Can the Poor Be Organized?

Cooperation and Public Goods in Rural India*

Raj M. Desai† **Shareen Joshi**‡ **Anders Olofsgård**§

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Abstract

High coordination costs are often identified as the reason for the low quality of public goods available to the poor. We report findings from a unique combination of a village-randomized controlled trial and a lab-in-the-field experiment. An in-depth survey of 1,600 women before and after an intervention establishing membership-based organizations in one of the poorest districts in India shows that the presence of these groups increased villagers' capacity to address water delivery problems, and improved access to, and quality of, water service. Public goods games with over 200 participants in a subset of control and treatment villages show that the presence of village groups increased cooperation among both members and non-members in treated villages. We find little evidence that cooperation is facilitated by more common tastes among group members. These results suggest that, in contrast to traditional community development programs, membership groups can help poor communities build social capital.

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† Edmund A. Walsh School of Foreign Service and Department of Government, Georgetown University, and The Brookings Institution (desair@georgetown.edu).

‡ Edmund A. Walsh School of Foreign Service, Georgetown University (sj244@georgetown.edu).

§ Stockholm School of Economics and Stockholm Institute of Transition Economics (anders.olofsgard@hhs.se).

A defining feature of poor communities is their limited capacity for collective action. Early assessments of political society—as detailed in Durkheim's concept of "mechanical solidarity" (1947 [1893]) or in Weber's description of "patrimonialism" (1968)—argued that where social ties are dominated by kinship, clan, or tribe, political life is characterized by the absence of coordinated behavior—what Banfield described as the problem of "concerting the behavior of large numbers of people in matters of public concern" (1958: 8). Recent writings have lamented the dearth of social capital in poor areas, and confirmed that the endemic factors associated with poverty can steeply raise coordination costs (Woolcock and Narayan 2000).

Without coordination, the bargaining position of the poor relative to public officials, middlemen, and frontline service-delivery agents is weakened (World Bank 2004; Banerjee, Iyer and Somanathan 2007). The result is that the quality, accessibility, and reliability of public services is almost uniformly worse for the poor, who are forced to rely on them almost exclusively (Devarajan and Reinikka 2004; DFID 2010).

Efforts have been underway for several years to increase coordination among the poor as a means of improving public goods. International donors, in particular, have sought to expand their support for "community-driven development" (CDD) projects that encourage the poor jointly to design, deliver, maintain, and monitor local public goods. The record of CDD initiatives in fostering cooperation, however, is mixed, with the balance of recent evidence showing that any behavioral changes among the poor do not last long.

We examine a related local institution, namely, the "self-help" group—a grassroots, membership-based organization that combines elements of classic rotating credit and savings associations with various other group-related activities, and whose effects have been relatively less explored (Chen, *et al.* 2007). There are compelling reasons to evaluate how the quality and

accessibility of local public goods are influenced by grassroots efforts such as self-help groups, which, in contrast to typical CDD initiatives, are typically focused on organizing the poor for a broader range of purposes beyond the implementation of specific projects.

Our investigation takes place in rural India, where we examine the ability of women to coordinate with respect to a public good that dramatically affects their quality of life: water. We exploit the random selection of villages for a self-help group program implemented in Rajasthan, the water-scarcest state in the Indian union. Although self-help groups were not established to address problems of water scarcity (or any other specific public goods problem), we postulate that the mutual trust and solidarity acquired in one domain (credit and savings) effectively translates into group cooperation along a number of other fronts. Residents of the region where the program was rolled out, moreover, are mostly from "scheduled tribes"—historically among the least mobilized groups in the Indian polity. Recruitment of these women into self-help groups can provide a strong-inference test of the capacity of grassroots movements to organize marginalized groups, and in so doing, improve accountability and quality with respect to public goods delivery.

We find three principal village-level effects. First, residents of villages where self-help groups were started acquire greater knowledge of how to address problems of water supply and water deficiency. Second, women in self-help group villages are more likely to contact local authorities regarding their grievances with respect to water service. Third, residents of self-help group villages report greater improvements in water access and infrastructure compared to their counterparts in control villages. We find that all of these effects are common to all residents in self-help group villages suggesting strong spillovers from members to non-members in villages were these groups were established.

Survey data suggest that the intervention resulted in both absolute and relative improvements in water services. The extent to which this is driven by increased cooperation, however, requires a better understanding of the channels by which self-help group membership may have enabled village residents to coordinate for improvements in water infrastructure. To test whether the presence of self-help groups fostered norms of cooperation we conducted a series of repeated public-goods games with both self-help group members and non-members in a sub-set of treatment and control villages. We find that self-help group members initially contribute significantly more than non-members. Thereafter, inter-village differences (between residents of treatment and control villages) dominate intra-village differences (between members and non-members within treatment villages), suggesting that learning takes place in subsequent rounds, and that non-members quickly become conditional cooperators. An alternative explanation is that self-help group members moved toward common preferences regarding public goods. Pre- and post-intervention concordances among groups within treatment and control villages do not support this argument.

We make two substantive contributions. First, we add to a small but growing body of evaluations using combinations of randomized-controlled trials (RCT) and lab-in-the-field techniques to understand better the underlying mechanisms generating observed impact (Levitt and List 2007).[1] Second, although self-help groups have proliferated in India (and around the world) evidence of their impact is limited compared to more traditional CDD programs. Ours is one of the first assessments of self-help groups' capacity to improve the quality of local public

[1] Recent lab-in-the-field experiments conducted in parallel with RCTs include evaluations of post-conflict reconstruction efforts (Fearon, Humphreys, and Weinstein 2009) and of community-based monitoring for education (Barr, *et al*. 2012).

goods. Our results suggest that SHGs can improve public service provision by creating an institutional basis for collective action emanating from the mutual trust generated within the framework of the rotating credit and savings association and other group activities. In sum, the *quality* of the interactions created by self-help group may be more important than the *quantity* of social interactions in reducing collective action costs. These findings suggest that self-help groups may be a more effective way of bolstering cooperation within marginalized communities than traditional CDD programs.

Community Organizations and Rural Development

Collective action has a major effect on the quality of public goods. Poor communities, however, face higher coordination costs due to factors such as: illiteracy (Narayan *et al*. 2000); limited access to information (Jones and Woolcock 2007); patron-client networks (Shami 2012); geographic isolation (Upton 2008); ethnic diversity (Miguel and Gugerty 2005); restricted asset ownership (Adhikari and Lovett 2006); vulnerability to shocks (Adger and Neil 2003); limited mobility (Gugerty and Kremer 2008); and an overall sense of "exclusion" (Warren, Thompson, and Saegert 2005), to name a few. High coordination costs and limited political agency diminish the weight of the poor in the design and implementation of public policy. Occasionally, marginalized groups such as smallholders, pastoralists, and women, have effectively used collective action to strengthen property rights (Baland and Platteau 2003; von Braun and Meinzen-Dick 2009), increase bargaining power in labor markets (Bardhan 2005), improve access to financial markets (Karlan 2007), and improve common pool resource management (Ostrom 1990; Ahn and Ostrom 2008). Still, poverty is almost everywhere characterized by under-provided public goods and badly functioning public services.

