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Can the Poor Be Mobilized?

Cooperation and Public Goods in Rural India*

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Abstract

High coordination costs are often blamed for the low quality of public goods available to the poor. Participatory development programs have sought to lower these costs by improving trust and cooperation, but the evidence of their impact is mixed. We examine financial "self-help" groups in one of the poorest districts in India, using a unique combination of a village-randomized controlled trial and a lab-in-the-field experiment. A survey of 1,600 women before and after the intervention shows that the presence of these groups improved access to, and quality of, a critical local public good: water. Public goods games played with 184 participants in a subset of control and treatment villages indicate that cooperative norms are stronger where self-help groups were present. We find little evidence that membership leads to a convergence of tastes among group members. These results suggest that, in contrast to traditional community initiatives, self-help groups can build durable social capital in poor communities.

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Effective states are underpinned by social cooperation. Tocqueville's observation in *Democracy in America* was that, in communities characterized by trust and collective efficacy, citizens are more likely to demand long-term investments that benefit all community members, to punish elites who use public institutions to extract rents, and to incentivize good behavior by administrators and officials.

It is unsurprising, then, that public goods and services in poor communities are inadequate. A defining feature of poor communities is the limited capacity of their inhabitants to engage in collective action. Early assessments of political society detailed in, for example, Durkheim's concept of "mechanical solidarity" (1947 [1893]) or in Weber's description of "patrimonialism" (1968 [1922]), 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). Others have confirmed that 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 uniformly worse for the poor, who must rely on them almost exclusively (Devarajan and Reinikka 2004; DFID 2010).

Efforts have been underway for several years to increase coordination as a means of reducing the scarcity of public goods available to the poor. International donors, in particular, have sought to expand their support for "community-driven" or "community-based" development projects that encourage the poor jointly to design, deliver, maintain,

and monitor local public goods. The record of community developmental 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 investigate the effects of a special type of community organization on the quality of public goods in rural villages. Our investigation takes place in India where we examine the "self-help" group—a membership-based organization that combines elements of classic rotating credit and savings associations with various other group-related activities, and whose effects are less understood than traditional participatory-development programs (Chen, *et al.* 2007). There are compelling reasons to evaluate how the quality and accessibility of local public goods are influenced by efforts such as self-help groups that, in contrast to other community-driven initiatives, are typically focused on organizing the poor for a broader range of purposes beyond the implementation of specific projects.

Indian self-help groups almost exclusively target rural women and their needs. We focus on 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, where women have traditionally prioritized investments in water over other public goods (Chattopadhyay and Duflo 2004). Although self-help groups were not established specifically to resolve water supply problems or any other public goods deficiency, we argue that the norms of mutual trust and reciprocity acquired in one domain (joint 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, who (unlike "scheduled castes") lack their own national political party, and who face high levels of civic exclusion (Chandra 2004). Recruitment of tribal women into self-help groups can provide a test of the capacity of marginalized groups to mobilize and, in so doing, improve the accountability and quality of public goods delivery.

We highlight three principal village-level effects. First, female residents of villages where self-help groups were established became better informed as to how to address problems of water supply and water deficiency. Second, women in self-help group villages were more likely to contact local authorities regarding deficiencies in water service. Third, women in self-help group villages report greater improvements in access to safe drinking water sources compared to their counterparts in control villages—a finding confirmed by the decadal Indian census.

We hypothesize that group interaction and mutual monitoring through the SHG's savings and credit scheme lowers individual-specific risks of cooperation and strengthens collective incentives to spur improvements in water quality. To test the presence of this possible mechanism, we conducted a series of simple, repeated public-goods games in a subset of treatment and control villages. We find that, in the first round, residents of treatment villages contribute almost 50% more than control village residents. Thereafter, although the gap closes, treatment village residents consistently contribute greater amounts than residents in control villages.

Finally, we test an alternative channel by which self-help groups may enhance citizens' capacity to demand improvements in public goods: shared preferences. If

residents in treatment villages moved toward common preferences regarding public goods to a greater extent than their control village counterparts, the cause could be better information sharing among villages, or the influence of village elites/group leaders, rather than intra-village cooperation. Pre- and post-intervention concordances across individuals within treatment and control villages, however, do not show any greater convergence of preferences among residents of villages where SHGs operated relative to residents of villages without SHGs.

Our analysis makes two contributions. First, we add to a small but growing body of evaluations using combinations of randomized-controlled trials and lab-in-the-field techniques to understand better the effects of development interventions as well as behavioral changes that may occur among program participants (Levitt and List 2007; Fearon, Humphreys, and Weinstein 2009). Second, although self-help groups have proliferated in India (and around the world) evidence of their impact is limited compared to more traditional community programs. 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 self-help groups on intra-group coordination. Our results suggest that self-help groups can strengthen norms of cooperation within the framework of the rotating credit and savings association and other group-identity fostering activities, and in so doing, improve public service provision by creating an institutional platform for collective action. These findings suggest that self-help groups may be a more effective way of bolstering cooperation within marginalized communities than traditional participatory development 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 a number of factors, including: 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); greater vulnerability to shocks (Adger and Neil 2003); limited residential mobility (Gugerty and Kremer 2008); and an overall sense of "exclusion" (Warren, Thompson, and Saegert 2005), to name a few. 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), enhance access to financial markets (Karlan 2007), or improve common pool resource management (Ostrom 1990; Ahn and Ostrom 2008). Still, poverty-stricken areas are almost everywhere characterized by under-provided public goods and badly functioning public services.

Community Initiatives, Self-Help Group, and Cooperative Behavior

Community-driven development (CDD) initiatives have become an important strategy to improve service delivery by allowing greater local control over developmental resources. In a typical CDD, communities receive external investment resources for local development, and are involved in all stages of planning, implementation, and monitoring of those projects. A CDD project also requires beneficiaries to establish a local

committee, council, or other organization open to all citizens, that acts as an interface between the community and counterpart local or municipal governments, as well as national governmental ministries or agencies. That participatory body is a hallmark of a CDD, enabling affected citizens to have a voice in decision-making, and to control the allocation of project funds. Reflecting a broader movement toward greater decentralization of service delivery, 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).

Somewhat differently conceived is the "self-help" group (SHG), one of a class of membership-based organizations that seek to facilitate the self-sufficiency of participants. In Indian villages, SHGs have typically been established with the help of local non-governmental organizations (NGOs) that generally have broad rural antipoverty agendas, although in recent years several state governments have sponsored their own SHGs. An Indian SHGs typically consists of 10 – 20 members who meet regularly to pool small amounts of money into a common savings fund until there is sufficient capital to begin lending in small amounts to those same members.

Although SHGs operate without any guarantee of external funding or subsidy, the National Agricultural Bank for Rural Development (NABARD) of India sponsors the SHG-Bank Linkage Program that connects the SHG with a rural bank, and enables SHGs to place their joint savings on deposit, as well as to borrow larger sums for group microenterprise activities from these banks. Besides acting as a classic rotating credit and savings association, SHGs can also sponsor village activities (leadership training for village residents, campaigns to expand birth control use or improve maternal nutrition,

are common). Financial intermediation, therefore, is often 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 a mechanism for long lasting, better coordination among participants to improve local governance.

Some observers consider SHGs to be one of several categories of CDD initiatives (e.g., Aveedko and Gilligan 2015; Deininger and Liu 2008). For our purposes, however, SHGs can be distinguished from the more common CDD in three critical ways. First, while CDD initiatives are typically oriented around an intended set of public goods and services, SHGs are established principally to build social cohesion among members by promoting self-employment and financial self-sufficiency, without reference to any "promised" public good (Chen, *et al*. 2007). CDD initiatives are set up for the utilization of earmarked funds: citizens of a community then form a local entity for the purpose of managing the investment of those funds. While some of the participatory bodies set up for the purpose of utilizing CDD project resources may survive, most do not outlast the grant or loan that funds the project. By contrast, SHGs are intended to be permanent fixtures of village civil society.

Second, in contrast to CDDs, SHGs tend to target marginalized populations within a community. Because CDD projects rely on the voluntary participation of the entire local population, they can quickly becoming controlled by elites if the opportunity costs of participation for the poor are high. Reviews of CDD projects indicate that participants tend to be from wealthier, more educated, and more politically connected households. They also tend to be men, and to belong to ethnic or tribal groups that enjoy higher status (Abraham and Platteau 2004). By contrast, SHGs almost exclusively recruit

poorer, rural women, who generally have lower incomes, fewer assets, and greater vulnerabilities than their male counterparts (King and Mason 2001). They are also 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 female-only.

SHGs are not established for the explicit purpose of addressing problems of public goods. But SHGs can exert an indirect influence on public goods by creating an environment in which members face incentives to coordinate their actions. First, in any given community, the probability that members who interact socially today will do so in the future is high, and thus individuals are encouraged to behave in ways that avoid retribution from others (Bowles and Gintis 2002). Within the SHG savings and credit scheme, and there are strong incentives for any individual SHG member to contribute savings and to approve loans to group members, since that member benefits from others doing the same. Second, repeated interaction is a fundamental component of SHG membership, and the frequency of interaction potentially lowers the costs associated with obtaining information about the characteristics as well as the past and future actions of other members (Duffy and Ochs 2009). The more easily acquired this information is, the more that members will favor collectively beneficial outcomes. Finally, the peer-based monitoring and sanctioning mechanism of SHG finance can potentially resolve incentive-compatibility problems that can arise when an individual member's actions affecting group welfare are not governed by legally-enforced contracts.

Cooperative behavior is more likely to emerge when individuals face low costs of information, have opportunities to coordinate their actions, engage in repeated

interactions, and can reward contributors and punish free-riders—all of which are facilitated by SHGs. In time, therefore, the presence of SHGs is expected to foster norms of mutual trust, increase group bargaining power with respect to service providers, and develop an institutional platform to address local issues of importance to members. Whereas CDD programs deliver public goods hoping that improvements to intra-group cooperation will follow, SHGs aim for the reverse: to enable group members to provide each other with mutual financial support with the expectation that public goods will improve in quality over time as a result of improved social cohesion.

Evidence

Examinations of CDD programs suggest that the impact on social capital tends to be weak or short-lived (Wong 2012). Much of this evidence is drawn from post-conflict states such as Sierra Leone (Casey, Glennerster, and Miguel 2012), Congo (Humphreys, Sanchez de la Sierra, and van der Windt 2012), Afghanistan (Beath, Christia, and Enikolopov 2013), and Sudan (Avdeenko and Gilligan 2015). These evaluations find little evidence of program impact on social capital, trust, or pro-social behavior. Even in more stable countries such as the Philippines (Labonne and Chase 2011) and Zambia (Chase and Sherburne-Benz 2001) the evidence of improvements to social capital is mixed.

