# Democratic Institutions and Collective Action Capacity: Results from a Field Experiment in Post-Conflict Liberia

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## Abstract

In the aftermath of Liberia's civil war, international development organizations introduced development aid projects in heavily affected areas with the goal of strengthening the ability of communities to solve collective action problems. The targeted communities were randomly selected by public lottery, providing a rare opportunity to assess the effects of external interventions on social organization. Five months after the intervention was completed, we used a community-wide public good game as a behavioral measure of a community's collective action capacity. Surprisingly—given the common view that local social and political institutions are highly robust and resistant to third-party efforts to change them—we find that treatment led to communities performing significantly better in the public goods game. We use evidence from surveys of the game players to try to understand the mechanisms by which the intervention affected contributions. Although our conclusions here are more tentative, the evidence suggests that the treatment resulted in greater mobilization and information-sharing efforts which may have produced more coordination around socially desirable outcomes. However, we also find that the effects of the intervention can only be seen when the public goods game is played by mixed-gender groups; when the collective action problem is presented to women-only groups there are high levels of cooperation independent of treatment status.

# 1    Introduction

Communities often confront the difficult task of inducing individuals to act in ways that may run counter to self-interest. The ability of political units to generate and sustain cooperation is part of what distinguishes successful states from those that fail, communities with high-levels of service provision from those that lack essential services, societies with well-functioning democratic institutions from those that underperform, and political groups that achieve power and influence from those that find themselves stuck on the sidelines. Importantly, prior research suggests that cooperative behavior is a relatively fixed characteristic of a political unit – reflective of demographic, economic, and political factors that are difficult to change – and thus is unlikely to respond to short-term interventions.

Given the centrality of a community's collective action capacity to a wide variety of consequential outcomes, aid agencies have increasingly embraced a programming model known as "Community Driven Development" (CDD), or "Community Driven Reconstruction" (CDR) when applied in post-conflict settings. In CDD programs, the delivery of development funds is combined with efforts to build local governance capabilities, typically by having project staff organize elections to Community Development Councils (CDCs) that select specific projects in a public process, and then help to implement and maintain them. Donor agencies believe that greater community involvement will make projects more successful in terms of their impact on material welfare. But they often often focus more on the idea that the institution-building component of CDD will improve local governance and the capacity of communities for collective action to provide and manage public goods. In particular, CDD projects are designed to promote more democratic institutions and practices in communities where, donors presume, traditional political structures are more autocratic and the possibilities for cooperation more limited.

In this paper we present the results of a field experiment in which a CDR program was implemented by the International Rescue Committee in two districts of northern Liberia, roughly from November 2006 to March 2008. Forty-two of 83 communities were randomly selected in the Fall of 2006 to participate in the program. Outcomes were measured in two main ways: by baseline and follow-up household surveys that asked about material well-being and attitudes regarding community governance, and by a "real life" public good game conducted in all 83 communities in the summer of 2008. In the public good game, randomly selected adults from each community were given 300 Liberian dollars (about $5) and asked to make a private decision about how much to keep and how much to contribute to a community fund for a new development project. While data from the surveys can be suggestive of changing attitudes, the public goods game allows us to assess whether the CDR program affected actual behavior

in response to a community-wide collective action problem. We exploit the CDR intervention in an effort to assess whether patterns of social cooperation are actually responsive to the introduction of new governance institutions, even when underlying demographic, economic, and political factors remain unchanged.

Relative to control communities, we observe several significant changes in the collective action capacity of treatment communities. Across a variety of survey-based measures, individuals in treatment communities report an increased sense of political and social efficacy, greater faith in local leaders and the value of democratic processes, and higher levels of community activity. Using our behavioral measures, we find that communities that received the CDR program contributed significantly more in the public goods game, collecting on average 82.1% of the total possible payout versus 75.8% in control communities. At the individual level, the average impact of exposure to the CDR program is approximately equivalent to the average impact of increasing the rate of return on a contribution from 100% to 400%. Forty two communities that were randomly assigned to have 12 men and 12 women playing the game rather than all women – a cross-cutting "gender composition" treatment discussed below. The CDR impact, we find, is concentrated entirely in these mixed group, for these, CDR raised average contributions from 67 to 82% of the total possible. Given the short duration of the treatment and the common belief that local social and political institutions are highly robust and resistant to third-party efforts to change them, this positive impact is surprising.[1] In addition, it is worth highlighting that, contrary to common beliefs about poor, post-conflict countries, we find evidence of high levels of social capital and behavioral evidence of a strong capacity for organizing and providing public goods in almost all of the sample communities, both treatment and control. For example, the average community received about 79% of the total possible payout in the public goods game, far higher than one might have anticipated given the rationale for donor-funded governance interventions.

We also use evidence from surveys of the game players to explore the mechanisms by which the CDR program increased community contributions in the public goods game. Although our findings here are more tentative, the evidence suggests that local leaders in CDR-treated communities engaged in greater mobilization and information-sharing efforts in the week between the initial community meeting that explained the game, and the play of the game itself. The main effect of the CDR program may have been to increase the number of community members with leadership and organizing experience relevant to solving a similar community-wide collective action problem. There is also some evidence that CDR-treated communities

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[1] Our prior expectation was that we would find no difference in aggregate contributions across treatment and control groups, but we thought we might find differences in the process by which communities mobilized and organized to play the game. These differences are discussed in section 6.

used more democratic processes to select community representatives and projects.

Subject to the usual (and significant) caveats concerning external validity, the results of this study speak to several larger debates and questions. First, at the level of policy, although the World Bank alone currently spends about $2 billion per year on CDD and CDR programs,[2] to date there have been almost no experimental assessments of the effectiveness of this development approach.[3] Our design does not afford a direct comparison between CDR and a more traditional, "top down" model of delivering aid, since our control communities received no CDR project, rather than a differently designed intervention. But it is still encouraging that we find evidence of both attitudinal and behavioral changes in the directions intended by advocates of community-driven development. Whether the positive changes are worth the cost of the project is harder to assess given that the objective is improved governance and stronger institutions as opposed to improvement of some specific service.

Second, our research design provides a rare opportunity to use random assignment to try to identify the causal impact of political institutions on public good provision and cooperation in a community. Many economists and political scientists believe that "good institutions" and "good governance" are critical determinants of economic growth, but estimating institutional (or policy) effects empirically is difficult because economies and political institutions normally co-evolve. The most compelling attempts try to use variation in institutions that is traceable to a historical accident or contingency very long ago, and thus cannot itself be caused by other more recent determinants of economic growth (Acemoglu, Johnson, and Robinson, 2001). These efforts are subject to the usual concerns about instruments, such as whether there are other paths by which presumed determinants of institutional variation centuries ago might be correlated with outcomes today, and whether institutional variation due to the historical accident has the same causal impact as variation due to policy interventions or other more proximate causes.

The focus on very long-run persistence in institutions also raises the question of whether anything can be done about institutions even if they matter. Since the end of the Cold War, Western-led political and military interventions in a long list of countries have attempted to transfer "good institutions" that would favor democracy, political stability, and growth. Foreign aid for economic development has also stressed efforts to change policies and institutions, both locally and nationally. For good reasons, skepticism about the prospects for success is common.

Random assignment of a CDR program involving elections gives us an opportunity to more

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[2]World Bank CDD website ( `http://go.worldbank.org/3RB76M9CU0`); consulted August 24, 2011.
[3]The main exceptions are Beath, Christia, and Enikolopov (2009) and Casey, Glennerster, and Miguel (2011); see below.

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cleanly estimate the impact of the introduction of a new, formally democratic institution at a local level. While we do not know if the effects we observed will persist, the positive short-run impact on contributions to a public good that we find is striking in light of the common view that local political and social institutions are robust and imperturbable. The simple mechanism of experience organizing to decide on and implement a public good in the CDR project helping "elites" do the same when they were later faced with a similar problem could plausibly work in other efforts to build more democratic local institutions.

The most closely related work is the randomized evaluation of a similar CDR program in Sierra Leone by Casey, Glennerster, and Miguel (2011). Their results are practically the reverse of ours. They find a significant positive impact of the program on measures of material welfare, but no effect on community decision-making processes or collective action capacity. Related work in Indonesia by Barron, Humphreys, Paler, and Weinstein (2009) using a regression discontinuity approach also finds effects on economic productivity but little impact on social outcomes. A partial explanation for the different findings may be that both the Sierra Leone and Indonesian CDR programs resulted in many projects aimed at economic production. In Sierra Leone 57% were focused on agriculture, livestock, fishing, or small business development; in Indonesia almost all funding was distributed as private cash transfers. By contrast, income-generating projects were disallowed in the Liberia CDR program that we studied.

Other studies have looked at the effects of varying institutional design and decision-making procedures within a CDD or CDR program. Beath, Christia, and Enikolopov (2009) randomly assigned different procedures for electing CDCs and choosing projects in a large-scale CDR effort in Afghanistan. They find that womens' preferences are not represented under any of their procedural configurations, and that elite capture is more likely when elections are "at large" and projects are selected by a consultation process rather than by secret ballot referenda. Similarly, Olken (2009) randomly assigns choice of projects in an Indonesian CDD initiative to plebiscites or representative-based meetings. He finds little impact on the projects chosen, but much greater satisfaction with them and reported willingness to contribute in the communities that chose by plebiscite. These two studies suggest that direct democracy may have some advantages as a method of project selection. Though the comparison is not exact and our measures on this dimension are relatively weak, we find no evidence that CDR associated with greater satisfaction with project selection in our community public goods game. We do find, however, some evidence of a positive effect on use of democratic procedures.

Another experimental literature asks whether providing information to constituents of various public services improves political accountability and service provision, typically finding that it does (Björkman and Svensson, 2009; Chong, De La O, Karlan, and Wantchekon, 2010;

Ferraz and Finan, 2008; Pande, 2011, for a review). The focus of CDR and CDD programs is on building new local-level institutions, although in the process information is conveyed about how democracy is supposed to work. We find some indication that the CDR program studied here increased expressed trust in community leaders, although this does not appear to be the pathway by which CDR increased community contributions in the public goods game.

In section 2, we provide background on the setting of the CDR program in Liberia, and then describe the intervention, the experimental assignment, and the general effects of the program. Section 3 describes in detail the community-wide public goods game implemented in all 83 treatment and control communities six to seven months after the completion of the CDR intervention. It also presents the main effects of exposure to CDR, and for two additional experimental manipulations we introduced into the game concerning the gender composition of the game players and the interest rate individual players faced on their contribution.

Section 4 turns to the problem of discerning the mechanism(s) by which the CDR program increased collective action capacity, beginning with a model of the decision problem faced by game players and by community elites considering how much to mobilize in preparation for play of the game. The model yields a set of hypotheses about how exposure to the CDR intervention might have had an effect. Sections 5 and 6 consider these hypotheses using our evidence from surveys of the game players, community representatives, and chiefs. The conclusion spells out implications for researchers and policy makers.

## 2    A CDR Program in Northern Liberia

Brutal and inept rule by Sergeant Samuel K. Doe in the 1980s had, by the end of the decade, attracted a variety of armed challengers for power in Liberia, the most successful of whom was Charles Taylor. Taylor's National Patriotic Front of Liberia (NPFL) fought the government, a rival rebel group (ULIMO), and international intervention forces (ECOMOG) until a peace agreement brought him to power by elections in 1996. Something of a peace then prevailed until new rebel groups, and chiefly the Liberians United for Reconciliation and Democracy (LURD; in some ways a successor to ULIMO), began trying to unseat Taylor in 1999. With the help of international pressure, they succeeded in 2003. A major United Nations peacekeeping operation (UNMIL) and election of Ellen Sirleaf Johnson as president, followed. UNMIL is still in the country though its numbers are much diminished. In 2006, the United Kingdom's Department for International Development (DFID) funded a $1.6 million project by the International Rescue Committee (IRC) for a CDR project in two districts of northern Liberia, Voinjama and Zorzor, which border on Guinea.

## 2.1 The program

The IRC project sought to support CDR programs in 42 "communities," where "communities" were to be constructed as groupings of a relatively large "hub village" (which in practice ranged from roughly 30 to 600 households) and smaller neighboring "satellite" villages (usually small clusters of households).[4] The authors participated with the IRC in the identification of a set of 83 potential communities, and in designing public lotteries held in September 2006 to select 42 to receive an IRC CDR program.

Voinjama and Zorzor districts were hard hit by the war, due in large part to several episodes when control of the area shifted between Taylor's NPFL and the rebel groups ULIMO and LURD. People of the majority ethnic group in the two districts, the Loma, mainly supported Taylor and the NPFL; members of the largest minority, the Mandingo mainly supported, or at least were identified with, ULIMO and LURD. Based on our household surveys, Voinjama district is about 59% Loma and 30% Mandingo; in Zorzor the proportions are 92% and 4%. Most communities, however, are relatively ethnically homogenous; for example, about 50 of the 83 communities are 90% or more from one group, and in only 16 is there a minority population of at least 15%.

Our baseline data record information on almost 6,000 household members living in the region in 1989. Of these, over 4% are reported to have died directly from war related violence and a further 6% suffered injury or maiming. 5% took active part in the fighting, with three fifths of these reporting that they were abducted. A similar share (4.9%) of approximately 1500 subjects we interviewed in our follow-up survey self-reported as ex-combatants. The most widespread impact, however, was one that could bear on communities' ability to cooperate (Richards, 2005): 85% of these individuals were displaced during the conflict and many were displaced multiple times, often to refugee camps in Guinea.