Community-Driven Development

CDD initiatives have become an important strategy to improve service delivery by allowing communities to control resources directly, and by requiring community participation in decision-making in order to put those resources to use. Communities typically receive external funds to build public goods and infrastructure, but participate in the design and monitoring of those goods. Reflecting a broader movement toward greater decentralization of service delivery and participation by the poor, the funding of CDD programs by international donors has expanded considerably; the World Bank alone has spent US$50 billion on CDD initiatives over the past decade. (Mansuri and Rao 2013).

The impact of CDD initiatives is mixed. Bjorkman and Svensson (2009) and Duflo, Dupas, and Kremer (2014) find that CDD programs increased participation in primary healthcare in Uganda and primary schooling in Kenya, respectively. A study of community-based education monitoring systems in Uganda finds evidence of beneficial effects to primary school performance (Barr *et al*. 2012). An evaluation of a community-based post-conflict reconstruction program in post-civil war Liberia shows that participating communities display more cooperative features than non-participant communities (Fearon, Humphreys, and Weinstein 2009). And evaluations of donor-funded CDD programs in the Philippines and Zambia find moderate, positive effects on measures of community "togetherness" (Chase and Sherburne-Benz 2001; Labonne and Chase 2011)

By contrast, Olken (2010) reports limited effects of community monitoring on corruption in road-building projects in Indonesia. More importantly, an examination of World Bank-sponsored CDD programs suggests that the impact on social capital tends to be weak or short-lived (Wong 2012). In post-conflict Sierra Leone, Casey, Glennerster, and Miguel (2012) find no

difference between treatment and controls when looking at political processes, fundraising for local public goods provision, or female empowerment. Similarly, Humphreys, Sanchez de la Sierra, and van der Windt (2012) find no impact on social capital from a CDD program in eastern Congo. In Afghanistan, a CDD program increased participation in local political decision-making, but showed no significant impact on measures of trust (Beath, Christia, and Enikolopov 2013). Finally, Avdeenko and Gilligan (2014) find no effects on the social networks or pro-social behavior of a community benefiting from a CDD implemented in Sudan.

The Self-Help Group Movement

A typical CDD program usually requires beneficiaries to establish a committee, council, or other organization to manage project implementation. Somewhat differently conceived is the "self-help" group (SHG), one of a class of membership-based organizations whose members provide each other with mutual support while attempting to achieve collective objectives through community action. Self-help groups typically combine elements of classic rotating credit and savings associations with various other group-related activities. Financial intermediation, however, is generally seen more as an entry point to other community goals, rather than as the primary objective. As with CDDs, the expectation is that, in time, these actions will develop an institutional platform for improved coordination among participants, in particular, to address deficiencies in public services over which local governments have jurisdiction.

Self-help groups can be distinguished from the more common CDD in three ways. First, CDD initiatives are typically oriented around an intended set of public goods and services, while self-help groups are established principally to build social cohesion among members without reference to any "promised" public good (Chen, *et al*. 2007), Second, whereas CDD programs

deliver public goods hoping that improvements to local institutions will follow, self-help groups aim for the reverse: to enable group members to provide each other with mutual support with the expectation that public goods will improve in quality over time. Finally, while the modalities of implementation may differ, CDD resources are generally funded through foreign aid flows, possibly suggesting to CDD participants that they face an unusual, possibly one-time, opportunity. Self-help groups, on the other hand, are typically grassroots efforts to organize the poor to deal with a country's perpetually underperforming public goods delivery mechanisms.

The self-help group (SHG) movement has proliferated in India (Krishna 2007; Chen, *et al*. 2007). SHGs have formed the cornerstones of development strategies of both central and state governments at least since the 1990s (Reddy and Manak 2005; Basu 2006; Chakrabarti and Ravi 2011).[2] Meanwhile, India's National Rural Livelihoods Mission envisions mobilizing all rural, poor households into SHGs and similar groups in several of the poorer Indian states—approximately 150 million citizens (Planning Commission 2011).

A typical Indian SHG consists of 10-20 members, usually women. The focus on women is critical to the SHG movement. Women generally have lower incomes, fewer assets, and greater vulnerabilities when contrasted with men (King and Mason 2001). Weak relative bargaining strength can restrict women's control over household decisions, particularly over decisions involving children (Rosenzweig and Schultz 1982; Browning and Chiappori 1998). Consequently, anti-poverty programs targeted to women are more likely to prevent the

[2] In 1992 the central government adopted the *Swarnajayanti Gram Swarojgar Yojana* (SGSY) to organize families below the poverty line into SHGs and provide them with training, capacity building and income generating assets through a mix of bank credit and government subsidies. At the same time the Reserve Bank of India (RBI) and the National Bank for Agricultural and Rural Development (NABARD) also piloted the SHG-Bank Linkage Program which granted SHGs with seed money to form "credit management groups" and to open savings accounts with rural banks (NABARD 2013).

transmission of poverty across generational lines than those whose main beneficiaries are men. Finally, there is also evidence that women are lower-risk borrowers, and more responsive to the threat of social sanctions that form the basis of group lending schemes (Armendáriz and Morduch 2005). Consequently, more than 80 percent of all Indian SHGs are women-only (NABARD 2013).

SHGs traditionally promote savings, link members to rural-credit institutions, and manage local childcare services, extra-curricular programs for school children, and job-training centers. They may also contract externally with non-state providers of healthcare and agricultural extension services. Traditionally, members, in addition to participating in savings schemes, receive some combination of job-skills training, agricultural support, and credit through linkages to rural banks.

The effective functioning of these group-based, rotating credit and saving schemes requires that members continue to contribute regularly and refrain from over-borrowing. SHGs are not usually established for the explicit purpose of addressing problems of water scarcity or other local public goods—although, as in the case we investigate, SHGs may be used as a mechanism to train members in certain aspects of public goods provision, maintenance, and monitoring. Direct effects of SHGs on improving public goods are possible. But it is far more likely that SHGs exert an indirect influence on public goods quality through the capacity of members to coordinate. First, it is reasonable to expect that experiences learned within the SHG framework would be useful in reducing coordination costs more generally. Collective action is more likely to emerge when individuals face low costs of information, opportunities to coordinate their actions and engage in repeated interaction, and the ability to reward contributors and punish free-riders—all of which are potentially facilitated by SHGs. Second, in time, the

presence of SHGs is expected to foster norms of mutual trust, increase group bargaining power with respect to service provision, and develop an institutional platform to address local issues of importance to members (including public service provision).

SHG Benefits: Preliminary Evidence

Several studies have focused on SHGs in Andhra Pradesh, which accounts for 40 percent of all SHGs in India (Galab and Rao 2003). Evaluations of the *Velugu* program—which included SHG-bank linkages, the provision of community investment funds, and the training of SHGs to address social problems such as child labor, gender and caste inequalities—found that the program increased incomes, reduced poverty, and improved women's participation in household decisions and civic engagement (Aiyar, Narayan, and Raju 2007). Deininger and Liu (2009a) find that 2.5 years extra exposure to the program resulted in higher consumption, nutrition levels, and asset accumulation for poor participants. Effects were confined to program participants, and no spillover effects into the wider communities were identified.