Unlike the case with CDD programs, the evidence regarding the impact of SHGs on either the creation of social capital, or public goods quality is limited. Several studies have focused on Andhra Pradesh, where over 40 percent of all registered Indian SHGs are located (Galab and Rao 2003). Evaluations of the *Velugu* SHG program, for example,

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 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 is strongly correlated with the likelihood that a woman will run for local political office. Deininger and Liu (2009a; 2009b) also find that the program increased trust in other villagers, elected representatives and government representatives, as well as women's attendance in village meetings. Similarly, Casini, Vandewalle, and Wahaj (2015) find that women in these groups are more likely to take collective action to rectify public goods deficiencies. Others have found that SHGs increase the likelihood of common action to correct public service deficiencies (Datta 2015; Khanna *et al*. 2015). The validity of these evaluations, however, is typically constrained by the non-random placement of programs across villages

Village Groups and Rural Water Supply

Research was conducted in Dungarpur district, a rural district of 1.4 million people located in the "tribal belt" of southern Rajasthan, and one of the poorest districts in India. 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). Literacy levels are only 60 percent among men and 38 percent among women (the Indian national averages are 82 percent for men and 65 percent for women). Two-thirds of the Dungarpur workforce is employed in agriculture.[1]

Dungarpur was part of the national "Backward Districts Initiative" which gave block grants to states in which India's 100 poorest districts were located. In 2007 Rajasthan's Ministry for Rural Development invited the Self-Employed Women's Association (SEWA), an NGO with a long history of supporting informal-sector women in neighboring Gujarat, to establish SHGs in Dungarpur as part of the initiative. SEWA's mission is to organize women to help them achieve "economic independence through self-reliance," and claims a membership of almost two million women in twelve Indian states and the Delhi municipality (SEWA 2014; Datta 2000; Bhatt 2006).

Randomization

SEWA rolled out its SHG program in two sub-district blocks (*tehsils*) in Dungarpur district in early 2008.[2] In cooperation with SEWA, we assigned villages as control or treatment as follows: all 475 villages on the census listing for Dungarpur from these two blocks (from 2001) were stratified based, first, on mean female literacy rate, next on total village population, and finally on average household size, thus creating eight sampling bins of roughly equal size. From each of these strata 4 and 6 villages were

[1] Additional details regarding Dungarpur district can be found in our appendix.

[2] As described in our appendix, the two *tehsils* were chosen by Rajasthan state authorities and SEWA due to the lack of other NGO operations as well as their low household participation rate in existing SHGs at the start of the intervention in 2007.

randomly selected for treatment and control, respectively, generating a sample of 32 villages for the SEWA program and 48 villages as controls (80 villages in total). The location of control and treatment villages may be seen in Figure 1.

Membership in SHGs cannot be randomized; all "self-employed" women in a village where SEWA is present are eligible to join the organization. Thus only the assignment of villages as treatment and control is random, and we focus on intent-to-treat (ITT) effects at the village level. Members, upon paying nominal dues, were organized into SHGs, each with approximately 20 members, and each with their own elected leaders (*Agewans*), with the number of SHGs per village depending on village population. *Agewans* were given initial training by SEWA facilitators over two weeks. Only women residing in the village were permitted to join the SHGs. Once set up, SHGs met once a month and set savings targets of Rs. 50 per member to be deposited into a group savings account at an SHG-linked bank.

Baseline and follow up surveys of the study areas 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 female residents selected at random from villages where SEWA programs were implemented.[3] 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

[3] SEWA's own target across treatment villages was to recruit half of the female residents into SHGs. In our sample, 55% of respondents in treatment villages are members of SEWA's SHGs (std. dev. = 7%). Additional details regarding SEWA, member recruitment, the nature of the intervention, and household sampling procedures can be found in our appendix.

women selected at random from villages where SEWA was not present over the two-year period, with 855 interviewed in 2007 and 940 in 2009.[4] Balance tests (shown in our appendix) show the villages to be well-balanced across a number of pre-intervention indicators. Women in treatment villages are slightly older, less likely to be a member of a Scheduled Tribe, and less likely to live in non-permanent (*kutcha*) houses, but the magnitudes of these differences are small.

Water Access and Quality

We examine several outcomes of interest including the diffusion of accurate information on notification procedures with respect to water supply problems, as well as effort undertaken to lodge complaints with the relevant authority. India's rural water supply and sanitation utilities are part of a complex institutional framework, with overlapping and often vague responsibilities allocated across the various levels of government (Chaturvedi 2011). As a result, water management in India varies widely by state and, within states, by district, with local councils of villages (*Gram Panchayats*) and state authorities all potentially involved in the provision and maintenance of water sources.

In Rajasthan, as in some other states, the *Gram Panchayat* is the statutory body responsible for use of central- and state-government funds, and for preparing and implementing plans for drinking water delivery and irrigation, rural water management,

[4] These villages may have had other SHGs operating besides SEWA's, but none participated in SEWA's SHG intervention. Two villages that were originally designated as treatment villages were reclassified as control villages because of delays in the implementation of SEWA programs. All results in this paper are robust to the exclusion of these villages from the sample.

and watershed development. Maintenance functions are split between the Rajasthan Public Health and Engineering Department (PHED)—through its network of sub-district *tehsil* branch offices—and the *Gram Panchayat*, which are jointly responsible for testing water quality and for the repair of existing water pipes, hand pumps, and public taps. Although the system is formally decentralized, the central government continues to cover a portion of the cost of rural water supply, with the result being that the District Collector—the central government's agent at the district level—retains considerable *de facto* authority over rural water supply schemes.

Given this fragmentation of authorities, identifying those responsible for deficiencies in water supply can be problematic. Nevertheless, we test the hypothesis that the presence of SHGs expands women's knowledge of authority structures in the community and motivates them to address water deficiencies in three ways. First, we measure learning by asking respondents' if they are aware of the public office or agency where they can report problems with water quality or access. We matched women's responses to a list of appropriate authorities at the village, *tehsil*, or district level, marking as wrong any responses that were incorrect (teachers, health workers, etc.) or vague ("the government"). This gives us a variable coded 1 if women picked an appropriate agency for water-related concerns, zero otherwise (in our pre-treatment sample, less than one-fifth of women correctly identified a water authority). Second, on the assumption that the more complaints received from villagers, the more likely relevant authorities are to respond, we ask respondents whether they have in the past six months, actually contacted any public official to complain about public water supply. Third and similarly, we ask

respondents whether they have, in the same time frame, contacted the *Sarpanch* (elected head of the *Gram Panchayat*), to complain.

We also examine three additional measures of water quality. First, we asked women to assess their village water quality on a 4-point Likert scale ("very good," "somewhat good," "somewhat bad," or "very bad").[5] We rescale this as a binary variable by coding "very" and "somewhat" good as 1, 0 otherwise. Second, we asked women whether they had access to drinking water through a piped connection.[6] Answers are coded 1 for affirmative responses, 0 otherwise. Third 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. The null-hypothesis of equal means cannot be rejected pre-intervention. Across both types of villages, however, improvements in water provision

[5] 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).

[6] The World Health Organization (WHO) considers an "improved" source to be water delivered through a tap, "tube" or sandpoint well, or hand pump, all of which access groundwater that is protected from surface contamination. Similarly, the Indian Government refers to "protected" water sources as being water delivered through hand pumps, tube wells, and bore wells. We emphasize "piped" water rather "improved" water sources for several reasons. First, although improved water is considered safe by the Indian public, data show that it is heavily contaminated (Shaheed, *et al*. 2014). Second, the Indian Government, under its 10th and 11th five-year plans (2002 – 2011) aimed to reduce the percentage of the rural population "living beyond the pipe," which entailed reducing reliance on hand pumps and wells, and increasing coverage of piped connections (Ministry for Water Resources 2002). Third and most critically, unlike piped connections, wells and pumps can be dug or installed privately without reference to any collective action. Rural piped systems (usually single-village systems) involve water being pumped into an overhead holding tank from which a small network of pipes carries water to communal collection points in the served villages (McKenzie and Ray 2004). There are no centralized piped systems for Dungarpur villages to access, making it impossible for households to obtain individual connections. Therefore, piped connections require villagers to act collectively in several respects. Villagers must agree on the location and construction of storage tanks and piping to distribute water. Villagers must also convince the *Gram Panchayat* to pay a small fraction of capital expenditure (although the state and central governments cover more than 75% of the costs). Villagers must also ostensibly take some responsibility for maintaining the storage tanks and pipelines (although much of the maintenance is handled by the state PHED in practice).

can be seen over the period of the two-year intervention in terms of: awareness of an appropriate water-related authority, interaction with public officials and with the *Sarpanch* regarding water supply, overall water quality, access to piped water, and access to irrigation for farming.

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

$$Y_{i,v,s,t} = \beta_0 + \beta_1(\text{SEWA Village})_v + \beta_2\text{Post-intervention}_t + \beta_3((\text{SEWA Village})_v \times \text{Post-intervention}_t) + \beta_4\mathbf{X}_{i,v,s,t} + \mu_s + \varepsilon_{i,v,s,t}$$

where $Y_{i,v,s,t}$ is the outcome of interest for individual *i* in village *v* in strata *s* during survey period *t*. *SEWA Village* 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 sampling strata-fixed effect, and $\varepsilon_{i,v,s,t}$ 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 makeshift (*kutcha*) dwelling; and zero otherwise.

Conditional results are shown in Table 2, and confirm the mean-difference tests in Table 1. Following the intervention, residence in a SEWA village following the intervention increases the likelihood that women will have accurate information as to where to report problems regarding water services, and that they will actually contact officials responsible. 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 access to irrigation for agriculture following the intervention (the result is marginally insignificant for access to piped water).[7]

We also examine whether these 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 SHG program. 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 (based on the overall quality of water assessment).

[7] In our appendix we examine conditional ITT effects on other local public goods and services, including roads and transport, electricity supply, primary education, and healthcare. SHG presence has small, positive effects on the quality of roads and healthcare, but no effect with respect to the propensity of villagers to contact public officials on matters to these services.

All dependent variables, thus far, have been survey based—either individual responses to survey questions regarding water supply, or responses aggregated by village. Moreover, although water delivered through piped systems is less likely to be contaminated than water extracted *via* wells and hand pumps—water in storage tanks being more easily protected from pathogens than groundwater—more than half of the pipes in rural India deliver untreated water (Safe Water Network 2014). Within the survey we cannot ascertain whether drinking water obtained by respondents is treated with chlorination or filtration, as respondents only see the water delivery method rather than the source. The village-wise "House-listing and Housing" module from the 2011 *Census of India*, on the other hand, collected data on the number of households whose main drinking water source is from a treated source. To examine whether our findings are supported by this external evidence, we take the percentage of households, by village, with access to treated water from the latest census. The previous census (2001) does not report village-wise access to various sources water (leaving us without a baseline). On the assumption that control and treatment villages resembled each other at the start of the intervention (confirmed by our balance tests), however, we can use differences between control and treatment villages in 2011 to validate SEWA village effects described above.

Column (3) in Table 3, therefore, is based on a cross section of villages in 2011. Despite the reduced degrees of freedom, we see that SEWA villages were five times more likely to have access to water from treated sources (note that a comparison of the sample means at the bottom of the table shows that, in both cases, the fractions of households 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.

Intermediate Outcomes

We replicate our benchmark specification across a range of mediating outcomes shown in Table 4. Residence in a SEWA village is significantly associated with positive ITT effects with respect to participation in SHG meetings and savings behavior. Although women in SEWA villages report more regularity in contributing to their private savings, we do not see evidence that SEWA's presence increases the likelihood of receiving credit from rural banks through the SHG-bank linkage.

