

## Can Development Aid Contribute to Social Cohesion after Civil War? Evidence from a Field Experiment in Post-Conflict Liberia

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Civil war is very common in the developing world, with harmful welfare effects when it occurs. Many fear that the devastation wrought by violent conflict destroys social capital, impedes economic development, and leads to the recurrence of violence (Paul Collier et al. 2003).

In response, donors are injecting large amounts of aid into post-conflict countries. A significant share of this assistance is spent on “community-driven reconstruction” (CDR) programs, which support the establishment of new local institutions in order to promote social reconciliation. Whether this assistance has this effect is, however, largely unknown. Can brief, foreign-funded efforts to build local institutions in fact have positive effects on local patterns of cooperation?

We address this question using a randomized field experiment to evaluate the impact of a CDR project in northern Liberia. The project was funded by the United Kingdom’s Department for International Development (DFID) and implemented by the International Rescue Committee (IRC). The project attempted to build democratic, community-level institutions for making and implementing decisions about local public goods. This model of support for participatory processes to enhance local public goods provi-

sion is now standard in post-conflict contexts, and is also a key component of donor-funded efforts to reduce poverty (“community-driven development”, or CDD). By one estimate, the World Bank alone lends upward of \$2 billion per year in support of such efforts (Ghazala Mansuri and Vijayendra Rao 2004).

Prior research suggests that such small-scale, externally driven interventions are 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 norms of social interaction are an outcome of long-run evolutionary mechanisms (Samuel Bowles and Herbert Gintis 2004); have deep historical roots in critical junctures that reshape social relations, such as the extraction of slaves from Africa (Nathan Nunn 2008); or reflect relatively fixed characteristics of communities, such as ethnic heterogeneity or the distribution of wealth (Alberto Alesina and Eliana La Ferrara 2005). Moreover, aid workers often return from the field demoralized by an impression that the benefits of foreign aid projects are easily captured by existing power brokers, a view that resonates with findings by economists (Mary Kay Gugerty and Michael Kremer 2008) and anthropologists (William Murphy 1990; Jean Ensminger 2007).

Yet the intervention we examine here is premised on the notion that increased cohesion can result from even a brief exposure to participatory politics—through a CDR program that involves the organization of community committee structures and support for those structures to help meet community needs. 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.

The data we have to assess the impact of the CDR program are rich, consisting of baseline and

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follow-up surveys and behavioral data collected from public goods games played by 1,979 randomly sampled individuals in the summer of 2008. We cannot present a full analysis of the findings here. Instead, we describe the program, present the measurement strategy employed to obtain behavioral outcomes, and provide some first-order results on behavioral change.

The outcome we examine is the amount of funding a community raises for a collective project through anonymous play in a public goods game. Our findings suggest that the CDR program improved community cohesion: treatment communities raised significantly more money than control communities. Although levels of social cooperation were high across all villages in our sample, 71 percent of households contributed the maximum amount in treatment communities, while 62 percent contributed the maximum in control communities. For total payouts, which averaged about \$333, treatment communities received almost 9 percent more on average for the community-selected public good. This effect is equivalent in magnitude to our estimate of the impact on individual contributions of quadrupling the social rate of return on a private investment.

### I. Research Design

Our research examines the impact of a CDR program implemented by a major international nongovernmental organization in 42 communities in Northern Liberia between September 2006 and February 2008.

The program area figured centrally in the violence that engulfed Liberia over the previous 15 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 record information on almost 6,000 household members living in the region in 1989. Of these individuals, the data suggest that over 4 percent died directly from war-related violence and a further 6 percent suffered injury or maiming. The most widespread effect, however, was one that could have a direct bearing on communities' ability to cooperate (Paul Richards et al. 2005): 85 percent of these individuals were displaced during the conflict and many were displaced multiple times. Of 1,500 individuals we interviewed in the project areas in May 2008, 25 percent still considered themselves internally displaced.