There is also some evidence suggesting that SHGs can build trust and social capital, mobilize women politically, and improve public service provision to a greater extent than CDD efforts. Sinha (2006) finds that SHG membership increases the likelihood that a woman will run for local political office. Deininger and Liu (2009a) also find that the program increased trust in other villagers, elected representatives and government representatives. They also find an increase in women's attendance in village meetings. Interestingly, these effects (contrary to the economic effects evaluated in Deininger and Liu (2009b) were not limited to group members, indicating spillover effects to other members of communities in which SHGs were formed.

Finally, Casini and Vandewalle (2012) find that women in these groups are more likely to take group action to rectify public goods deficiencies.

The limited evidence of the impact of SHGs, while more favorable than that for traditional CDD programs, is typically constrained by the non-random placement of programs, the non-random assignment of individuals to groups, and wide variations in the methods employed by these organizations. To our knowledge ours is the first exploration that combines the use of a randomized-controlled trial with lab-in-the-field techniques to explore the impact of SHGs on intra-group coordination.

Village Groups and Water Quality

Research was conducted in Dungarpur district, a rural district of 1.1 million people located in the "tribal belt" in southern Rajasthan, and one of the poorest districts in the country. In 2005 the per capita income of Dungarpur stood at Rs. 12,474 (approximately $312) compared to the state average of Rs. 16,800 (approximately $420). Twenty-one percent of the population lives below the rural poverty line. Literacy levels are only 66 percent among men and 31 percent among women, and 76 percent of the population is engaged in agriculture (Government of Rajasthan 2009). In 2004, the Indian Planning Commission included Dungarpur in its *Backward Districts Initiative*, which aimed to address the problems of low agricultural productivity and rural unemployment, and to fill critical gaps in physical and social infrastructure through both central and state-level government interventions in the 100 least-developed districts.

In 2007, Rajasthan state authorities invited the Self-Employed Women's Association (SEWA), a grassroots organization with a long history of working with informal-sector women in neighboring Gujarat, to establish SHGs in Dungarpur. SEWA's mission is to organize women

to help them achieve "economic independence through self-reliance," and now claims a membership of almost two million women in twelve Indian states along with the Delhi municipality (SEWA 2014; Datta 2000; Bhatt 2006).

The Integrated Rural Livelihoods Program

SEWA rolled out a rural SHG development pilot as a randomized-controlled trial in Dungarpur district in 2007-2008. Villages were assigned as control or treatment villages as follows: all villages on the census listing for Dungarpur (from 2001) were stratified according to female literacy rate, total village population, and average household size. From these strata, 32 villages were randomly selected for the SEWA program and 48 villages were selected as control villages (80 villages in total). All adult women in a SEWA village were invited to become SEWA members. Members, upon paying nominal dues, would then be eligible to join SHGs and participate in basic training programs meant to create solidarity and a common understanding of SEWA's objectives. Next, members were organized into SHGs, each with approximately 20 members, and each with their own (group-elected) leader (*agewan*). SHG leaders were given initial training by SEWA facilitators over several weeks. Once set up, SHGs met once a month and set savings targets of Rs. 25-100 per member to be deposited into a group savings account at an SHG-linked bank.

The core function of the SHG is to establish and manage the group savings and credit scheme. As we have indicated, however, SHGs also provide a forum in which local issues of importance to women are discussed, and act as a vehicle for a diverse range of livelihood and financial-support programs organized by the SHGs that are open to all female village residents. In Dungarpur, SEWA's SHGs serve as a focal point for training and education on water

management, in particular, with respect to: (i) technology choices, locations, and cost-sharing for maintenance of water sources; (ii) village-wide needs for hand-pump repair as well as construction of community and private-roof rainwater harvesting tanks; and (iii) advocacy in demanding a more reliable water supply.

Two aspects of this intervention potentially enhance the generalizability of our findings. First, we focus on a group for whom collective action constraints are likely to be particularly binding: rural, tribal Indian women, who face some of the lowest levels of literacy, labor-force participation, and autonomy in the world (King and Mason 2001). More importantly, tribal communities are among the politically weakest groups in India, and in contrast to low-caste groups, have been slow to organize politically (Chandra 2005). In general, divisions along the lines of religion, class, caste, ethnicity, and tribe have hindered the formation of a unified women's movement (Agnihotri and Mazumdar 1995). Overall participation by women in civil society organizations also remains low (Chhibber 2001). Second, the intervention was implemented in Rajasthan where annual mean rainfall is the lowest of any Indian state—ranging between 20 and 45 percent of the national average. As in much of India, rainfall is concentrated in the 3-4 months of the monsoon. The critical link between water and gender has been well-documented: women are principally responsible for obtaining water in rural India and consistently weight drinking water higher than other types of public services such as roads which are primarily used by men (Chen 1991; Chattopadhyay and Duflo 2004; Joshi 2011).

Results

Baseline and follow up surveys of the study area were conducted in late 2007 and in late 2009, respectively. These survey data form a pooled cross-section with treatment and control

samples. The sample of treated women includes a total of 1,410 women who resided in the villages where SEWA programs were implemented. Of these, 748 women were interviewed in the 2007 baseline and 662 interviewed in the 2009 follow up surveys. The sample of control women includes 1,795 women who did not reside in SEWA villages over the two-year period, with 855 interviewed in 2007 and 940 in 2009.[3]

We examine several outcomes of interest including the diffusion of accurate information on how to address grievances with respect to water supply problems, as well as effort undertaken to address such grievances with the relevant authority. Water management in Indian states varies widely by district, with village councils (panchayats), sub-district offices, and state authorities all potentially involved in the provision and maintenance of water sources. In Rajasthan, as in some other states, the sub-district block (*tehsil*) office exercises certain fiscal and administrative powers over the villages and municipalities within its jurisdiction. In Dungarpur, within each *tehsil*, rural water supply is under the authority of a Public Health and Engineering Department (PHED). We test the hypothesis that participation in SHGs expands women's knowledge of authority structures in the community, and motivates them to redress grievances about public issues. We measure learning by examining respondents' knowledge of the PHED as the place for reporting problems with water supplies, and we measure collective action by examining the extent to which respondents actually contact the PHED.

[3] These villages may have other SHGs operating besides SEWA's, but none received SEWA's Integrated Rural Livelihoods intervention. Five villages that were originally designated as treatment villages were reclassified as control villages because SEWA programs were not implemented until after the completion of the follow up survey in December 2009. The delay in establishing programs in these villages is attributed to the presence of another NGO that was operating in these villages. All results in this paper are robust to the exclusion of these villages from the sample.

We also examine four measures of water quality. First, we asked women to assess their overall (household) water quality on a 4-point Likert scale ("very good," "somewhat good," "somewhat bad," or "very bad").[4] We rescale this as a binary variable by coding "very" and "somewhat" good as 1, 0 otherwise. Second, we asked women whether they had direct access to piped water, either through tapped water directly into their house or their land plot, or that of a neighbor's to which they had use. Answers are coded 1 for affirmative responses, 0 otherwise. Third we coded responses of women who claim they had only unprotected, undeveloped sources of water such as rivers, lakes, ponds ("no safe source") as 1, 0 otherwise. Finally, we ask whether women have access to irrigated water for farming, again coding affirmative responses as 1, 0 otherwise.

