

Decentralization, elite capture, and private  
contributions:  
Experimental evidence from the Solomon Islands

Andrew Beath<sup>1</sup>, Ariel BenYishay<sup>2</sup>, Giovanna d'Adda<sup>3</sup>, Pauline Grosjean<sup>4</sup>  
& Roberto Weber<sup>5</sup>

October 2014

*Keywords:* Decentralization, elite capture, private contributions

---

<sup>1</sup> World Bank <sup>2</sup> College of William & Mary <sup>3</sup> Politecnico di Milano <sup>4</sup> University of New South  
Wales <sup>5</sup> University of Zurich

Corresponding author: Ariel BenYishay, abenyishay@wm.edu.

We thank seminar participants at SEEDEC and the University of Oxford for helpful comments. This work benefited from the collaboration of the Solomon Islands Ministry of Development, Planning and Aid Coordination (SIMDPAC) and the World Bank. Excellent fieldwork was conducted by Heather Belfor, Alpana Modi, Ananta Neelim, Tom Sackman, Patrick Schneider, Juliana Silva Goncalves, Erin Steffen, Joe Vecci and Mark Walsh. Danielle Hayek provided superb research assistance. The project was supported by a grant from the U. of New South Wales. The views expressed are those of the authors and do not necessarily represent the views of SIMDPAC or the World Bank.

## **Abstract**

We design a field experiment to test a new mechanism intended to limit local elite capture in the context of decentralized control over public resources. We offer residents in randomly assigned communities vouchers that can be contributed to a public good or cashed out (at a lower rate). Control communities receive block grants. We observe that discussions regarding the public goods to be improved with these funds last longer in treatment communities and include a broader set of participants. The choices of project types in treatment communities are also less likely to reflect the preferences of leaders and more likely to reflect those of individuals who had not previously participated in local meetings, particularly in communities where leaders are least altruistic and where these sets of preferences diverge. Our results suggest that decentralizing resources to the individual level via vouchers may be an effective mechanism for aggregating information on the marginal value of local public goods.

# 1 Introduction

Decentralization of spending authority for local public goods to communities has become a prominent policy in developing countries. The past two decades have seen a range of anti-poverty programs enacted specifically to put local communities in charge of the selection and implementation of these public goods. These policies have been implemented partly in response to the low level of fiscal decentralization historically prevalent in developing countries, compared to developed nations (Gadenne and Singhal, 2014). Decentralization reforms include community-driven development (CDD) programs, social investment funds, and other interventions. The World Bank alone has devoted almost \$85 billion toward “local participatory development” (Mansuri and Rao, 2013).

Decentralization is often justified on the basis of informational advantages about local needs and the implemented quality of these goods. The argument laid out is that individuals are better informed about the marginal value of different public investments in their communities, and that accurately transferring this information to the center is costly. Alatas et al. (2012) document that community-based information on household-level poverty can be more accurate than other more centralized versions. Moreover, community members may be better positioned to monitor the quality of implementation and operation of public infrastructure and provision of public services.

The empirical findings on the effects of decentralization appears to contradict these expected advantages. Evidence from Indonesia suggests that community monitoring is less effective than traditional top-down monitoring in reducing corruption of local public spending (Olken, 2007). Moreover, the potential informational benefits of decentralization may be offset by elite capture. As Mansuri and Rao (2013) note, participants in civic activities across a variety of settings are generally wealthier, more educated, hold higher social status and are more politically connected than non-participants. Pradhan, Rao and Rosenberg (2010) document these characteristics among groups making funding allocation decisions in Indonesia, while Arcand and Fafchamps (2012) and Mansuri (2012) show similar patterns in Burkina Faso and Pakistan, respectively. Even among participants, control of the process may be further concentrated among elites. Not surprisingly, elite control of project decisions is pervasive, as Fritzen (2007) documents.

In theory, elite capture may not be harmful if it reflects “benevolent” capture,

wherein elites are better informed about the optimal projects and act altruistically on behalf of their communities. There is mixed evidence on how pernicious this capture is: Labonne and Chase (2009) find extensive capture in local project proposals as part of a CDD program in the Philippines, which are much more likely to match the preferences of local leaders than other participants. Rao and Ibanez (2005) find that Jamaican Social Investment Fund choices were not well aligned with preferences of community members at large, but the latter largely expressed satisfaction with the outcomes. Bardhan and Mookherjee (2005) provide theoretical support for the premise that, when block grants given to communities exceed the funds required to complete a lumpy public project, the excess is likely to be siphoned off by elites. Nonetheless, estimates of impact on actual community welfare under differing levels of capture are not documented anywhere in the literature.

We offer a new mechanism through which to limit elite capture in decentralized project selection: anonymous voluntary contributions, subsidized via transfers to community members. We call these “vouchers,” which are provided directly to community members and can be redeemed either for funding a public good or for private consumption (at a lower rate). Vouchers offer a more direct link between local public revenues and the representative preferences of individual community members. In a sense, they represent direct cash transfers with an incentivized public good contribution option.

We design a field experiment to test the effects of these vouchers on project selection dynamics and outcomes. We conduct our study across 80 villages in the Solomon Islands, a country in which dispersed, largely subsistence communities are often dominated by “big men.” We implement the vouchers by providing 20 randomly selected adults in each study village with 10 notes, which can be redeemed for either 10 Solomon dollars each (SI\$, approximately US\$1.4) if contributed to a public project, or 5 SI\$ each if destined to private consumption. The public fund is provided as a credit at the nearest hardware supplier, which can be used to purchase items from a pre-set menu of materials. Participants discuss to decide what community project the funds will be used for. Importantly, the participants make their decisions anonymously, thereby avoiding coercion and retribution.

We evaluate the effect of providing funds through vouchers rather than through a standard block grant. In a control group of villages, the maximal public fund amount (2000 SI\$) is provided to the community with no individual contributions required. Participants in the control treatment simply decide how to spend the grant,

and cannot retain any part of it for private consumption. All other features of the process are the same across experimental arms.

We study the impacts of the voucher scheme on the discussions held by communities and on their choices of the public goods to be funded. We find that, under the voucher scheme, village discussions last longer, with more community members speaking. The project choices under the voucher scheme are less likely to reflect the *ex ante* top preferences of village leaders and more likely to reflect those of individuals who had not participated in recent village meetings, with this effect most pronounced in villages where leaders' survey responses indicate the lowest altruism. Community members also perceive project choices to be more fair under the voucher treatment.

The voucher scheme does provide fewer funds to communities than our control arm: communities earn 79.1% of the funds available to them (58.1% of the total available in public good contributions, 20.9% in private cash). The welfare gains from this allocation need not necessarily be lower; this depends in part on the actual productivity of public projects in generating private consumption, and whether this exceeds our 100% contribution matching rate. Nonetheless, ignoring the value of the private transfers, we recognize that limiting elite capture by providing these transfers does entail costs (in our case, private transfers represent 36.0% of the public funds provided). At the same time, the vouchers provide community members with additional private "ownership" of the public good (paid for in the form of foregone private transfers), and this "ownership" may induce complementary effort or contributions by community members in the implementation of the projects. As our data on implementation quality is limited, we do not observe differences in the speed with which communities obtain their materials and implement their selected projects. However, the large effects of our voucher mechanism on participation in the project selection process and in the nature of selected public goods suggest that this mechanism has significant potential benefits.

Our study adds to a growing literature on a number of alternative mechanisms to limit the potential for elite capture in decentralization efforts. First, under some conditions, local elections may discipline leaders into adopting policies that reflect the needs of the broader population (Foster and Rosenzweig, 2004), although clientelism can also emerge in local democracies (Wantchekon, 2009).<sup>1</sup> Pradhan et al. (2011)

---

<sup>1</sup>In some cases, public goods are provided at a more local level (say, at the village level) than local government administrative units, making elections of leaders a much broader policy than

conducted a randomized experiment varying the election of Indonesian school committee members and linkages of these members with village councils, finding higher subsequent test scores in schools with these electoral reforms.