The CDR program had the following core components. First, villages were grouped into approximately equal-sized "communities" based on geographic proximity and preexisting ties. Then the IRC undertook initial activities to sensitize communities to the new development project, including meetings with local chiefs and elders to solicit their cooperation. In each community, the IRC oversaw the establishment of a new institution—a community development committee (CDC)—that was charged with managing a community-wide process to select and implement a quick-impact project (approx. \$2,000–\$4,000 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 oversaw implementation and continue to have responsibility for project maintenance over time.

The implicit hypothesis underlying the program was that the introduction of CDCs, and exposure to their operation, would enhance the ability of community members to act collectively for mutual gain. The program also aimed to improve households' material welfare and to inculcate democratic values—outcomes that are not the focus of this paper.

To test the core hypothesis, the IRC agreed to randomly assign communities to a treatment group (with 42 units) that received the CDR program and a control group (with 41 units) that did not. The lottery was conducted in public, with chiefs representing each community in attendance.

The IRC tracked implementation of the CDR program over the course of 18 months. Comparative data on the rollout and staffing of CDR programs implemented by the IRC in other countries suggest that the quality of implementation of this project was similar to experiences elsewhere.

### II. Measurement Strategy

The standard approach to measuring social cohesion involves surveying households to assess levels of trust, patterns of community activity, and the extent of associational life. We followed this approach, drawing on a subset of the battery of social capital questions developed by the World Bank. But we were conscious that, particularly in the context of a program designed

TABLE 1—CDR PROGRAM IMPACT

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

*Notes:* 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. \*Significant at 95 percent.

to promote trust and cooperation, individuals in treatment communities may have learned how to respond in ways that would please outside funders.

For this reason, we designed a public goods game with the goal of observing whether communities exposed to the CDR treatment actually behave differently from 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 two visits the community had time to choose their community representatives, select potential projects, and discuss what strategies to use in the public goods game.

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 percent and 400 percent). Thus each community had the opportunity to earn up to 25,200LD. 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.

### III. Empirical Results

#### A. Impact of Community-Driven Reconstruction on Social Cohesion

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 percent 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>1</sup> We see that exposure to the CDR program led to an average 6.5 percentage point (or 8.7 percent) 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 percent); and the share of households contributing the full amount increased by 9 percentage points (15 percent). These effects are each significant at the 95 percent level.

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 percent to 400 percent. This change in interest rates yields an estimated 5.8 percentage point change in individual contributions, an effect significant at the 99 percent level.<sup>2</sup>

### B. Potential Confounds

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 play in subsequent "cycles."

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 from 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. The magnitude of the estimated impact of the program drops slightly, but the estimates remain significant at the 95 percent 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.

### C. Measurement Validity

We believe that our approach to measuring behavioral outcomes is an improvement over existing attitudinal measures. Nevertheless, we recognize that the approach raises challenges of interpretation, two of which we consider here.

First, the intervention was complex: treatment communities received both higher levels of public investment and exposure to the political and

<sup>1</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>2</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.

social components of CDR programs. In principle, the economic component could affect contributions by changing the community's value for public 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 cohesion 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. Finally, our survey data show little difference in treatment and control communities in the extent to which people value the project or believe it will be of broad benefit to the community.

It is also possible that CDR, rather than affecting a general aptitude for cooperation, simply taught treatment communities how to act in order to please outsiders. While this possibility cannot be completely ruled out, we think it is unlikely that this explanation accounts for the results. First, the games were implemented by an organization not linked to the IRC intervention in any way (and by teams that did not know which communities had received the CDR treatment). Second, unlike with survey responses, there is a real private cost in the game to taking an action motivated purely by a desire to please outsiders; moreover, the actions are taken in private and the benefits diffused over the community.

#### IV. 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 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 are striking. They 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. These findings suggest that post-conflict development aid can have a measureable impact on social cohesion. In future work, we hope to use the survey data to uncover the mechanisms that account for this main finding.

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