Differences between pre- and post-treatment means for control and treatment villages are in Table 1. Across both types of villages, improvements in water provision can be seen over the period of the two-year intervention. However, with the exception of reliance on unsafe water sources, the null-hypothesis of equal means cannot be rejected pre-intervention. Post-intervention means show significant improvements in treatment villages over control villages in terms of: knowledge of the PHED, interaction with the PHED office, overall water quality, household access to piped water, and access to irrigation for farming. With respect to safety of water sources, prior to the intervention residents in treatment villages were more reliant on unsafe and surface water. Two years later, there is no significant difference between residents in control and treatment villages, suggesting some catch-up by treatment villages.

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[4] We use a 4-point scale without the neutral mid-point of the 5-point scale to force choice. Use of the 4-point can yield greater inter-respondent reliability under conditions where respondents are well-acquainted with the issue being rated (Chang 1994; Cummins and Gullone 2000).

We explore these effects in the framework of a more completely specified regression model. As individual SEWA membership is potentially endogenous to our outcome variables, and since we are primarily interested in the effect of the opportunity of the intervention on women in targeted villages, we use residence in SEWA village as our treatment variable and estimate intent-to-treat (ITT) effects with the following linear probability model:

$$Y_{i,v,b,t} = \beta_0 + \beta_1 \text{SEWA}_v + \beta_2 \text{Post-intervention}_t + \beta_3 (\text{SEWA}_v \times \text{Post-intervention}_t)$$

$$+\beta_4 \mathbf{X}_{i,v,b,t} + \mu_b + \varepsilon_{i,v,b,t}$$

where $Y_{i,v,b,t}$ is the outcome of interest for individual *i* in village *v* in block (sub-district) *b* during survey year *t*. *SEWA* takes value 1 if the respondent resided in a village selected for SEWA's program, *Post-intervention* is a dummy variable that takes value 1 if the household was interviewed after the treatment program, **X** is a vector of household-control variables, μ is a block fixed-effect, and $\varepsilon_{i,v,b,t}$ is a standard disturbance. β_1 is the pre-program difference between control and treatment villages, β_2 is the trend, i.e., the changes in the outcome in the absence of the treatment, and β_3 is the ITT effect. All standard errors are clustered at the village-level.

Control variables include the respondent's age, literacy, and marital status, and number of children below the age of 15. In addition, we include dummies coded 1 if the respondent: is a member of a Scheduled Tribe; a member of a Scheduled Caste; is head of household; is part of a household that owns its own land; lives in a non-permanent (*kutcha*) dwelling (considered a proxy for income and assets that is likely to be unaffected by a two-year intervention); and zero otherwise.

Conditional results are shown in Table 2, and confirm findings in Table 1. Following the intervention, residence in a SEWA village following the intervention increases the likelihood that women will know where to report grievances regarding water, and that they will actually

field such grievances. Compared to control group means, results suggest an increase in knowledge of and action regarding local water supply by some fifty percent. Results also suggest a relative improvement in SEWA village women's water quality and safety, and access to irrigation for agriculture post intervention (the result is marginally insignificant for access to piped water).

As a test of robustness, we examine whether these ITT effects are present at the village level. For our key dependent and independent variables we aggregate household (female) responses in order to derive village-level percentages. These data form a panel of village-year observations comprised of each village before and after SEWA's rural livelihoods program. All village-year estimations use OLS with error correction for contemporaneous correlation across villages ("panel-correct" standard errors). Our results are reported in Table 3. Columns (1) – (3) confirm the effects of SEWA's intervention on improvements in the percentage of households with access to piped water, and concomitant reductions in those who have access to "very bad" water, as well as those who rely on unsafe water sources at the village level.

All dependent variables, thus far, have been survey based—either individual responses to questions regarding water supply, or aggregated responses by village. We examine whether the intervention, therefore, has yielded improvements in village water infrastructure. Village-wise data on water supply that are coterminous with the SEWA intervention are not available. However the 2011 *Census of India* includes a "Houselisting and Housing" module that records household access to drinking water, among other amenities. We take the percentage of households, by village, whose main source of drinking water is tap water from a treated source. The previous census (2001), unfortunately, does not report village-wise access to drinking water, and thus we have no baseline. On the assumption that control and treatment villages resembled

each other at the start of the intervention, however, we can use differences between control and treatment villages in 2011 to validate SEWA village effects described above.

Column (4) in Table 3, therefore, is a cross section of villages in 2011. Despite these reduced degrees of freedom, we see that SEWA villages were five times more likely to have access to piped water (note that a comparison of the sample means at the bottom of the Table shows that, in both cases, the fractions of household with access were relatively small). In sum there is evidence that SHG villages saw improved water provision along multiple dimensions during the course of the intervention compared with control villages.

Fostering Cooperation: Evidence from a Public Goods Game

We have seen that the organization of marginalized women into village-based groups facilitated tangible improvements in a critical local public good, namely, water. However, we do not know precisely why collective action increased among these group members. Understanding the channels by which coordination is facilitated is important for thinking about the long-term consequences of the intervention. One such channel is that SHG participation fosters norms of cooperation and collective action.

Within SHGs, cooperation can emerge as a preferred strategy where one believes that other group members will similarly cooperate. This type of strategy selection mechanism has been examined in the context of reciprocity within ethnic communities (Habyarimana, *et al*. 2007), as well as infinitely repeated transactions (Camera and Casari 2009). The basic theoretical result is that cooperation is an equilibrium when agents are involved in long-term interactions, are sufficiently patient, and have information on the actions of others. To the extent that SHGs provide a stable environment for members, facilitate intra-group communication, and enable

members to commit to and enforce mutual cooperation, one would expect that an efficient outcome would be achieved due to a combination of positive and negative inducements. On the one hand, SHG membership along with the repeated interactions occurring in the group context can decrease the variance in strategy selection by members—that is, create expectations that cooperation will be reciprocated—and thereby reduce risks in contributing effort. Additionally, exclusion from group membership can serve as punishments for non-cooperative behavior. Habyarimana, *et al*. (2007) find that "social sanctioning" is a major contributor to greater cooperation among co-ethnics.

To assess the mechanisms that lead to cooperation among members of SHGs, we played a variant of a repeated provision-point public goods game (Davis and Holt 1993; Ledyard 1995; Eckel and Grossman 2008). The game was implemented in each of seven treatment and control villages in 2011. Female participants were randomly selected in each village. Group sizes ranged from nine to 14 women, but were made comparable across treatment and control villages as much as possible.[5]

In our setup, each subject was assigned a number, to ensure anonymity in the distribution of payouts. At the beginning of each round of the game, subjects were given coupons worth Rs. 20 and were told that these would be redeemable for cash at the end of the game. The players were then asked to contribute privately any portion of the Rs. 20 into an envelope on which their identifying number was written, which they would then place into a box. If the total amount contributed by all players exceeded a certain provision point, initially set at $N \times 10$ where N is the number of players, that amount would be doubled and distributed back to the participants in

[5] Variants of this game have been widely played in both classrooms and field settings (see, e.g., Abbink, Sadrieh, and Zamir 2002; Semmann, Krambeck, and Milinski 2003).

equal amounts.[6] If the provision point was achieved in the current round, it would be raised by 20 percent in the subsequent round, and increased steadily to a maximum of $(N - 1) \times 20$. The game ends when this maximum provision point is reached, or after 10 rounds, whichever comes first. If the combined contributions did not exceed the provision point, all contributions were lost for that round. Contributory decisions were anonymous and players were instructed not to discuss their actions with others, minimizing the risks that outside pressure or expectations should influence the outcomes.[7] Players were not told when the game would end. Each game was played without revealing the number of rounds until the end of the game.