Second, decentralization efforts can mandate and invest in specific processes and structures to limit capture. Many CDD programs, for example, employ facilitators who guide communities through a needs identification and implementation process (Mansuri and Rao, 2013). The presence of facilitators appears to shift the project choices toward the preferences of the facilitators themselves (Platteau and Gaspart, 2003). Many programs also require specific procedures be followed, such as the number of meetings to be held and implementation via a committee of local citizens with specific characteristics (often including demographic quotas). Olken (2010) studies the effects of one such procedure—direct voting for project choices by community members in Indonesia. He finds no effects on project choices, but higher overall satisfaction with these choices under voting. In Afghanistan, Beath, Christia and Enikolopov (2014) find that secret-ballot referenda can reduce the influence of local elites over the type and location of village projects. These referenda also improve villagers’ satisfaction with the local economy and governance structure. These mixed findings indicate that democratic selection procedures may limit capture in some contexts.

Finally, elite capture may also be restricted when communities must compete for resources on the basis of project proposals that are assessed on the basis of their representativeness or targeting. Chavis (2010) studies the effect of more intense competition for these resources among villages in sub-districts in Indonesia, finding such competition can reduce local road construction costs. However, such competition may have smaller gains when expected welfare impacts from alternative project choices are less observable by external individuals (e.g., higher level administrators) who select the “winners” on this basis.<sup>2</sup>

The remainder of the paper proceeds as follows: In section 2, we lay out our experimental design and data collection. In section 3, we lay out our hypothesis and present results on the discussion dynamics, project selection, and participant perceptions in section 4. We test likely mechanisms in section 5, and briefly discuss project implementation in section 6. We conclude in section 7.

---

implementation in a given locale.

<sup>2</sup>Competition can also impose costs: some communities must be excluded from funding for local public goods, and the competition for a fixed set of funds may generate “prize” effects at the margin (in which communities undertake other costly investments in order to win the prize).

## 2 Experimental Design and Data

### 2.1 Setting

The study took place in June - August 2013 in a sample of 80 randomly selected villages across all wards in each of four provinces in the Solomon Islands (Choiseul, Malaita, Temotu, and Western), with 20 villages sampled in each province. Sampled villages were drawn from the population of villages receiving funds under the Solomon Islands Rural Development Program (henceforth RDP). RDP was a US\$22 million initiative implemented by the Solomon Islands' Ministry of Development and Planning and Aid Coordination (MDPAC), and supported by AusAID, IFAD, and the World Bank. RDP was launched in 2008 and closed in November 2013. It was operational in eight provinces of the Solomon Islands (Choiseul, Western, Isabel, Central, Guadalcanal, Malaita, Makira and Temotu). RDP used CDD modalities with the goal of financing investments identified by villagers themselves as the most urgent preconditions for improved living standards, ensuring that villagers themselves played a greater role in determining investment priorities and that a greater share of funds actually reached communities. Existing local institutions (e.g., tribal councils and churches) planned and managed RDP activities at the community-level and arranged implementation of small works where applicable.

### 2.2 Experimental Design

Villages were assigned randomly to the voucher treatment arm or control arm, with province-level stratification. Leaders in each village were contacted in advance and asked to invite all available adults to a community meeting on a specific date. The attendees of this meeting represent our individual-level sampling frame. From this frame, 18 villagers (9 male and 9 female) were randomly drawn via random name drawing. In addition, two leaders (one male, one female) were selected (typically the highest ranking village leaders of each gender present at the meeting). The community meeting was then adjourned, with only the individuals selected to participate asked to remain. All the activities described below were conducted indoors, in spaces protected from outsiders' intrusions, such as local schools or public buildings, so as to ensure the privacy of participants' decisions. Importantly, experimental treatment assignment was not revealed to community members until after participant selection had taken place.

The main component of the study was a structured community activity (henceforth SCA). The SCA allocated SI\$2000 (approx. USD\$300) to each village for a community project, which could be selected among the following: (i) maintenance or improvement of a local public building or other space (excluding churches or other religious buildings); (ii) maintenance or improvement of a local drinking water facility; or (iii) maintenance or improvement of a local irrigation system. The money could be spent at a local hardware store to purchase the materials needed for the construction or repair work, such as paint, roofing iron, cement, etc. Community members were required to provide labor and complementary material to implement the project. One participant from each session was selected to keep a record of how the funds were spent and of community members contributions. The method of delivery of the money allocated to the village project was identical across all villages. The total amount allocated to the village-level project was to be redeemed by the village as credit at a local hardware store.

Participants were invited by the local enumerator conducting the session to participate in an open discussion, closely resembling community meetings, to decide which type of project to implement. No arbitrary structure was imposed on the format of the discussion nor on the method of selection of the project. The enumerator and assistants simply stepped aside until an agreement was reached, and recorded who spoke and for how long. This will allow us to investigate whether the voucher treatment lead to longer and more inclusive project selection discussions.

In the voucher treatment arm, prior to the discussion, the local enumerator conducting the session issued each of the 20 selected participants 10 paper vouchers worth SI\$5 each. Facilitators informed villagers that the vouchers could either be used to fund a village-level project or could be individually redeemed for cash. Any individual contribution towards the village-level project would be doubled by the researchers. After selecting the project, participants indicated, in private, how many of their vouchers they wished to redeem and how many they wished to contribute to the village-level project. All subjects were paid a participation fee and any private voucher redemption from the activity at the end of all tasks.

In both experimental arms, at the conclusion of the discussion participants completed a simple form that identified which building would be improved and which materials they intended to purchase. Subjects were also asked to decide who would be responsible for obtaining the materials from the hardware store.

## 2.3 Data

Within each village, a series of questionnaires were conducted to collect additional information. Before the SCA, participants were administered a short questionnaire about their preferences over a group of potential community projects, such as health facilities, schools, market building, etc. Each participant was asked to rank the three most needed projects within his or her community. This information allows us to map participants' preferences over local public goods before the SCA to the project selection in the SCA.

The SCA discussion was directly observed by the local enumerator conducting the session and other team members. The number of speaking interventions by each participant was recorded at five minute intervals. The total length of the discussion was also recorded. We use these data to investigate how the voucher treatment affects participation in project selection.

After the SCA, participants were administered an additional survey, collecting demographic, socio-economic, prior experience with community organizations, social preferences, and satisfaction information. The social preference questions included questions about trust, reciprocity and altruism. In particular, the questionnaire employed three questions to measure altruism: the first about subjects' willingness to share their resources with others, the second asking how much of an income windfall subjects would donate to someone in need, and the third eliciting an opinion on people who spend time and energy for others without expecting anything in return. These questions are shown to correlate strongly with choices in the dictator game, a behavioral game commonly used to capture altruism (Falk et al., 2012). We use these measures to both correlate behavior in the SCA with underlying social preferences and to understand how the leaders' social preferences relate to elite capture and our treatment effects.

We also collected information on community characteristics from a sample of key informants, such as village elders, community group leaders, etc., within each village. These data are useful to investigate how treatment effects vary depending on local power structures, experience with RDP and other village traits, and control for them in the analysis.

Finally, approximately three months after the SCA, a local enumerator returned to each community to assess progress in obtaining and installing the materials selected as part of the experiment.

We present summary statistics for our primary outcome variables and covariates

of interest in Table 1.