The IRC's CDR program adapts for a post-conflict context the Community Driven Development model that is now widely supported by the World Bank and other donors for aid programs aimed at poverty reduction. The goals are to "improve material welfare, build institutions and promote community cohesion ... [and to facilitate] the creation of sustainable community (governance) structures and communities participating within those structures through a system responsive to community rights and needs – paying particular attention to

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[4]In the region these settlements are generally called towns, not villages. The size and number of the CDR communities was determined in part by funder requirements on the number of people to be served by the project, in part by the logistical capacity of IRC in northern Liberia, and in part by distribution of villages and people in these two districts. In practice, our impression is that the IRC projects focused on the hub villages (and town quarters), which are natural communities in the sense that they have a traditional authority structure.

the most vulnerable and those most impacted by war (women, youth, excombatants and vulnerables)."[5] One premise of the aid project is that poverty, "lack of democratic governance" at the grass-roots level, and marginalization of vulnerable subpopulations are causes of violent conflict. Another is that past conflict increases tensions and distrust within and between communities, so creating a need for projects that will promote reconciliation.

The IRC's CDR program in Liberia had the following core components. After the communities were identified and the treatment set selected in September 2006, the IRC undertook initial activities to explain the program to local communities, including meetings with chiefs and elders to solicit their cooperation on an advisory board. In each community, the IRC then oversaw the election of community development councils (CDCs), with 5 to 15 representatives (the average was 9). All adults in the community were eligible to vote, and the IRC staff encouraged though did not require the CDC to include female members (in practice, all communities had at least one and in the median case, one-third of members were women). CDCs were then empowered to oversee a community-wide process to select and implement a "quick impact" project (median value of $2,700), followed by a larger development project (median value of $12,000). Communities were also encouraged to consider using part of the total block grant (median value of about $13,000) for a "marginalized project" intended to address needs of vulnerable groups, although in practice these projects, when undertaken, were similar to the quick-impact and larger projects.

All three types of projects tended to involve construction of community facilities, such as community meeting houses, guest houses and latrines for important visitors, school rooms, and wells. The IRC staff helped to conduct a needs assessment with the CDC and in community meetings, but, subject to a few constraints, the "community driven" philosophy deliberately leaves project selection to the community.[6] For all projects, communities were supposed to supply labor or in-kind contributions worth 10% of project value. IRC staff also assisted the CDCs with project design and tendering bids from local contractors. CDCs managed the implementation process and continue to have responsibility for project maintenance over time.

By March 2008, construction had been completed on 55 of 131 projects in all 42 treatment communities, and had at least begun on almost all of them.[7] Delays were ascribed mainly to an initial overestimation of the capacity of the local construction sector, and perhaps also to to what may have been an unusual level of IRC staff turnover.

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[5]This is from the IRC's final proposal to DFID for the project.

[6]Projects must be for community-wide rather than private or narrowly targeted benefit, and it seems that purchase of capital equipment for income-generating projects (such as a rice mill) was also not allowed in this case.

[7]Note that some communities, particularly the larger ones, which received larger block grants, pursued multiple projects.

## 2.2   Treatment Assignment and Covariate Balance

In September 2006, IRC staff randomly assigned collections of villages to treatment. The method used was block randomization, with 21 of 40 clusters of villages selected with equal probability in Zorzor and 21 of 43 communities selected with equal probability in Voinjama. Selection was implemented by IRC staff by drawing lots during public lotteries with participants from the community clusters. Reports from the IRC suggest that representatives of communities were in general positive about the process of random allocation on the grounds that it seemed both transparent and fair relative to the standard approach of selection by NGOs and government officials.

In March and April 2006, before the community boundaries were decided, we implemented a baseline household survey of 1,702 households in the two districts. Of these, 1,606 lived in communities ultimately assigned to treatment or control status. The baseline data allow us to assess whether the treatment and control communities are similar on various dimensions such as material well being, conflict experience, ethnic composition, as well as a large set of indicators of attitudes about governance.

Figure 1 shows the distribution in treatment and control communities of a core set of variables that are plausibly associated with collective action capacity: basic population data (number of households, persons per household), a set of three wealth indicators (two composite measures of material wellbeing and percent with primary school education), exposure to conflict (percent household members injured or killed in conflict since 1989 and share that are former combatants), a measure of ethnic heterogeneity (percent Mandingo), and a measure of rurality (percent of communities that are "quarters" of a larger town). With one exception, balance is very good; our many attitudinal indicators show excellent balance as well. An $F$-test for the hypothesis that these variables are jointly uncorrelated with treatment has an associated $p$ value of 0.56 indicating that we cannot reject the null hypothesis that the randomization was faithfully implemented by IRC field agents.

The variable for which balance is poor is "quarters." Twenty-eight communities in the five largest towns, are classed as quarters — an administrative level within a town that has a chief or sub-chief, and more or less well-delimited boundaries. Chance allocated 10 quarters to CDR treatment and 18 to control, a somewhat skewed distribution. This difference corresponds to a 0.40 standard deviation difference between treatment and control group values. In the public goods game discussed below, we found that the quarters generated markedly lower contributions, an outcome consistent with other observations suggesting that these communities were less well organized on average than more rural communities. There is disagreement on the merits of trying to "control" for variables on which there is imbalance of this form.

Introducing controls does not reduce bias since bias does not depend on the realization of the randomization. Moreover, it may *introduce* bias if controls are selected using a 'conservative' approach in which controls are introduced precisely because they lead to smaller estimated effect sizes or larger standard errors. Introducing controls may improve efficiency, although the efficiency rationale for introducing controls is weakened, not strengthened, by the failure of a balance test (Mutz and Pemantle, 2011). Nevertheless, many researchers view invariance of estimated effects to the introduction of controls as evidence of robustness. For this reason, in most analyses we report results with and without a control for "quarters" although we emphasize that our preferred specification, the unconditional estimate of treatment effects, provides an unbiased estimate.

## 2.3   Estimation of Effects

Unless otherwise noted, we report estimates of the average treatment effect. These estimates take account of the blocked randomization by using district as strata; in addition, strata are used to account for other treatment arms where relevant and, where noted, to account for 'quarters' as a potential confound. These estimates and their associated standard errors are calculated as follows:

$$\hat{\tau}_{ATE} = \sum_{S \in \mathcal{S}} \frac{n_S}{n} \left( \frac{1}{n_{S1}} \sum_{S \cap T} y_i - \frac{1}{n_{S0}} \sum_{S \cap C} y_i \right) \tag{1}$$

$$\hat{\sigma}_{ATE} = \sqrt{\sum_{S \in \mathcal{S}} \left( \frac{n_S}{n} \right)^2 \left( \frac{\hat{\sigma}_{S1}^2}{n_{S1}} + \frac{\hat{\sigma}_{S0}^2}{n_{S0}} \right)} \tag{2}$$

where $y_i$ is the observed outcome of interest in unit $i$; $\mathcal{S}$ is a set of strata with typical element $S$; $T$ and $C$ are the collections of units in treatment and control; $n$ and and $n_S$ denote the number of all units and units in stratum $S$ respectively; $n_{S1}$ and $n_{S0}$ denote the number of treated and untreated units in stratum $S$; and $\hat{\sigma}_{Sj}^2$ is the estimated variance of potential outcomes under treatment condition $j$ in stratum $S$. All analyses of CDR effects use the community as the unit of analysis since this was the level of treatment assignment (or in the analysis of heterogeneous effects, subsets of community responses are analyzed); in our analysis of the behavioral game, we also analyze a composition treatment at the level of communities and interest rate effects at the level of individual players.

Exact $p$ values are estimated using randomization inference and taking account of the structure of blocking in the randomization scheme. In general, results for the average treatment effect, or a $t$-test on the difference of means without matching, yield similar results. Asymptotic
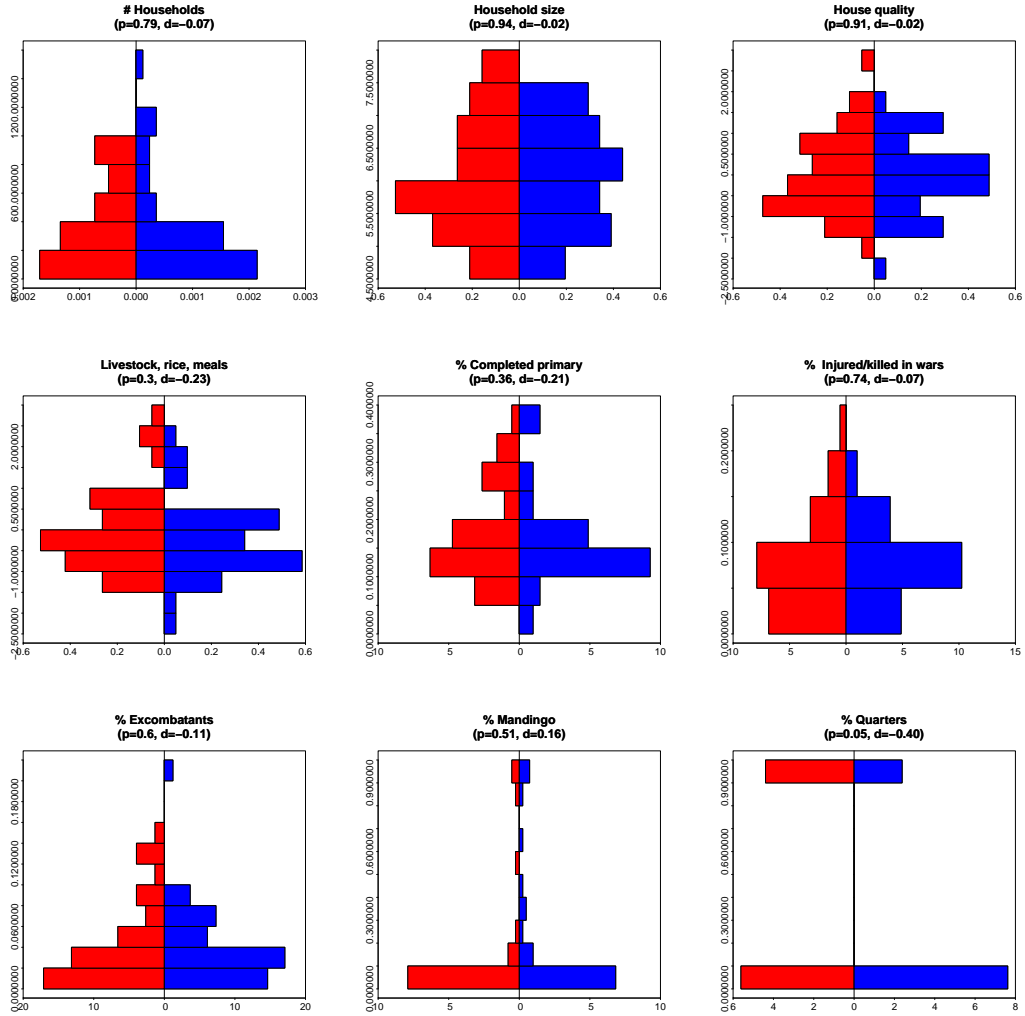
Figure 1: Notes: Figures show histograms of each variable for treatment (blue) and control group (red). Graph titles report $p$ values from $t$-tests of the null hypothesis of no relation between each variable and treatment assignment and "$d$" values which report the difference in means between treatment and control groups in standard deviations of the control group values. For variable definition see Table 3.

approximations are extremely close to the estimated exact $p$-values.

## 2.4 Multiple Comparisons

For many of the outcomes of interest we have access to a large number of possible measures. This multiplicity gives rise to a well known problem. With so many questions an item-by-item analysis will find some differences between treatment and control groups to be "statistically

significant" even if the null hypothesis of no impact is true.[8]

In situations in which we have multiple measures we address this problem following the approach of Kling, Liebman, and Katz (2007) and create a set of standardized indices of outcomes on related items. Within each set of variables we first define items so that higher values imply a positive treatment effect, we then subtract the mean for the control group and divide by the control group standard deviation. The index is then constructed as the standardized average of the standardized variables for each respondent. More formally, define:

Define:

$$\zeta_j(X_1, X_2, \ldots, X_m) = \frac{1}{m} \sum_{k=1}^{m} \left( \frac{\overline{x}_{kj} - \frac{1}{n_c} \sum_{j \in C} \overline{x}_{kj}}{\sqrt{\frac{1}{n_c} \sum_{h \in C} \left( \overline{x}_{kh} - \frac{1}{n_c} \sum_{j \in C} \overline{x}_{kj} \right)^2}} \right) \tag{3}$$

where each variable $X_i$ is coded so that positive values have a substantively positive interpretation, $C$ is the set of of control community indices and $\overline{x}_{kj}$ is the average outcome on measure $k$ in community $j$. Then our index is given by:

$$\xi_j(X_1, X_2, \ldots, X_m) = \frac{\zeta_j - \frac{1}{n_c} \sum_{j \in C} \zeta_j}{\sqrt{\frac{1}{n_c} \sum_{h \in C} \left( \zeta_j - \frac{1}{n_c} \sum_{j \in C} \zeta_j \right)^2}} \tag{4}$$

In practice we define variables at the community level (the level at which treatment is assigned) and calculate the averages and standard errors in equations 3 and 4 using non-missing data only, thus in some cases for a given unit, $\zeta_j$ may use data for only a subset of variables from all units.