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). We asked women whether they or others (including their husbands, other family members, or outsider) make decisions regarding: whether to keep their children in school, whether to take their children to medical-care providers if they are sick, and whether to use contraception. By no means is this an exhaustive list of family priorities; rather, the ability to make unilateral decisions about children is a proxy for their broader "autonomy." In all three cases, SEWA village residents report they have greater voice in these decisions relative to their counterparts in control villages.

In addition to having a stronger role in household decision-making, SEWA village women interact and engage with the village-level decision-making bodies (the *Gram Panchayat and Gram Sabha*) to a greater degree than women in villages without SEWA. In particular, women living in SEWA villages tend to attend more meetings of the *Gram*

Sabha and more likely to interact with, or have business pending before the *Gram Panchayat*.

We also see evidence of what SEWA's presence did not change over two years. There is no change, relative to cohorts in control villages, in farm or non-farm income. Neither is there any change in indicators of voting. Improvements in rural water supply cannot, therefore, be attributed to changes in wealth or in increases in overall political engagement.

Measuring Cooperation: Evidence from a Public Goods Game

We have seen that organizing marginalized women into village-based groups facilitated improvements in a critical local public good, namely, water—an important effects, since women are principally responsible for collecting water. Surveys, however, can only measure coordination among group members indirectly. Moreover, if SEWA facilitators press SHG members to take group action on particular issues (including water supply), or if SEWA facilitators have somehow orchestrated group responses to village problems, then the sustainability of any increase in social capital may be weak.

We posit that repeated intra-group interaction within the framework of the SHG's mutual savings and microcredit scheme lowers collective action costs by enhancing intra-group trust, and by enabling members to internalize norms of reciprocity. There is behavioral evidence that repeated and long-term interaction can lead to cooperative behavior even among anonymous players (Camera and Casari 2009). Credible monitoring and punishment further strengthens incentives to cooperate. Habyarimana, *et al*. (2007), for example, find that "social sanctioning" is a major contributor to greater

cooperation among co-ethnics. Experimental evidence also shows strong group-identity effects where individuals grouped together tend to play more cooperatively towards each other, even within highly artificial groups whose boundaries carry little meaning for group members (Chen and Li 2010).

SEWA's SHG intervention features a number of opportunities to strengthen cooperative behavior among its membership. SHGs require repeated interaction and intra-group communication and, through the threat of exclusion, provide a means to enforce mutual cooperation. The mutual liability requirement of the rotating credit and savings mechanism, additionally, fundamentally relies on intra-group trust.

To gauge the extent to which the SHGs have contributed to the development of norms supporting cooperative behavior 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 a subset of fourteen villages that had been surveyed in 2007 and 2009—in seven treatment and seven control villages. To test whether the habits of collective action had been sufficiently internalized to survive without external prompting or incentive from organizers games precluding any opportunities for outside pressure were played in 2011, some four years after the initial launch of the Dungarpur SHG intervention. Games were conducted in two batches (8 games played in winter, 6 games played in the following summer). Group sizes ranged from nine to 14 female players.[8]

In our setup, each participant was assigned a random number to ensure anonymity in the distribution of payouts. At the beginning of each round of the game, subjects were

[8] Details regarding selection of villages and recruitment of subjects can be found in our appendix.

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 equal amounts.[9] If the combined contributions did not exceed the provision point, all contributions were lost for that round and game play continued into another round with the provision point unchanged. If the provision point was achieved in the current round, it would be raised by 20 percent in the subsequent round. The game ends when the maximum provision point of $(N - 1) \times 20$ is reached or after 10 rounds, whichever came first. 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.[10] After each round, all players learned how much had been contributed in total to the common pool, whether the threshold had been reached, and their individual earnings (paid out at the end of the game). Players were not told when the game would

[9] 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. 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).

[10] 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.

end. Each game was played without revealing the number of rounds until the end of the game.

The game features two types of Nash equilibria. In the first (non-cooperative) equilibrium all players contribute nothing to the public good, if they are self-interested. In the second (cooperative) equilibrium a subset of players contributes just enough to meet the provision point (many such equilibria exist with different subsets of players and different combinations of contributions).[11] 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.

Game Results

We expect that SHG village residents will display higher levels of trust and contribute greater amounts to the public goods than their control village counterparts. Game players were briefly surveyed in order to obtain basic information regarding their age, socio-economic status and education; these groups were well balanced.[12] We also differentiate between the first round and the subsequent rounds, as the first round picks

[11] Details specific game protocols and a more detailed formal representation of the game may be found in our appendix.

[12] Means for treatment- and control-village players, along with summary statistics for differences in some key game characteristics in control and treatment villages, are presented in our appendix

up norms of cooperation and trust most cleanly (Berg, Dickhaut, and McCabe 1995). Subsequent rounds, additionally, can show any learning of others' behavior that takes place under conditions of repeated interaction.

Figure 2 indicates that 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.[13] The difference persists over subsequent rounds of the game as shown in Figure 3. 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 cooperation.[14] All villages played at 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.

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:

$$Z_{i,r,v} = \gamma_0 + \gamma_1(\text{SEWA Village})_v + \gamma_2\mathbf{X}_i + \gamma_3\mathbf{R}_r + \upsilon_{i,r,v}$$

where $Z_{i,r,v}$ is the outcome of interest indexed by individual *i* in round *r* of the game in village *v*. *SEWA Village* takes value 1 if an individual was a resident of a treatment village, **X** is a vector of individual-specific control variables, **R** is a vector of round-

[13] 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).

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

specific variables, and $v_{i,r,v}$ is a random disturbance. Control variables include age and literacy (measured on the basis of whether game players could write 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.[15]

In our previous survey-based estimations we relied on cluster-robust standard errors, clustering at the village level (with 80 villages). In our public goods game, however, a major limitation of inference is that the asymptotic justification assumes that the number of clusters goes to infinity. With fourteen villages, there may be too few clusters in our sample, even if we do not face the problem of high variability in the number of observations per cluster. We use two alternatives to address this concern. First we rely on bias-reduced linearization rather than standard cluster-robust standard errors. Bias-reduced linearization, as proposed by Bell and McCaffrey (2002), can reduce bias by, dropping the OLS assumption of i.i.d. errors, and assuming only that errors from

[15] As with our survey results previously, we estimate ITTs. It is useful, however, to examine the behavioral effects of SHGs on those who receive treatment, or the average treatment-on-the-treated (ATT) effect, for a number of reasons. First, with group members and non-members evenly divided within treatment villages, it is possible that the ITT understates the treatment effect. Second, from a policy perspective, if SHG programs are to be scaled and to be used as platforms for member-specific training and vocational programs, then the key longer-term outcomes of interest (e.g., employment and income) would obviously be more relevant to members rather than non-members. Third, comparison of ATTs alongside ITTs can determine the extent to which cooperative norms held by members transfer to non-members within treatment villages (if the ATT significantly exceeds the ITT, the spillover is expected to be low). Naturally, estimation of ATTs raises issues akin to those raised by observational studies, since confounding variables may be related to receipt of treatment, as opposed to treatment assignment. In our appendix, we analyze the extent to which game results are driven by SEWA members rather than SEWA village residence. We rely on various techniques to adjust for potential imbalances between members and non-members across covariates, and conduct sensitivity analyses to test for the possibility of hidden bias due to unobserved heterogeneity. These results show village effects and SEWA membership effects to be highly similar. See the appendix for full details.

distinct clusters are uncorrelated. This method replaces ordinary residuals by residuals inflated to better approximate the covariance of the true errors.[16] Second, we use the wild-cluster bootstrap-*t* procedure as recommended by Cameron, Gelbach, and Miller (2008), in which the resampling procedure draws residuals at the level of the village cluster. We present *p*-values testing for the null hypothesis of zero-effect of residence in a SEWA village (i.e., where $\gamma_1 = 0$).

Table 5 presents results from the first round. We look at *three* outcomes: (i) individual contributions; (ii) gross payouts (total earnings from the common fund distributed equally); and (iii) net earnings (gross payout less the contributed amount). Contributions, which most directly reflect norms of trust and cooperation, are significantly higher for residents of SHG villages, who contribute some 15% (Rs. 2.5) more than residents of control villages. Cooperation does not initially benefit treatment villagers, who do not receive larger payouts as a result of their contributions.

In Table 6 we present regressions including data from all rounds. Estimates in Table 6 suggest that on average women in treatment villages contribute Rs. 4 (20 percent of their per-round endowment) more than women in control villages. Unlike the case in the first round alone, higher contributions are associated with overall greater returns: treatment village residents receive on average an extra Rs. 21 in gross payouts, and an extra Rs. 17 in net earnings, compared to their counterparts 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

[16] In this sense, bias-reduced linearization is considered a generalization of the finite-sample correction applied to OLS estimates.

(payout less contributions) to determine whether the propensity to contribute is affected by previous round net gain/loss. In column (2), the coefficient for the interaction of lagged net earnings with the treatment indicator is positive in the case of contributions, suggesting that women in treatment villages who profited in the previous round from contributing less increased their 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 Alternate Channels: 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 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 orchestrated by local elites. If village elites have instructed or otherwise influenced SHG members to prioritize a specific public good, then endemic inequities of class or caste may increase in response to the presence of organizations like SHGs (Bardhan and Mookerjee 2006; Platteau and Gaspart 2003; Dasgupta and Beard 2007).[17]

Repeated group interactions within the context of the SHG may also lead to preference convergence—in this case, with respect to emphasizing water infrastructure over other things. Once preferences have converged, collective action is more easily facilitated. Distinguishing between elite and member preferences is challenging as preferences of elites may be internalized, and the line between pressure and influence on the one hand and information sharing and discussion on the other is thin. In either case group formation may cause a convergence of observed preferences, and this convergence may lead to coordinated group action.

Estimating Concordance

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

[17] In the study by Sinha (2006), group leaders are typically better off and have more education than the average group members, and they tend to borrow more frequently from the joint savings (this partly reflects greater creditworthiness, though).

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, $0 \leq \alpha \leq 1$, 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).[18]

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

[18] 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).

scale. The upper panel of Table 7 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 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, local 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 a simple intervention requiring women to form group-based savings and credit societies affected the delivery and quality

of water. We find three principal effects: (i) female residents of villages where self-help groups were started possess greater knowledge of how to address problems of water supply and water deficiency; (ii) women in self-help group villages are more likely to contact local authorities regarding their grievances with respect to water service; and (iii) 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 a subset control and treatment villages strongly suggest that the presence of self-help groups contributed to trust and cooperation among village women. Women in treatment villages contribute significantly more than women in control villages in the first round, when norms of cooperation are captured most cleanly. In subsequent rounds, when learning about other players' behavior becomes increasingly salient, the pattern persists, with women in treatment villages contributing more than their counterparts 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.

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.