### 3 Testable hypotheses

We make a series of hypotheses concerning the effect of the voucher treatment on the discussions held by communities over the selection of the public goods to be funded. Overall, we expect individual community members to be empowered by the opportunity to keep funds for themselves, rather than contributing them to a public project. The increase in individual bargaining power due to personal control over project finances is predicted to translate into stronger engagement in the project's selection, especially by typically marginalised community members, and improved matching between chosen projects and group preferences.<sup>3</sup> Namely, we make the following hypotheses:

1. Vouchers lead to lower levels of public good funding, given by the sum of individual contributions, relative to the block grant.
2. Vouchers increase participation in project selection, measured by the duration and inclusiveness of discussions over project choice.
3. Vouchers reduce elite capture of decision-making over local public good provision. Namely, vouchers lead to project choices that more closely reflect preferences of median voters and marginalised individuals, such as the poor, women and youths, rather than preferences of formal and informal elites.
4. Vouchers lead to more democratic decision-making. In particular, vouchers increase perceived fairness of decision-making, measured through survey questions at the end of the session.
5. Vouchers increase satisfaction with the project, stated by participants at the end of the session.

---

<sup>3</sup>The pre-analysis plan for this study also included hypotheses on project implementation. Namely, we expect the voucher treatment to increase the likelihood of projects being implemented and to improve the quality of the public goods. The available data does not allow us to test these hypotheses, since only a small number of communities had collected the material and started to work on the projects three months after the SCA. For this reason, we do not report any of the hypotheses we formulated on project implementation, nor discuss any results on this topic in the remainder of the paper.

## 4 Results

### 4.1 Voucher contributions

We begin by documenting the contribution of vouchers to the public good observed in our treatment communities. Figure 1 offers a histogram of these contributions, which span the full range of SI\$ 0-50 and peak at SI\$ 25 (with more than 28% of the participants contributing at this level). Contributions generally decrease monotonically on both sides of this modal contribution, although a disproportionately large share of participants contribute the full amount possible (13.3% of participants do so, while only 0.6% contribute zero).

Voucher contributions are correlated with a variety of individual characteristics, shown in Table 2. In column 1, we regress individuals' voucher contributions on their demographic characteristics (including province fixed effects). We note that the purposively selected leader participants contribute nearly SI\$ 3 more than the average participant, as do individuals who obtain some income from off-farm work (either wage or business income). Young participants (under 30 years of age) contribute 2.2 fewer dollars. Gender differences in contributions are small and insignificant. In column 2, we regress contributions on participants' survey responses for social preferences, finding that responses on two of the three altruism questions are significant predictors of voucher contributions. Concerns about neighbors cheating the participant are negatively related to contributions, but only noisily so, while trust and reciprocity appear less directly related to contribution levels.

These results suggest that the vouchers operate in line with other voluntary public good contribution mechanisms, wherein wealthier and more powerful individuals often contribute larger absolute amounts, as do those who exhibit greater altruism.

Voucher contributions are also correlated with individual participation in the discussion. Figure 2 shows average voucher contribution by individuals who spoke or didn't speak during the meeting. Subjects who spoke during the discussion contributed on average SI\$30.1, while those who did not donated SI\$ 27.2. This difference is statistically significant in a two-sided t-test ( $p = .001$ ).

### 4.2 Impacts on project discussion

We next consider whether the voucher treatment altered the open-ended discussion of the public good to be funded. These discussions last an average of 12.7

minutes in control communities, while those in treatment communities last 15.4 minutes, with the difference significant at the 5% level. The difference appears to come from shifting the right tail of the distribution: only 15% of control communities have discussions that last 20 minutes or longer, while 38% of treatment communities do so.

We also assess how the treatment affects the share of individuals participating in this discussion, and the number of intervals during which they speak. To do so, we regress individuals' participation outcomes on a treatment dummy and province fixed effects, with standard errors clustered at the community levels, with results shown in Table 3. In control communities, 39.5% of individuals spoke during the discussion, while 43.6% of treatment individuals did so (the difference is not statistically significant). Among these speakers, the voucher treatment raises the number of five minute intervals during which a participant speaks by 0.22 (significant at the 10% level).

Which individuals are more likely to speak during the discussion due to the voucher treatment? In Table 4, we regress the probability of being a speaker and, for speakers only, the number of five-minute intervals spoken, on individual characteristics and their interaction with treatment. While roughly 33.7% of all individuals speak during the discussion, leaders do so twice as often. Male leaders speak more than female leaders (although female leaders speak significantly more than the average participant), but the gender pattern is reversed in the rest of the population. Among non-leaders, women are more likely to speak than males (by 20.1 pp). Other characteristics associated with status predict intervention and speaking time in a consistent way. For example, those with off-farm income are 11.4% more likely to speak, while younger participants are 20.7 pp less likely to do so. Notably, none of these discussion participation rates are heterogeneously different for treatment and control communities.

If none of these demographic traits identify the marginal individuals who respond to the treatment by speaking at higher rates, are there less easily observed characteristics which nonetheless do so? Survey responses by participants allow us to identify individuals who had not attended any community meetings in the preceding five years (30.3% of participants) as well as those who had attended but had not spoken at these meetings (27.8%). Both of these groups of individuals are significantly less likely to speak in the control communities than are average participants (20.0 pp less so for non-attendees and 25.2 pp less so for non-speaking attendees). In other words,

although our experiment randomly sampled individuals who do not routinely take part in community meetings and enlisted their participation in such a meeting, we observe these individuals behaving similarly to their reported past behavior in other settings.

Importantly, the voucher treatment significantly raises the probabilities of speaking for specifically these types of individuals. Figure 3 shows the effect of the voucher treatment on the number of times participants intervene in the project selection discussion. We disaggregate this effect depending on whether subjects attended community meetings over the previous 5 years (Panel A) and spoke at these meetings (Panel B). The effect of the voucher treatment on participation in the discussion is positive overall and for each group of subjects, but the increase is statistically significant at the 1% level only for individuals who did not speak at previous community meetings (two-sided t-test  $p = .001$ ).

The figures provide only average effects. We therefore test their statistical significance, taking individual and village controls into account within our regression framework. When these heterogeneous effects are jointly included in column 4 of Table 4, the treatment eliminates roughly a third of their disparities in speaking rates relative to other participants. Column 5 confirms this is the case even controlling for the aforementioned demographic characteristics. Finally, in column 6, we find that these effects exist on the extensive margin rather than the intensive one: conditional on improving their probability of speaking at all, the treatment does not increase the frequency with which these participants speak relative to other participants.

Thus, we observe that the voucher treatment extends the open-ended discussion of the public good project and increases the share of individuals participating by drawing in community members who would not normally take part in such meetings.

### **4.3 Impacts on project selection**

We next consider the impacts of the voucher treatment on the types of projects selected by communities as part of the SCA. Our primary typology of projects reflects the main building or space each community chose to improve. Before the SCA, we elicited each participant's ordinal ranking of the top three buildings that should be improved with a hypothetical SI\$2000 grant, with the options limited to nine types: kindergarten, primary school, health clinic, water supply (typically a community standpipe), sanitation, market, road/bridge/wharf, church, or another non-church community building. More than 1/3 of participants ranked the local kindergarten

as their top preference, while sanitation (16%) and water supply (13%) were the second and third most popular first choices respectively (and most frequently ranked as second choices). In fact, in 59% of villages, kindergarten was the most frequently reported top ranked preference among all participants. Sanitation was the most frequently reported top ranked preference in 15% of villages.

Aggregating individual preferences to the community level is naturally sensitive to alternative mechanisms. Here, we focus largely on simply using the modal top-ranked preference of participants; in our sensitivity checks, we test whether using alternatives, such as whether a project was ranked among the top three preferences, without any weighting, alters our main results.

Leaders' reported preferences often diverged from those of the rest of their communities. In 67.5% of communities, leaders' top ranked preferences and those of the rest of the participants differed. This is largely because, in only 10% of villages were kindergartens the top ranked preference of leaders, although, as previously mentioned, these were frequently the modal top ranked preference among the rest of the participants.

