

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

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## **NOT FOR CIRCULATION OR CITATION**

**COVER NOTE:** Dear EGAP participants: This is the first draft of a paper describing the core results from a field experiment on an intervention that introduced local democratic governance institutions in northern Liberia in 2006-8 in order to strengthen community cohesion. Using a public goods game to measure cohesion, we find that the intervention achieved its objectives. Here we provide tables with our main findings as well as supplementary data on the mechanisms that might account for the relationship between the intervention and the outcomes. Our main questions for EGAP members relate to (1) how best to frame the paper and whether the democratic innovation framing is successful or whether a broader institutional framing would be more compelling (2) the mechanisms that could be at work, whether the set we examine is compelling, and how better to parse evidence in favor of one or other account.

## Introduction

Communities often confront the difficult task of inducing individuals to act in ways that may run counter to their individual self-interest. These problems of “joint action”—which arise, in the words of Josiah Ober, “whenever groups composed of self-interested and interdependent individuals seek to develop and carry out cooperative plans” (2008, p. 7)—are a universal feature of political organization (Olson 1965). In facing these shared challenges, the ability of political units to generate and sustain cooperative behavior is part of what distinguishes successful states from those that fail (Ober 2008), communities with high levels of public goods provision from those that remain without essential services (Habyarimana et al 2009), well-functioning democracies from those that underperform (Putnam 1994), and political movements that achieve power and influence from those that find themselves stuck on the sidelines (Chong 1991; Tarrow 1994).

Given the centrality of a political community’s “collective action capacity” to a wide variety of consequential outcomes, the determinants of social cooperation have long been a focus of scholarly attention. Two key facets of the conventional wisdom stand out. First, prior research suggests that cooperative behavior is a relatively fixed characteristic of a community—reflective of demographic, economic, and political factors that are difficult to change—and thus is unlikely to respond to short-run interventions. Second, while political institutions likely matter for collective action capacity, it is hierarchical, command-and-control arrangements that have an advantage in mobilizing individuals to take actions that are not individually rational. Democratic institutions, on the other hand, are thought to impede joint action (Ober 2008).

This paper investigates these claims in the context of a field experiment to evaluate the impact of a community-driven reconstruction (CDR) project carried out by a major international NGO in post-conflict Liberia. The project introduced democratic, community-level institutions for making and implementing decisions about local public goods into a political environment long-dominated by hierarchical, hereditary institutions of chieftainship. We exploit this intervention in an effort to assess whether patterns of social cooperation are actually responsive to these new institutions, even when underlying demographic, economic, and political factors remain unchanged. Moreover, we compare observed levels of collective capacity across villages in an effort to evaluate whether democratic institutions in fact undermine a community’s capacity to achieve joint action.

Our field experiment advances the study of social cooperation in several ways. First, we develop a novel strategy for measuring collective action capacity which involves the administration of an anonymous, community-wide public goods game designed to capture actual patterns of behavior. This represents an improvement over standard, survey-based methods that, especially in the context of a program designed to promote social cooperation, are vulnerable to respondents misrepresenting their real views in an effort to please funders or simply because they understand the “social desirability” of reporting high levels of cooperation. Second, in assessing whether collective action capacity is responsive to the introduction of new institutions, we gain significant explanatory leverage from the fact that the democracy-building program was randomly assigned across villages in northern Liberia. As a consequence, our estimates represent the *causal* effect of creating new democratic institutions, bypassing typical concerns about endogeneity and omitted variable bias that plague most studies of the impact of political institutions on outcomes. Strikingly, we find that

collective action capacity is responsive to the introduction of new institutions, even without fundamental changes either to the structure of economic relations or to more macro-level processes, and that political communities in which new, democratic institutions were constructed outperform villages in which traditional, hierarchical institutions remained unchallenged. Finally, given the powerful evidence of a *democratic advantage* in social cooperation, we explore the mechanisms that might explain how democratic institutions create the basis for more effective joint action.

## **Collective Action Capacity**

Political units—regardless of their scope or scale—face a common challenge of generating and sustaining cooperative behavior. Composed of individuals with distinct preferences but a shared instinct to put self-interest above all else, political organizations often struggle to identify a common goal, to coordinate the expectations of their members about how others will behave, and to police the behavior of individuals who often have strong incentives to deviate from an agreed upon path.

Self-interest is a big part of what gets in the way of social cooperation. Standard models of self-interested behavior generate predictions of collective action failure as individuals can benefit from the production of public goods or the preservation of shared resources even if they do not contribute to the effort to produce or protect them (Olson 1965; Hardin 1982; Ostrom et al 2002). Expectations are critical as well, because even if individuals wish to cooperate with others in achieving a common outcome, they may not do so unless they believe that others will cooperate with them. Without shared expectations about how to behave, the chance for productive cooperation is often lost (Weingast 1997; Chwe 2001).

While social cooperation is a useful tool for achieving many desired outcomes (such as greater material wellbeing, security, political influence, and so on), the level of collective action capacity varies considerably across political communities. The most pessimistic models—which predict no successful collective action—offer little explanatory leverage over this variation, as instances of successful collective action are many. At the same time, collective efforts do not always succeed, even though a number of distinct mechanisms for generating successful cooperation (i.e. repeated interaction, selective incentives, norms of reciprocity, other regarding preferences, etc.) have been identified. Thus the question of why some communities exhibit greater collective action capacity than others remains a central issue in political science.