Figure 1: Location of SEWA and non-SEWA villages in Dungarpur District, Rajasthan

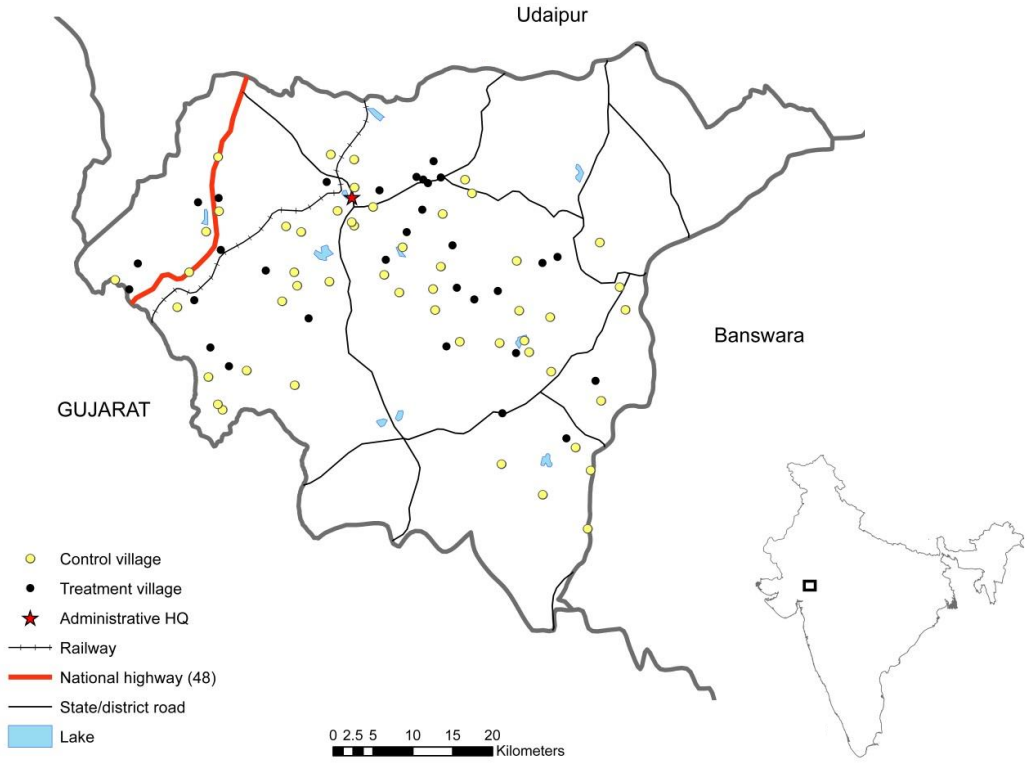


Table 1: Unconditional differences, pre- and post-treatment water access

	Pre-treatment		Post-treatment		
	Mean	**Difference**	**Mean**	**Difference**	**Difference in difference**
Knowledge of public authority	0.156 *0.180*	0.025	0.233 *0.378*	0.144***	0.119***
Contacted public official	0.164 *0.183*	0.019	0.250 *0.406*	0.156***	0.137***
Contacted *Sarpanch*	0.139 *0.160*	0.021	0.220 *0.349*	0.128***	0.107***
Overall water quality	0.174 *0.166*	-0.008	0.241 *0.370*	0.297***	0.137***
Access to piped water	0.112 *0.134*	0.022	0.088 *0.182*	0.093***	0.071***
Access to irrigation for farming	0.011 *0.007*	-0.004	0.025 *0.061*	0.035***	0.039***

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 public authority	**Contacted public official**	**Contacted *Sarpanch***	**Overall water quality**	**Access to piped water**	**Irrigation for farming**
SEWA Village	0.015	0.009	0.013	-0.024	-0.002	-0.006
	(0.023)	(0.022)	(0.022)	(0.040)	(0.046)	(0.010)
Post-Intervention	0.085***	0.094***	0.088***	0.071**	-0.008	0.010
	(0.024)	(0.025)	(0.023)	(0.034)	(0.027)	(0.010)
SEWA Village × Post-Intervention	0.111**	0.125**	0.097*	0.148**	0.084	0.040**
	(0.053)	(0.052)	(0.053)	(0.062)	(0.053)	(0.017)
Age	-0.000	-0.001	-0.001	0.001	0.001**	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Head of Household	0.000	0.007	0.007	0.017	0.063**	-0.011
	(0.031)	(0.031)	(0.029)	(0.027)	(0.028)	(0.009)
Scheduled Tribe	-0.034	-0.036	-0.026	-0.127***	-0.235***	-0.002
	(0.027)	(0.028)	(0.027)	(0.041)	(0.050)	(0.010)
Scheduled Caste	0.042	0.045	0.057	-0.073	-0.111	-0.002
	(0.061)	(0.062)	(0.063)	(0.051)	(0.082)	(0.016)
Literate	0.150***	0.165***	0.128***	0.091***	0.064***	0.006
	(0.032)	(0.031)	(0.032)	(0.026)	(0.022)	(0.009)
Married	-0.038	-0.053	-0.040	-0.039	0.034	0.003
	(0.039)	(0.034)	(0.037)	(0.043)	(0.028)	(0.014)
Children	0.002	-0.000	-0.001	-0.002	-0.004	0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.002)
Landowner	-0.035	-0.039	-0.029	0.028	0.009	-0.004
	(0.023)	(0.024)	(0.026)	(0.022)	(0.014)	(0.007)
Kutcha	-0.066***	-0.075***	-0.054**	-0.115***	-0.112***	-0.016*
	(0.021)	(0.022)	(0.021)	(0.026)	(0.027)	(0.008)
N	3,205	3,205	3,205	3,195	3,198	3,205
R^2	0.091	0.105	0.080	0.135	0.293	0.023
Control group mean	0.196	0.208	0.181	0.209	0.100	0.134
Treatment group mean	0.275	0.291	0.251	0.265	0.157	0.178

Notes: Results are OLS with standard errors clustered by village in parentheses. Selection-strata fixed effects and intercepts are estimated but not reported. Control and treatment means are pre- and post-treatment. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

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

	(1)	(2)	(3)
	Percent with piped water (Survey)	**Percent with poor access to water (Survey)**	**Percent with access to treated water (*Census* 2011)**
SEWA Village	1.474**	-1.093	5.417**
	(0.681)	(0.812)	(2.456)
Post-Intervention	1.334*	-21.297***	
	(0.753)	(1.485)	
SEWA Village × Post-Intervention	5.358***	-8.276***	
	(1.301)	(1.298)	
Literate (%)	0.162***	-0.460***	0.037
	(0.029)	(0.141)	(0.101)
Married (%)	-0.167	0.324	-0.162
	(0.249)	(0.297)	(0.235)
Kutcha (%)	-0.239***	0.019	-0.080
	(0.054)	(0.106)	(0.051)
Scheduled Tribe (%)	-0.339***	0.198**	-0.132**
	(0.071)	(0.098)	(0.058)
Scheduled Caste (%)	-0.417**	0.021	-0.286**
	(0.170)	(0.146)	(0.127)
N	157	157	79
Villages	80	80	
R^2	0.506	0.383	0.356
$(p > \chi^2)$	0.000	0.000	
Control village mean	7.310	66.421	1.260
Treatment village mean	14.465	58.877	7.193

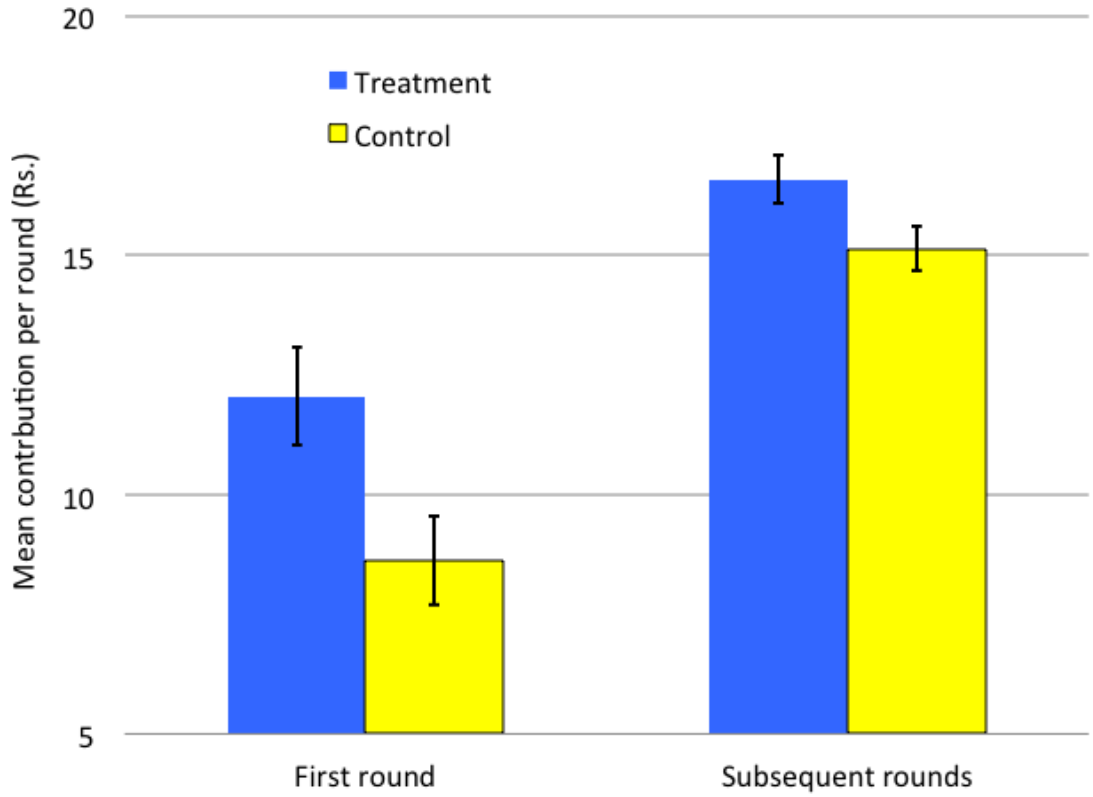
Notes: Observations are village-year (before/after) aggregated from baseline and endline surveys (columns 1 and 2) or taken from the *Census of India 2011* (column 3); census figures are for a village cross-section. Survey-based results are generated using OLS with panel-correct standard errors, corrected for contemporaneous correlation across villages. Census-based results are OLS with robust standard errors. Control and treatment means are pre- and post-treatment. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table 4: Intermediate outcomes, conditional effects

	Outcome	SEWA Village × Post-Intervention	Std. error	R^2
(1)	Participates in SHG activities	0.221***	0.051	0.101
(2)	Saves regularly	0.097**	0.044	0.029
(3)	Received a bank loan in the past 5 years	0.026	0.037	0.016
(4)	Has final say in children's schooling	0.053**	0.024	0.176
(5)	Has final say in medical decisions	0.063**	0.027	0.151
(6)	Has final say in family-planning	0.067***	0.017	0.032
(7)	Farm income in Rupees (past 3 months, Ln)	-0.000	0.109	0.021
(8)	Non-farm income in Rupees (past 3 months, Ln)	-0.327	0.318	0.053
(9)	Engaged with *Gram Sabha* and *Gram Panchayat*	0.026*	0.015	0.032
(10)	Generally votes in local elections	-0.010	0.011	0.051
(11)	Generally votes in state elections	-0.011	0.018	0.042
(12)	Generally votes in national elections	-0.019	0.027	0.045

Notes: Table shows intent-to-treat effects generated from OLS estimations based on table 2, with standard errors clustered by village, strata-fixed effects and an intercept. R^2 for the full specification is also shown (N = 3,205 except for equation 9, for which N = 2,399). * p<0.10; ** p<0.05; *** p<0.01.