With these descriptive statistics in mind, we test whether the voucher treatment altered the likelihood that the building types chosen by each community reflected the preferences of leaders versus those of other community members. In Figure 4, we compare the overall shares of each building type selected in the treatment and control communities, as well as the frequencies with which each type was the modal top-ranked preference of all participants and of leaders. We observe that 22.5% of control communities selected the local kindergarten—the most frequently cited top choice of non-leader participants—as the building to be improved, while 27.5% of treatment communities did so. Conversely, 32.5% of control communities chose water supply as their designated improvements, while 25.0% of treatment communities did so. We observe similar reductions in the selection of sanitation systems between control and treatment communities (15.0% vs. 2.5%). From this aggregate picture, we observe that control communities were more likely to opt for project types that in the aggregate correlate with the preferences of leaders.

Of course, the preferences of each community may differ depending on the specific conditions of local buildings and infrastructure, as well as demographics and other considerations. Thus, we test whether the voucher treatment decreased the probability that each community's chosen project matches the specific preferences of its leaders and increased the probability that it matches those of non-leaders. To do

so, we construct a community-level dummy variable indicating that the project type chosen matches the modal top-ranked preference of all participants (and, separately, of leaders as well as other subgroups). We then conduct community-level regressions of this dummy on a treatment indicator and province fixed effects. Shown in Table 5, the results are consistent with those at the aggregate level: in treatment communities, the selected project type is 5 pp more likely to match the preferences of all participants and 12.5 pp less likely to reflect those of leaders (although these effects fall short of standard statistical significance levels). We observe a 7.5 pp increase in the probability that the selected type matches the preferences of individuals who had not attended or spoken in a community meeting in the preceding 5 years (although again, at our sample size, these effects are not statistically significant). These results are qualitatively robust to the use of different preference aggregation mechanisms.<sup>4</sup>

In the previous sub-section, we observed that the voucher treatment led to wider participation in the discussion. We now examine whether increased participation affects the likelihood that the selected project matched individual preferences. We construct an indicator variable equal to 1 if the selected project is equal to a participant’s top-ranked preference. Figure 5 shows that speaking in the discussion does not significantly increase the probability that one’s preferences are reflected in the outcome of the discussion. Combined with the result, mentioned above, on voucher contribution, it appears that improving participation in group decision-making increases individual valuation of the selected project, even though it doesn’t lead to project choices more closely reflecting one’s own preferences.

## 4.4 Participant perceptions

We also use survey responses on satisfaction with the project selection process and outcomes to test whether within-community differences are consistent with the

---

<sup>4</sup>We consider an alternative preference aggregation mechanism and define the preferred project at the village level as the one most frequently ranked among the top three by the members of a community. This index and the one used in the main analysis represent extreme formulations of individual preferences while the former mechanism gives equal weight to all top-three projects, the latter considers only top-ranked projects in the preference aggregation. The former measure thus ignores any differences in the intensity of preferences over these top-three alternatives, potentially making this a noisier measure of individual preferences. Table 6 replicates our main regressions, now with the dependent variable equal to one if the selected project is the project most frequently ranked in the top three by village leaders (Columns 1 and 2) or by village members who didn’t speak at previous meetings (Columns 3 and 4). Our results are qualitatively robust to the alternative preference aggregation mechanism used. While the coefficients generally have the same sign as those in previous regressions (with the exception of Column 3), they are not statistically significant.

cross-community results. To do so, we utilize individuals’ survey responses to the question “*Do you think the project was chosen in an equitable and fair way?*” and the the question “*Are you personally satisfied with the project that was selected today?*”. Each was answered categorically (not at all, not very, quite, very), and we convert these responses to a [0-1] scale. In Table 7, we show results regressing these measures on our treatment indicator and province fixed effects (with standard errors clustered at the community level). We observe that reported fairness and satisfaction are high in our control communities (0.86 and 0.87, respectively), but the voucher treatment increases these rates by 5.4 pp and 5.9 pp, respectively, with these effects being significant at the 5% level (column 1 of Table 7). Leaders are more likely to perceive the process as fair (9 pp more so) and satisfactory (8 pp) in both control and treatment communities, but somewhat less so in voucher treatment communities (column 2). In column 3, we consider the treatment effects among individuals who had not previously participated in a recent community meeting. These individuals are significantly less likely to consider the process as fair and satisfactory in control communities (-12 pp in both cases), but the voucher treatment counteracts this completely, with the heterogeneous treatment effects significant at the 1% level. To ensure that these effects are not due to these individuals’ demographics or underlying social preferences, we separately interact the treatment with a set of demographic variables (column 4) and survey-based social preferences (column 5), finding similar effects.

It is also possible that the vouchers directly raise participants’ satisfaction with the process—possibly because individuals prefer the private redemption option to our control process—and that this is differentially true for individuals with the lowest prior participation rates. Nonetheless, the pattern of results suggests that vouchers induced individuals who had not previously participated in community meetings to do so, altering the selection of projects by the communities, and, as a result, these individuals were more satisfied with the process and outcome.

This interpretation is consistent with the effects of participation in the discussion on subjects’ perceptions. We observe a positive and statistically significant correlation between participation in the discussion over the project to be selected on one hand, and perceptions of fairness and satisfaction with the chosen project on the other. Figures 6 and 7 show these effects: 92.8% of subjects who spoke during the SCA discussion believe that the project selection process was fair, relative to 85.2% of non-speakers (two-sided t-test  $p = .000$ ). Similarly, 93.9% of speakers are satisfied

with the selected project, compared to 86.5% of non speakers (two-sided t-test  $p = .000$ ). This result is consistent with the increased valuation of the selected project, measured by SCA contributions, demonstrated by individuals who spoke during the discussion. It thus appears that the voucher treatment increases the legitimacy of the project selection process, individuals' satisfaction and their valuation of the outcome, potentially even beyond its effect on project selection. This interpretation is consistent with previous findings on the positive effect of increased participation in project choice on community members' satisfaction, even in the absence of effects on the projects selected (Olken, 2010).

## 5 Mechanisms

In settings where the degree of elite capture varies across villages, one might expect to observe voucher treatment effects that vary across the extent of existing capture. In particular, if vouchers directly constrain the ability of elites to channel public goods toward their private preferences, we should observe larger treatment effects in more “captured” communities. In this section, we test this mechanism by examining the heterogeneity in treatment effects on project selection across communities with different levels of capture. Because such capture may also be correlated with other community-level characteristics, we also test whether other covariates produce similar heterogeneity in the treatment effects. Throughout, we find evidence consistent with the constraint of capture as the primary mechanism through which our results operate.

Measuring “capture” directly is naturally challenging. We thus consider two measures that reflect leaders' own social preferences. First, leaders' social preferences—particularly their altruism—determine their motives for capture. This is consistent with Kosfeld and Rustagi (2011), who observe that leaders' preferences correlate closely with cooperation in commons management in their communities, in part because leaders with preferences for equality effectively punish free riding residents. In our setting, survey-based social preferences are correlated with pro-social behavior in the form of contributions to the community project. We thus use our survey-based measures of leaders' altruism to identify the degree of likely capture in a community, with the most altruistic leaders less likely to pursue private utility at the expense of public welfare. Second, we consider communities where there is a disagreement between the top-ranked preferences of leaders and those of participants who had not

spoken in or attended a recent meeting. In these cases, we hypothesize that voucher treatment effects should be larger than in communities where these parties' ex ante preferences were already aligned.<sup>5</sup>

Table 8 shows regressions interacting our treatment dummy with these measures of leader preferences. Because leaders' altruism responses vary by province, we include *treatment x province* interactions as controls so that we do not misinterpret province-level variation in treatment effects related to other factors as due to leaders' altruism. In column 1, the coefficients suggest that the voucher treatment raises the likelihood that the selected project will reflect the preferences of participants at large, but much less so in cases where leaders are more altruistic (partly because the direct effect of leaders' altruism on the agreement between selected type and participants' preferences is positive). Again, although the effect sizes are not small, our sample size limits the statistical significance of these coefficients. At the same time, we observe opposite-signed effects on the likelihood that the selected type will match the preferences of leaders: the main treatment effect is large and negative, as is that of leaders' altruism, while the interaction of leaders' altruism with the treatment counteracts the main treatment effect (again, with large standard errors). Effects on the likelihood that the selected type matches the preferences of individuals who had not spoken or attended a previous recent meeting are also consistent with this pattern. The treatment increases this likelihood, but much less so in communities where leaders were very altruistic (both the main treatment and interaction effects are significant at the 10% level). Finally, in column 4, we interact the treatment with a dummy indicating disagreement in the preferences of leaders and the aforementioned non-speakers. We find that the treatment effect on the probability that the selected type matches the non-speakers' preferences is significantly different (at the 5% level) across communities where leaders' and non-speakers' preferences do not agree and those where they do; the overall treatment effect is positive only in those places where there is initial disagreement. These results are consistent with the vouchers limiting the ability of the least altruistic leaders to bend the selection process in favor of their own preferences.