## **The Impact of Institutions on Collective Action Capacity**

Recognizing the central role that community-wide collective action can play in the development process, international donors have focused increasing attention on small-scale interventions designed to increase collective action capacity by engaging beneficiaries in the design and management of development projects. This emphasis on process is an explicit attempt to foster greater social cooperation so that aid-driven interventions can be sustained after donors leave and subsequent shared challenges can be more effectively addressed by communities on their own. Interventions of this type—often referred to as “community-driven development” (CDD) or “community-driven reconstruction” (CDR) programs—typically involve the introduction of

participatory, democratic institutions that are then employed in the process of selecting, designing, and implementing development initiatives. By conservative estimates, the World Bank's lending alone for CDD projects exceeded \$2 billion in 2003 (Mansuri and Rao 2004)

Can these brief, foreign-funded efforts to build local institutions in fact have positive effects on local patterns of cooperation? Moreover, does the introduction of (and experience with) *democratic* institutions erode or enhance a community's subsequent collective action capacity?

Prior research suggests that the introduction of new political institutions by outsiders is unlikely to substantially alter patterns of social interaction in a community, and that the ability of a community to act collectively is the result of a slow and necessarily indigenous process. Scholars have argued that patterns of cooperation are an outcome of long-run evolutionary mechanisms (Bowles and Gintis 2004); have deep historical roots in critical junctures that reshape social relations, such as the 11<sup>th</sup> century in medieval Italy (Putnam 1994) or the era of the slave trade in Africa (Nunn and Wantchekon 2009); or reflect relatively fixed characteristics of communities, such as ethnic heterogeneity or the distribution of wealth (Alesina and La Ferrara 2005; Boix and Posner 1998). Recent work that measured patterns of social cooperation in fifteen small-scale societies linked differences across communities to the extent of market integration, a variable that reflects where a given community rests on the long, slow path toward modernization (Henrich et al 2002). In addition, aid workers often return from the field demoralized by an impression that, rather than reshaping underlying power relations, foreign aid projects are easily captured by existing power brokers, a view that resonates with findings by economists (Gugerty and Kremer 2006) and anthropologists (Murphy 1990; Ensminger 2007) who have studied the implementation of development programs. So in short, without substantial changes to the underlying demographic, economic, and political factors that shape people's preferences and interests, one might be skeptical that the introduction of new institutions (designed and implemented by outsiders) can have much of an impact on a community's collective action capacity.

Moreover, while participatory, democratic institutions are justifiably embraced for the values they embody including freedom, equality, and dignity—and thus favored by international donors interested in cultivating institutional change—it is not at all apparent that such institutions will enhance, rather than undermine, the ability of a community to act jointly in pursuit of a common goal. Indeed, paraphrasing Ober (2008), successful social cooperation makes a lot of sense when it is predicated on hierarchy—an empowered individual issues an authoritative command, the recipients of that command receive instructions that are publicly known as to how to behave, and there are consequences for not behaving in the prescribed way.

Participatory democracy, on the other hand, seems to have a number of features that might get in the way of successful social cooperation. Processes that engage relatively large and diverse groups of community members in determining how to achieve social cooperation and for what purposes are transactions-intensive (and therefore costly and time-consuming), bring to the fore wildly divergent preferences (without necessarily yielding a commonly-held position), and generally do not produce a leadership structure with the unambiguous mandate to herd individuals toward meeting a single, common objective. Indeed, long-time observers of democratic processes, such as de Tocqueville, warned that participatory institutions generate instability and unpredictability; later theorists formalized some of these concerns, showing that the stable aggregation of diverse preferences is impossible under democratic voting rules (McKelvey 1976) and that, in distributive environments, majority rule equilibria involve instability over time (Kalandrakos 2004).

Nevertheless, beyond its moral virtues, there are some reasons to think that participatory democracy may have an advantage over more hierarchical, authoritarian alternatives when it comes to the promotion of successful collective action. First, democratic institutions might increase the ability of a community to coordinate on an equilibrium which stipulates that people should cooperate and that *social sanctions* should be applied when individuals fail to cooperate, especially if such sanctions must be administered through a decentralized process of monitoring and oversight by fellow citizens.

A second possibility is that participatory democracy produces outcomes (i.e. a common goal, a structure for managing and distributing the benefits of cooperation, etc.) that are more highly valued by a broad array of community members than decisions made in more hierarchical, authoritarian systems. By increasing *the value of the prize*, democratic institutions may make social cooperation more feasible.

Third, it may be the case that, independent of sanctions and monitoring, democratic institutions have an *organizational* advantage in the dissemination of information; perhaps by mobilizing large numbers of people to deliberate over the aims and strategies for achieving social cooperation, and setting in place institutions to engage individuals in political participation, democratic politics can more effectively align people's expectations and coordinate their actions. Democratic institutions may also increase the number of people with leadership responsibilities and experience, which may facilitate mobilization for collective action.

Finally it may be the case that individuals are more likely to take the interests of others into account when decisions are made via participatory processes. Democratic institutions might shape *preferences* in this way either because individuals are more aware of the interests of others (with participatory processes bringing these to the fore) or because the emphasis on equality embedded in democratic structures leads individuals to place a higher value on other's needs.