## 2.5 Broad impacts of the CDR program

As noted, the CDR program had many goals besides that of building collective action capacity. In addition the program sought to have a direct impact on welfare as well as improve the situation of minorities and improve local governance.

To assess the effects of the program on collective action capacity in the context of these multiple goals we provide here a basic analysis of treatment effects on a cross-section of broader outcomes, using data from a follow-up household survey we implemented in March and April 2008, shortly after the formal end of the program but 5 months before the gathering

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[8]Our preliminary analysis proceeded item by item, noting greater item-by-item "significance" for some groups of questions, and also a general pattern of positive CDR treatment impact that was unlikely to be explained by chance even though for most individual questions the CDR effect was not "statistically significant" (Fearon, Humphreys, and Weinstein, 2009).

of our measures of collective action capacity. The endline survey gathered data on 1,382 of 1,606 study area households interviewed in 2006 (we also added 166 new households to ensure a minimum of ten surveys per community). Similar to the baseline, this survey collected data on material welfare, public services, social capital, attitudes about local governance and marginalized groups, and participation in community affairs.

The survey comprised 141 questions, most with multiple parts, so we have 514 response variables excluding the "roster" data on socio-demographic characteristics of household members. For the purposes of this paper, we focus our analysis on the subset of questions related to community governance and collective action, and group questions in sets that correspond to CDR program objectives (see Appendix A for the questions and groupings). In particular, we explore the impact of the CDR intervention on faith in local leaders and the value of democratic processes, the degree of political and social efficacy, attitudes toward marginalized groups, and the extent of community activity. The CDR program is hypothesized to strengthen collective action capacity in part by changing attitudes about the value of open and democratic processes and the importance of inclusiveness, and changing beliefs about the extent to which individuals can make a difference in community affairs.

The first seven rows of Figure 2 summarize results for indices of attitudes related to community governance practices. We see strong evidence of a positive CDR impact, with estimated effects on average about 20% of a standard deviation and statistical significance for all categories except the questions about women's political rights (which nevertheless border on significance at conventional levels). "Democracy" questions tap respondents' attitudes about the value of democratic process for community decision-making. For example, should community members or chiefs have the most influence over scenarios involving a land dispute and use of development funds; should town members question or "respect" authority; should leaders favor their family or ethnic group; should community leaders be elected? "Efficacy" covers whether the respondent feels he/she has influence or capacity to influence community decisions, whether they feel free to express their views, and whether they are generally satisfied with the way community decisions are made. "Participation" includes reports on community initiatives and willingness to work together, individual participation in community meetings and contacts with various authorities, frequency of community meetings, and intention to vote and work with a political party. "Inclusion of ex-combatants" combines questions about attitudes towards ex-combatants, their treatment in community meetings and perceptions of their willingness to work for the community. "Inclusion of migrants" uses the same questions but for "returnees" and "new migrants." "Trust in leaders" includes direct questions about how "trustworthy" are leaders in this town compared to other towns, what share of development

funds they would likely keep for private use, whether making complaints to leaders is likely to have any good effect, and whether they "do better" out of community meetings.

The last two rows of Figure 2 display results for indices of social capital (membership in organizations and clubs) and the degree of conflict within the community (reports of "divisions" and/or violence along the lines of income, gender, age, migrant status, religion, ethnicity, and education, and reports of crime or fighting). Again we find evidence of positive effects although estimates are weakened when we condition on quarter. (The more urban quarters had more reports of conflict, social divisions, and crime, and were somewhat underrepresented in the treatment set.)

These results are suggestive of striking social impacts in line with the expectations of the program; although in the context of a program designed specifically to affect these outcomes there is cause for concern that the results reflect a desire of respondents to reflect the values of international donors. These concerns motivate the use of behavioral measures, described in the next section.

A final point about the survey results is worth emphasizing: the focus on estimating CDR impact should not lead us to miss the fact that respondents in both treatment *and* control communities generally reported high levels of social capital, trust in community leaders including chiefs, sense of individual and community efficacy, participation in community efforts, support for democratic procedures, and tolerance and acceptance of ex-combatants and new migrants. CDD and CDR programs attempt to foster these beliefs and capacities in communities presumed to suffer from their absence. At a higher level of generality, some theories of economic development argue that low levels of trust and volunteer community organizations are important barriers to growth in poor countries. These survey data from two war-ravaged districts in northern Liberia are at odds with these assumptions.[9]Large majorities of respondents reported attending a community meeting in the last six months and speaking or making a suggestion in a meeting. Many also reported having raised an issue with the town chief (about 46%). Asked if divisions or disputes along the lines of wealth, gender, age, migrant status, religion, ethnicity, or education hindered cooperation in the community, on average around 80% or more of respondents said "not at all" rather than "somewhat" or "very much."

## 3    Main Results

The analysis presented in the last section on broad outcomes of the CDR program followed a relatively standard approach of using survey evidence to assess collective action capacity and

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[9]To give just one example, over 90% of respondents reported belonging to at least one community or social organization, and over 50% said they belonged to at least three.

**Average treatment effects (endline survey measures)**



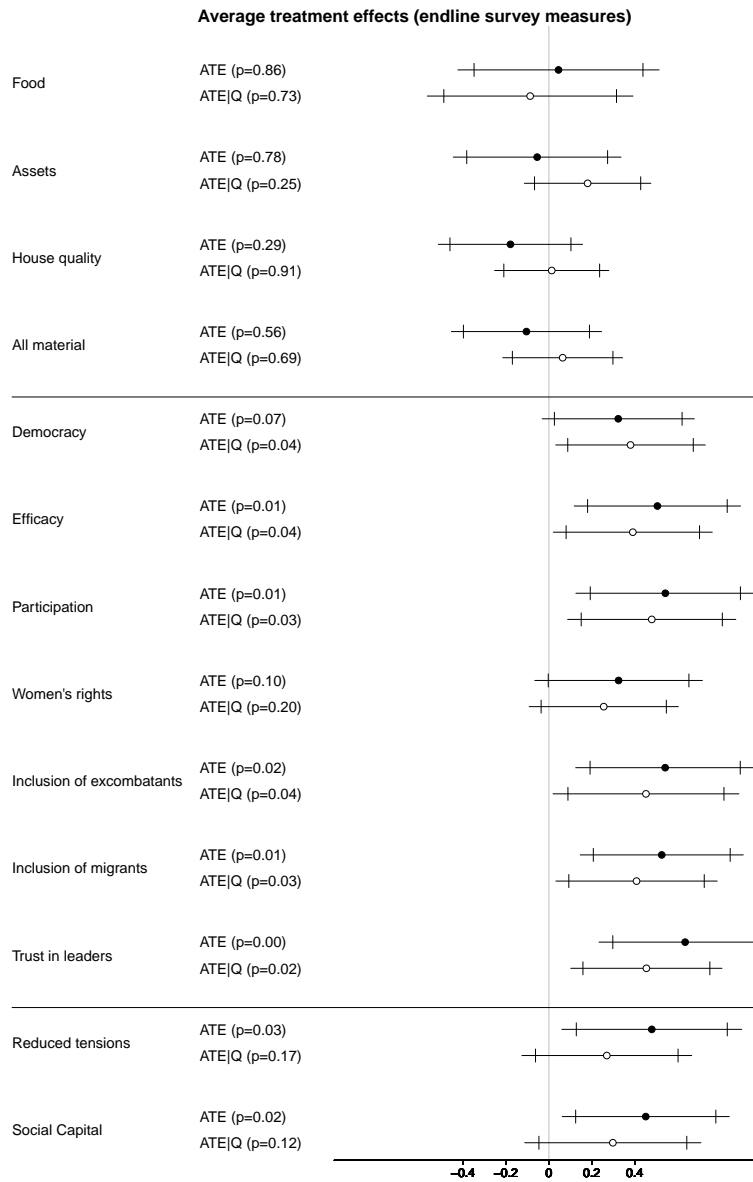| | | |
|---|---|---|
| Food | ATE (p=0.86) | |
| | ATE\|Q (p=0.73) | |
| Assets | ATE (p=0.78) | |
| | ATE\|Q (p=0.25) | |
| House quality | ATE (p=0.29) | |
| | ATE\|Q (p=0.91) | |
| All material | ATE (p=0.56) | |
| | ATE\|Q (p=0.69) | |
| Democracy | ATE (p=0.07) | |
| | ATE\|Q (p=0.04) | |
| Efficacy | ATE (p=0.01) | |
| | ATE\|Q (p=0.04) | |
| Participation | ATE (p=0.01) | |
| | ATE\|Q (p=0.03) | |
| Women's rights | ATE (p=0.10) | |
| | ATE\|Q (p=0.20) | |
| Inclusion of excombatants | ATE (p=0.02) | |
| | ATE\|Q (p=0.04) | |
| Inclusion of migrants | ATE (p=0.01) | |
| | ATE\|Q (p=0.03) | |
| Trust in leaders | ATE (p=0.00) | |
| | ATE\|Q (p=0.02) | |
| Reduced tensions | ATE (p=0.03) | |
| | ATE\|Q (p=0.17) | |
| Social Capital | ATE (p=0.02) | |
| | ATE\|Q (p=0.12) | |

−0.4  −0.2  0  0.2  0.4

Figure 2: Figure shows unadjusted average treatment effects (ATE) and average treatment effects with covariate adjustment for quarter (ATE|Q). Lines are 95% and ticks are at 90% confidence intervals. See Table 3 (Appendix) for variable definitions and summary statistics.

institutional performance. For the measurement of collective action outcomes, however, we were particularly concerned that CDR could lead to a change in reported responses without changing capacity or inclination for collective action. For example, the NGO's intervention might influence people's understanding of what they are "supposed to say" but not their willingness or ability to act and coordinate in line with those expressed beliefs.

For this reason, we designed a public goods game with the goal of observing whether communities exposed to the CDR program were in fact more effective at acting collectively. The game involved the following steps. An advance team visited each of the 83 hub towns and gained consent for a community meeting to describe an opportunity for the community to receive new funds for development. One week later, we ran a meeting in which community members were told that they could receive up to $420 to spend on a development project.[10] Receipt of funds would depend on whether the community completed a form indicating how the funds would be spent and the names of three community representatives to receive and handle the funds. The specific amount received would depend on how much money a random sample of 24 people contributed to the project in a community-wide public goods game. One week after that, a team returned to the village, collected the form, sampled 24 households, played the game, and publicly announced and provided the total payout to the village. Between these two visits, the community had time to select their community representatives and potential projects, and to spread information about the game and how it should be played. On game day, detailed surveys were completed with all 24 game players (after they made their private contribution decisions), the three community representatives, and the village chief.

## 3.1   Game Description

The public goods game itself was straightforward: 24 randomly selected adults (from randomly selected households) were given three 100LD notes, worth in total about $5 US or close to a week's wages. They then chose, in private, how much to contribute to the community and how much to keep for themselves. It had been explained in the community meeting that half of the players would have their contributions multiplied by two, while the others would be multiplied by five, corresponding to interest rates of 100% and 400%. Thus each community had the opportunity to earn up to 25,200 LD. For this "interest rate treatment," players were randomly assigned to the high and low rate conditions (with blocking on gender and location);

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[10]The initial community meeting and the game itself were administered by a Liberian NGO – National Excombatant Peacebuilding Initiative – working with a team of Stanford graduate students under our oversight. NEPI members did not know that we were studying the effects of the IRC CDR program, and our graduate students typically did not themselves know which villages were treatment and control (although in some cases signs advertised IRC projects).

players knew their interest rate when choosing how much to contribute.[11]

## 3.2 Gender Composition Treatment

In addition, we ran a cross-cutting experimental treatment: in half of the communities all 24 game players were women, while in the other half, we selected 12 men and 12 women players. The gender composition assignment was implemented using a matched pair design in which units were matched based on estimated population size. This 'gender composition' treatment related only to the makeup of the players for the public goods game, and not necessarily the set of beneficiaries of the potential development project. This was explained to communities via the verbal instructions delivered at each community meeting in which it was made clear that, regardless of the gender of the game players, in all communities both women and men could participate in meetings to decide on projects, serve as community representatives, and be beneficiaries of the project itself.[12]

Table 1 gives the overall distribution of treatments and reports the number of communities and treatments in each condition.

## 3.3 Implementation of Games

Eighty-two communities successfully completed the behavioral game.[13] The average payout to villages was 20,020LD, or 79.4% of the total possible, with a standard deviation of 13.3%. Among individuals, fully two-thirds contributed the maximum amount (300LD), with the rest almost evenly divided over giving 200 (10%), 100 (12%), or 0 (11%). The average contribution was about 235LD, which is 78.3% of 300.[14] In lab experiments, contributions in the first play of analogous public goods games are typically around 50% of individual endowments (Ledyard, 1995). Arguably, then, these rural Liberian communities managed an impressive level of

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[11]In the community meetings, our presenters stressed that the contribution decision was up to the game player and that there could be good reasons for an individual to keep the money for private use. It was evident, however, that attendees immediately grasped the conflict between private and social good.