Although we posit that these impacts are due to the vouchers limiting leaders' capture, it is possible that leaders' preferences are correlated with other community-level characteristics that are in fact the primary mechanism for our observed impacts.

---

<sup>5</sup>Both measures reflect conditions at the time of our experiment; we do not address the origins of these differences in leaders' social preferences, be they idiosyncratic for each individual leader or historically persistent at the village level

Thus, in Table 9, we further test whether our treatment effects vary across other community characteristics. In column 1, we consider community size (in terms of households). We note that the treatment effects on the match between the selected project type and the leaders' preferences are more pronounced in small villages, with the direct effect of treatment now significant at the 10% level. In fact, in all but the largest communities (the 80% with fewer than 100 households), the treatment effect is a 20 pp reduction in this likelihood that the selected type matches leaders' preferences.<sup>6</sup> Importantly, however, we observe a very weak correlation between leader altruism and village size, with very large villages exhibiting *lower* leader altruism. As such, the channel through which village size alters the voucher impacts is likely to be different than that through which leader altruism operates.

It may be that, in very large villages, tribal factions struggle to reach consensus, even in the presence of vouchers (consistent with the wide literature on the impacts of ethnic fractionalization on public good provision (Alesina, Baqir and Easterly 1999)). In column 2 of Table 9, we interact our treatment indicator with the number of tribal groups within the village (which range from 1 to 14 in the largest communities). We find coefficients that are in the direction of this mechanism but are too noisy to significantly differentiate from zero.

Because they introduce a trade-off with private consumption, vouchers may also differentially affect communities that are relatively poorer, as the marginal utility of private consumption in such communities may increase disproportionately (under most assumptions about the shape of the utility function). To the extent that individuals in such communities are trading off private consumption that yields larger marginal utility for public contributions, they may demand larger changes in elite behavior in exchange for each dollar contributed. In column 3 of Table 9, we test whether the voucher treatment varies across communities with varying shares of participants who earn some of their income from off-farm wage or business sources. Recall that off-farm income is positively and significantly correlated with SCA contributions. Moreover, earning a wage is negatively correlated with the likelihood that a subject does not own any assets, such as vehicles, radios, etc. We find results that are indeed larger in poorer communities, with the main effect controlling for off-farm income significant at the 5% level and the interaction effect significant at the 10% level. Moving from the 25th percentile of the distribution of off-farm income to the

---

<sup>6</sup>We also consider the effect of the number of households linearly, interacting it with our treatment dummy: the results are robust and qualitatively the same in this different specification.

75th percentile reduces the treatment effect by approximately half.<sup>7</sup>

We further test whether the treatment effects vary by distance to the provincial center, as the value of both the private payouts and the public good may vary across this distance (in part because many of the materials to be purchased with the public good contributions would be sourced in these provincial centers). In column 4, we find no significant differences among communities that are farther than the median in terms of travel time relative to those that are close.

Finally, it is also possible that the leaders' altruism reflects a broader set of social preferences in each community, and that the heterogeneity in treatment effects we observe above is due to differences in altruism or social capital among all participants in each community. In columns 5, we interact the treatment indicator with the mean survey-based altruism responses of all participants in each community. We find weak evidence that more altruistic communities exhibit larger treatment effects (recall that *less* altruistic leaders were associated with larger treatment effects), but these effects are not significantly different from zero. The effects across the principal component of social preferences are small and insignificant. Thus, it appears that it is the preferences of leaders that matter for the effectiveness of vouchers rather than the underlying social preferences of the broader community, consistent with the vouchers limiting the behaviors of elites.<sup>8</sup>

## 6 Project implementation

Survey staff returned to 65 of the 80 communities approximately three months after the SCA to identify progress toward implementation of the selected projects (with communities excluded based on distance from provincial centers, given the low likelihood of implementation in the short time frame, as discussed below). Given the remoteness of many communities and the infrequency of regular transport services, many communities had not had many opportunities to obtain the needed materials from the hardware suppliers where their credits were issued. Nonetheless, we consider as our primary short-term progress indicator a dummy indicating whether the community had obtained the materials targeted for its project. Only roughly 1/4 of

---

<sup>7</sup>We also consider the effect of income, proxying it through a dummy equal to one if a subject does not own any assets. The results are robust and qualitatively the same in this different specification.

<sup>8</sup>We run a similar regression replacing mean altruism with the first principal component of the responses on all of the social preferences questions (8 questions reflecting altruism, trust, and reciprocity). Our results are robust to this different specification.

communities had done so at this follow-up stage. We find no significant differences in this rate among treatment and control communities (Table 10). To better understand the role of distance to the supplier in determining this pace, we consider the heterogeneous treatment effect for communities that are closer than the median travel time from their provincial centers. Treatment communities within this band were 25 pp more likely to have obtained their materials than control communities, although these differences are not statistically significant, given the sample size. Communities beyond the median exhibit no differences between those assigned to treatment and controls.<sup>9</sup>

## 7 Conclusions

A growing number of efforts have decentralized allocation decisions over public goods to local communities in developing countries, aiming to benefit from informational advantages on community needs. These programs use a range of strategies to limit the extent to elite capture in these decisions, including extensive facilitation and encouragement, elections of leaders, project referenda, and competition among communities. We offer an alternative mechanism that relies on private incentives of community members to discipline local leaders. Our experimental design compares the allocation decisions under this voucher scheme to those made under the standard block grant design. We find that vouchers can provide a useful mechanism through which to limit elite capture.

Strikingly, vouchers appear to engage particularly marginalized community members in project selection: we find that our voucher treatment leads to choices that better reflect the preferences of individuals who do not normally participate in community meetings. Moreover, the effect of the voucher treatment is stronger in poorer villages, suggesting that a decentralised form of delivery can be particularly effective in communities where public resources are particularly scarce. Finally, the vouchers are also most binding in communities where leaders are otherwise less inclined to pursue projects that reflect the community's broader preferences. Taken together, the voucher mechanism appears to generate particularly large gains in communities where counterfactual outcomes are worst.

---

<sup>9</sup>Although we collected data on individuals' contributions of labor and other inputs into project implementation, we do not report any results using those data, given the small number of communities where materials were available.

The available data, particularly due to the low implementation rate of projects three months after the study, does not allow to make any claims on the effect of vouchers on the use of materials and project implementation more in general. It may be the case that communities where participants privately contributed to the public good may exhibit differential behaviors once materials are fully available. Even if this is not the case, however, we interpret our results as indicating that vouchers are a potentially useful mechanism to limit elite capture in allocation decisions.

A full cost effectiveness comparison of vouchers relative to other mechanisms for reducing elite capture is not possible, partly because studies of these alternative mechanisms do not document their costs. Such a comparison would also depend on participants' contribution rates—which may vary across contexts—as well as the welfare gains from private consumption provided by the vouchers. Finally, it is possible that participants' perceived “ownership” over public contributions might induce better monitoring or otherwise improve implementation quality, further improving vouchers' cost effectiveness. These features offer grounds for fruitful additional research.