Thus participatory democracy may have a set of inbuilt advantages which sometimes outweigh the disadvantages that have led observers to worry about whether democratic institutions erode collective action capacity.

Answering the question of whether collective action capacity is responsive to the introduction of new institutions—and more specifically whether democratic institutions erode or enhance patterns of social cooperation—is made difficult by the fact that whatever political institutions exist in a given locale may be a function of a wide variety of other characteristics of that political unit. For example, if we found using observational data that collective action capacity increased markedly in the wake of a change in the institutions governing a society, we could not be sure that the institutional change caused this increase in collective action capacity. In fact, the institutional change might itself be a function of patterns of social cooperation. With respect to democracy, if the evidence suggested that democratic institutions outperform authoritarian institutions in resolving collective action problems, it would be difficult to ascribe that effect to the institutions per se, when the characteristics of democratic and authoritarian political units are so different on other dimensions. A field experiment—in which the “institutional treatment” is randomly assigned across political units—offers a means of isolating the impact of institutions and testing the claims in the literature about how institutions shape social cooperation.

## Research Design

To study the effect of new institutions on collective action capacity, we undertook a field experiment to evaluate the impact of a community-driven reconstruction project carried out by the International Rescue Committee (IRC), a major international NGO, in Lofa County, Liberia. Northern Liberia figured centrally in the violence that engulfed Liberia over the past fifteen years. It was a hotspot during Charles Taylor's rebellion between 1990 and 1996 and reemerged as the epicenter of a second war against Taylor's government after 2000.

Our baseline data testify to the extent of damage done to the region by the conflict. The 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 having been 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 effect, however, was one that could have a direct bearing on communities' ability to cooperate (Richards et al 2005): 85% of these individuals were displaced during the conflict and many were displaced multiple times. Of 1500 individuals we interviewed in the final survey, 25% still considered themselves internally displaced.

The CDR program, which was implemented between 2006 and 2008, had the following core components. First, villages were grouped into approximately equally-sized "communities" – based on geographic proximity and preexisting ties, each with a population of between 2000 and 3000 individuals. Then, IRC undertook initial activities to sensitize local communities to the new development project, including targeted meetings with local chiefs and elders to solicit their cooperation. In each community, IRC oversaw the election of community development councils (CDCs), with representatives chosen from among all voting-age adults in the villages. CDCs were then empowered to oversee a community-wide process to select and implement a quick impact project (approx \$2000-\$4000 in value), followed by a larger development project (approx \$17,000 in value). The members of the CDCs were selected in direct elections from among all voting age adults in the villages. CDCs managed the implementation process and continue to have responsibility for project maintenance over time.

In order to evaluate the impact of these new institutions on subsequent collective action, we employed an approach known as randomized evaluation. 42 communities were randomly assigned to receive the CDR program. These communities were selected from among 83 equally-deserving and accessible "communities," constructed from over 400 villages that were clustered together (by IRC) on the basis of preexisting ties and geography in order to facilitate program implementation. This lottery was conducted in a public place, with chiefs representing each community in attendance.

This randomization process is key to ensuring that differences in outcome measures for the project and comparison villages truly reflect the *causal* effects of the CDR program and not other features that determine selection into treatment in the first place. Those communities not exposed to CDR (the "control" communities) are, at least in expectation, no different than those who received the treatment, except for the fact of receiving treatment.

In practice, the randomization process was effective in the sense that baseline data shows that treatment and control areas are similar with respect to a large set of measures: wealth, exposure to conflict, community size, and so on (Fearon, Humphreys, and Weinstein 2006). There are, however, some differences that arose by chance: specifically, treatment areas were somewhat more likely than control areas to be rural and distant from services such as markets, transportation, and health clinics. Given the small sample size, it is not surprising that some pre-treatment differences do exist. Where differences do exist, we can control for the different characteristics of the treatment and control groups as we conduct the analysis of program impact.

Finally, it is important to 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 funds for public goods provision coupled with the creation of participatory, democratic institutions. In short, the design allows us to assess the impact of receiving the IRC CDR program versus not getting the IRC CDR program. We refer to this issue as the “complex treatment” problem. Given our interest in isolating how the institutional aspects of the treatment impact collective action capacity, we must rule out the possibility that any observed impacts of the intervention reflect economic effects that follow from the funding for public goods provision—a task we undertake in our discussion of the main results.

## Measurement Strategy

The standard approach to measuring collective action capacity involves surveying households to assess levels of trust, patterns of community activity, and the extent of associational life. But we were conscious that, particularly in the context of an intervention designed to promote trust and cooperation, individuals in treatment communities might have incentives to misrepresent their real views in an effort to please outside funders or simply because they had learned about the “social desirability” of reporting pacific, trusting relations.<sup>1</sup>

For this reason, we designed a public goods game with the goal of observing whether communities exposed to the CDR program actually behave differently than control communities after the project came to an end. The game involved the following steps. An advance team visited each of the 83 communities and gained consent for a meeting to be held to describe an opportunity for the community to receive funds for development. One week later, a meeting was convened in which community members were told that they could receive up to \$420 to spend on a development project. They were also told that the receipt of funds would depend on whether the community completed a form indicating three community representatives who would handle the funds and how the funds would be spent; 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

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<sup>1</sup> A related possibility is that the experience of working with the IRC on the CDR program increased peoples’ belief that foreign NGOs would deliver on their promises, thus making them more likely to contribute in our public goods game due to some expectation of reciprocity. However, data from a survey of chiefs suggest that, even in control communities, the level of interaction with foreign donor agencies was quite high so this expectation of reciprocity would need to have been specific to the IRC program.

two visits the community had time to select their community representatives, select potential projects, and discuss what strategies to use in the public goods game. Detailed surveys were also completed with all 24 households, the three community representatives, and the village chief.