[12]We added the gender treatment because it was simple to implement and could allow us to speak to donor and academic interest in the question of how women's involvement in community decision-making affects outcomes; we also expected, as noted above, that we would find no CDR treatment effect. We did not have the statistical power to have a third set of communities where all game players were men. Because our focus in this paper is on CDR, we do not provide a full analysis of the gender composition treatment.

[13]Play was halted prematurely in one community after a player changed her mind about her contribution decision and a public scene developed when she and her sister made this known. The community was later given an approximately average payout to avoid hard feelings.

[14]A handful of players disobeyed instructions and put amounts other than 0, 100, 200, or 300 into the envelope. We use their actual contributions in the individual-level analyses that follow.

Table 1: Distribution of treatments

|  | CDR Intervention | | Total Communities |
| Gender composition | Control | Treatment | (Participants) |
| --- | --- | --- | --- |
| Mixed groups | 20 | 22 | 42 (1008) |
| Women only | 21 | 20 | 41 (984) |
| Total communities (Participants) | 41 (982) | 42 (1008) | 83 (1992) |

Notes: In mixed groups 12 men and 12 women were selected as game players; in 'women only' communities, all 24 were women. In all communities, 12 players were randomly assigned to have a high interest rate and 12 to low. In areas with mixed groups half the men and half the women were assigned to each interest rate condition. The CDR assignment was blocked on district. The women gender composition assignment was blocked using a matched pair design with matching on village size.

cooperation.[15]

## 3.4  Main Effects from Behavioral Measures

The focus of our analysis here is on the effect of the CDR treatment on contributions by players. The central outcome of interest is the degree to which communities were able to act collectively, as reflected in individual decisions about how much to contribute for a project to be managed by three community representatives. We provide only summary results of the effects of the gender composition and interest rate variations, but use them throughout the analysis as strata and to examine heterogeneous effects. With all treatments randomly assigned across villages (or individuals), the behavioral game allows us to measure the causal effect of the CDR program on actual behavior and also to see how that impact varies with gender composition of the players and the interest rates accruing to individual contributions. Since the main intervention was administered at the community level, we run our core analysis using a community level outcome – the average contribution by players in a community.

We emphasize that our research design does not isolate the impact of introducing new democratic institutions per se. Instead, we are in a position to identify the overall effect of receiving CDR funds for public goods provision coupled with the creation and use of participatory, democratic institutions for selection and implementation of community projects. To try to assess the effect of the institutional aspects of the CDR program on collective action

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[15]Lab experiments usually involve smaller stakes (relative to wealth and income) and smaller groups, both of which favor contributions relative to our case. On the other hand, our game involved actual communities that had a week to mobilize and exhort people to contribute if chosen to play.

capacity, we have to consider other information, a task we undertake below.

Table 2 presents estimates of the main effects of our three randomized treatments on contributions in the game.

Table 2: Experimental Effects

|  | CDR Treatment | Gender Composition Treatment | Interest Rate Treatment |
|---|---|---|---|
| Level in control group | 226 | 223 | 226 |
| Average treatment effect | 17 | 27 | 18 |
| $p$-value | 0.03 | 0.003 | 0 |
| *Conditioning on quarter:* | | | |
| Level in control group | 231 | 224 | 226 |
| Average treatment effect | 8 | 23 | 18 |
| $p$-value | 0.3 | 0.002 | 0 |

**Note:** CDR and gender composition treatment effects are estimated at the community level; district and the other treatment form strata. For the interest rate, strata are formed by community gender groups. The $p$ values are calculated using two tailed tests and randomization inference.

The upper half of Table 2 shows that all three treatments had substantial effects in terms of dollars contributed. In terms of share of total payout, the effect of CDR on collective action was to raise the average share contributed from 75.8% to 82.1% of the maximum possible, a difference that is close to half of the standard deviation of the payouts in our sample. Another way to scale the magnitude of the CDR effect is to note that it is about the same as the impact of raising the rate of return on individual contributions from 100 to 400%: both increased average individual contributions by about 17LD, which is 5.6% of 300. The gender composition treatment ("all women") had a somewhat larger positive impact, almost 10% greater payouts than the mixed gender groups on the 0-100 scale. The lower half of the table shows estimates of average treatment effects when quarter is included in the list of strata. We see here that this substantially reduces the estimated CDR treatment effect.

Figure 3 shows the heterogeneity of the measured CDR treatment effect across strata. Looking across the rows, we see that the CDR program is associated with a very large effect on contributions where both men and women played the game, and a marginally negative but statistically insignificant effect in the communities where only women played. Moreover, when we take account of quarter, the estimated CDR treatment effect in mixed areas, though marginally smaller, remains statistically significant.

The CDR treatment effect does not vary substantially by either interest rates or gender

of the game player. That is, although the treatment effect is stronger in mixed gender groups than in women only groups, this is not because CDR affected men's contributions but not women's in the mixed groups. Interest rate effects are strong in both the mixed and women only groups but surprisingly are found among women only. Male players ignore the interest rate in both treatment and control conditions. Importantly, and a point we return to below, we see little evidence of an interaction between the interest rate and the CDR treatment: it is not the case that experiencing the CDR program increased game player's sensitivity to higher interest rates, for example.
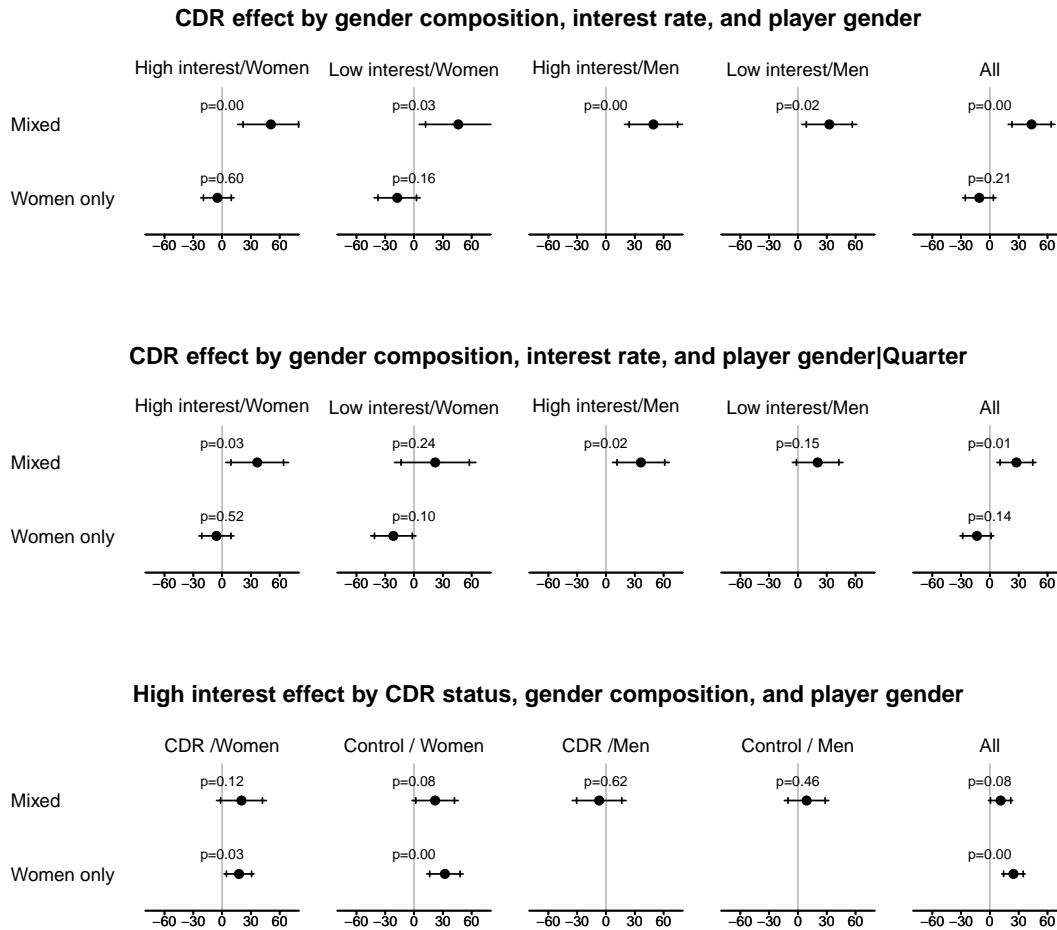


Figure 3: Heterogeneous effects. Each panel shows estimated treatment effects for six disjoint strata as well as overall effects for women only and mixed groups. Top rows show estimated effects, with 95% confidence intervals, for participants in mixed groups; bottom rows show effects in women only areas (in which there are no male participants). Columns further disaggregate by other treatment conditions and gender of participant. *p*-values are generated using randomization inference.

To explain why and how this CDR program increased collective action capacity, it is clearly necessary to understand why the effect appears only in the communities where we had 12 men and 12 women as game players, and not when only women played. In the next section, this finding will prove useful for evaluating some hypotheses about causal mechanisms, though it remains an important puzzle.

Before proceeding, we note that the absence of a CDR impact in women only groups is probably not explained by a ceiling effect. One might conjecture that if women just gave substantially more than men, we might not pick up a CDR effect in the women only groups because they were already close to the maximum feasible contribution in this setting. It is true that women contributed more than men on average, but it is interesting to note that they contributed more only when they knew they were playing with other women.

If the absence of a CDR effect in the women only groups was the result of a ceiling effect, then we would expect the effect of a higher interest rate in the all women groups to be lower than in the mixed groups. But this is not the case.

Despite statistical objections to the practice (Freedman, 2008), multiple regression is often used to check whether a treatment effect remains when covariates are considered. This is indeed the case for the CDR effect in the mixed gender groups when we 'control for' community size, whether the community is a Quarter in a larger town, and percent Mandingo. Larger communities generated significantly lower payouts in the game, as did the more urban quarters and predominantly Mandingo communities. The estimated CDR effect in the mixed gender groups diminishes some when we control for these factors (and especially quarter, which was not well balanced), but remains substantively and statistically significant (see Table A1, Web Appendix).

## 4   Mapping Mechanisms

The CDR program could have affected play in the public goods game by a number of different causal paths. The data we collected through our surveys and behavioral measures provides valuable information that enables us to explore the different mechanisms that may account for the CDR impact. A simplified model of the decision problem faced by players in our behavioral game can help to distinguish between five plausible families of mechanism.

### 4.1   The Contribution Game

Consider a game with $n$ players who simultaneously choose whether to contribute a share of a dollar $x_i \in [0, 1]$ to a public good. Let $\epsilon_i - \theta$ denote the player's marginal private

cost of contribution to the public good; the common term, $\theta$, which may be positive or negative, captures community average net benefits to the act of contribution. The idiosyncratic component $\epsilon_i$, distributed according to cumulative density $F$ on some interval of $\mathbb{R}$, describes individual deviations from the average. Then let $i$'s preferences be represented by:

$$u_i(x) = (1 + \beta)\mu \left( \sum_{-i} r_j x_j + r_i x_i \right) - (\epsilon_i - \theta)x_i$$

where $\mu \in \mathbb{R}$ captures the rate of transformation of public investments into public goods (measured in units of the value of own consumption of the public good), and $\beta$ captures social preferences, given here by the extent to which $i$ values the public good beyond own-consumption. Player specific interest rates are given by $r_i$.[16]

In this simple environment, individuals contribute the full amount, $x_i = 1$, if $(1+\beta)\mu r_i + \theta > \epsilon_i$ and contribute nothing otherwise. Define $\phi(r) = (1 + \beta)\mu r + \theta$. With half the population facing interest rates $\bar{r}$ and half interest rates $\underline{r}$ and interest rates distributed independent of types, the expected value of the public good contributions, $\pi$, is

$$\pi = \frac{n}{2} \left( \bar{r} F(\phi(\bar{r})) + \underline{r} F((\phi(\underline{r}))) \right)$$

A number of features of the model are worth noting. First if participants saw zero value to the public investments ($\mu = 0$) – for example due to expected theft of all funds by leaders – then the decision to contribute would depend on the sign of $\epsilon_i - \theta$ only, reflecting a focus on the direct costs and benefits of contribution rather than the effects arising through the public good.

Second, there is an observable difference between effects that work through considerations that apply to the contribution decision by itself, $\epsilon_i - \theta$, and effects that work through the impact of a contribution on the public good, $\mu$ or $\beta$. If an action increases $\mu$ or $\beta$ then we expect the effect of this action to be magnified by the interest rate. This is not so for actions that affect $\theta$, however, since the effect of $\theta$ is independent of the interest rate.

Third, although developed for a linear public goods technology, similar logic holds if citizens perceive diminishing marginal returns to spending on the public good, although in this case players may optimally choose a contribution that is in between all and nothing. In the unique Bayes-Nash equilibrium of such a game, players with lower values of $\epsilon_i$ contribute more (conditional on their interest rate). Alternatively, suppose that players perceive *increasing* marginal returns from contributions to the public good, or view the marginal cost

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[16]Or more precisely $r_i$ denotes the multiplier which is equal to one plus the interest rate.

of contributing as decreasing in the number of others who contribute (say, because people feel discomfort for deviating from what most others are doing). Then multiple equilibria can arise, with players contributing all or nothing but in larger or smaller sets depending on the equilibrium. In this case outcomes depend on whether and how communities coordinate on one or another equilibrium.[17]

## 4.2 Mobilization

Below we discuss ways in which exposure to the CDR program could affect the key parameters from this model, $\beta$, $\mu$ and $\theta$. Of special interest, however, is the role that mobilization activity by leaders might play in determining these values. Recall that an initial meeting was held for each community at which attendees learned that any 24 adults might be selected to play the game a week later. This allowed time to spread information about the game, to encourage people to contribute if chosen, and perhaps to hold meetings to choose projects and representatives.[18] Mobilization could increase the average motivation to contribute. But at the same time, community elites might condition mobilization effort on their expectations about the benefit from doing so.