## References

- Alatas, Vivi, Abhijit Banerjee, Rema Hanna, Benjamin A Olken, and Julia Tobias. “Targeting the Poor: Evidence from a Field Experiment in Indonesia.” *American Economic Review* 102 (2012), 1206–1240.
- Arcand, Jean-Louis, and Marcel Fafchamps. “Matching in community-based organizations.” *Journal of Development Economics* 98 (2012), 203–219.
- Bardhan, Pranab, and Dilip Mookherjee. “Decentralizing antipoverty program delivery in developing countries.” *Journal of Public Economics* 89 (2005), 675–704.
- Beath, Andrew, Fotini Christia, and Ruben Enikolopov. “Direct Democracy and Resource Allocation: Experimental Evidence from the Solomon Islands.” *Working Paper*, (2014).
- Chavis, Larry. “Decentralizing development: Allocating public goods via competition.” *Journal of Development Economics* 93 (2010), 264–274.
- Foster, Andrew D., and Mark R. Rosenzweig. “Democratization and the distribution

- of local public goods in a poor rural economy.” *Economics Department, Brown University* , (2004).
- Fritzen, Scott A. “Can the Design of Community-Driven Development Reduce the Risk of Elite Capture? Evidence from Indonesia.” *World Development* 35 (2007), 1359–1375.
- Gadenne, Lucie, and Monica Singhal. “Decentralization in Developing Economies.” *Annual Review of Economics* 6 (2014), 581–604.
- Kosfeld, Michael, and Devesh Rustagi. “Leader Punishment and Cooperation in Groups: Experimental Field Evidence from Commons Management in Ethiopia.” Working Paper, (2011).
- Labonne, Julien, and Robert S. Chase. “Who is at the Wheel When Communities Drive Development? Evidence from the Philippines.” *World Development* 37 (2009), 219–231.
- Mansuri, Ghazala. (2012). “Harnessing Community: assortative Matching in participatory Community Organizations.”
- Mansuri, Ghazala, and Vijayendra Rao. (2013). *Localizing Development: Does Participation Work?* World Bank.
- Olken, Benjamin A. “Monitoring Corruption: Evidence from a Field Experiment in Indonesia.” *Journal of Political Economy* 115 (2007).
- Olken, Benjamin A. “Direct Democracy and Local Public Goods: Evidence from a Field Experiment in Indonesia.” *American Political Science Review* 104 (2010), 243–267.
- Platteau, Jean-Philippe, and Frederic Gaspart. “The Risk of Resource Misallocation in Community-Driven Development.” *World Development* 31 (2003).
- Pradhan, Menno, Daniel Suryadarma, Amanda Beatty, Maisy Wong, Armida Alishjabana, and Arya Gaduh. “Improving educational quality through enhancing community participation: results from a randomized field experiment in Indonesia.” , (2011).

Pradhan, Menno, Vijayendra Rao, and Cristina Rosemberg. “The Impact of the Community level activities of the Second Urban Poverty Project (UPP).” *Department of Economics, University of Amsterdam, Amsterdam* , (2010).

Rao, Vijayendra, and Ana Maria Ibanez. “The social impact of social funds in Jamaica: A Participatory Econometric analysis of targeting, collective action, and participation in community-driven development.” *Journal of Development Studies* 41 (2005), 788838.

Wantchekon, Leonard. “Can informed public deliberation overcome clientelism? Experimental evidence from Benin.” *New York* , (2009).