The literature suggests that contributions to the public good are higher in the presence of thresholds or provision points (Marwell and Ames 1980; Isaac, Schmidtz, and Walker 1989; Cadsby and Maynes 1998), and that these games are more efficient at soliciting the true valuation of public goods (Rondeau, Poe, and Schulze 2005). In our setting, the provision points give us an opportunity to study whether women from treated villages are more likely to converge towards the cooperative outcome when a cooperative focal point is provided in the game structure.

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[6] In threshold games, any contributions above the threshold are wasted, while in provision-point games, any contributions above the threshold also contribute to total repayment. Formally we can write the payoff of a representative player in the stage game as

$$U_i = 20 - c_i + I2\frac{\sum_{j=1}^{N} c_j}{N},$$

where

$$I = \left\{ \begin{array}{l} 1 \; if \; \sum_{j=1}^{N} c_j \geq \theta \\ 0 \; if \; \sum_{j=1}^{N} c_j < \theta \end{array} \right\}.$$

The parameter c_i is the individual specific contribution, N is group size, θ is the provision point, and I is an indicator function taking on the value of 1 if total contributions exceed the provision point and 0 otherwise

[7] We are cognizant that measuring and scrutiny can exaggerate pro-social behavior (Levitt and List 2007), and therefore played all rounds and all games without any counting of individual balances until the game concluded.

Game Results

Games were played in 14 villages (7 treatment, 7 control) beginning one year subsequent to the follow-up survey in two batches (8 games played in winter, 6 games played in the following summer). We distinguish between three types of players; SHG members in treatment villages, non-members in treatment village, and women in control villages. We hypothesize that SHG members and non-members will behave differently in these group games. First, membership itself can create group identity, as detected in experimental studies using highly artificial means to divide players into groups (e.g. Chen and Li 2009). Second, SHG members have been told the benefits of collective action and are accustomed to making decisions in groups. Most of them are also part of rotating credit and savings scheme, implying repeated interaction that requires a certain amount of trust and cooperation to operate properly. Third, to the extent that SHG membership is highly valued, the potential threat to be excluded from the group also creates an additional sanctioning tool for non-cooperative behavior. We therefore expect SHG members to display higher levels of trust and contribute greater amounts to the public goods than their non-member village counterparts, something that should be measurable in the first round of play. Given our survey evidence on the externalities of SEWA programs, however, we also do expect that non-members in treatment villages will feature more cooperative behavior than women in control villages.

Game players were briefly surveyed in order to obtain basic information regarding their age, socio-economic status, education and occupation. Differences between the characteristics of treatment and control populations are presented in Table 5, showing no significant difference between the two. Summary statistics for differences in some key game characteristics in control and treatment villages are listed in Table 6. We also differentiate between the first round and the

subsequent rounds, as the first round picks up norms of cooperation and trust most cleanly (Berg, Dickhaut, and McCabe 1995). Subsequent rounds, additionally, can show any learning that takes place under conditions of repeated interactions.

Figure 1 indicates that average contributions in the first round are the highest for SHG members in treatment villages, and the differences compared to the other groups are statistically significant. Women in treatment villages contributed, on average, Rs. 12.5 compared to women in control villages who gave Rs. 8.6 in the first round—a difference of over 45 percent representing almost one-fifth of the initial endowment.[8]

There are also differences between SEWA members and non-members within treatment villages. Recall that we played "mixed group games", that is, with groups containing both members and non-members, in two treated villages. In the first village, the group was evenly split between members and non-members. In the second group, 4 out of the 14 players were non-members. First-round average contributions were Rs. 4 lower in the first village (with an even split) than the second village (with a 70-30 split) and the difference is statistically significant at the 1% level. Small sample size renders these results far from conclusive, but they point in the same direction as other results.

The difference persists over subsequent rounds of the game. Comparing average contributions over the whole game is complicated by the fact that game length varied, and control villages tended to play more rounds, as they were slower to converge towards

[8] Reviews of public-goods games in other settings find wide variations in cooperativeness, but players typically contribute 40 – 60 percent of the initial endowment, even as theory predicts zero contributions (Heinrich, *et al*. 2001; Davis and Holt 1993; Janssen and Ahn 2003). Contributions tend to be lower if initial endowments are more unequal or if groups are more heterogeneous (Cadsby and Maynes 1998).

cooperation.[9] All villages played least three rounds; for these three rounds women in treatment villages contributed an average of Rs. 11.7 while their counterparts in control villages contributed an average of Rs. 9.4.

SHG members contributed more than non-members in the first round, but, as shown in Figure 2, non-members in treatment villages then increased their contributions to match that of SHG members. In the end average contributions across all rounds were almost identical, and the difference between members and non-members is statistically insignificant, suggesting that non-members may initially be uncertain of the extent to which members have internalized cooperative norms, but after learning act as conditional cooperators. The main divide in subsequent rounds is between residents of treatment and control villages, rather than between members and non-members.

Since the games varied in the number of players and rounds, and the provision points could vary across villages in the same round of play, a more robust comparison requires controlling for these differences. Regressions take the following form:

$$Z_{i,r,v} = \gamma_0 + \gamma_1 \text{SEWA Member}_i + \gamma_2 \text{SEWA Non-member}_i + \gamma_3 \mathbf{X}_i + \gamma_4 \mathbf{R_i} + \upsilon_{i,r,v}$$

where $Z_{i,r,v}$ is the outcome of interest indexed by individual i in round r of the game in village v. *SEWA Member* takes value 1 if an individual was a member of an SHG in a treatment village, *SEWA Non-member* takes value 1 if an the individual resides in a treatment village but was not a member of an SHG, **X** is a vector of individual control variables, **R** is a vector of round-specific variables, and $\upsilon_{i,r,v}$ is a random disturbance. Of the parameters to be estimated, γ_1 is the direct treatment effect (SHG membership), γ_2 is the indirect treatment effect (within-village spillover).

[9] Players did not know whether another round would be conducted. We announced each round as a "surprise".

Additional individual control variables include age and literacy (measured on the basis of whether game players could sign their name). We also include round-specific controls, including round dummies and provision point. Finally, we include dummies for the days of the week, and the particular season in which the games were played. All errors are clustered by village.

Table 6 presents results from the first round. We look at four outcomes: (1) individual contributions; (2) gross payouts (total earnings from the common fund distributed equally); (3) the difference between a woman's payout and the amount she would have earned from playing the Nash strategy of contributing nothing ("gap"); and (4) net earnings (gross payout less the contributed amount).

Contributions, which most directly reflect norms of trust and cooperation, are significantly higher for SHG members who contribute some 15% (Rs. 3) more than non-members in both treatment and control villages. That cooperation also benefits SHG members, who receive larger payouts than they would have from defecting. Note that the "SEWA village, non-member" dummy is insignificant, indicating that the main differences in first-round strategy selection fall between SHG members and all non-members.