The public goods game itself was straightforward: 24 randomly selected individuals (from randomly selected households) were given three 100LD notes (worth in total about \$5 US or close to a week's wages) and asked to decide, anonymously, how much they wished to contribute to the community and how much they wanted to keep for themselves. Half of the players were randomly assigned to have their contributions to the community multiplied by two, while the other half had their contributions multiplied by five (corresponding to interest rates of 100% and 400%). Thus each community had the opportunity to earn up to 25,200 LD.

In addition, we ran a cross-cutting experimental treatment in which in half of the communities all 24 players were women, while in the other half there were 12 men and 12 women players. We employed blocking to ensure (near perfect) balance across all treatments. The "gender composition" treatment related only to the makeup of the players for the public goods game. This treatment was announced to communities via a small change in the verbal instructions delivered at each community meeting. In all communities, however, women and men were eligible to participate in meetings to decide on projects, to serve as members of community committees, and to be beneficiaries of the project itself.

The central outcome of interest for our purposes is the degree to which communities were able to act collectively, as reflected in individual decisions (among a sample of randomly selected households) about whether (and how much) to contribute in the community-wide anonymous public goods game. With all treatments randomly assigned across villages (or individuals), the behavioral game puts us in a position to measure the causal effect of introducing new institutions on collective action capacity and to see how that impact varies with gender composition of the players and the interest rates accruing to individual contributions.

## Results

### **The Impact of Democratic Institutions on Collective Action Capacity**

Eighty-two communities successfully completed the behavioral game. In one community, the game was halted during play as a result of a rule violation. The average payout to villages was 20,022LD and the median received was slightly higher at 20,850LD (out of total possible earnings of 25,200LD). Among individuals, nearly two thirds contributed the maximum amount (300LD). Only 10% kept the endowment in its entirety.

Table 1 presents estimates of the impact of the CDR program on contributions in the public goods game. The first row shows the share of the total available funding earned, the second the average share of 300LD contributed by community members, and the third the share of individuals contributing the full amount. The final column reports average treatment effects estimated by taking

a weighted average of the differences in outcomes between treated and untreated units in the women only and in the mixed gender sites.<sup>2</sup>

[Table 1]

We see that exposure to the CDR program led to an average 6.5 percentage point (or 7.5%) gain in the share of available funds earned by the community; the average share of the 300LD contributed by households increased by an estimated 5.7 percentage points (7.6%); and the share of households contributing the full amount increased by 9 percentage points (15%). These effects are each significant at the 95% level. Levels of collective action capacity are responsive to the introduction of new institutions; moreover, communities in which democratic institutions were created exhibit higher levels of social cooperation than those in which traditional, hierarchical institutions remained unchallenged.

We can get some sense of the magnitude of the effect by examining how players responded to different interest rates. Quantitatively, the 5.7 point effect of the CDR treatment on individual contributions is about the same as the effect of a change in the social rate of return of an individual's investment from 100% to 400%. This change in interest rates yields an estimated 5.8 percentage point change in individual contributions, an effect significant at the 99% level.<sup>3</sup>

## How Institutional Treatment Effects Vary by Gender Composition

Table 2 describes how the impact of CDR varies with the gender composition of the players in the community-wide public goods game. We focus here on only one outcome measure—the average share of the 300LD contributed by a given player—although the results are similar with the other dependent variables described in Table 1.

[Table 2]

There is clear evidence that the aggregate effects identified in the last section mask considerable heterogeneity between the mixed gender and women only cases. As seen in Table 2, the impact of the institutional treatment is visible *only* in communities in which the entire community was eligible to participate in the public goods game. In these communities, the effect is very strong: exposure to the CDR program increased the average share of the 300LD contributed by households by an estimated 14.3 percentage points. This effect is significant at the 99% level. By contrast, when eligibility for the public goods game was limited to women alone, we find *no* evidence that the CDR program is associated with greater collective action capacity.

At the same time, we find that the effect of gender composition is substantial. In communities that did not receive the CDR program, women's only groups exhibit contribution rates (in terms of the average share of 300LD contributed) that are 17.7 percentage points higher than in

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<sup>2</sup> We report the average treatment effect on the treated, matching on gender treatment because of a slight lack of balance in its assignment. Results from the simple average treatment effect, or a *t*-test on the difference of means without matching, yield very similar results.

<sup>3</sup> These estimates are calculated by examining differences between contributions by individuals facing a high and a low interest rate within villages under the assumption that behavior is not strongly sensitive to the composition of interest rates facing other players. For this analysis standard errors are clustered at the community level.

mixed gender groups—an effect similar in magnitude to that of the CDR program. The gender composition treatment, however, does not produce higher levels of cooperation in CDR treatment communities, where cooperation rates are universally higher and nearly identical for mixed and women only cases.