Figure 1: Voucher contributions

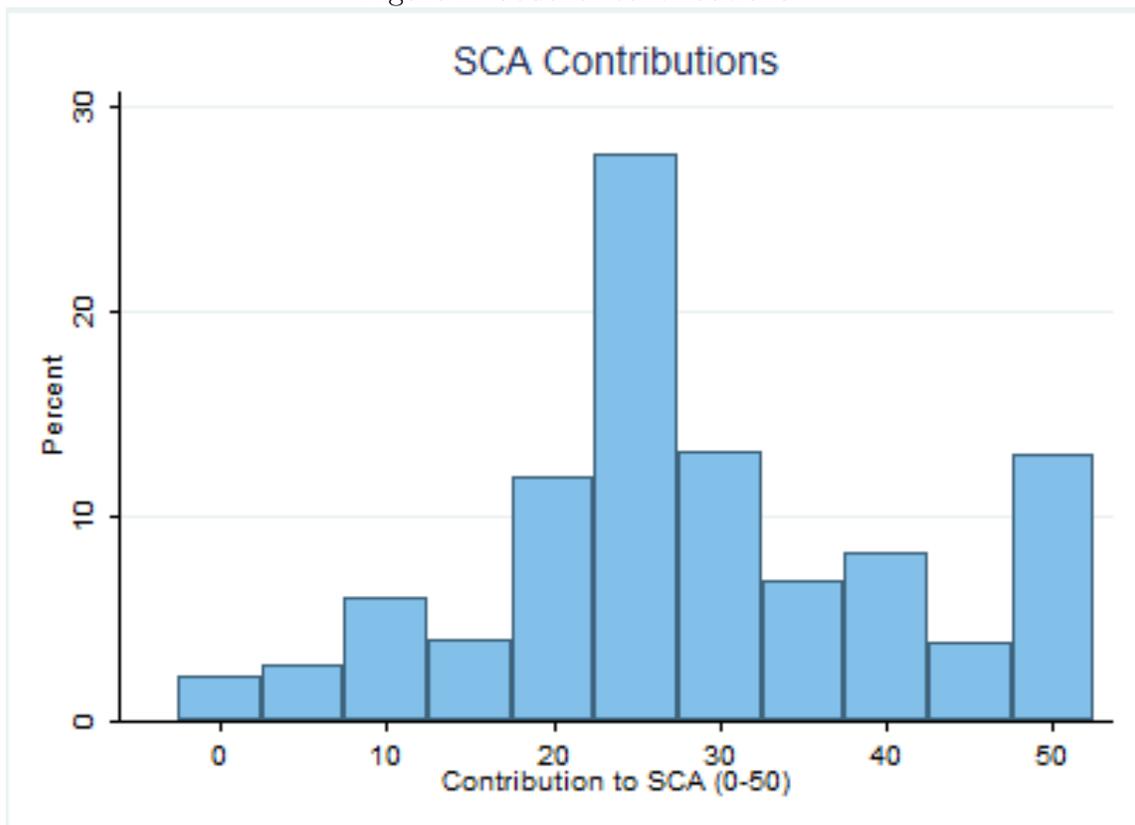


Figure 2: Voucher Contributions and Participation in Prior Community Meetings

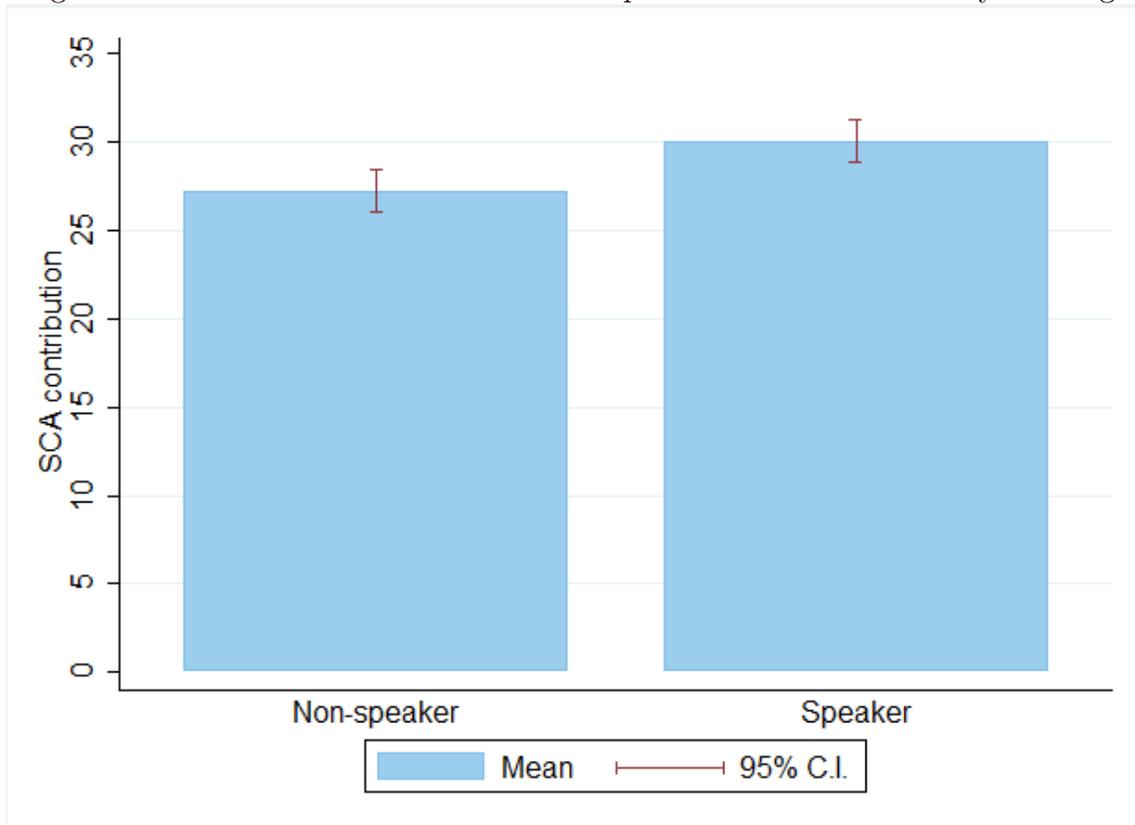


Figure 3: Participation in Prior Community Meetings and in SCA Discussion

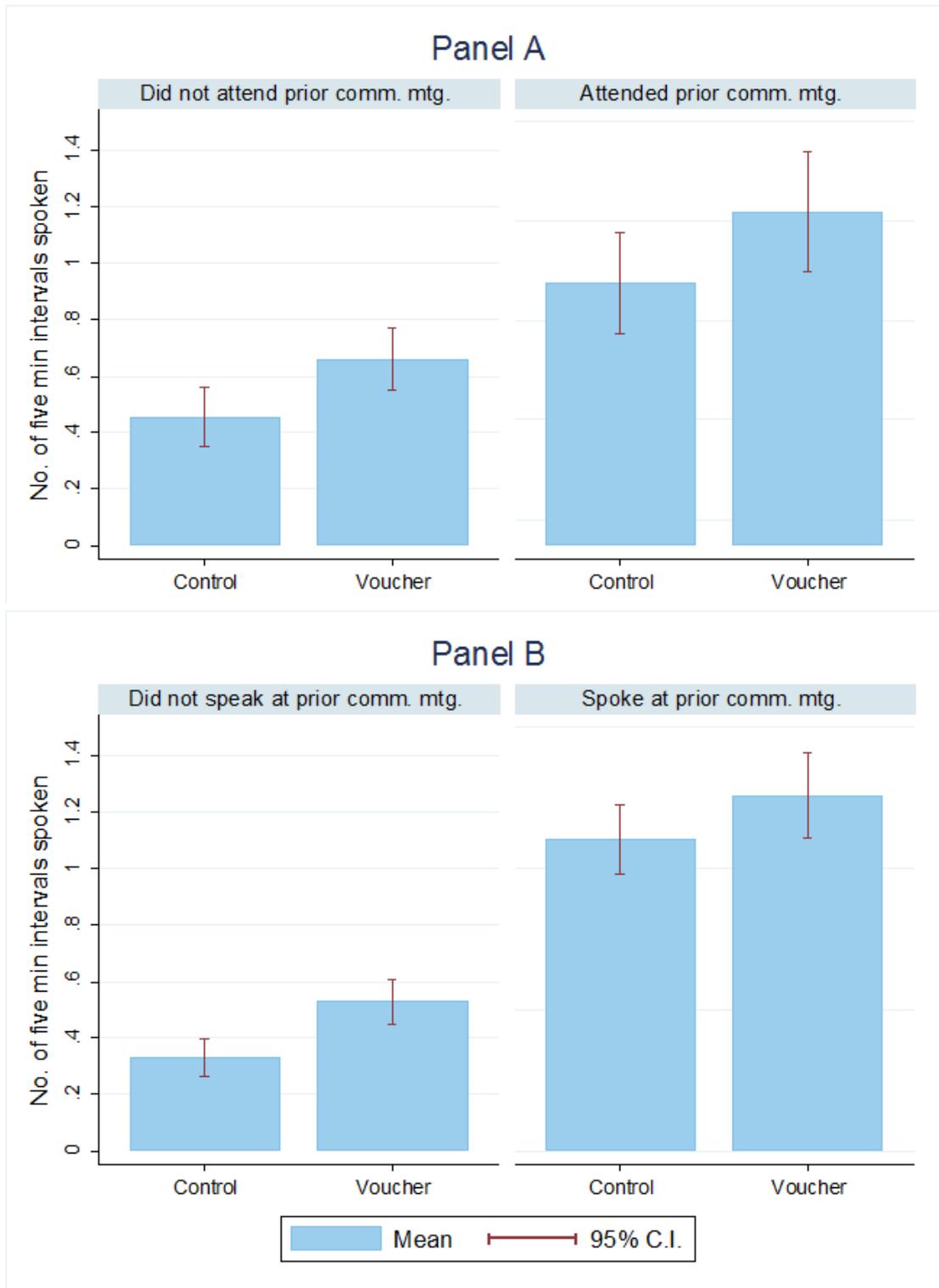


Figure 4: Project preferences and choices