Assume for simplicity that $F$ is uniform and let $\pi(\gamma)$ denote the expected contributions to the public good for parameter vector $\gamma = (\beta, \mu, \theta)$, which we take to be an element of a convex and compact parameter space $\Gamma$. Note that $\pi$ is weakly increasing in each element of $\gamma$. To represent investments in mobilization, say that there exists a baseline (no mobilization) value of the parameters, $\underline{\gamma}$, and that leaders can choose parameter vector $\gamma \geq \underline{\gamma}$ to maximize:

$$u(\gamma) = \pi(\gamma) - c(\gamma, \alpha | \underline{\gamma})$$

Here $c()$ denotes a smooth convex cost function that is increasing in each element of $\gamma$; $\alpha$ denotes organizational capacity and we assume that marginal costs of organizing are decreasing in capacity in the sense that the cross partial $c_{\gamma\alpha}$ is negative.[19]

In this case there is a unique solution to this problem, $\gamma^*$ (existence is guaranteed by the Weierstrass theorem; uniqueness follows from the concavity of the utility function). The

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[17]For example if $n = 2$, $\epsilon_i \sim U[0, 1]$ and $u_i = \frac{3}{4}(x_i + x_{-i})^2 - (1 + \epsilon_i)x_i$, then there is an equilibrium in which all contribute zero, and another where players with $\epsilon > .5$ contribute the full amount, and others contribute 0, and another where all contribute all.

[18]Attendance varied at the initial community meeting, averaging perhaps one quarter of the adult population of the hub community.

[19]Note we rule out the possibility that leaders mobilize to reduce contributions to the public good. Note also that defining the objective function as increasing in contributions in this way does not imply that the elite are necessarily public-spirited. They could, for example, value contributions precisely so that they can subsequently appropriate them for themselves.

(positive) incremental expected contributions attributable to mobilization is given by:

$$\Delta(\alpha, \underline{\gamma}) \equiv \pi(\gamma^*(\alpha, \underline{\gamma})) - \pi(\underline{\gamma})$$

We have then that a drop in the costs of mobilization, $\alpha$, weakly increases total contributions, but may do so through changes in any of the parameters affecting individual preferences over the contribution decision.[20]

## 4.3 Model Implications

The model highlights four major families of mechanism that may affect play in the basic contribution game, corresponding to factors that affect the parameters $\mu$, $\beta$, $\theta$, and ability to coordinate. The CDR program might have increased contributions in the public goods game by directly affecting community members' values for these parameters and coordination ability, or indirectly by increasing community leaders' mobilization efforts in the week before play of the game. We consider the immediate, or individual-level pathways first, and then discuss the possible impact of mobilization efforts on these pathways.

### 4.3.1 The economic value of the public good

The CDR program could have increased or decreased individuals' value for their own consumption of the public good as it is produced by contributions. This is captured by $\mu$ in the model. Though the term is not exact, we will refer to these as mechanisms that affect the *economic value* of the realized public good. Examples include factors that affect how much an individual values having a new meeting house, for example, relative to cash, and also factors that affect how much of the money raised is actually spent on the project.

We highlight two such ways that CDR could affect the economic value of public investments.

1.1 *Project selection effects:* If more democratic methods were used to select projects for our game in CDR communities, this may increase individuals' satisfaction with the planned projects.

1.2 *Trust in leaders:* The experience of the CDR program might have increased trust that money contributed to the project would actually be delivered and properly spent by community leaders.

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[20]From the implicit function theorem, $\gamma_\alpha^* = -c_{\gamma\alpha}/c_{\gamma\gamma} > 0$.

### 4.3.2 The social value of the public good

Second is the extent to which individuals place value on the *social benefits* of the public investments, captured in the model by $\beta$. In this case, CDR works by increasing the weight individuals place on community welfare.

2.1 CDR may increase the *social value* of the public good by increasing members' sense of obligation to the community, experience with the benefits of public goods, or reconciliation that increased the value members put on other community members' welfare;

### 4.3.3 Costs and benefits of contributing per se

Third are mechanisms that work by affecting community members' value for contributing independent of the actual value of the public good produced. These are captured by $\theta$ in the model.

3.1 *Income effects:* If the CDR program significantly increased incomes in the community, this could lower game players' value for cash versus public projects.

3.2 *Sanctioning effects:* The CDR program may increase cooperation by increasing individuals' expectation that they might be sanctioned for failing to contribute.

3.3 *Social (NGO) desirability effects:* Experience with the CDR project could have increased the weight community members put on taking actions that please NGOs.[21]

3.4 *Legitimacy effects from participation:* If CDR increased the use of participatory procedures in community decision-making, this could lower individuals' costs of contributing independent of value for the project and trust in leaders, by increasing the perceived legitimacy of the action.

### 4.3.4 The ability to coordinate

Finally, the experience of the CDR program could have improved or, in principle, worsened, the community's ability to coordinate on a high contribution equilibrium in a situation where there are multiple equilibria. As noted above, multiple equilibria arise only if community members perceive sufficiently increasing returns to public goods contributions (for instance if

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[21]In the community meetings to explain the game, people very quickly saw that the decision problem asked them to choose between personal and community welfare. Some commented along the lines of "they are testing us," with the idea being that "good performance" would be to show how community-minded their community was. Conceivably, the desire to please a foreign NGO in the hope of future benefits could have been increased by experience with the CDR program.

they value what they get from raising \$400 more than twice as much as what they get from raising \$200) or if individuals value contributing more when others contribute (for instance, if they feel discomfort for failing to match what others are doing). On the positive side, the CDR experience might have increased social ties or the propensity to discuss community affairs in such a way that information about how others would act in the game diffused more rapidly. Alternatively, if the program created rival sources of power or influence to the traditional structure, this could have made coordination more difficult.

4 CDR may affect the ability of communities to *coordinate* on socially desirable outcomes.

### 4.3.5   Mobilization capacity

CDR might have affected individuals' calculations directly, or indirectly by increasing the motivation or efficacy with which community leaders organized in the week prior to the play of the game. CDR can affect community leader motivations to mobilize by altering their own values for $\theta$, $\mu$, or $\beta$; this could have increased or decreased the effort they spent trying to influence these parameters in the community.[22] In the case of graft (affecting $\mu$), note that there can be offsetting effects: higher expectation of graft can reduce individuals' incentive to contribute, but increase elites' incentive to mobilize to induce more contributions. It may also affect incentives to mobilize by altering default levels of community contributions.

The CDR program could also have lowered the costs to leaders of mobilizing for collective action problems of this form. CDR may have increased the number of community members with experience cooperating to run elections, select projects, mobilize contributions, and spread information about the program. Faced with a similar task, people, networks, and practices used in the CDR program could be reactivated, and "learning by doing" might have made them more efficient than in communities that had not had the experience of CDR. These gains in efficiency may in turn have allowed for more successful action through any of the channels listed above.

5 The experience of the CDR program might increase the community's *ability to organize and mobilize* for a collective action problem.

## 5   Evidence on Mechanisms

Identifying the mechanisms through which an intervention operates is in general a difficult task (Green, Ha, and Bullock, 2010; Imai, Keele, and Tingley, 2010). Our design provides

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[22]This would correspond to lower or higher cost of mobilizing in the model ($\alpha$), which are implicitly relative to the mobilizers' value for contributions produced.

exogenous variation in the application of the CDR program, but not on intermediary variables that can themselves affect levels of cohesion. Nevertheless, ancillary data from our surveys provides preliminary evidence that can be used to assess the extent to which these mechanisms are plausibly in operation. In particular, we provide evidence on the effects of the CDR treatment on an array of intermediary variables. We also provide a set of correlations between these intermediary variables and the main outcome of interest, contributions in the public goods game. We emphasize, however, that while the former relationships are identified, the latter are not and the analyses in this section should be considered only as suggestive of possible mechanisms.

## 5.1 Economic Value of the Public Good

We begin by examining mechanisms that work by altering the value to game players of the public good. We can see from Table 2 that the interest rate applied to investments has a very strong effect on contributions. This suggests that the economic value of the investments was an important consideration, at least for women (see Figure 3). Recall however that following the logic of our model, if the effect of CDR works through this value then we would expect a positive interaction between CDR and the interest rate – especially in mixed groups where the positive effects of CDR are concentrated. That is, if CDR worked by increasing individuals' value for public goods, then facing a higher interest rate should induce greater contributions from people in CDR than in control communities. From Figure 3, we see weak evidence of such an interaction – in mixed gender groups, the treatment effect is moderately stronger for high interest players; the difference, however, is not statistically significant, suggesting that this family of mechanisms may not be important. We now examine more closely two specific members of this family.

### 5.1.1 Project selection

It is important to note that how project funding was to be used was itself a choice variable for communities. If CDR-treated communities used more democratic procedures to select community representatives and projects, then this might have increased players' value for the project and thus the marginal value of their contributions.

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**Effects of CDR Treatment on Intermediate Variables**
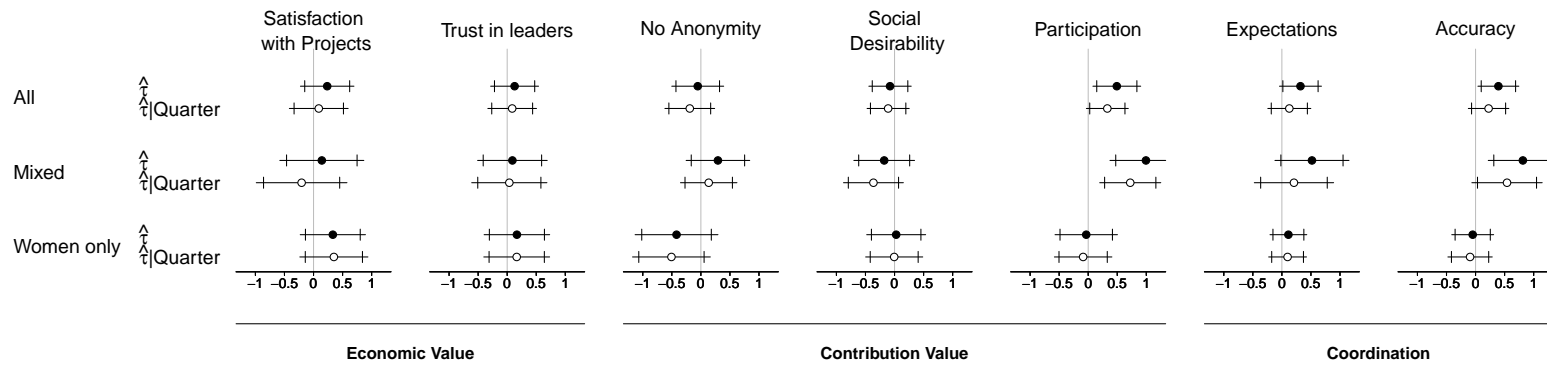


**Relation between Intermediate Variables and Contributions (Non Experimental)**
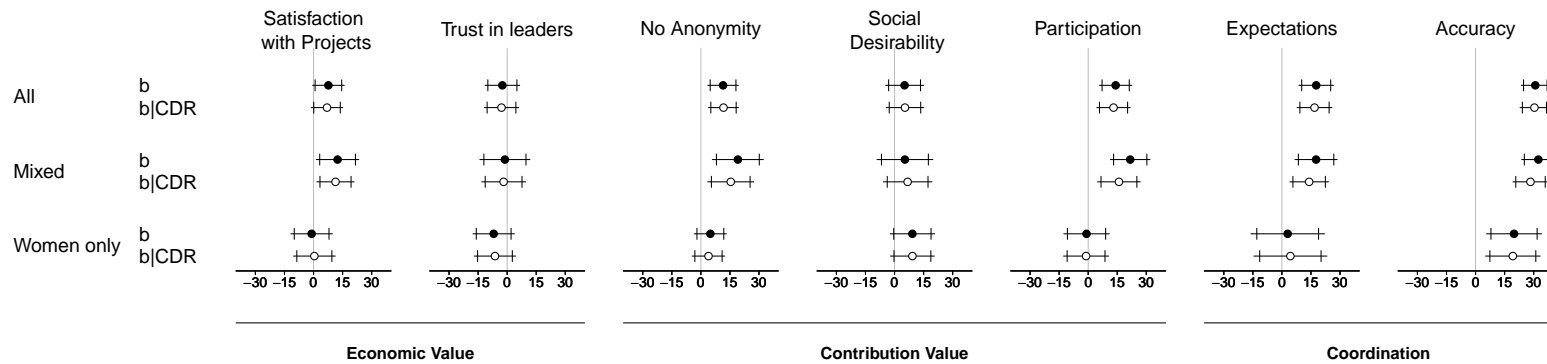


Figure 4: Upper panel shows the effects of CDR on various intermediary variables; lower panel shows the relation (regression coefficients) between these intermediary variables and outcomes. Note that while estimates in the upper panel are identified due to randomization, those in the lower panel are not. See Table 3 for variable definitions and summary statistics.