In Table 7 we present regressions including data from all rounds. Estimates in Table 7 suggest that on average all women in treatment villages—both SEWA members and nonmembers—contribute Rs. 4 (20 percent of their per-round endowment) more than women in control villages. As in the first round, higher contributions are also associated with higher benefits: treatment village residents receive on average an extra Rs. 19.5 in gross payouts, and an extra Rs. 16 in net earnings, compared to their counterparts in control villages.

Overall, the results suggest that once learning becomes a factor in subsequent rounds, differences in strategy selection between treatment and control villages are greater than those

between members and non-members. Returns to cooperative behavior become stronger in subsequent rounds as non-members raise contributions, and as all women from treatment villages receive higher payouts and achieve greater earnings from game play than women in control villages.

These data can also be used to examine differences in strategies for women as a function of their own gains/losses throughout the game. We include lagged net earnings (payout less contributions) to determine whether the propensity to contribute is affected by previous round net gain/loss. In column (2), the coefficient for lagged net earnings is negative in the case of contributions, suggesting that women in control villages who received higher payouts in the previous round by contributing less lowered their contributions in the following round. However, the positive, significant coefficient for lagged earnings indicates that women in treatment villages behaved in the opposite way: a woman who profited in the previous round from contributing less increased her contributions in the current round. Synchronization of strategies ensured that these women collectively achieved higher payouts than the zero-contribution strategy. This may be the strongest piece of evidence in support of the hypothesis that SHGs foster norms of cooperation. Note that lagged earnings and their interaction with the treatment indicator only affect current-round contributions and the difference between actual payout and the case where players simply kept the Rs. 20; neither variable affects gross or net payouts.

Testing an Alternate Channel: Preferences

An alternative, potential channel by which cooperation may be achieved is through the convergence of preferences for public goods. The management of disagreements over which public goods to prioritize plays a crucial role in facilitating (or obstructing) cooperation. A well-

known problem in multi-ethnic societies, for example, is that group identity is associated with public goods preferences. Thus Rabushka and Shepsle (1972) argue that democracy is infeasible in ethnically divided societies because polarized preferences will prevent effective power sharing. Alesina, Baqir and Easterly (1999) argue that ethnic diversity, along with resulting divergent preferences over types of public goods, leads to lower aggregate provision. This central idea—that divergent preferences undermine cooperation—transfers to the problem of cooperation among the rural poor. In our empirical setting, it is possible that group interaction within the framework of the SHG caused preferences among women regarding public goods to converge through indoctrination by SEWA, through shared information, or through group pressure.

Converging preferences, if observed in SHG villages, can also suggest elite capture: that group activities are appropriated by local elites. If village elites have pressured, cajoled, or otherwise influenced SHG members to select a specific public good, then endemic inequities of class or caste may increase in response to the presence of organizations like SHGs (Platteau and Gaspart 2003). Moreover, group members may internalize elite preferences over time.

Concordance Estimates

To test whether preferences for public goods are converging we estimate an inter-rater reliability coefficient that quantifies the extent of agreement across subjects on the rating or ranking of a set number of variables. We provide concordance coefficients pre- and post-intervention, for control as well as SEWA villages, and for the latter, for both SEWA members and non-members post-intervention. We use Krippendorff's alpha (α) coefficient, a measure widely used to assess the reliability of subjectively-coded response data by multiple coders, $0 \leq \alpha$

≤ 1, with $\alpha = 1$ being perfect agreement, $\alpha = 0$ perfect disagreement. Comparisons of various measures of inter-coder reliability have shown that Krippendorff's α carries lower annotator bias and bias due to "category prevalence," i.e., where a disproportionate amount of annotated data falls into a particular coding category (Antoine, Villaneau, and Lefeuvre 2014; Arstein and Poesio 2008).[10]

Treating all residents within a particular group as "raters" we estimate alpha coefficients for the group based on how individual respondents rank public goods priorities, as well as how they rate the quality of individual public goods. If the SHG intervention has caused convergence of preferences we expect to see a difference-in-difference effect, that is, an increase in concordance among women in SEWA villages post-intervention in excess of that observed among women in control villages. Women were asked rank the top three village-level priorities among water, sanitation, health, electricity, education, work/employment, and roads. They were then asked to rate each of these services with respect to their quality and availability on a standard (5-point) Likert scale. The upper panel of Table 4 shows alpha coefficients for rankings of public goods priorities; the lower panel shows coefficients for ratings of public good quality.

All alpha coefficients are increasing over time, suggesting some convergence. This increase, however, is larger in the control than in treatment villages, suggesting that the temporal increase has little to do with the intervention. Alpha coefficients for quality ratings, similarly, show weaker convergence in villages where SEWA was present compared to those where

[10] The basic form for Krippendorff's α coefficient is $\alpha = 1 - (D_o/D_e)$ where D_o is the observed disagreement and D_e is the disagreement one would expect when the coding of units is attributable to chance rather than to the properties of these units themselves. Where $D_o = 0$ and $\alpha = 1$, there is perfect reliability. When observers agree as if chance had produced the results, $D_o = D_e$ and $\alpha = 0$, which indicates the absence of reliability. For details on the calculation of Krippendorff's α see, e.g., Krippendorff (2011), and Hayes and Krippendorff (2007).

SEWA was absent. Overall we find little evidence of the SHG intervention causing preferences for public goods to converge.

Conclusion

Two decades of research indicates that public goods provision is impeded by the inability of individuals to coordinate for a common purpose, and that in this regard, the poor face steep coordination constraints. The resulting, weaker relative bargaining strength of low-income groups has been shown to decrease investments in, and raise the cost of access to, public goods and services available to poor communities.

In this paper we have sought to examine whether a marginalized group of women lacking any history of mobilization, and suffering from costly deficiencies in a particular public good, can organize itself to improve local public goods provision. Finding that it can, we then seek to determine why. Our investigation consists of two parts; an evaluation of the effects of self-help groups on local public goods relying on a randomized controlled trial, and a lab-in-the-field experiment to better understand intra-group cooperative strategies.

We focus principally on the extent to which the intervention positively affected water delivery and quality, and find three principal effects. First, female residents of villages where self-help groups were started possess greater knowledge of how to address problems of water supply and water deficiency. Second, women in self-help group villages are more likely to contact local authorities regarding their grievances with respect to water service. Third, women in self-help group villages report greater improvements in water access and infrastructure compared to their counterparts in control villages. Indicators of the quality of water, access to piped water, and reliance on unsafe surface/rainwater are confirmed by data from the 2011

census on village-level water infrastructure. Results suggest strong spillovers from members to non-members in villages where self-help groups were established.

A series of public goods games played in control and treatment villages strongly suggest that the presence of self-help groups contributed to trust and cooperation among village women. SHG members initially contribute more than both non-members in SEWA villages and women in control villages. In subsequent rounds, when learning becomes a factor, non-members in SEWA villages contribute roughly the same amount as members, and significantly more than women in control villages. Meanwhile women initially benefitting from non-cooperation in the previous round tend to persist with that strategy in control villages. But in treatment villages norms of cooperation trump selfish behavior even when women benefit from free riding; we observe that those early "winners" increase their contributions in subsequent rounds in villages where self-help groups are present.