One possible explanation for the powerful impact of the women only treatment is that women are more altruistic and thus we are capturing the effect of having 24 women playing the game rather than just 12. The results in Table 3, however, suggest that this is not the case. In fact, contribution rates are nearly identical among men and women in mixed gender communities. Women give much more, however, when they are making decisions about how much to contribute in a game in which only women are participating—suggesting that the effect is truly due to composition and does not reflect a women’s “main” effect.

[Table 3]

This pattern will become important as we examine the mechanisms that account for collective action success. To move beyond a black box analysis of the CDR treatment effect, we need to understand why the effect is observed so strongly in the mixed gender cases but not in the women’s only cases, and why a gender composition effect is so clearly seen in the CDR control areas but not in the treatment areas.

## **The Impact of Interest Rates on Patterns of Cooperation**

Perhaps we observe no impact of the CDR program on collective action capacity when only women are participating in the game because of a “ceiling effect.” Cooperation rates in women only games may be so high that it is difficult to detect an additional impact of the institutional treatment.

In order to assess whether a ceiling effect is in evidence, we explore the impact of the random variation in interest rates on contribution rates in both treatment and control communities. If there is a ceiling above which it is difficult to drive cooperation rates, then we should observe no impact of the higher interest rate in environments in which cooperation is already high. The results are presented in Table 4.

[Table 4]

The bottom line is clear: although levels of cooperation are high in CDR treatment communities, they are not so high that the additional impact of a higher interest rate cannot be detected. Indeed, the impact of offering a higher return is greater in CDR treatment communities than in the control group (although the interaction term is not statistically significant); moreover, the impact of the CDR program is especially strong for subjects who were offered higher interest rates on their contributions to the public good.

These results are nearly identical if we focus on the gender composition treatment rather than the CDR treatment. Even in women’s only communities where rates of cooperation are already high, a higher interest rate drives higher contributions in the public goods game. Thus the fact that the introduction of new, democratic institutions drives higher rates of cooperation in mixed gender

settings, but not when only women are participating in the public goods game, cannot be accounted for by a ceiling effect.

## **Robustness**

Covariates are balanced across the treatment and control communities in expectation but not necessarily in their realization. To check that our results are not driven by omitted variables, we focus attention on two potential confounds. The first stems from a practical development in the field. During the first week in which the games were played, we received a report that leaders in one community had gathered villagers together after we left and asked people to report how much they had contributed. This was a violation of the protocol agreed to by the community. We moved quickly to prevent any retribution in that village, but also decided to alter the protocol for subsequent games to ensure greater protection for game participants. These changes included stronger language about the importance of protecting anonymity, random audits of community behavior in the days following the game, facilitation of anonymous reporting of violations of game protocol by participants, and a new opportunity to receive supplemental funds in a post-project lottery if no reports of harassment were received. It is possible that these protocol changes affected subsequent play in subsequent “cycles” (indeed we exploit this variation below).

Second, some of the games were played in communities that are subsections of large villages or towns. These “quarters” mirror other villages in the sense that they have an existing leadership structure (e.g., a “quarter chief” and elders) and are known to people in the area, but the dynamics of collective action in subsections of larger or more urban areas could be different than those in smaller, more isolated villages. As it happened, the treatment was assigned at a somewhat lower rate to quarters.

To take account of these potential confounds, we estimated average treatment effects using exact matching to compare treatment and control observations that share each of these characteristics. This ensures that our results are not driven by the fact that treatment assignment probabilities may differ across systematically different groups of villages. Matching on cycle (in addition to the gender treatment) has only a small impact on the core results in Table 1. The magnitude of the estimated impact of the program drops slightly, but the estimates remain significant at the 95% level. After matching by quarter as well, the estimated magnitude of the program impact is diminished, from 6.5 to 4.3 points (for share of available funds earned), from 5.7 to 3.7 points (for average share contributed) and from 9.1 to 5.8 points (for the share contributing the maximum), as is the precision of our estimates, with  $p$ -values rising to 0.07, 0.10 and 0.11 respectively. This drop in estimated effects reflects the fact that contributions were lower in quarters and quarters received treatment at a lower rate than villages. If we separate the sample into those villages in which all women played the game versus those with mixed gender composition, the finding of a significant CDR program impact in mixed gender groups remains robust, even after accounting for both cycle and quarter. There is still no impact of the CDR program in communities in which only women participated in the game.

## **Discussion**

The results of the behavioral game provide powerful evidence that the introduction of new local level institutions can alter patterns of social cooperation in a way that persists after the program’s

conclusion. Villages exposed to a community driven reconstruction program exhibit higher subsequent levels of social cooperation than those in the control group, as measured through a community-wide public goods game. This suggests not only that patterns of cooperation are responsive to short-run interventions, but also that the introduction of participatory, democratic institutions increases the collective action capacity of communities rather than undermining it.

At the same time, the result of the cross-cutting gender treatment presents a puzzle that requires further exploration. The beneficial impact of the institutional treatment is visible only in those communities in which all villagers were eligible to participate in the public goods game. Where only women were eligible to play (even though both men and women could participate in project selection, serve as community representatives, etc.), cooperation rates were very high, but the institutional treatment had no impact. As we have ruled out the possibility that a “ceiling effect” is at work, we must turn now to an investigation of the mechanisms through which democratic institutions enhance collective action capacity, in order to ascertain why their effects are visible in mixed gender settings but not when only women participate in the public goods game.