In the surveys of game players, community representatives, and chiefs, we asked a number of questions about the procedures used to select community representatives and the projects. We also asked a smaller set of questions about game player satisfaction with the planned projects and the process of selecting them.

Although it does appear that greater satisfaction with planned projects is associated with higher rates of contribution, the first column of Figure 4 shows little evidence that satisfaction with the projects or process was greater in the CDR communities, whether or not one conditions on the gender treatment.

### 5.1.2 Trust in leaders

If CDR had the effect of reducing the scope for graft – for example, by rendering leaders more accountable to citizens – then game players could be more confident that their contributions would indeed be used for the production of public goods. We find, however, little or no evidence that the CDR effect is working through such a mechanism. As seen in Figure 3, there is no relationship between the CDR treatment and the level of trust in community leaders. Further, consistent with the model's implication of offsetting effects regarding graft, average trust in leaders in a community is unrelated to average contributions. In mixed gender groups, we see no correlation at all; in women-only groups, the relationship is marginally negative.[23]

The lack of a relationship between the CDR treatment and the trust of game players in leaders is somewhat in tension with our results from the May/June 2008 follow up survey, where we found a positive CDR effect (Figure 2). It could be that the effect was transient, or that the later measure is noisier because game players exaggerated their trust in leaders out of concern that if they expressed low levels of trust, we might be less likely to provide the funds. It is also true that our questions in the post-game survey asked about trust in both the community representatives and "community leaders" rather than just "community leaders."

Curiously, if we look at how average contributions vary with trust in leaders as measured in the follow-up survey, we find a strong positive association in the mixed gender groups, but no association at all (or very slightly negative) in the all-women groups. It is probably not that men cared about the trustworthiness of leaders while women did not: Using the post-game survey measure of trust and looking at individual contributions, we find that both men and women who express greater trust in leaders give significantly more in the game (though it is

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[23]Interestingly, at the individual level (within communities) individuals who express greater trust in leaders contribute much more (not shown), despite there being no relationship at the community level. This 'micro-macro' disconnect could arise if leaders favor those connected with them. Then there may be low levels of trust overall but those that are well connected report relatively high trust and contribute more.

true that men's giving was somewhat more sensitive to higher levels of trust than women's). It is something of a puzzle, then, why greater trust in the women-only CDR communities (as measured in the follow-up survey) does not translate into higher payouts in these communities versus the women-only, no CDR communities. At a minimum, this suggests to us that a CDR effect on trust in leaders is not the primary mechanism behind our results. Either the trust effect of CDR was very short-lived, or it persisted, but did not lead to higher contributions in the communities where only women played the public goods game.

## 5.2 Social Value for the Public Good

There is also little evidence that the effect of CDR worked by increasing the weight individuals put on benefits to the community. As with private values, social values mechanisms imply an interaction between CDR treatment and the effect of interest rates, which we do not observe. There is other evidence against this mechanism as well.

First, note that if the CDR program's impact worked primarily through an increase in civic-mindedness in treated communities – or even an impact that was stronger for one gender than the other – then we would not expect to observe the effect only in the mixed groups, and for both genders in the mixed groups, contrary to the patterns seen in Figure 3.

Second, a number of our game-player survey questions might be thought to more directly tap civic mindedness, and also whether there were fewer differences and conflicts among community members. We asked if the respondent thought that any of the following divisions made it hard for people in the community to work together: income, gender, religion, ethnicity, education, old versus young, and new migrants versus long-term residents. Even more so than in the follow-up survey, game players in CDR communities did not report significantly lower differences on these dimensions.

We also asked if the respondent had voted in the last presidential election, spoken to the chief or attended a community meeting in the last six months, and was a member of a credit association, a religious association, or a committee to support community development. Despite some of these being mechanically related to having had a CDR program – for instance, CDR programs imply community meetings and possibly more reasons to speak to the chief – game players in CDR communities were only marginally more likely to respond positively, with the differences not significant. Nor are there systematic differences when we condition on gender treatment (mixed versus all women players).

## 5.3 Costs and Benefits of Contributing Per Se

We identified four channels through which CDR may have altered the perceived value of participation for any level of public good provided.

### 5.3.1 Income effects

The most obvious channel is that by increasing income, the CDR program produced a decline in the marginal value of cash. The fact that in general wealthier participants contributed more (see Fearon, Humphreys, and Weinstein (2009) ) is consistent with this logic. However, we find little evidence that the CDR program had a positive impact on material well-being, making it unlikely that higher contributions in the public goods game are the result of an income effect. Specifically, we use data from the follow-up survey to generate composite indicators for questions on access to food and livestock, minor household assets, and the quality of household dwellings. None show any consistent or significant CDR treatment effects, whether or not we condition on quarter. Further, if contributions increased for this reason, we would expect greater contributions in both the mixed and all-women communities, which is again not what we observe.

### 5.3.2 Fear of sanctioning

Second, many accounts of collective action suggest that a key mechanism through which communities foster cooperation is by threatening to sanction non-compliers. In principle, CDR may have increased the ability of communities to sanction – for example by affecting the flow of information about the actions of community members, or their willingness to do so – by establishing norms of cooperation. It is also possible that CDR could weaken the capacity to sanction, for example by weakening traditional authority structures.

We asked game players whether they thought others would find out what they contributed and whether this concern affected their contribution decision. A perception that others could learn of one's decision is a precondition for the sanctioning mechanisms to work (Habyarimana, Humphreys, Posner, and Weinstein, 2007). Although we went to considerable lengths to ensure that players would know that only they knew what they put in the envelope, players nonetheless entertained diverse beliefs about the probability that their actions could become known. These concerns may have been well founded if, for example, village elites are able to force public admissions of behavior after the fact. About one in six of our respondents reported such fears to us. Moreover, these reports are clustered at the community level; village fixed effects account for about 17% of the variance in responses to this question. The lower panel

in Figure 4 suggests that fears of loss of anonymity are associated with higher contributions, particularly in the mixed gender groups. However as seen in the upper panel, the estimated effect of CDR on perception of lack of anonymity, though pointing towards a positive impact in the mixed groups, is not distinguishable from zero. Thus while fear of sanctioning may matter for contribution rates, we do not see evidence of a CDR effect on these fears.

### 5.3.3 Social desirability bias

A third way in which CDR may have affected the direct value of contributing (independent of the public good produced) is through the signal value of contributions to external development agencies. Communities could have surmised that we were assessing community-spiritedness and that better performers might be rewarded by future projects. Conceivably, experience with the CDR program could have made such beliefs more likely, or have increased the value communities put on good relations with NGOs.

In the follow-up survey, we asked respondents about twelve actions they might take "to try to change the situation ... if you had concerns about how things were going in your village." Two of the actions offered were "appeal to local NGOs for assistance" and "appeal to the international community for assistance," with possible responses being that this would "make things worse," "make no difference," "help a little," or "help a lot." We also asked each respondent to select from the list of actions the ones they thought would be most effective and second most effective. These questions allow us to create a measure of trust in NGOs and "the international community." Our composite index, comprises questions about whether appeals to national or international NGOs would help and whether this action would be either most or second-most effective.

The fifth column of Figure 4 shows that there is no significant CDR treatment effect on this measure. Indeed, in mixed areas, conditioning on quarter, the relationship is negative and close to significant at the 90% level. If we disaggregate the measures by questions about NGOs and "the international community" we find that in CDR communities, slightly greater percentages say they think appeals to NGOs may be helpful and among the most effective actions, but the differences are not close to significant. For the questions about the international community, CDR treatment associates with *lower* trust, though the differences are again insignificant. Finally, we note that these measures of trust in non-governmental organizations do not correlate with contribution decisions at the village level.

Thus, experiencing the CDR program did not increase community members' beliefs that NGOs or "the international community" would be able to "make things better" if one had "concerns about how things are going" in one's village. Since respondents certainly associated

our public goods game with NGOs and the international community, this is evidence against the hypothesis that CDR increased contributions in the game by increasing community members' desire to please, or belief in the efficacy of, foreign NGOs. It should also be noted that this mechanism cannot account for the existence of a CDR treatment effect in the mixed gender but not the all women groups.

### 5.3.4 Participatory Decision Making

A fourth possible channel is that by promoting participation in collective decision making, CDR raised the value of contributing to joint projects, for any level of public good actually provided. Three recent studies support a logic of this form. In lab experiments, Dal Bó, Foster, and Putterman (2010) find that use of democratic procedures independently increased the effect of a given policy on cooperation in a public goods game (that is, use of elections themselves increased cooperation, rather than only via the policy chosen). Hamman, Weber, and Woon (2011) find that electoral delegation to a leader increased contributions despite moral hazard temptations facing the leader and Baldassarri and Grossman (2011), deploying lab experiments in rural Uganda, that participation in elections to select third-party enforcers increased contributions to public goods without direct effects on the characteristics of leaders.

The fifth column in Figure 4 provides evidence broadly supportive of this channel. CDR has a large positive effect on use of participatory approaches as measured by an index that includes responses on whether the selection of leaders was by election, the selection of projects was done by voting, as well as four other questions on popular participation in decision making. The effect exists, moreover, only for communities in which the game was played by both men and women; there is no evidence of a CDR effect on more democratic procedures in areas where the game was played by women alone. Finally, there is a strong correlation between participatory procedures and meeting participation and contributions at the community level. The evidence from the first column that participants were not more supportive of projects (particularly in the mixed areas) suggests that the effect of participation is not due to its bringing about more desired projects.

We note, however, that while these results are consistent with a legitimacy channel, other interpretations are also possible, including the possibility that collective meetings indicate greater mobilization efforts and/or provided a setting for strategy coordination.

## 5.4 Coordination

Under some conditions, contribution to a public good can take the form of a coordination dilemma in which some participants want to contribute if and only if others also contribute.

This can arise if the benefits to the public good exhibit increasing returns or if individuals have preferences consistent with 'strong reciprocity' (Bowles and Gintis, 2004). Under such conditions, two communities that are identical with respect to the three families of mechanisms described above may fare differently depending on their ability to coordinate. There are two distinct ways in which CDR could affect the logic of coordination. First, it may generate the kind of preferences that turn public goods problems of this form into a coordination game, rather than a prisoners' dilemma. Second, CDR could make coordination on good outcomes easier (for example by aligning expectations).

Regarding the first possibility, we asked game players what they thought other players were likely to do; their answers allow us to look at the correlation between their beliefs about others' behavior and their own actions. Figure 5 shows the estimated marginal effect of higher reported beliefs about others' actions on one's own contribution decision. The effects are estimated using ordinary least squares and controlling for village fixed effects, so that not only are features such as quarter accounted for, but also the 'truth' about village level contributions. Thus the results reflect the effects of having unusually high or unusually low beliefs about the actions of others. Two patterns stand out. First, in general there is a strong positive relationship, which is consistent with coordination preferences. Second, the size of the correlation is similar in treatment and control communities overall, although among women players, CDR is associated with a higher correlation between one's own actions and beliefs about others' actions (the difference between the effects in treatment and control areas for women is significant at the 90% level). This might be construed as weak evidence in favor of the proposition that CDR increased coordination preferences in this subgroup; however, it does little to explain variation in contributions since as seen in Table 3, the marginal effect of CDR is not in fact greater for women.

There is more support for the second possibility – that CDR facilitated coordination on higher contributions in a situation with multiple equilibria. We constructed indicators of each player's beliefs about the contributions of other players as well as indicator of the accuracy of those beliefs. From the final two columns of Figure 4, we see that in CDR areas players do indeed expect that other players will contribute more. This effect is concentrated entirely in communities where the game was played by both men and women, although it is weakened when we account for quarter. Expectations are also associated with actual contributions at the village level, suggesting that players do have reasonable knowledge regarding the likely play of their peers on which they could condition their strategy if they wanted to; moreover, this relationship remains reasonably strong after accounting for quarters.