That the main difference lies between treatment and control villages, not between SHG members and non-members in treatment villages, suggests that the intervention of SEWA, which is meant to encompass all women in the village, indeed increases trust and cooperation beyond just the group members. From a group identity perspective this means that SEWA has succeeded in having self-help group members internalizing other-regarding preferences beyond just membership, identifying also other women in the village as being part of the group whose interests they share. We find no evidence that women in treatment villages—whether self-help group members or non-members—have moved towards holding common preferences regarding which public goods are deficient or about which public good improvements should be prioritized.

Although these findings may not be applicable to all cases where marginalized groups face collective action constraints, our results—however preliminary—suggest that self-help groups have achieved increased coordination among marginalized communities not by forcing members to value the same things, but rather, by changing incentives members face by minimizing individual risks of cooperation. This result stands in contrast to much evidence concerning the lack of social capital formation in community-driven development type programs. We argue that the specific characteristics of self-help groups programs may lie behind the results, in particular, the repeated interactions not necessarily oriented towards the design and implementation of specific development projects, but simply as a means of institutionalizing effective local collective action.

Table 1: Unconditional differences in pre- and post-treatment water access

	Pre-treatment		Post-treatment	
	Mean	**Difference**	**Mean**	**Difference**
Knowledge of PHED	0.164 *0.183*	0.019	0.250 *0.406*	0.156***
Contacted PHED	0.139 *0.160*	0.021	0.220 *0.349*	0.128***
Overall water quality	0.174 *0.166*	-0.008	0.241 *0.370*	0.297***
Access to piped water	0.112 *0.134*	0.022	0.088 *0.182*	0.093***
No safe water source	0.162 *0.281*	0.119***	0.069 *0.052*	-0.017
Access to irrigation for farming	0.011 *0.007*	-0.004	0.025 *0.061*	0.035***

Notes: Differences are generated from two-sample t-tests by control/treatment group (treatment means are italicized). * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 2: Water access and quality, conditional effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Knowledge of PHED	**Contacted PHED**	**Overall water Quality**	**Access to piped water**	**No safe source**	**Irrigation for farming**
SEWA Village	0.011	0.015	-0.032	-0.008	0.120*	-0.006
	(0.021)	(0.022)	(0.039)	(0.042)	(0.067)	(0.008)
Post-Intervention	0.093***	0.087***	0.067*	-0.013	-0.091**	0.014
	(0.025)	(0.023)	(0.035)	(0.029)	(0.041)	(0.011)
SEWA Village × Post-Intervention	0.122**	0.095*	0.145**	0.080	-0.141**	0.039**
	(0.051)	(0.052)	(0.062)	(0.052)	(0.071)	(0.017)
Age	-0.001	-0.001	0.002*	0.001**	-0.001*	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Head of Household	0.010	0.010	0.021	0.071***	-0.024	-0.013
	(0.031)	(0.030)	(0.027)	(0.026)	(0.023)	(0.009)
Scheduled Tribe	-0.058**	-0.051*	-0.157***	-0.260***	0.040	0.002
	(0.028)	(0.026)	(0.037)	(0.047)	(0.039)	(0.010)
Scheduled Caste	0.044	0.052	-0.074	-0.086	0.038	-0.001
	(0.065)	(0.063)	(0.054)	(0.084)	(0.043)	(0.015)
Literate	0.168***	0.130***	0.095***	0.075***	-0.025	0.006
	(0.030)	(0.032)	(0.027)	(0.023)	(0.016)	(0.009)
Married	-0.058*	-0.044	-0.042	0.029	-0.067**	0.004
	(0.034)	(0.037)	(0.043)	(0.028)	(0.030)	(0.014)
Children	-0.001	-0.002	-0.002	-0.004	0.011**	0.002
	(0.005)	(0.005)	(0.005)	(0.003)	(0.005)	(0.002)
Landowner	-0.035	-0.026	0.029	0.013	0.018	-0.006
	(0.024)	(0.026)	(0.022)	(0.015)	(0.016)	(0.007)
Kutcha	-0.080***	-0.059***	-0.117***	-0.113***	0.018	-0.014
	(0.021)	(0.020)	(0.027)	(0.026)	(0.017)	(0.009)
N	3,205	3,205	3,195	3,198	3,198	3,205
R^2	0.101	0.076	0.123	0.288	0.080	0.021
Control group mean	0.208	0.181	0.209	0.100	0.114	0.134
Treatment group mean	0.291	0.251	0.265	0.157	0.171	0.178

Notes: Results are OLS with errors clustered by village. Block (*tehsil*) fixed effects and intercepts are estimated but not reported. * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table 3: Water quality and access, village-level results

	(1)	(2)	(3)	(4)
	Percent with piped water (Survey)	**Percent with poor access to water (Survey)**	**Percent with no safe source (Survey)**	**Percent with piped water (Census 2011)**
SEWA Village	1.474**	-1.093	6.579***	5.417**
	(0.681)	(0.812)	(0.891)	(2.456)
Post-Intervention	1.334*	-21.297***	-12.644***	
	(0.753)	(1.485)	(0.801)	
SEWA Village ×	5.358***	-8.276***	-6.609***	
Post-Intervention	(1.301)	(1.298)	(2.189)	
Literate (%)	0.162***	-0.460***	-0.085	0.037
	(0.029)	(0.141)	(0.202)	(0.101)
Married (%)	-0.167	0.324	0.363	-0.162
	(0.249)	(0.297)	(0.228)	(0.235)
Kutcha (%)	-0.239***	0.019	-0.097	-0.080
	(0.054)	(0.106)	(0.080)	(0.051)
Scheduled Tribe (%)	-0.339***	0.198**	0.089	-0.132**
	(0.071)	(0.098)	(0.149)	(0.058)
Scheduled Caste (%)	-0.417**	0.021	-0.001	-0.286**
	(0.170)	(0.146)	(0.104)	(0.127)
N	157	157	154	79
Villages	80	80	77	
R^2	0.506	0.383	0.132	0.356
$(p > \chi^2)$	0.000	0.000	0.000	
Control village mean	7.310	66.421	12.070	1.260
Treatment village mean	14.465	58.877	14.945	7.193

Notes: Observations are village-year (before/after) aggregated from baseline and endline surveys (columns 1 – 3) or taken from the *Census of India 2011* (column 4); census figures are for a village cross-section. Survey-based results are generated using OLS with panel-correct standard errors, corrected for contemporaneous correlation across villages. Census-based results are OLS with robust standard errors. * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table 4: Differences in demographic characteristics, game players