Before doing so, one important task remains: to convincingly demonstrate that the gains in social cooperation flow from the institutional intervention, we must rule out the possibility that higher contribution rates in treatment communities reflect the economic consequences of the CDR program. In principle, the economic component could affect contributions by changing the community’s value for public goods (relative to private goods). A number of arguments speak against this interpretation. First, public investments plausibly exhibit decreasing marginal returns, in which case the effect of past investment would be to bias our estimates of collective action capacity downward. Second, increasing returns would have to be very strong to account for the magnitude of the effect we find (recall that our estimate of CDR’s impact on individual contributions is approximately equal to the estimated impact of quadrupling the social rate of return). Third, our survey evidence suggests that the direct impact of the CDR program on welfare is modest. We find little evidence that the CDR program improved overall household welfare, access to services, and the infrastructure of community public goods, although there is some weak evidence of small gains in education (Fearon, Humphreys, and Weinstein 2009).

## Mechanisms

We outlined four mechanisms through which the institutional innovation we examine could affect collective action capacity: a *social sanctioning* mechanism, a *prize-value* mechanism, an *information-organizational* mechanism, and a *preferences* mechanism.

Identifying the mechanisms through which an intervention operates is in general a difficult task (Imai et al 2009). Our design provides exogenous variation in the application of the CDR program, but not on intermediary variables that can themselves affect levels of cohesion. Nevertheless, ancillary data provides preliminary evidence that can be used to assess the extent to which these mechanisms are plausibly in operation in this case. In particular, we provide evidence on the existence of the sanctioning mechanism, the prize-value mechanism, and the organizational mechanism. The evidence is supportive of the latter two of these three mechanisms. We do not yet have evidence on the preferences mechanism.

## Sanctioning

We examine two pieces of evidence to assess the plausibility of the sanctioning account. The first exploits the change in protocol introduced after the second cycle of data collection. As described above, the change of protocol was designed to reduce the feasibility of coercion in the communities under study. If indeed sanctioning was important for sustaining high levels of cooperation, then we would expect lower contribution rates in later cycles. Moreover, if sanctioning was particularly effective in treatment communities, we would expect the diminished scope for sanctions to reduce cooperation rates more strongly in treated areas.

[Table 5]

The evidence in Table 5 supports the hypothesis that sanctioning was important for sustaining high contribution rates and that the protocol changes reduced the scope for such sanctioning. Cooperation rates were between 13 and 15 percentage points lower in later cycles than they were in Cycle 1. However, the evidence does not support the second hypothesis: the decline in contribution rates was no greater (and was in fact marginally smaller) in treatment areas compared to control areas. Thus, while sanctioning appears to be important the evidence does not support the claim that it was *more* important in treatment areas. Finally, we note that if we examine the mixed gender and women's only sites separately we find that both the marginal effect of the CDR treatment in the mixed areas and the marginal effect of the women's only treatment in the control areas are stronger in later cycles, suggesting similar effects of sanctioning for both processes.

This evidence is striking, but imperfect: While scheduling was undertaken in a way that ensured balance in treatment and control areas over time, the schedule was not fully randomized. First, only rural areas were scheduled in the first cycle (the core results from Table 5 are, however, robust to taking account of rurality and to conditioning only on rural areas). Second, cycle is correlated with time in the sense that all first cycle areas participated before all later cycle areas (we do not however find evidence for any time trend over cycles 2, 3 and 4).

We provide a second piece of evidence in Table 6 drawing on survey data. In our participant survey, we asked whether subjects expected that their actions would be observed. As reported in Table 6, these concerns were expressed at a higher rate both in treated mixed areas (compared to control mixed areas) and in women only control areas (compared to mixed control areas). The former relationship is not significant, while the latter although weak, is significant at the 90% level. This provides weak support for the claim that sanctioning could account for the gender composition effect, but does not support the claim that sanctioning accounts for the CDR treatment effect. Moreover, we note, that expectations that others know of one's actions poorly predict contribution rates (see Table 7)

[Table 6]

[Table 7]

Overall, this evidence then does not support the view that the effect of the CDR program was to strengthen cohesion by improving the community's capacity to sanction non-participants.

## Prize Value

The second mechanism we consider is the possibility that the new institutions provided subjects with greater confidence that contributions made to the public good would translate into a high quality public good. In short, the private value of contributing to a public good rises when institutions selecting projects better reflect the preferences of potential contributors and implement these projects more efficiently.

[Table 8]

Table 8 provides evidence from survey data to test this hypothesis. We use survey data from participants to examine three dimensions that could affect the prize value of the public goods. First a measure of whether participants broadly, had a say in the selection of projects, second whether the projects actually selected were the relevant ones, and third whether respondents believed that the funding made available for the projects would be effectively employed.

The data finds effects in the hypothesized direction on all measures. For two, the first and the third, the effect size is substantively large and statistically significant, suggesting that participants in treatment areas did indeed have a greater say in the selection of projects and had greater confidence that the funds would be used well. This we take as positive evidence for the prize mechanism.