Thus, on coordination, we find that in communities exposed to CDR, individuals expect

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others to contribute more (and are more likely to be correct in their expectations). Moreover, in general, players that expect others to contribute more also contribute more themselves (Figure 5). The first pattern is stronger in the mixed gender condition but not the second, suggesting that the game may have had a coordination component in all areas, but CDR treatment played a role in improving coordination in mixed areas only. While stronger than other evidence we have surveyed so far, it is important to emphasize that this evidence too is speculative and that the relationship between expectations and behavior is not identified.
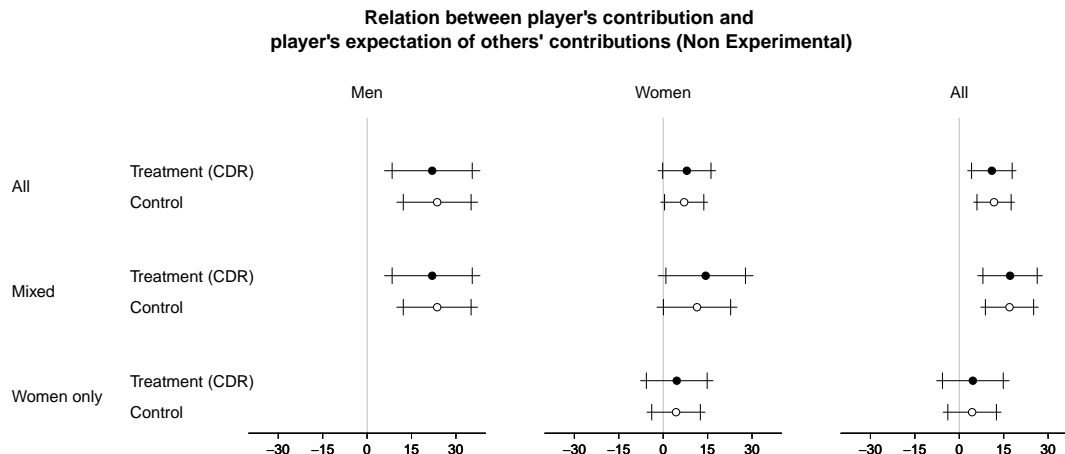
**Relation between player's contribution and
player's expectation of others' contributions (Non Experimental)**



Figure 5: Regression estimates for player's beliefs about other's average contribution (scaled to 1 sd in control communities). Dependent variable is player's contribution in Liberian Dollars. Sample broken into 8 disjoint strata according to gender of respondent, CDR treatment status, and gender treatment status. All estimates account for village-level fixed effects. For variable definitions see Table 3.

## 5.5 Mobilization

We have examined a set of mechanisms through which CDR may have altered behavior in the public goods game. Broadly, the evidence is negative on the mechanisms by which CDR might have directly affected community members' economic and social value of the public good. It is mixed on mechanisms by which CDR might have affected individuals' costs and benefits of contributing per se, with some positive evidence that CDR increased the perceived legitimacy of the community effort. There is also evidence consistent with CDR improving communities' ability to coordinate expectations about how to play the game.

Any of these mechanisms could have been affected directly by CDR or indirectly through mobilization efforts in the week between our first visit and the play of the game.

CDR could have increased the capacity for collective action by giving communities and

their leaders experience with a similar problem. As in the CDR program, communities were being asked to select a project, choose a set of representatives, and, if they wished, to mobilize community members to contribute to a public good (recall that the CDR program involved community contributions of labor). In principle, the CDR experience might have created or added to a cadre of leaders with experience organizing and mobilizing community participation, and so better able or more likely to coordinate to spread to information about the game to all potential players.

To assess whether CDR improved collective action capacity by strengthening the ability of communities to mobilize cooperation, we asked the game players questions about whether community meetings had been held to select community representatives and to choose the projects, about their personal attendance and overall attendance at these meetings, and about whether they had personally been contacted and urged to stay home on game day, to contribute if asked to play, or given information about the projects and the game. We also asked a range of questions that tapped their knowledge of the exercise: Had they heard of the planned project selected by the community? If meetings were held, did they know who organized them and how many people attended? Did they know the names of the community representatives and what projects were chosen? In the chief and community representative surveys, we asked leaders whether they had organized community meetings about the game and projects, and whether they had contacted individuals about staying home and how they should play the game.

Figure 6 summarizes the results regarding the impact of CDR on these measures of mobilization activity and game-player knowledge. In communities where both men and women were selected to play the game, CDR treatment is associated with significantly greater reports of community meetings, game-player knowledge, game-player reports of contact by elites, and elites' reports of contact with potential players. Among the three sets of respondents, CDR impact on the community representative reports of contact effort are positive but not statistically significant. For the game player reports, these effects in the mixed groups weaken in terms of statistical significance when we block on quarter status, though the estimated ATEs remain positive and non-trivial in substantive terms. Elite reports on contact activity remain strong and significantly positive even when controlling for quarter.

By contrast, in communities where only women participated in the game, there is no evidence that exposure to CDR is associated with more community meetings, game player knowledge, or contact efforts. Thus, we have a mechanism that mirrors the pattern observed with contributions: A positive impact of CDR in mixed gender groups, and zero effect in the communities where only women could play the game.

These same patterns hold at the individual level, when we compare women in the mixed groups to women in the all women groups on their responses to the questions about community meetings, knowledge of the game and projects, and contact experience. We find generally strong positive effects of CDR treatment on women in the mixed groups, but no effects at all for women in the all women groups (including controlling for quarter). CDR is associated with evidence of significantly greater mobilization efforts of women (and men) in communities where both genders had to play the game, but not for women in the all women groups.

Mobilization effects can also be observed in the choice of leadership. Recall that communities were asked to select a set of representatives to receive and manage the funds for the public goods project.

The final columns in Figure 6 shows the effect of CDR on survey responses to questions about the prior leadership activity of these community representatives. According to the game players, the CDR treatment is associated with significantly more experienced representatives where the game was played by a mixed gender group, but no significant difference in experience where only women were permitted to play the game.[24] The differences may in part stem from the use communities made of structures put in place by the CDR program. We asked the community representatives directly "were you a member of a donor-sponsored community development council?" Since the CDR program created CDCs, we might expect to find a positive effect and indeed we do: about 68% say Yes in the CDR communities, versus 46% in the control communities, a strongly significant difference that remains just as large and statistically significant when we condition on quarter. However, when we distinguish between mixed and all women groups, we find that the effect is larger in mixed, while smaller and statistically insignificant (though positive) in the all women groups (final column, Table 6.

Finally, we also see from the lower panel that more "mobilized" communities give more in the public goods game; this is true both for the mixed gender and all women groups suggesting that mobilization may matter everywhere, even if the CDR program only affected the extent of mobilization in communities where both men and women played the game.

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[24]Similar patterns hold if we examine the representatives' own accounts of leadership experience, but not if we use reports by the chiefs. The pattern is not explained by the gender composition of community representatives. The all women groups did have a somewhat higher frequency of women as community representatives (59% versus 48% in the mixed groups), but the pattern persists when we condition on gender of the representatives.

**Effects of CDR Treatment on Mobilization Variables**
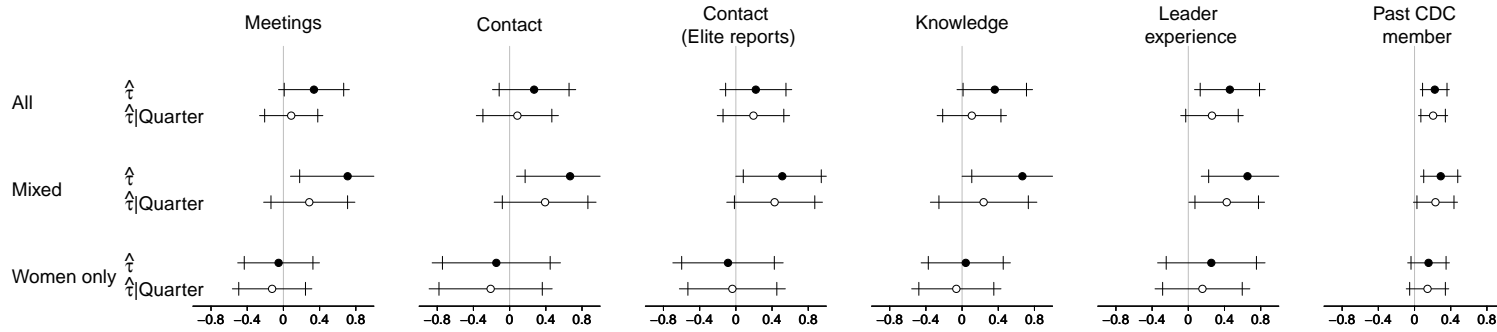


**Relation between Mobilization Variables and Average Contributions (Non Experimental)**
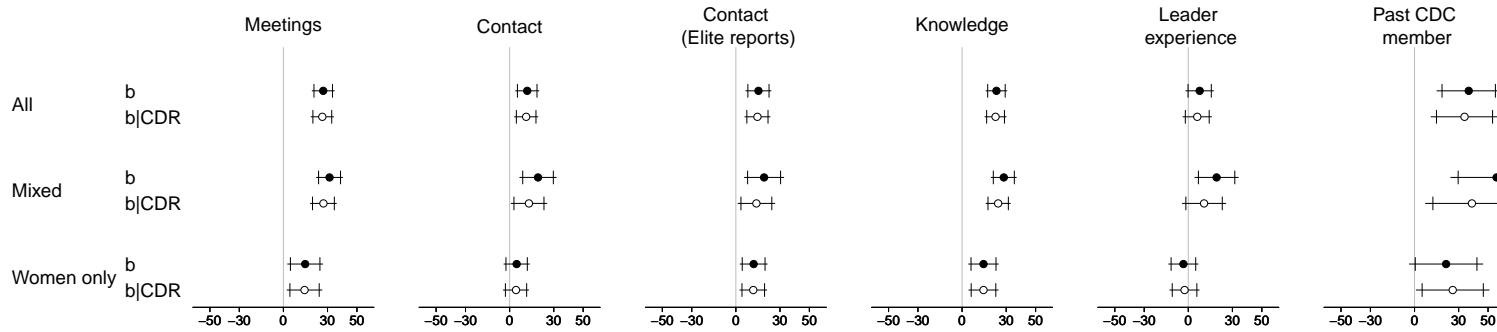


Figure 6: The upper panel show the CDR effect on mobilization variables; the lower panel shows the relation (regression coefficients) between these intermediary variables and outcomes. Note that while estimates in the upper panel are identified by the randomization, those in the lower one are not. For variable definitions see Table 3.

**Differences between Women only and Mixed areas
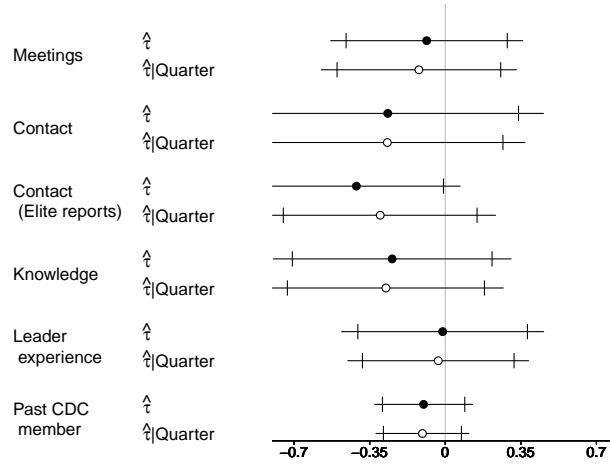among CDR communities on Mobilization Variables**



Figure 7: Comparison of intermediary outcomes between women-only and mixed gender CDR areas for mobilization measures. For variable definitions see Table 3.

# 6 Why Did CDR Increase Contributions in Mixed Groups Only?

The evidence on the impact of the CDR program is striking, but the results suggest heterogeneous effects. Exposure to CDR increased collective action capacity as measured by a public goods game, but only in those communities where 12 men and 12 women were randomly selected to play the game. In those communities where the behavioral game involved only women, there is no evidence that the CDR program had an impact on the ability of communities to solve collective action problems. Likewise, there is a powerful effect of having only women play the public goods game, but one that depends on CDR treatment status. Contributions are higher in women-only groups, but only in communities that did not receive the CDR program.

Examination of Figures 4 and 6 suggest that key 'second-stage' relationships work similarly in communities, regardless of the gender composition of game players. For example, the relationship between most measures of mobilization and contributions is similar across areas, and the relationship between expectations (and accuracy of expectations) and contributions

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is also similar. However, in the first stage where we examine the impact of CDR on interme-diary variables, communities exhibit different patterns of behavior depending on the gender composition of game players. The CDR program is powerfully associated with patterns of mobilization in areas where both men and women played the game, but has no impact on mobilization where only women had to make contribution decisions (Figure 6, upper panel). Similarly, exposure to CDR is related to measures of the expected contribution and accuracy of prediction in mixed gender areas but not in women-only areas.

The results on mobilization, in particular, plausibly account for higher contribution levels in CDR-treated communities, where both men and women played the game. CDR increased mobilization in mixed areas but not in women only areas. This does not mean however that as a result of CDR there were greater mobilization efforts in the mixed gender CDR areas than in the women only CDR areas. As shown in Figure 7, conditional on CDR, levels of mobilization do not differ systematically depending on the gender composition of game players. Among communities that received the CDR treatment, local leaders invested in mobilization activities at about the same level, regardless of who was expected to participate in the game.

A possible explanation for the difference is that communities drew on different people and networks for mobilization in the mixed versus the women-only communities. Where communities understood that a random sample of community members would participate in the game, their task was to generate high levels of contribution from both men and women in order to maximize the funds available for a community project. In women-only areas, leaders knew that the funds available to the community would depend only on the actions of a random sample of women. Thus, the impact of the CDR program may have been to develop mixed-gender structures (CDCs) and prior experience with community-wide mobilization that were relevant for solving the collective action problem presented to mixed gender groups. However, where only women were involved in the public goods game, alternative networks – including well-established women's organizations, such as secret societies – could be used to generate high levels of contribution. In these cases, the structures and experiences generated by CDR simply may not have been relevant given the nature of the collective action problem they confronted.

What little evidence we have that bears directly on this hypothesis is mixed. We asked the community representatives both if they had been a leader of a "woman's action group" and if they had previously been a chief, which in the case of female community representatives would most likely have been "lady chief."[25] On the one hand, in the all women groups, female community representatives are twice as likely to report having been a lady chief – 19% versus

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[25]In general, women do not serve as chiefs of communities, but all these communities have other formal positions, including "lady chief."