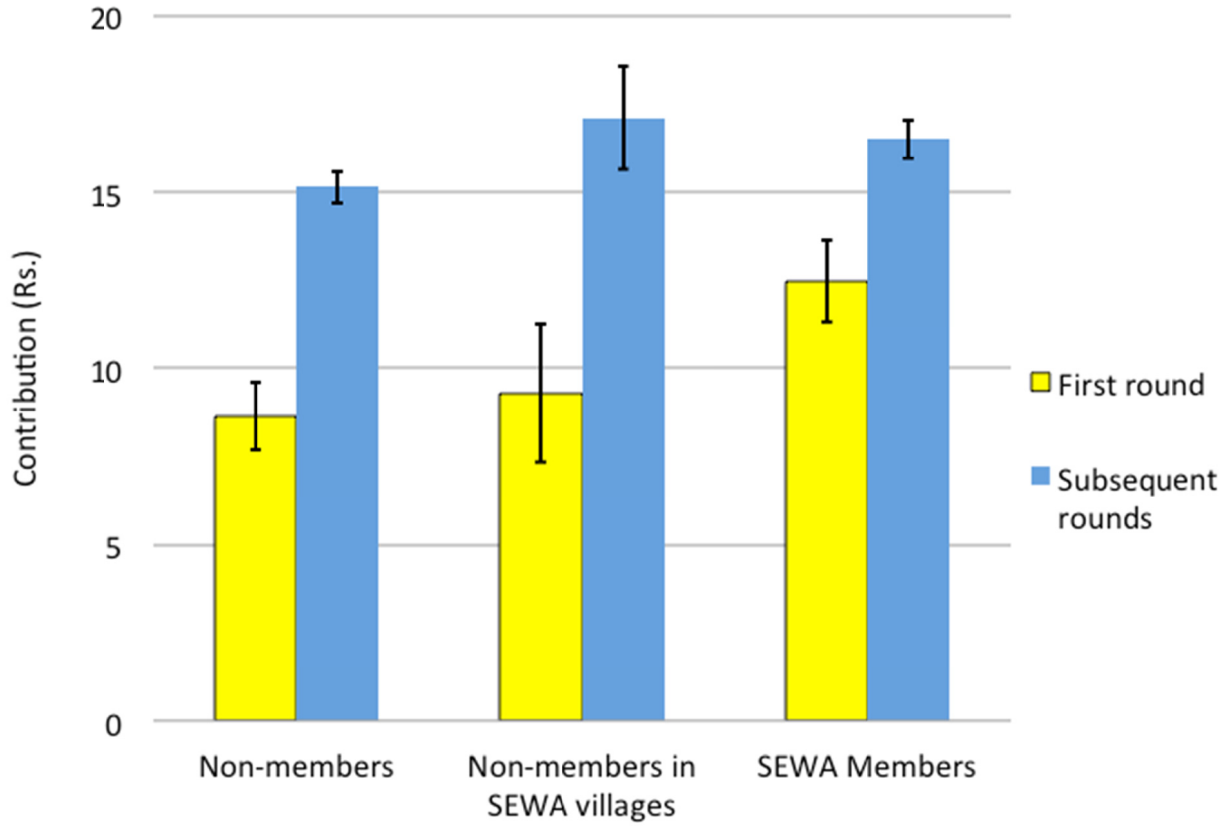
	Control mean	Treatment mean	Difference
Age	32.950	32.604	-0.349
Education	1.440	1.416	-0.025
Literacy	0.233	0.313	0.080

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Summary statistics for all rounds, treatment and control areas

	Control	Treatment	
	Non-Members	SEWA Members only	SEWA Members and Non-Members
Number of rounds played	6.5	4.6	4.75
Number of players in each round	12	12	15
Fraction of rounds in which cooperation was achieved	0.51	0.59	0.62
Average of payouts per round – Payouts from Nash strategy of contributing 0 in each round (Rs.)	6.05	31.23	25.46

Figure 1: Average contributions by group



Notes: Bars are average contributions of different cohorts for first round and all subsequent rounds, with ±95% confidence intervals.

Figure 2: Average contributions by group-round

Table 6: Regressions results for experimental games, first round

	(1)	(2)	(3)	(4)
	Contribution	**Payout**	**Gap**	**Net Earnings**
SEWA Member	2.926***	2.935	3.142***	0.009
	(0.757)	(1.951)	(0.719)	(1.823)
Non Member,	-0.317	-1.474	-0.189	-1.157
SEWA Village	(0.683)	(2.753)	(0.730)	(3.180)
Provision Point	0.143***	0.304***	0.102***	0.162***
	(0.017)	(0.054)	(0.012)	(0.043)
Age	-0.007	0.027	-0.016	0.034
	(0.041)	(0.038)	(0.040)	(0.048)
Literate	-0.061	-0.317	-0.197	-0.257
	(0.456)	(1.015)	(0.473)	(1.093)
N	186	186	186	186
R^2	0.415	0.756	0.354	0.354

Notes: Village-clustered standard errors are in parentheses. Intercepts and seasonal dummies are included but not reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Regression results for experimental games, all rounds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Contribution		**Payout**		**Gap**		**Net Earnings**	
SEWA Member	3.861***	4.032***	20.946***	24.371***	4.446***	4.414***	16.997***	20.178***
	(1.082)	(1.022)	(4.743)	(5.150)	(0.902)	(0.698)	(3.966)	(4.496)
Non-Member, SEWA Village	3.254**	4.128***	19.511***	22.784***	3.925***	4.609***	16.157***	18.479**
	(1.296)	(1.305)	(5.984)	(7.227)	(1.019)	(0.970)	(5.232)	(6.460)
SEWA Village × Net Earnings$_{r-1}$	0.017**	0.012**	-0.113***	-0.134***	-0.033***	-0.037***	-0.131***	-0.147***
	(0.006)	(0.005)	(0.027)	(0.028)	(0.007)	(0.006)	(0.024)	(0.025)
Net Earnings$_{r-1}$		0.082**		0.058		0.069		-0.021
		(0.038)		(0.202)		(0.041)		(0.184)
Provision Point		-0.060**		-0.172		-0.075**		-0.117
		(0.027)		(0.129)		(0.027)		(0.132)
R^2	1,034	848	1,046	860	1,046	860	1,046	860
N	0.417	0.368	0.435	0.510	0.114	0.135	0.448	0.499

Notes: Village-clustered standard errors are in parentheses. Intercepts, coefficients for age and literacy, along with dummies for round played, day, and season in which games were played are estimated but not reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Concordances for control and treatment groups

		Pre-intervention	Post-intervention	Difference
Rankings				
	Control	0.514*	0.756***	0.242
	Treatment	0.479*	0.620***	0.140
	SEWA members		0.599***	0.120
	Non-members, SEWA villages		0.655***	0.176
Ratings				
	Control	0.102	0.149	0.048
	Treatment	0.113	0.156	0.042
	SEWA members		0.137	0.024
	Non-members, SEWA villages		0.182*	0.068

Notes: Figures are Krippendorff's α inter-rater concordance/reliability coefficients, calculated for each cohort pre- and post-intervention. "Raters" are individuals within each group. Control refers to all residents of non-SEWA villages, treatment refers to both members and non-members in SEWA villages. Upper panel shows reliability coefficients when individuals are asked to rank the top three village-level priorities: water, sanitation, health, electricity, education, work, and roads. Lower panel shows reliability coefficients when individual are asked to score each of these issues with respect to their quality and availability. Significance levels are generated from bootstrapped distributions of Krippendorff's α to obtain confidence intervals. For SEWA members and non-members in SEWA villages, differences are calculated against pre-intervention treatment means.

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