## Organization

Finally, we consider three types of evidence for the hypothesis that the impact of introducing community-level, democratic institutions works through improvements in organizational capacity for mobilizing participation. First, in Table 9, we examine the extent to which participation in CDR is associated with a greater propensity to hold and for villagers to attend community meetings to discuss the behavioral games.

[Table 9]

The evidence suggests that patterns of participation in community meetings broadly reflect patterns of contribution in the public goods games. In particular, participation rates were greater in treated mixed areas (compared to control mixed areas) and in women only control areas (compared to mixed control areas).

[Figure 1]

This attendance at meetings is, moreover, strongly associated with greater contributions in the public goods game (see Figure 1). “Controlling” for attendance at meetings cuts the magnitude of the CDR treatment effect almost in half. While our randomization does not identify the effects of meetings on contributions we take this as suggestive evidence in favor of the hypothesis that the both the CDR treatment effect and the gender composition effect worked through greater organization.

The second type of evidence we examine relates to contact rates. Table 10 reports the extent to which individuals report being contacted regarding how to take part in the behavioral game in the week prior to play. We again see the pattern that contact rates were higher in treated mixed areas (compared to control mixed areas) and in women only control areas (to mixed control areas), although the effects are strongest on this measure for treatment in mixed areas. Moreover, it is also the case that higher contact rates in villages is associated with considerably higher contribution rates (results not shown), although individuals that are contacted contribute significantly less suggesting perhaps that organized communities contact more people but target the hard cases. Again, this evidence is broadly supportive of the organization mechanism.

[Table 10]

Finally, Table 11 examines who gets selected onto the committees organized by communities as part of the behavioral games. In particular, we examine the number of CDC (Community Development Committee) members on these committees. All communities have some form of CDC although the pool of CDC members is plausibly higher in treatment areas. The results in Table 11 suggest that CDC members are indeed drawn on to a great extent in treated areas, with on average one extra committee member being selected from CDC member pools in mixed areas. The tendency to select CDC members as a function of treatment in women's only areas is weaker however and, moreover, women only areas do not show a greater propensity to engage CDC members. This suggests that while this type of organizational skill was drawn upon in treated mixed areas, a different process must account for the effectiveness of women only groups in control areas.

## **Conclusion**

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 a community driven reconstruction program exhibit higher subsequent levels of social cooperation than those in the control group, as measured through a community-wide public goods game. These results suggest that changes in community cohesion can take place over a short period of time; can occur in response to 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.

Understanding the mechanism(s) underlying this relationship is more difficult and our results are more speculative. Nevertheless, they suggest that the effect of the institutional innovation does not lie in an increased capacity of communities to sanction participants for failing to act in the collective interest. Rather two other, closely related mechanisms find support: first the prize value of the projects appears higher in CDR treatment areas, with subjects having a greater say in the selection of projects and more confidence that funds will be put to good use; second, organizational capacity in CDR treatment areas appears enhanced, which may lead directly to more effective coordination of strategies for solving the collective action problem we study.

A variation in our public goods game in which in half of the research sites only women took part points to important heterogeneity in CDR treatment effects and poses a challenge for interpreting the processes that might account for collective action in this setting. The CDR treatment effect, we found, is strong only in cases where we measure cohesion by examining mixed groups. In addition to the CDR treatment effect, we also find an effect of gender composition – women’s only groups do better, but this is true only in the CDR control areas. Thus women’s only groups do better and CDR treatment areas do better but these two effects do not cumulate.

This lack of accumulation of treatment effects could be due to a ceiling effect, although the variation in other parts of our data casts doubt on that account. Alternatively it could be that there is a substitution effect of the following form. In mixed gender areas, our study of mechanisms suggests that CDR treatment communities engage in greater organization and draw strongly on existing CDC structures. This organization plausibly accounts in large part for their greater success. In control areas, the women’s only groups also engage in more organizational activities. In principle, as they do so, they might draw on very different structures than those drawn upon in the mixed gender CDR treatment areas. In CDR treatment areas, the mixed gender groups draw on the mixed gender CDC structures. These structures are not drawn upon so extensively in the women’s only areas, however. The most likely alternative structures to be drawn upon are the well established women’s organizations in these communities. We do not, however, have evidence that this is the case; indeed our data suggest that women leaders are *least* likely to be engaged on committees in the women’s only treatment areas. For us, this remains a puzzle. While the data suggests that alternative organizational structures are employed we have yet to identify what these structures are.

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**Table 1: CDR Program Impact**

Outcome	Control communities	Treatment communities	Difference ( <i>se</i> )
Share of available funds earned	75.9%	82.5%	+6.5** (2.6)
Average share of 300LD contributed	75.1%	80.8%	+5.7** (2.6)
Share contributing full amount	62.3%	71.3%	+9.1** (3.7)

*Notes:* \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table reports the average treatment effect on the treated, with matching according to assignment to the gender composition treatment; standard errors allow for heteroskedasticity across strata. Results are reported for village level outcomes, for 41 treatment and 41 control communities.