Figure 2: Average contributions by group



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

Figure 3: Average contributions by group-round

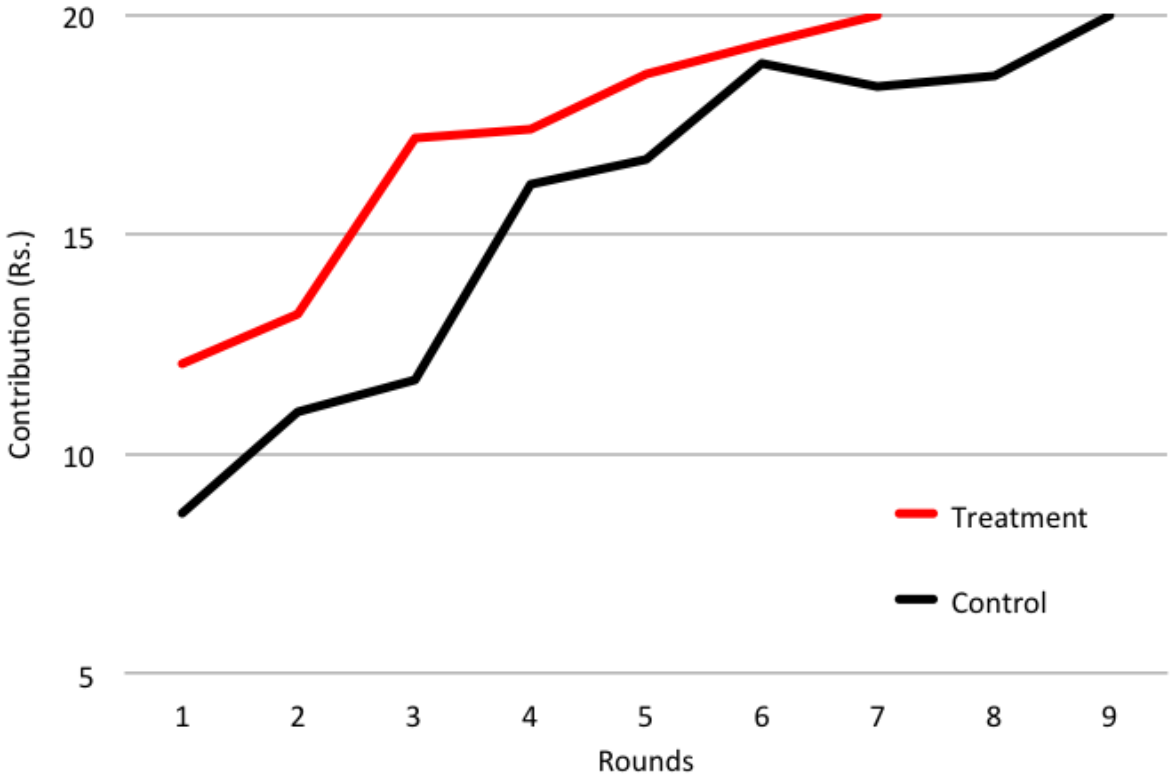


Table 5: Regressions results for experimental games, first round

	(1)	(2)	(3)
	Contribution	**Payout**	**Net Earnings**
SEWA Village	2.447***	2.284	-0.163
	(0.851)	(2.372)	(2.080)
Provision Point	0.144***	0.306***	0.162***
	(0.021)	(0.067)	(0.052)
Age	0.002	0.039	0.038
	(0.044)	(0.042)	(0.051)
Literate	0.122	-0.069	-0.191
	(0.541)	(1.268)	(1.289)
p-values from small-cluster adjustment:			
Cluster-robust SE	0.006	0.295	0.931
Bias-reduced linearization	0.005	0.337	0.937
Wild bootstrap-*t*	0.023	0.366	0.932
N	186	186	186
Adjusted R^2	0.393	0.737	0.352

Notes: Standard errors adjusted for (village-level) clustering with bias-reduced linearization in parentheses. Intercepts and seasonal dummies are included but not reported. Reference lines show *p*-values for "SEWA Village" coefficient from linear regression with cluster-robust standard errors, bias-reduced linearization, and cluster wild bootstrap-*t* with 5,000 resamples. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table 6: Regression results for experimental games, all rounds

	(1)	(2)	(3)	(4)	(5)	(6)
	Contribution		**Payout**		**Net earnings**	
SEWA Village	3.859**	4.033**	20.943***	24.360***	16.995***	20.167***
	(1.910)	(1.712)	(7.788)	(8.434)	(6.305)	(7.260)
SEWA Village × Net Earnings$_{r-1}$		0.082**		0.058		-0.020
		(0.041)		(0.220)		(0.206)
Provision Point	0.017*	0.012*	-0.113***	-0.134***	-0.131***	-0.147***
	(0.009)	(0.007)	(0.031)	(0.032)	(0.026)	(0.029)
Net Earnings$_{r-1}$		-0.060*		-0.172		-0.117
		(0.031)		(0.151)		(0.157)
p-values from small cluster adjustment:						
Cluster-robust SE	0.003	0.002	0.001	0.000	0.001	0.001
Bias-reduced linearization	0.044	0.019	0.007	0.004	0.007	0.006
Wild bootstrap-*t*	0.006	0.031	0.024	0.048	0.032	0.047
N	1,034	848	1,046	860	1,046	860
Adjusted R^2	0.404	0.354	0.425	0.498	0.438	0.487

Notes: Standard errors adjusted for (village-level) clustering with bias-reduced linearization in parentheses. Age and Literate (as controls) as well as intercepts and seasonal dummies are included but not reported. Reference lines show *p*-values for "SEWA Village" coefficient from linear regression with cluster-robust standard errors, bias-reduced linearization, and cluster wild bootstrap-*t* with 5,000 resamples. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table 7: 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 alpha (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 α to obtain confidence intervals. For SEWA members and non-members in SEWA villages, differences are calculated against pre-intervention treatment means. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

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Appendix

The Empirical Setting: Dungarpur District

One of the poorest districts in India, Dungarpur district lies on the Southern border of the state of Rajasthan.[23] Members of the "Scheduled Tribes"—designated indigenous groups whose disadvantaged status is acknowledged by the Indian constitution and national legislation—make up some 70% of the district population. The rural population (93 percent of the total) has traditionally relied on three major sources of subsistence: forest gathering, animal husbandry, and agriculture. Forests that used to cover 60 percent of the district in the 1940s shrank to less than 12 percent by 1999 (FSI 1999). Deforestation prompted many to turn to settled agriculture. Over three-quarters of the district labor force is now engaged in agriculture as cultivators or casual laborers.

Although the Aravalli range to the north separates Dungarpur from the Great Indian Desert, deforestation and the removal of soil cover from most of the hill slopes have caused considerable rainfall variability, with well-known consequences for agriculture and livestock grazing. The local duration of the rainy season (calculated based on the onset and withdrawal of the monsoon) shrank from 101 days in 1973 to 40 days in 2004 (Rathore and Verma 2013). Meanwhile, major droughts occurred four times in a two-decade period prior to the intervention examined here: in 1986-87, in 1987-88, in 2003, and in 2004-05.

One of the consequences of forest degradation and its effects on agriculture has been a massive increase in migration from Dungarpur. Income from migratory work accounts for more than half of average household income (compared to 19 percent from

[23] Unless otherwise noted, all figures cited here and in the following paragraphs, are drawn from *Census of India* (2011) data.

agriculture, 5 percent from livestock, and the rest from wage labor and self-employment). 91 percent of migrants from Dungarpur are men (Haberfield *et al*. 1999).

As a result, women play a prominent role in the district workforce: the female labor participation rate in Dungarpur is 43 percent, almost double the Indian average.[24] In the 2001 Census, Dungarpur was one of the few districts in Northwest India where the female-male sex ratio was relatively close to the biologically "normal," at 1,019 women for every 1,000 men, and the only district in Rajasthan where the ratio was in favor of women. Basic demographic indicators for Dungarpur district from the 2011 Census are listed in Table A1, and show that the sex ratio has since fallen to 963 (still higher than the district mean within Rajasthan of 928).

In 2004-05, finally, the per-capita income of Dungarpur stood at Rs. 12,474 (approximately $312[25]) compared to state average of Rs. 16,800 ($420), while 21 percent of the population lived below the national rural poverty line, placing Dungarpur among the country's poorest 10 districts (Government of India, 2009). A report of the Indian Planning Commission considers Dungarpur "perhaps the most underdeveloped district in India" (Planning Commission 2011, p. 79).

The Self-Help Group Intervention

In 1992 the Indian government adopted the *Swarnajayanti Gram Swarojgar Yojana* (SGSY) to organize families below the poverty line into self-help groups (SHGs) and

[24] A higher-than-average female labor-force participation rate than is typical of India's tribal regions (Bhalla and Kaur 2011).

[25] Here and throughout, for currency conversion, we use official Rupee-Dollar exchange rates (annual averages) for the relevant year(s).

provide them with 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 seed money to form "credit management groups" and to open savings accounts with rural banks (NABARD 2013). SHGs formed the cornerstones of rural development strategies of some state governments in the 1990s (Reddy and Manak 2005; Basu 2006; Chakrabarti and Ravi 2011). In the past decade, however, the SHG movement has proliferated across India in rural, and to a lesser extent, urban settings (Krishna 2007; Chen, *et al*. 2007).

In 2004, the Indian Planning Commission included Dungarpur in its "Backward Districts Initiative" (*Rashtriya Sam Vikas Yojana*) which aimed to "address the problems of low agricultural productivity, unemployment, and to fill critical gaps in physical and social infrastructure" through block grants to state governments, which would then distribute funds to eligible districts.[26] Rajasthan's state Ministry of Rural Development invited the Self-Employed Women's Association (SEWA), headquartered in neighboring Gujarat, to facilitate the establishment of SHGs in Dungarpur. Although SEWA was originally established in 1972 as a trade union for informal sector and self-employed women, it has transformed into one of the world's largest organizations of female workers.

SEWA began a rollout of its own SHG pilot in Dungarpur district in late 2007. Of the four sub-district blocks (*tehsils*)—Aspur, Dungarpur, Sagwara, and Simalwara—other

[26] The identification of backward districts within a state was made on the basis of three measures with equal weight: (i) value added per agricultural worker; (ii) average agriculture wages; and (iii) percentage of scheduled caste/scheduled tribe population in the districts (Planning Commission of India 2004).

organizations had established a presence in Aspur and Simalwara. Beginning in the 1980s, the People's Educational and Development Organization (PEDO) established village committees in Simalwara villages to improve primary education. In the 1990s PEDO began to establish SHGs in these villages, later expanding to Aspur (PEDO 2006). In Simalwara, a number of NGOs along with the Rajasthan state government's Tribal Development Commissioner's Office began to establish SHGs alongside efforts to improve watershed and common resource management in the mid-1990s (Simalwara having the highest proportion of Scheduled Tribe residents among the Dungarpur *tehsils*). Remaining dense and open forests are also concentrated in Simalwara, making this *tehsil* the least agriculture-based in the district.

By contrast, Dungarpur *tehsil* (where the town of Dungarpur and the district government seat are located) as well as Sagwara were relatively untouched by NGOs, and had a lower degree of village organizational density. These are the two *tehsils* in which SEWA chose to roll out its SHG pilot program.[27] Our sample indicates that about 13% of women in Dungarpur and Sagwara across control and treatment villages were SHG members pre-treatment.[28] By contrast, a comparison of rural Indian SHGs notes that in Rajasthan about one quarter of households had a female SHG member in 2006 (Wilson and Sinha 2006).