8% in the mixed groups ($p = .13$). But the difference goes the other way for female community representatives who said they were leaders of a "woman's action group" (38% in all women versus 60% in mixed, $p = .03$).

So while the data suggests that alternative organizational structures were employed to mobilize cooperation in communities exposed to different gender composition treatments, we have not identified what these structures are.

# 7    Conclusions

A field experiment in which villages in northern Liberia were randomly assigned to receive international development assistance provides evidence that the introduction of new local-level democratic institutions can alter patterns of social cooperation in a way that persists after the program's conclusion. Villages exposed to the development program exhibit higher subsequent levels of social cooperation than those in the control group, as measured through a series of survey based indicators and in behavior in a community-wide public goods game. The estimated effect in the public goods game is of a similar magnitude to the effect of a quadrupling of interest rates. These results suggest that changes in community capacity for collective action can take place over a short period of time; can be the product of outside intervention; and can develop without fundamental changes either to the structure of economic relations or to more macro-level political processes. Random assignment of communities to treatment provides confidence in the causal nature of the relationship and the use of behavioral outcome measures reinforces our sense that the effects are real.

Unexpectedly, however, we found strong heterogeneity in the measured effect of the CDR program. For collective action problems that were to be solved by mixed gender groups, we found CDR very large effects; for problems to be solved by women only groups, the estimated effects were zero or negative.

Understanding the mechanism(s) underlying this relationship is more difficult and our results are more speculative. Nevertheless, they suggest that the effect of the introduction of elected community development councils does not stem from them increasing public satisfaction with the projects chosen or greater trust that leaders will not abscond with community finances. Nor is there evidence that CDR increased the weight that community members put on community welfare. The evidence also does not support the possibility that CDR reduced the perceived costs or benefits of contributing either by raising the prospects of sanctioning for failure to contribute, by increasing the expected gains from signaling community cohesion to NGOs, or by reducing the marginal utility of income. There is some evidence that the CDR program resulted in greater use of electoral and participatory processes for project

selection and it is possible that, even though the content of the projects seemed unaffected by this, increased legitimacy surrounding the community projects increased the benefits from contributing. There is also evidence that coordination dynamics were at work — individuals in (mixed gender) treated areas were more likely to know how others would play and correctly expected higher contributions. Moreover, individuals who thought others would contribute highly contributed more themselves.

We also find relatively strong evidence that experiencing the CDR program strengthened community mobilization capacity which suggests that the effect of CDR worked through greater mobilization efforts. In the mixed-gender communities, CDR areas in general had leaders with greater experience who ran more meetings, communicated more with village members, and imparted greater understanding of the process. All of these features are associated with greater levels of contributions. They are all features, moreover, that may have provided a level of common knowledge that could facilitate the operation of the coordination dynamics described above.

In sum, the explanation that we think makes the best sense of the data is a simple one. Communities saw the collective action problem as a kind of coordination problem and the CDR program provided experience in organization that helped them to solve it. The problem we gave communities was similar in some basic respects to that faced by the communities that had gone through the CDR program. Where communities knew that both men and women could be selected to play the game, prior CDR treatment was associated with greater and more effective mobilization by more experienced leaders. This suggests that prior experience with organizing for cooperative tasks through deliberately mixed-gender CDC committees and community-wide processes may have created human resources that could be drawn upon to resolve coordination dilemmas, at least for mixed gender groups.

Under this interpretation, the finding of sharply different CDR treatment effects in the mixed versus women-only groups has important implications for understanding when CDR programs are effective. When we introduced the gender variation to the behavioral games, we conceived of it as a cross-cutting manipulation distinct from the CDR treatment. However, it can also be thought of as providing two ways of measuring the impact of the CDR program. In effect, we have one measurement of the effect of the CDR program on collective action problems facing mixed gender groups, and one for problems facing women-only groups. We found evidence that CDR improves collective action capacity for the former but not the latter.

Although this finding emerged as an artifact of design variations in our behavioral game, it may have broader significance, including for donor agencies. It highlights that what might matter is not *how* decisions are being made, but *who* is making them—a feature that we did

not allow communities to determine for themselves when we took our measurements. Insofar as collective action problems need to be addressed by broad cross sections of a community, the exposure to inclusive decision making of the form promoted by external actors may have large effects. However existing structures of authority are often organized around single-gender or other more narrowly constituted groups. Our results suggest that effects may be much more modest, or absent, if these structures are deployed when groups face collective action problems.

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# 8 Appendix: Variable Definitions and Summary Statistics

| Variable | Definition | Source | $n$ | Min | Max | sd |
|---|---|---|---|---|---|---|
| CDR | Treatment: Village Participation in IRC CDR project | Project files | 83 | 0 | 1 | 0.5 |
| Mixed | Treatment: Communities in which 50% men and 50% women took part in the public goods experiment | Project Files | 83 | 0 | 1 | 0.5 |
| Women only | Treatment: Communities in which only women took part in the public goods experiment (1-Mixed) | Project Files | 83 | 0 | 1 | 0.5 |
| Interest rate | Treatment: Multiplier on individual contributions ($0 = \times 2, 1 = \times 5$) | Project Files | 83 | 0 | 1 | 0.5 |
| Quarter | Indicator for whether a hubtown is a neighborhood of an urban area | Project Files | 83 | 0 | 1 | 0.47 |
| **Items in Figure 1** | (Baseline measures) | | | | | |
| # Households | # Households in village | United Nations | 83 | 33 | 1500 | 315 |
| Household size | Village Average | BL: 16 | 79 | 4.5 | 7.9 | 0.8 |
| House quality | Mean effects index: # Rooms and doors, mud brick or better walls, zinc roof, piped water (6 items). | BL: 45-47. | 79 | -2.1 | 2.7 | 0.93 |
| Food / livestock | Mean effects index: # Chickens, sheep, guinea fowl, # meals/day, tins of rice planted (5 items). | BL: 45, 49, 56. | 79 | -2.2 | 2.8 | 0.97 |
| Primary education | Household members that completed primary education | BL: 28 | 79 | 0 | 38 | 8.2 |
| Share injured or died | Percentage | BL: 32, 34 | 79 | 0 | 23 | 4.6 |
| Share excombatant | Percentage | BL: 33 | 83 | 0 | 22 | 4.1 |
| Share Mandingo | Percentage | BL: 27 | 83 | 0 | 100 | 28 |
| **Items in Figure 2** | (Endline survey measures) | | | | | |
| Food | # Mean effects index: Chickens, sheep, guinea fowl, # meals/day, # tins of rice planted (5 items). | EL: 34, 38, 45, | 83 | -1.96 | 3.94 | 1.09 |
| Assets | Mean effects index: # Wooden beds, foam mattresses, radios, and buckets (4 item). | EL: 45. | 83 | -1.42 | 2.58 | 0.91 |
| House Quality | Mean effects index: # Rooms and doors, walls of mud brick or better, zinc roof or not, piped water or not (5 items). | EL: 34-36, 40 | 83 | -1.5 | 3.19 | 0.84 |
| All material | Mean effects index comprising elements in previous three measures (14 items). | See above | 83 | -1.69 | 2.79 | 0.87 |
| Democracy | Mean effects index: attitudes on individual right to question leaders, clientelism, role of all in decisions, importance of elections, community as primary decision makers for collective problems (6 items) | EL: Q105-107, 109, 84, 89 | 83 | -1.88 | 2.49 | 0.83 |
| Efficacy | Mean effects index: attitudes on need for top down decision making, ability to influence decisions, satisfaction with decisions. Effectiveness of: making complaints, organizing, making appeals, protesting, voting. Willingness to talk in town meetings (15 items). | EL: 95-96, 98-100, 129 | 83 | -2.12 | 2.34 | 0.92 |

| Variable | Definition | Source | $n$ | Min | Max | sd |
|---|---|---|---|---|---|---|
| **Items in Figure 2** | (Continued) | | | | | |
| Participation | Mean effects index: Community initiatives for public goods, initiatives for various community matters or projects (27 items). | EL: 121, | 83 | -1.97 | 2.77 | 1.17 |
| Women's Rights | Mean effects index: women should have equal rights, women should be decision makers, women do participate in decisions (3 items). | EL: 97, 108, 129E | 83 | -3.48 | 2.65 | 0.96 |
| Ex combatant | Mean effects index: whether excombatants should be tolerated, welcomed, allowed to lead, friends, kin; whether they add to quality of life; would provide loans, are trustworthy, benefit too much (11 items). | EL: 66, 69, 72, 73, 93, 132, 163 | 83 | -2.2 | 2.72 | 1.07 |
| Migrants | Mean effects index: whether migrants should be tolerated, welcomed, allowed to lead, friends, kin; whether they add to quality of life in town; would provide loans, are trustworthy; divisions between natives and migrants (21 items) | EL: 73, 132, 126 | 83 | -3.29 | 2.29 | 1.02 |
| Trust in leaders | Mean effects index: can subjects borrow from leaders? trust them to mind their children? complain to them? Are leaders' relatives favored? Are they trustworthy (2 items)? Would they steal from community? Would subjects act as guarantor for them? (8 items) | EL: 72(4), 73(4), 93, 100, 102-104, 132(G) | 83 | -2.6 | 2.74 | 1.03 |
| Social Tensions | Mean effects index: Are there divisions based on class, gender, age, migration, religion, ethnicity, education, other (8 items)? Have these resulted in violence (8 items)? Has there been theft of animals or items? fighting? armed violence in town? violence in household? (21 items) | EL: 126, 127, 130 | 83 | -2.66 | 1.97 | 0.99 |
| Social Capital | Mean effects index: Existence and membership in eight kinds of social organization. | EL: 125(A-H) | 83 | -2.31 | 2.56 | 0.99 |
| **Figure 4** | | | | | | |
| Satisfaction with projects | Mean effects index: Project was among the most important things for village; liking projects was a factor in decision making (2 items) | GS: 47 (or 50(2)), 67a, | 83 | -3.42 | 1.51 | 1.02 |
| Trust in leaders | Mean effects index: Are leaders trustworthy (2 items)? Would they steal from community? Would they use money effectively? Would representatives benefit disproportionately? Or the chief? Was believing money would be used well important for decisions? (7 items) | GS: 18-20, 32 (or 41), 51, 52, 67c | 83 | -2.09 | 2.13 | 0.97 |
| No anonymity | Did subjects think it likely that others villagers would find out how much they contributed? | GS: 67 | 83 | -1.07 | 3.06 | 1.03 |

| Variable | Definition | Source | $n$ | Min | Max | sd |
|---|---|---|---|---|---|---|
| **Figure 4** | Continued | | | | | |
| Social Desirability | Mean effects index: Index on whether individuals would appeal first to local or international community for assistance? likely effectiveness of the appeal? (4 items) | EL: 100 | 83 | -2.54 | 2.12 | 0.87 |
| Participation Index | Mean effects index: were there community meetings to discuss game? who organized them? were community representatives elected? were meetings public? was there competition for positions? did some say that they preferred different representatives? are Community Representatives relatives of the chief? was the process of selecting projects considered important for decisions? (8 items) | GS: 25, 27, 33-36, 37 (or 39), 67b | 83 | -2.17 | 2.28 | 0.97 |
| Expectations | Mean effects index: Of the other players, how many contributed all? how many nothing? what did most do? | GS: 58–59 | 83 | -3.5 | 1.32 | 0.88 |
| Accuracy of expectations | Mean effects index: Squared deviation between expected and actual number contributing 300, squared deviation between expected and actual number contributing nothing. | GS: 58–59 | 83 | -2.97 | 1.43 | 0.89 |
| **Figure 6** | | | | | | |
| Meetings | Mean effects index: Existence and participation in community meetings (5 items) | GS: 24–26, 28, 54 | 83 | -1.98 | 1.99 | 0.89 |
| Contact | Mean effects index: Were game players contacted about the game, or the project? were they asked to stay home? urged to contribute? | GS: 55, 56, 57, 66 | 82 | -0.8 | 1.4 | 0.5 |
| Contact (Elite Reports) | Mean effects index: Did elites contact population to discuss the game to discuss the project? Encourage people to spread the word? did they tell them to stay home? urge them to contribute? (3 items) | CR: 64(1), 65, 66, 67 | 83 | -2.22 | 1.33 | 0.93 |
| Knowledge | Mean effects index: Do players know what the project is? do they know who organized the meetings? how many attended? who the community representatives are? what the projects are? (5 items) | GS: 22, 27-9, 42 | 83 | -1.72 | 2.46 | 0.98 |
| Leader Experience | Mean effects index: Have community representatives played a leadership role in the community before?(2 items) | GS: 31, 40 | 83 | 1.61 | 5.52 | 0.89 |
| Past CDC member | What share of community reps were former CDC leaders? | CR: 34 | 75 | 0 | 1 | 0.37 |

Table 3: Table of Variables used in text. Baseline survey (BL) (`http://tinyurl.com/fhw2011hhbls`), Endline survey (EL) (`http://tinyurl.com/fhw2011hhel` ), Postgame survey (GS) (`http://tinyurl.com/fhw2011gs`), Community representative (CR) survey (`http://tinyurl.com/fhw2011cr`), Chief Game survey (CG) (`http://tinyurl.com/fhw2011cg`)