (0.079)
Security	0.00	43	0.01	102	-0.01 (0.015)
Other	0.02	43	0.11	102	-0.08 (0.050)
Please specify the geographic location to which (NAME) left:					
Same village	0.00	48	0.01	102	-0.01 (0.014)
Other village in the same admin unit	0.02	48	0.04	102	-0.02 (0.032)
Other admin. unit in the same locality	0.23	48	0.13	102	0.10 (0.064)
Other locality in the same state	0.33	48	0.20	102	0.14 (0.074)
Other state within sudan	0.38	48	0.63	102	-0.25** (0.085)
From Sudan to South Sudan	0.04	48	0.00	102	0.04* (0.020)
Has (NAME) ever attended school?					
Yes has attended school.	0.28	943	0.30	1867	-0.02 (0.018)
Yes, is currently attending school.	0.24	943	0.32	1867	-0.07*** (0.018)
No.	0.47	943	0.39	1867	0.09*** (0.020)
If no, what is/ was the reason for not attending school? (Selected main answers.)					
School not present	0.49	435	0.41	701	0.07* (0.030)
Too expensive	0.02	444	0.05	723	-0.04** (0.012)
Help at home/ farm work/ family business	0.10	435	0.12	701	-0.03 (0.019)
Forbidden by parents	0.20	435	0.20	701	0.01 (0.024)
For how many days in the last 7 days did (NAME) do this work?	6.39	371	6.30	667	0.09 (0.072)
Was (NAME) available for work during the past 7 days? Yes.	0.47	737	0.41	1411	0.05* (0.022)
Did (NAME) look for work during the last 7 days? Yes.	0.04	388	0.04	867	0.00 (0.012)
Why was (NAME) not available/did (NAME) not look for work during the past 7 days? (Selected main answers.)					
Student	0.48	393	0.49	873	-0.00 (0.030)
Housewife	0.28	393	0.30	873	-0.02 (0.028)
Is the illness related to the war (e.g., wounded, trauma)?	0.29	7	0.33	18	-0.05 (0.216)
Observations	3472				

Source: Household Questionnaire (2012). Information was collected on all people who have lived (slept and eaten) in a household in the last six months and all people who have left the household within this period of time.

Table A.6: Community Demographics

	Control	*N*	**Treated**	*N*	**Diff.**/*SE*	***p* value**
Households (HHs)	364	8	559	16	-194 (141)	0.182
Female-headed HHs	40.12	8	79.88	16	-39.75 (27.228)	0.158
HHs migrated out in the past year	9.57	7	3.79	14	5.79 (6.011)	0.348
HHs migrated in past year	0.75	8	12.00	15	-11.25 (7.786)	0.163
Former combatants	9.33	6	4.69	13	4.64 (5.620)	0.420
Male war-crippled victims	10.71	7	5.56	16	5.15 (7.837)	0.518
Female war-crippled victims	5.00	6	0.00	12	5.00 (3.423)	0.163
Boys without parents	22.14	7	39.64	14	-17.50 (26.015)	0.509
Girls without parents	23.71	7	36.57	14	-12.86 (20.096)	0.530
Overall migrated in (last 3 yrs.)	3.83	6	14.47	15	-10.63 (10.512)	0.324
Overall migrated out (last 3 yrs.)	12.67	6	8.27	15	4.40 (8.381)	0.606
Overall migrated in (last 3 m.)	0.83	6	0.13	15	0.70 (0.451)	0.137
Overall migrated in (last 3 m.)	166.83	6	133.33	15	33.50 (236.859)	0.889
IDPs migrated in (last 3 yrs.)	1.40	5	3.13	15	-1.73 (4.778)	0.721
IDPs migrated out (last 3 yrs.)	0.40	5	0.33	15	0.07 (0.627)	0.916
Observations	24					

Source: Community Questionnaire (2012).