[27] Prior to the 2011 census, the western section of Dungarpur block was split of to form Bichhiwara block, increasing the number of sub-district divisions within Dungarpur from four to five. This separation occurred after the intervention examined here.

[28] A two-sample means test cannot reject the zero-difference null ($p = 0.542$).

All (adult) women in a village were invited to become members of SEWA by paying (as annual dues) Rs. 5 ($0.12), and then asked to join village-based SHGs.[29] These groups typically consist of groups of 10-20 women with usually two elected leaders (*Agewans*). The number of SHGs, therefore, varies according to the female population of the village. Once established participants met once a month and contributed approximately Rs. 50 ($1.20) per member, per meeting, which would be deposited into a common, SHG-linked savings account.[30] The pooled savings could then be used to make small, interest-bearing loans to individual SHG members over predetermined periods, with payment installments and interest rates determined jointly.

After six months of operation, an SHG becomes eligible to borrow from its linked rural bank. RBI guidelines exempt banks from requiring "collateral security" for loans up to Rs. 500,000 ($12,000 in 2007) to SHGs (RBI 2000). SHG members determine the uses for which loans to SHGs are to be put, but NABARD guidelines encourage loans for "productive purposes," such as, to establish SHG-operated microenterprises or to finance vocational programs for women (NABARD 1992).

SHG meetings last, on average, between 30 minutes and an hour and typically take place in a suitable, common village-based meeting place (such as a school or *Panchayat* office) or in the home of one of the group members. Group members come to meetings with their cash savings contribution and passbook—in which all contributions and, if any,

[29] Recruitment of members is carried out by making announcements about SEWA at village Panchayat meetings, and/or private meetings with educated and influential members of the village who then spread awareness about SEWA's programs.

[30] SEWA guidelines indicate that this amount may be adjusted upwards or downwards by as much as Rs. 25, depending on the targeted monthly saving amount that the group decides will be suitable.

loans that may have been withdrawn by the member are recorded. One of the *Agewans* conducts each meeting, while the other records the minutes, as well as each member's savings deposit (and any loan repayments) both in members' passbooks and on a separate bank ledger. The collected amount is deposited in the linked bank on the following day, normally by one of the *Agewans*. Other members, however, are also occasionally given responsibility for deposits in order to expose them to bank procedures. Deposit receipts from the bank are preserved until the deposited amount appears on the SHG's account. For the first four meetings for each SHG, local SEWA staff act as facilitators, managing the meeting agenda, advising *Agewans*, demonstrating account keeping and note taking, and encouraging participation by group members.

The meeting begins, and all members take turns to contribute their required amount and to have it recorded in their passbook (usually with a thumbprint). The meeting then opens to new individual loan requests from members. Any new loan is then discussed by group members, who settle on a loan period, interest rate, and number of repayment installments. The loan contracts are group-liability, and the group asks the new lender to take an oath promising regular repayment. The *Agewan* records the loan amount and transfers cash to the borrower. During SHG meetings, other related matters may be discussed such as, but not limited to, requests for postponement of repayments due to extraordinary circumstances or emergencies. If the SHG is seeking to take a group loan, an officer from the linked bank may also be in attendance, in which case savings contributions are passed directly to the bank officer who issues a deposit receipt.

Finally, group members are encouraged to discuss issues of mutual importance concerning the village. This includes, but is not limited to: specific issues facing neighbors

(ranging from health matters, to domestic abuse, to alcoholism, to social relationships), matters pending before the *Panchayat*, and complaints about the quality of local public services. They also have time to socialize with one another in an unstructured way before and after meetings, and while waiting for one's turn to deposit their savings into the group account.

Household Sampling Procedures

Village sampling was provided by the authors. All villages on the 2001 census listing for the Dungarpur district were stratified according to average female literacy rate, total number of households, and average household size, creating eight sampling bins of equal size. From these strata 4 and 6 villages were randomly selected for treatment and control, respectively, generating a sample of 32 villages for the SEWA program and 48 villages as controls (80 villages in total). All adult women in a SEWA village were invited to become SEWA members.

IMRB-Social and Rural Research Institute (New Delhi), a private survey firm with no relationship to SEWA, carried out household sampling and enumeration for both the baseline and follow-up surveys in villages selected by authors. Household sampling procedures were implemented in three steps in each village. First, enumerators held meetings with village leaders, including elders, elected representatives such as the village ward representative in the *Panchayat* (Ward *Panch*), *Panchayat* head (*Sarpanch*), and/or caste representative (*Mukhiya*) to obtain information related to current demographic characteristics of the village (number of households, male and female population, boundaries of the main sub-village neighborhoods or "hamlets"), and the distribution of

households among the hamlets. Where possible, more than one village leader was consulted in order to gauge the reliability of the information provided, particularly on the population estimates. In the event of stark differences in estimates given by different village leaders, further investigation of *Panchayat* records would be undertaken to obtain accurate estimates of population and the number of households in each hamlet.

Second, the village was divided into enumeration segments for the purposes of the survey. Enumerators designed a schematic representation of the village, distribution of hamlets and the population distribution in each of the hamlets for this purpose—the survey supervisor would physically walk around the key boundaries of the villages to draw a rough map with markings for natural and man-made boundaries, as well as the locations of clusters of households. Where the total number of households in all the hamlets combined was 200 or less, all households in the village would be listed in a common segment. Where the number exceeded 200, the village would be divided into segments of approximately 100 households each. Once the total number of segments was identified, in the event of more than one segment, half would be randomly chosen for household selection.

Third, a household listing exercise would identify all eligible households within these identified segments. All such households which had a female member 18-60 years of age who is a "key decision maker" in the absence of a male member of the family were considered eligible. Households that were locked at the time of screening and whose members were likely to return (as notified by neighbors) on the same day would, similarly, be treated as eligible. Locked households where household members were not expected to return by the end of the listing exercise were marked as ineligible.

Each eligible household within identified segments was then coded with a running serial number. The ratio of village-wide eligible households (H) to the desired households to be sampled (N) yields the sampling interval ($n = H/N$). A random number (r) would be picked from within the sampling interval for each of the villages, and eligible households were selected by adding the sample interval to the previous serial number of the selected household selected, r, $r + n$, $r + 2n$, $r + 3n$, …, N to reach the required number of eligible households to be sampled within each segment.

Surveys for the baseline were conducted between October and December in 2007. These steps were repeated prior to the follow-up surveys, which were completed between October and December in 2009. SEWA was not involved in the sampling of households or in the implementation of either survey.

Balance Tests and Other Local Public Goods

Panels (A) and (B) of Table A2 shows a simple test of pre-intervention means for control and treatment groups, for both village- and individual-level characteristics. In panel (A) we see that villages are well-balanced across several indicators, with none of the means tests able to reject the null of a difference between SEWA villages and non-SEWA villages. While the villages are randomly assigned, the same is not possible with individual women within the village, as SEWA procedures permitted all adult women in a village to join SEWA and to participate in its SHGs. We see some evidence in panel (B) that individuals surveyed in in SEWA villages pre-treatment were slightly older, less likely to be members of Scheduled Tribes, and less likely to reside in semi-permanent or temporary (*kutcha*) housing (considered a proxy for income and assets). The magnitude of these differences is

small: women in control and treatment villages are about one year apart in average age; six percent less likely to be members of Scheduled Tribes, and eight percent less likely to live in *kutcha* dwellings. By contrast the sample is well balanced across groups of individuals with respect to literacy, head of household, marital status, and land ownership.

Table A3 shows conditional intent-to-treat (ITT) effects regarding women's perceptions of public goods quality. We focus on four other categories: roads and transportation, electricity supply, primary education, and healthcare. As with rural water supply, governmental responsibility for each is fragmented across central, state, district, sub-district block, and village level authorities; all are under the authority of the public sector. SEWA's presence has some (positive) effect on the quality of roads and healthcare, indicating that the SHG program's benefits may not be limited to water.

The extent to which women were mobilized in pursuit of these goals, however, is less clear. Table A4 shows effects on the capacity and willingness to address problems regarding local public goods and services apart from water. Respondents were asked if they knew to whom to report problems with respect to access or quality of each category of local public good or service. They were also asked if they had contacted an official at that public agency. SHG presence has no conditional effect on increasing women's level of engagement in these areas.

Public Goods Game Recruitment and Protocols

Seven villages were selected from a list of 12 treatment villages in which the SHG intervention was implemented in the first month of SEWA's program. An additional seven control villages were chosen from the full list of 48 control villages through a one-to-one

matching process based on proximity to one of the seven treatment villages, as well as similarity with respect to female literacy and total population (based on data from the Indian census). In control villages, female participants were randomly selected by SEWA with the help of the village *Sarpanch*, and in coordination with the authors. In treatment villages, women were randomly selected by SEWA from the SHG member list. We complemented this with a smaller number of randomly selected non-members.

Game protocols were designed and implemented by the authors, and were played in July 2011 and again in December 2011 in a subset of control and treatment villages. Female game players were gathered into a common area (usually an outside courtyard, a school ground or inside a classroom or meeting hall. Women were instructed that they were going to play a game involving real cash but were not informed that this was a public goods game. Each player was then interviewed to obtain basic demographic information.

Players were each given two envelopes—one large manila envelope and one smaller white envelope—both with the same (random) number clearly visible on the front. In the large envelope were seven colored cards or "coupons" with numbers written on one side signifying the following denominations: one yellow card with a "10," one pink card with a "5," and five blue cards with a "1."

The game leader then explained the rules of the game as follows:

> In this game you have been given several colored coupons with numbers that represent different amounts of money. Please treat these as different types of rupee notes: the yellows are ten rupees [show yellow card], the pinks are five rupee notes [show pink card], and the blues are one rupee notes [show blue card]. All in total you have twenty rupees. At the end of the game, any coupons you have will be exchanged for cash. You have also been given two envelopes—one is large [show large manila envelope] and one is small [show small white envelope], but they both have the same number. Please remember this number.
>
> Now you will have to decide how much to contribute to a *lokphada* [village surety fund]. You can contribute any amount you want—from nothing, zero rupees, to the entire amount, twenty rupees. Please place whatever contribution you want to make in the small envelope and please put the rest in the large envelope [demonstrate with sample cards]. We will then count the total amount

of all contributions. If all of you together contribute more that [the game provision point] we will double this total amount and will distribute it back to all of you in equal amounts.

There are [e.g., 10] of you in this room. This means the total in the *lokphada* must be more than 100 rupees in order for the amount to be doubled and distributed back to each of you in equal amounts. If you all contribute 100 rupees or less, then your contributions will be taken and you will receive nothing except what you keep in the large envelope. Suppose some of you contribute the full 20 rupees and some of you contribute nothing, and others give amounts in-between. Let us say that when we count the money in the *lokphada* we see 150 rupees. We would double that to 300 rupees and give it back to everyone equally, which would mean that everyone would get 30 rupees each. It does not matter whether you contributed zero or twenty Rupees; each of you would still receive 30 rupees. If we distribute money back to you, please put these in the large envelope as well.

But suppose together you contribute less than [the game provision point]. If that is the case, each person will lose her contribution. In other words, suppose we count the *lokphada* and it holds 90 rupees then each person who contributed money would lose that amount they contributed.