**Table 2: CDR Program Impact, by Gender Composition**

	Mixed Gender	Women Only	Difference ( <i>se</i> )
Control communities	66.8%	84.6%	17.7*** (3.6)
Treatment communities	81.2%	80.3%	-0.8 (3.5)
Difference ( <i>se</i> )	14.3*** (3.9)	-4.3 (3.2)	-18.6*** (5.0)

*Notes:* \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table presents results of a two-sample t-test. The difference-in-difference estimate is the coefficient on the interaction term from an OLS regression. Results are reported for village level outcomes, for 41 treatment and 41 control communities.

**Table 3: The Relative Impact of Gender and Gender Composition**

	Mixed Gender	Women Only	Difference ( <i>se</i> )
Female	74.5%	82.6%	8.0*** (3)
Male	74.2%		
Difference ( <i>se</i> )	0.3 (0.3)		

*Notes:* \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table presents results of a two-sample t-test. Results are reported for village level outcomes, for 41 treatment and 41 control communities.

**Table 4: CDR Program Impact, by Interest Rate**

	Low Interest Rate	High Interest Rate	Difference ( <i>se</i> )
Control communities	73.8	78.0%	4.2* (2.3)
Treatment communities	78.0	84.5%	7.5*** (2.2)
Difference ( <i>se</i> )	3.2 (2.4)	6.5*** (2.0)	3.3 (3.1)

Notes: \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table presents results of a two-sample t-test. The difference-in-difference estimate is the coefficient on the interaction term from an OLS regression. Results are reported for individual level outcomes.

**Table 5: Cycles and Sanctions**

	Cycle I	Later Cycles	Difference ( <i>se</i> )
Control communities	0.87	0.72	-0.15*** (0.05)
Treatment communities	0.9	0.77	-0.13*** (0.04)
Difference ( <i>se</i> )	0.03 (0.03)	0.06* (0.03)	-0.03 (0.06)

Notes: \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table presents results of a two-sample t-test. The difference-in-difference estimate is the coefficient on the interaction term from an OLS regression. Results are reported for individual level outcomes.

**Table 6: Share Believing Their Actions Will Be Known to Others**

	Mixed Gender	Women Only	Difference ( <i>se</i> )
Control communities	0.12	0.21	0.09* (-0.05)
Treatment Communities	0.18	0.16	-0.02 (0.06)
Difference ( <i>se</i> )	0.06 (0.05)	-0.06 (0.06)	-0.11 (0.08)

Notes: \*\*\* Significant at 1%; \*\* Significant at 5%; \* Significant at 10%. The table reports the average treatment effect on the treated, with matching according to assignment to the gender composition treatment; standard errors allow for heteroskedasticity across strata. Results are reported for village level outcomes.

**Table 7: Information and Sanctions**

Do you think it is likely that other people in the village will find out how much money you contributed?			
	No	Yes	Difference
Share of Endowment Contributed	0.78 (1572)	0.80 (321)	0.02 (0.02)

**Table 8: Measures on the Prize Value**

	Control communities ( <i>N</i> )	Treatment communities ( <i>N</i> )	Difference ( <i>se</i> )
Share of reps reporting that projects were selected at a community meeting	.82 (37)	.94 (36)	.12* (.07)
Avg. share who reported selected projects were among “most important” for village	.94 (41)	.95 (41)	.01 (.02)
Avg. share who reported funds would “certainly” be put to good use	.80 (41)	.87 (41)	.07** (.03)

*Notes:* \*\*\* Significant at 1%; \*\* Significant at 5%; \* Significant at 10%. The table reports the average treatment effect on the treated, with matching according to assignment to the gender composition treatment; standard errors allow for heteroskedasticity across strata. Results are reported for village level outcomes.

**Table 9: Participation in Subsequent Community Meetings**

	Mixed Gender	Women Only	Difference ( <i>se</i> )
Control communities	0.4	0.55	0.14** (0.07)
Treatment communities	0.55	0.52	-0.03 (0.06)
Difference ( <i>se</i> )	0.15** (0.07)	-0.03 (0.07)	-0.18* (0.09)

*Notes:* \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table presents results of a two-sample t-test. The difference-in-difference estimate is the coefficient on the interaction term from an OLS regression. Results are reported for individual level outcomes.

**Table 10: Contact Rates (Number of people contacted in advance of play)**

	Mixed Gender	Women Only	Difference ( <i>se</i> )
Control communities	2.6	3.9	1.25 (0.86)
Treatment communities	4.9	3.8	-1.07 (1.23)
Difference ( <i>se</i> )	2.26** (0.97)	-0.6 (1.17)	-2.32 (1.51)

*Notes:* \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table presents results of a two-sample t-test. The difference-in-difference estimate is the coefficient on the interaction term from an OLS regression. Results are reported for individual level outcomes.

**Table 11: Leader Selection (Number of committee members that are “CDC leaders”)**

	Mixed Gender	Women Only	Difference ( <i>se</i> )
Control communities	1.2	1.3	0.18 (0.33)
Treatment communities	2	1.7	-0.43 (0.33)
Difference ( <i>se</i> )	0.95*** (0.33)	0.33 (0.34)	-0.62 (0.47)

*Notes:* \*\*\* Significant at 99%; \*\* Significant at 95%; \* Significant at 90%. The table presents results of a two-sample t-test. The difference-in-difference estimate is the coefficient on the interaction term from an OLS regression. Results are reported for individual level outcomes.

Figure 1: Meeting Attendance and Village Average Offers