At the end of the game, please bring us the large envelope and we will give you rupees in the amount of the face value of the coupons you have.

Players were not told that the game would last more than one round. In each round, contributions were collected by having the women place their white envelopes in a large box. Amounts in the white envelopes were then counted, and the total amount was announced. If the total exceeded the provision point, payouts were distributed back to the players in equal increments. If game play continued to the next round, empty white envelopes were distributed back to the players, keeping track of the numbers on the envelopes to ensure that they were returned to their same players. When game play concluded, women were asked to step into a private area where their large envelopes were emptied, counted, and their coupons exchange for cash. Summary statistics from game results are in Table A5.

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

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

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

The stage game has two types of pure strategy Nash equilibria. In the first of these, each player contributes zero. There are no incentives for any player to deviate unilaterally from this strategy profile, even if by doing so the provision point would be met, since the amounts are distributed back to all participants in equal shares irrespective of whether they contributed or not.[31] The second type of pure strategy equilibrium is a strategy profile in which the provision point is just satisfied, and all players who contributed a strictly positive amount get back at least as much as they gave.[32] If the latter condition is not satisfied, then a contributing player would be better off contributing nothing. On the other hand, there are no incentives for any player to contribute any amount above the provision point since that amount would be shared equally with all other players.

Formally, the conditions for a "cooperating equilibrium" to exist are that there is a sequence of $c = \{c_1, c_2, \ldots., c_n\}$ such that

$$\sum_{i=1}^{n} c_i = t, \text{ and}$$

[31] Strictly speaking, this statement requires that $N > 2$, since the contributions are doubled.

[32] This can be written as $c_i \leq 2\bar{c}\ \forall c_i$, where $\bar{c} = \frac{t}{N}$ is the mean contribution.

$$c_i \leq \frac{2t}{N} \; \forall c_i \in c.$$

There are several such equilibria with different combinations of contributions to satisfy minimally the provision point. Given the nature of equal contributions in the SHG savings groups, however, it may be particularly relevant to consider symmetric equilibria in which all players contributing a strictly positive amount contribute the same. The existence of symmetric equilibria requires that $\hat{n} \geq N/2$, where $\hat{n}$ is the number of players contributing a strictly positive amount. Participants did not know how many rounds would be played. This is typically formally treated as a game with an infinite horizon. In such an infinitely repeated game, it is possible to use trigger strategies to sustain an infinite number of equilibria, some of which involve even higher contributions. The welfare maximizing Nash equilibrium (as long as all players are sufficiently patient) is a strategy profile in which all players contribute their full amount in every round as long as all other players contribute, but turn to contribute zero for the rest of the game after a deviation. The repeated nature of the game should thus in principle make it possible to sustain a higher level of cooperation between the players.

Treatment Effects and Robustness Checks

On the assumption that improvements to village-level public goods are due to greater cooperative norms and trust inculcated within SHGs, the effects of the treatment specifically for those to whom treatment is administered is of potential interest. ITT effects within the context of the public goods game demonstrate the behavioral effect of offering the SHG program to village residents; they do not tell us the effect of participating in the program, or the effect of the average treatment on the treated (ATT). If we are interested

in estimating the behavioral effects of SHGs on those who join the group, then there is an important reason to focus on the actions of SHG members (rather than the effects across members and non-members within treatment villages). In the recruitment of players, moreover, we face a trade-off between being able to identify accurately the various behavioral responses of women directly affected by the intervention, and potential selection bias given that as those who chose to become members may not be representative of village residents.

Our main game results rely principally on residence in a SEWA village, rather than SEWA membership, as the key treatment indicator. In this section we run a series of tests for balance among observed covariates as well as for robustness of our main results to potential unobserved heterogeneity when examining the ATT (where we use SEWA membership as the measure of treatment). We use matching methods to ensure balance between covariates, and re-run our estimations of player contributions adjusting weights for any imbalances between members and non-members. Finally, we conduct sensitivity analysis to examine the robustness of these results to the presence of hidden confounders.

Differences between treatment and control village residents, as well as between SEWA members and non-members, are shown in Table A6. Panel (A) shows comparisons between residents of different types of villages, panel (B) between members and non-members (who reside in both control and treatment villages). The table shows some differences between groups; however for none of these can we reject the null that the difference in means is non-zero. Moreover, the means tests yield similar results for the village comparison and for the member/non-member comparison, suggesting that the

recruitment of SEWA members and non-members for games resulted in a well-balanced sample resembling random selection, at least with respect to observed characteristics.

Further, we use propensity scores to predict SHG membership in both treatment and control villages, and rerun our mean-difference tests comparing the game behavior of "predicted" members and "predicted" non-members in both types of villages. Using nearest-neighbor matching without replacement we create a matched dataset by estimating SEWA membership using a logit model based on the following individual-player characteristics: age, age squared, years of formal education, literacy (based on whether they could write their name on a sheet of paper), marital status, and whether their husband had migrated out of the village for employment (the latter three covariates being coded as 1 or 0). All individuals with a propensity score above the median (0.458) are classified as "predicted" members. Table A7 shows tests of differences in means for per-round game contributions to the public good across and between all combinations of predicted members and predicted non-members in both control and treatment villages. Results are shown both for the first-round contribution as well as average per-round contributions for the full game. In the first round shown in panel (A) predicted members in treatment villages contribute on average Rs. 5 more than predicted members in control villages. Meanwhile predicted non-members in treatment villages contribute almost Rs. 3 more in the first round than similarly-predicted non-members in control villages. Both of these differences are statistically significant. Comparisons of average contributions between predicted members and predicted non-members—whether within treatment villages or control villages—by contrast, are not significant. Taken together these results indicate that selection based on observed covariates is unlikely to drive the treatment effects. Residence in a SEWA village

has a stronger effect on behavior than membership in SEWA itself. These results carry over to the analysis of the full game in panel (B).

As an additional test of the symmetry between village- and member-level results we substitute in our benchmark specification the village-level treatment indicator with indicators of individual membership in SEWA:

$$Z_{i,r,v} = \beta_0 + \beta_1 \text{SEWA Member in SEWA Village}_{i,r,v} + \beta_2 \text{Non-Member in SEWA Village}_{i,r,v} + \beta_3 \mathbf{X}_{i,r,v} + \nu_{i,r}$$

where $Z_{i,r,v}$ is the outcome of interest for individual *i* in round *r* of the game in village *v*. *SEWA Member in SEWA Village* is the treatment indicator (treatment being individual membership rather than village residence), ***X*** is a vector of control variables, and v_{jt} is a random disturbance. Control variables include provision point (the total contributed amount, above which the total funds are doubled and distributed back to players in equal shares), age, and literacy (measured on the basis of whether game players could sign their name). We also include dummies for the individual days over which the games were played and the round number. We also estimate a coefficient for *Non-Member in SEWA Village*, which allows us to determine the spillover from members to non-members within SEWA villages (β_1 being the treatment effect, β_2 the within-village spillover)

We use entropy balancing to pre-process the data by generating unit weights that effectively adjust for systematic and random differences between treatment and control groups (Hainmueller and Yu 2013). Entropy balancing provides a "maximum entropy reweighting" scheme for causal inference with a binary treatment, based on a set of constraints that force covariate distributions of treatment and control groups to match

exactly on known, pre-specified moments. It then incorporates covariate balance into the weight function applied to the sample.

As with the matching method used above, we require that the samples be balanced with respect to the following covariates: age, education, literacy, marital status, and whether the husband has migrated out of the village. We specify the first moments for all covariates; given possible non-linearity with respect to age, we specify an additional second moment for age (the equivalent of including age and age^2 in the matching equation) based on individual game player characteristics. The resulting reweighting thus assures complete balance across these covariates between SEWA members and non-members (these weights vary by individual players, but for each player, remain constant for all rounds of play). In contrast to other pre-processing methods such as weighting the regression by the propensity score—which can fail to balance covariates in finite samples—entropy balancing directly adjusts the weights to the known sample moments, and thus eliminates the need for balance checking for the included moments (Hainmueller 2012).

Results are in Table A8, which replicates Tables 5 and 6, but where we substitute SEWA membership for residence in a SEWA village as the treatment indicator. As in those tables, we use bias-reduced linearization to correct for the small number of village clusters. The two sets of results are highly symmetric. Note that beyond the first round, non-members in SEWA villages also contribute greater amounts than their counterparts in control villages. The treatment effect of village residence is, more or less, indistinguishable from the treatment effect of SEWA membership.

For all their advantages, matching and balancing methods only rely on observed characteristics. As a final robustness check, then, we explore the possibility of hidden bias due to unobserved variables that might affect the choice of whether to join SEWA. We use the bounding approach first proposed by Rosenbaum (2002), which indicates how strongly an unmeasured variable must influence selection in order to undermine matching based on the measurable factors (DiPrete and Gangl 2004). This analysis defines a sensitivity parameter Γ, which can be considered the size of the effect of any unobserved covariate on the outcome. Where average treatment effects are free of hidden bias ($\Gamma = 1$), the upper and lower bounds (in this case, of the Hodges-Lehmann confidence interval) of the treatment effect are the same. In the presence of unobserved selection bias (where those joining SEWA are also more generous) then the treatment effects would over-estimate the true treatment effect, and the test statistic must be adjusted downward.

These adjustments, along with critical p-values for each, are in Table A9, which shows that robustness to hidden bias is strong. Because we are interested in the presence of over-estimation of treatment effects, we focus on the upper bounds of the confidence interval, and their associated significance. Based on these *p*-values, at the 95% threshold, the magnitude of hidden bias required to explain observed relationships is very large; SEWA members would have to be more than 50 times more likely to join SEWA due to unobserved factors, and these unobserved variables would have to predict perfectly contributions in the public goods game in order to undermine treatment effects. An alternative interpretation is that SEWA members would have to contribute, on a per-round average, more than 50 times the amount that non-members contribute, a highly implausible

scenario. We conclude, therefore, that treatment effects at the member-level are not sensitive to hidden bias.

Table A1: Dungarpur district, basic indicators

	Rural	Urban	Total
Population	1,299,809	88,743	1,388,552
Female-Male Ratio (women per 1,000 men)	996.5	951.0	993.5
SC (% of total population)	3.4	9.0	3.8
ST (% of population)	74.4	18.4	70.8
Male literacy rate	58.3	79.7	59.7
Female literacy rate	36.4	68.2	38.4
Male labor-force participation rate	49.7	50.1	49.8
Female labor-force participation rate	44.4	16.1	42.6
Agricultural workers (% of labor force)	67.0	8.7	64.3
Marginal workers (% of agricultural workforce)	67.8	0.3	68.2

Source: Office of the Registrar and Census Commissioner of India (2011).

Table A2: Pre-treatment differences, SEWA and non-SEWA villages and individual members

	Non-SEWA	SEWA	Difference
(A) *Villages* (*N* = 78)			
Scheduled Tribe (%)	77.540	71.239	-6.301
Scheduled Caste (%)	5.734	5.227	-0.507
Literate (%)	17.967	16.001	-1.966
Married (%)	95.373	95.404	0.031
Kutcha (%)	77.393	70.462	-6.931
(B) *Village residents* (*N* = 1,603)			
Age	36.345	37.389	1.044**
Head of household	0.139	0.122	-0.018
Scheduled Tribe	0.730	0.668	-0.061***
Scheduled Caste	0.062	0.060	-0.002
Literate	0.188	0.184	-0.004
Married	0.952	0.947	-0.006
Children	2.270	2.136	-0.134*
Landowner	0.835	0.861	0.026
Kutcha	0.746	0.667	-0.079***

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

Table A3: Local public goods and services, conditional effects

	(1)	(2)	(3)	(4)
	Roads	**Electricity**	**Education**	**Health**
SEWA Village	-0.051	0.008	-0.063	-0.019
	(0.047)	(0.037)	(0.053)	(0.034)
Post-Intervention	0.215***	0.021	0.152***	0.264***
	(0.045)	(0.033)	(0.048)	(0.036)
SEWA Village × Post-Intervention	0.115*	-0.005	0.113	0.127**
	(0.065)	(0.046)	(0.076)	(0.059)
Age	0.000	0.000	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Head of Household	0.016	0.016	0.001	0.006
	(0.032)	(0.036)	(0.033)	(0.027)
Scheduled Tribe	-0.040	-0.101***	-0.143***	-0.104***
	(0.045)	(0.035)	(0.039)	(0.036)
Scheduled Caste	0.104**	0.025	0.035	0.077**
	(0.044)	(0.050)	(0.052)	(0.038)
Literate	0.044*	0.046*	-0.006	0.022
	(0.026)	(0.025)	(0.028)	(0.026)
Married	-0.019	0.002	-0.024	0.060
	(0.045)	(0.036)	(0.046)	(0.039)
Children	-0.002	-0.002	0.004	0.001
	(0.005)	(0.004)	(0.005)	(0.006)
Landowner	0.064***	-0.075***	-0.022	-0.061**
	(0.024)	(0.025)	(0.032)	(0.028)
Kutcha	-0.075***	-0.129***	-0.081***	-0.058**
	(0.026)	(0.023)	(0.025)	(0.026)
R^2	0.124	0.104	0.083	0.176
N	3,154	3,190	3,156	3,186

Notes: Results are OLS with standard errors clustered by village (= 80) in parentheses. Selection strata-fixed effects and intercepts are estimated but not reported. * $p<0.10$; ** $p<0.05$; *** $p<0.01$

Table A4: Interactions with officials regarding public goods and services, conditional effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Contacted any public official (1) – (4)				Contacted *Sarpanch* (5) – (8)			
	Roads	**Electricity**	**Education**	**Health**	**Roads**	**Electricity**	**Education**	**Health**
SEWA Village	0.009	0.031	0.011	0.004	0.021	0.025	0.011	0.000
	(0.025)	(0.025)	(0.024)	(0.028)	(0.025)	(0.022)	(0.020)	(0.023)
Post-Intervention	0.118***	0.211***	0.039	-0.030	0.120***	0.160***	0.024	-0.006
	(0.032)	(0.036)	(0.026)	(0.025)	(0.030)	(0.030)	(0.021)	(0.021)
SEWA Village × Post-Intervention	0.030	0.075	0.027	0.057	0.005	0.055	0.005	0.057
	(0.056)	(0.067)	(0.048)	(0.044)	(0.052)	(0.058)	(0.041)	(0.039)
Age	0.000	0.001	-0.001	-0.000	0.000	0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Head of Household	0.022	0.034	0.037	0.034	0.009	0.016	-0.008	0.015
	(0.034)	(0.036)	(0.034)	(0.034)	(0.035)	(0.030)	(0.020)	(0.023)
Scheduled Tribe	-0.004	-0.002	0.003	0.030	-0.004	0.045**	0.034**	0.044***
	(0.031)	(0.024)	(0.026)	(0.026)	(0.024)	(0.022)	(0.015)	(0.016)
Scheduled Caste	-0.058	-0.013	-0.034	-0.035	-0.028	0.018	0.008	0.019
	(0.040)	(0.060)	(0.042)	(0.044)	(0.037)	(0.042)	(0.029)	(0.032)
Literate	0.131***	0.118***	0.134***	0.093***	0.095***	0.073***	0.062***	0.057***
	(0.029)	(0.024)	(0.026)	(0.024)	(0.028)	(0.024)	(0.021)	(0.021)
Married	-0.009	-0.021	-0.016	-0.046	0.003	0.004	-0.012	0.010
	(0.033)	(0.043)	(0.036)	(0.044)	(0.033)	(0.040)	(0.024)	(0.029)
Children	-0.001	0.005	0.005	0.001	0.000	0.003	0.003	-0.001
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)
Landowner	-0.054**	-0.011	-0.036	-0.017	-0.047*	-0.014	-0.034*	-0.029
	(0.027)	(0.021)	(0.023)	(0.021)	(0.026)	(0.021)	(0.018)	(0.019)
Kutcha	-0.032	-0.016	-0.031*	-0.025	-0.015	0.015	-0.009	-0.007
	(0.022)	(0.019)	(0.018)	(0.019)	(0.020)	(0.018)	(0.012)	(0.013)
R^2	0.058	0.107	0.042	0.027	0.044	0.071	0.017	0.017

Notes: Results are OLS with standard errors clustered by village in parentheses. Selection-strata fixed effects and intercepts are estimated but not reported (N = 3,205). * p<0.10; ** p<0.05; *** p<0.01.

Table A5: Summary statistics for all rounds, treatment and control areas

	Control Villages	Treatment Villages
Number of rounds played	6.5	4.75
Number of players in each round	12	15
Fraction of rounds in which provision point was achieved	0.51	0.62
Average of payouts per round – payouts from Nash strategy of contributing 0 in each round (Rs.)	6.05	25.46

Table A6: Differences in demographic characteristics, game players

(A)	**Treatment village residents**	**Control village residents**	**Difference**
Age	35.792	35.267	0.525
Education	0.938	1.689	-0.751
No formal education	0.833	0.789	0.044
Literacy	0.365	0.278	0.087
Married	0.958	0.944	0.014
Husband is migrant	0.021	0.022	0.001

(B)	**SEWA members**	**Non-members**	**Difference**
Age	35.627	35.466	0.160
Education	0.988	1.553	-0.565
No formal education	0.819	0.806	0.013
Literacy	0.373	0.282	0.092
Married	0.964	0.942	0.022
Husband is migrant	0.024	0.019	0.005

Notes: means are based on demographic characteristics of game players (N = 184). "No formal education," "Literacy," "Married," and "Husband is migrant" are coded 1 or 0. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table A7: Mean person-round contributions, predicted members and non-members

		Predicted members	**Predicted non-members**	(*p*-value)
(A) First round	**Control villages**	7.400	8.880	(0.250)
	Treatment villages	12.759	11.746	(0.376)
	(*p*-value)	(0.001)	(0.006)	
(B) All rounds	**Control villages**	13.274	14.284	(0.107)
	Treatment villages	16.121	15.438	(0.201)
	(*p*-value)	(0.000)	(0.003)	

Notes: predicted membership in SEWA is estimated by nearest-neighbor propensity matching without replacement, with the following logistic equation: Prob.(*SEWA member*) = 0.315 (±0.149) × *Age* – 0.004 (±0.002) × Age^2 – 0.058 (±0.070) × *Education* + 0.671 (±0.374) × *Literate* – 0.924 (±1.421) × *Married* – 0.696 (±1.719) × *Husband Migrant* –5.054 (±2.564), $N = 184$, $R^2 = 0.043$.

Table A8: Regression results for experimental games with entropy balancing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Contribution			Payout			Net earnings		
SEWA Member in SEWA Village	3.206***	4.213*	4.254**	3.103	21.501***	24.984***	-0.103	17.284***	20.669***
	(0.000)	(0.050)	(0.024)	(0.140)	(0.005)	(0.002)	(0.953)	(0.003)	(0.002)
Non-Member in SEWA Village	0.042	3.465	4.401*	-0.644	19.523**	22.861*	-0.686	16.022**	18.303*
	(0.938)	(0.145)	(0.057)	(0.849)	(0.027)	(0.052)	(0.853)	(0.023)	(0.081)
SEWA Member in SEWA Village × Net Earnings$_{r-1}$			0.104**			0.075			-0.025
			(0.030)			(0.732)			(0.906)
Non-Member in SEWA Village × Net Earnings$_{r-1}$			0.057			0.014			-0.030
			(0.527)			(0.981)			(0.954)
Provision Point	0.146***	0.019**	0.014**	0.309***	-0.115***	-0.138***	0.163***	-0.135***	-0.154***
	(0.000)	(0.029)	(0.033)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	-0.029	-0.018	-0.012	-0.033	0.079	0.106	-0.004	0.094*	0.113
	(0.493)	(0.517)	(0.644)	(0.580)	(0.217)	(0.156)	(0.940)	(0.070)	(0.104)
Literate	-0.650	-0.331	-0.109	-0.970	-0.698	-0.434	-0.320	-0.377	-0.343
	(0.337)	(0.548)	(0.826)	(0.584)	(0.560)	(0.688)	(0.837)	(0.651)	(0.669)
Net Earnings$_{r-1}$			-0.077*			-0.196			-0.127
			(0.061)			(0.196)			(0.449)
Adjusted R^2	0.437	0.408	0.364	0.770	0.403	0.482	0.347	0.421	0.473
N	186	1,034	848	186	1,046	860	186	1,046	860

Notes: OLS results are reweighted with entropy balancing weights generated from adjustments to known covariates of game players. Adjustments are made to the 1st and 2nd moments for age, and the 1st moments for education, literacy, marital status, and whether the respondent's husband has migrated out of the village for work. Standard errors are clustered by village and adjusted for small-cluster bias-reduced linearization, and are in parentheses. Day, round, and seasonal dummies are included but not reported. Columns (1), (4), and (7) are first-round results only; all others are for the full game. * p<0.10; ** p<0.05; *** p<0.01.

Table A9: Sensitivity analysis for average treatment effects

Γ	θ^+	θ^-	p^+	p^-
1.00	10.0	10.0	<0.001	<0.001
1.25	10.0	10.0	<0.001	<0.001
1.50	10.0	10.5	<0.001	<0.001
1.75	10.0	12.0	<0.001	<0.001
2.00	10.0	12.5	<0.001	<0.001
3.00	7.5	12.5	<0.001	<0.001
4.00	7.5	13.5	<0.001	<0.001
5.00	7.5	14.5	<0.001	<0.001
10.00	5.5	15.0	<0.001	<0.001
15.00	5.5	15.0	<0.001	<0.001
20.00	5.0	16.5	0.004	<0.001
30.00	3.0	17.5	0.014	<0.001
40.00	3.0	17.5	0.029	<0.001
50.00	3.0	17.5	0.045	<0.001
60.00	3.0	18.0	0.061	<0.001
70.00	3.0	18.5	0.076	<0.001
80.00	3.0	19.0	0.090	<0.001

Note: Γ is the odds of differential assignment due to unobserved factors. Upper and lower bounds are Hodges-Lehmann (HL) point estimates of treatment effects where those effects are over- or under-estimated, respectively, and where HL $\in [\theta^+, \theta^-]$. Estimates at $\Gamma = 1$ assume no hidden bias and so upper- and lower-bound estimates are equal. At higher values of Γ, upper-bound estimates adjust the test statistics downwards for positive selection. *p*-values are from one-tailed tests. N = 186 matched pairs.

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