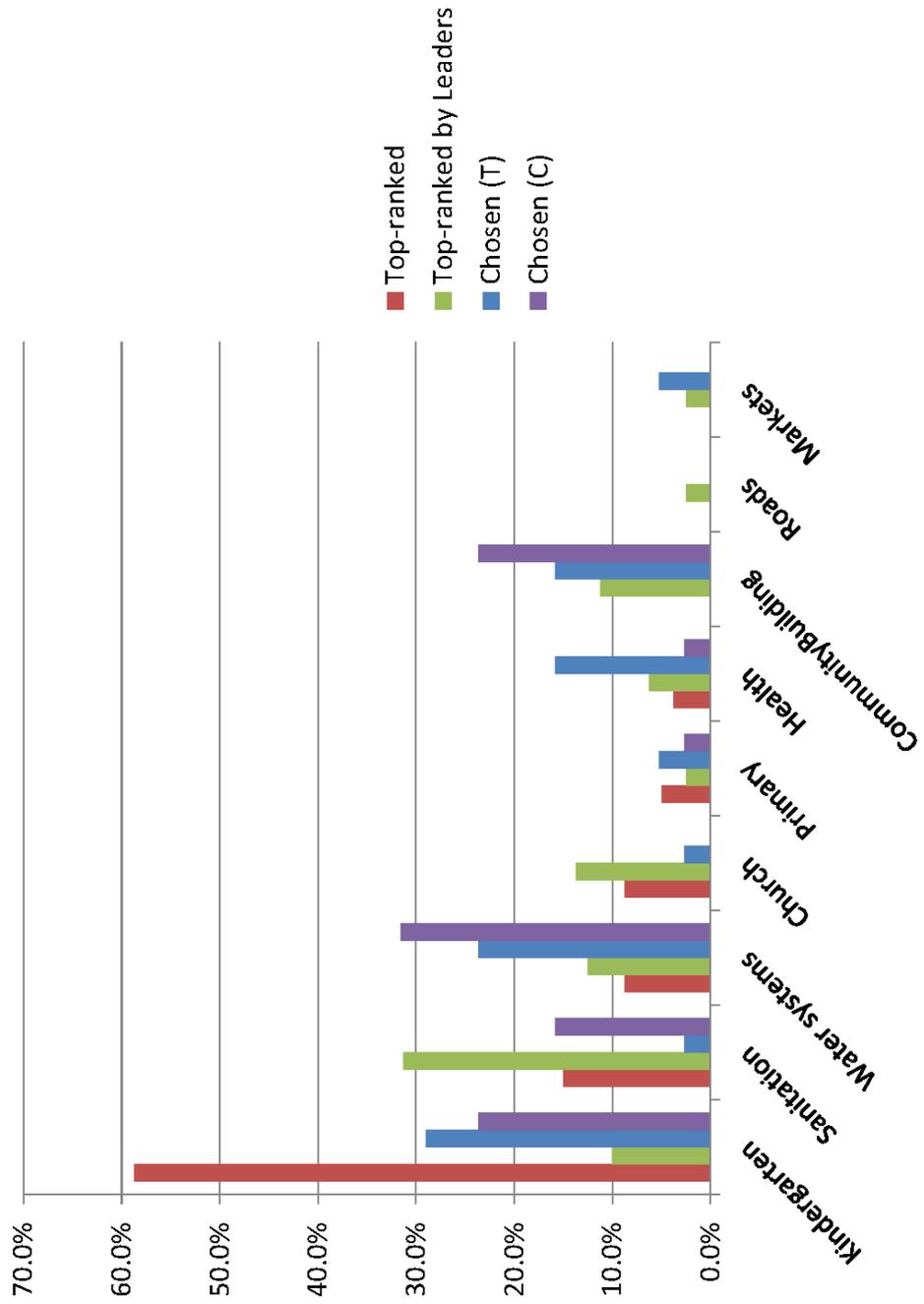


Figure 5: Effect of Treatment on Match Between Preferences and Project Type Selected

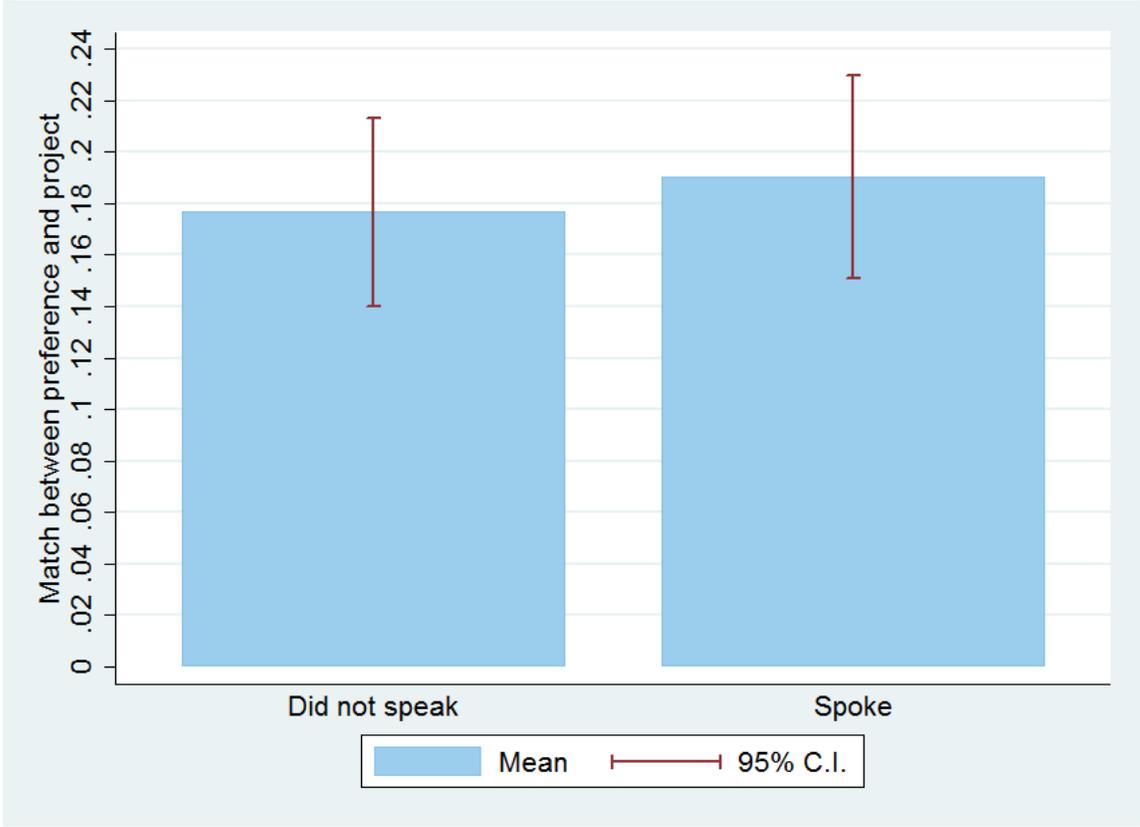


Figure 6: Effect of Participation in SCA Discussion on Fairness Perception

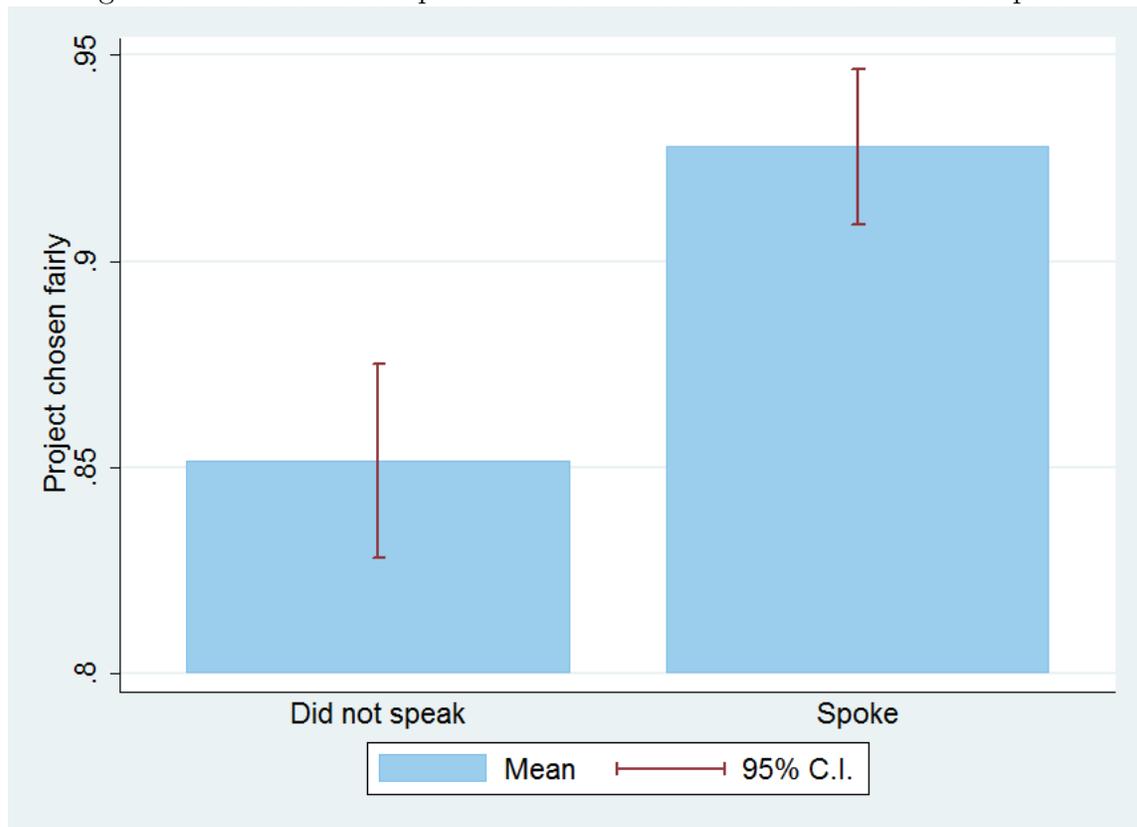


Figure 7: Effect of Participation in SCA Discussion on Satisfaction with Project

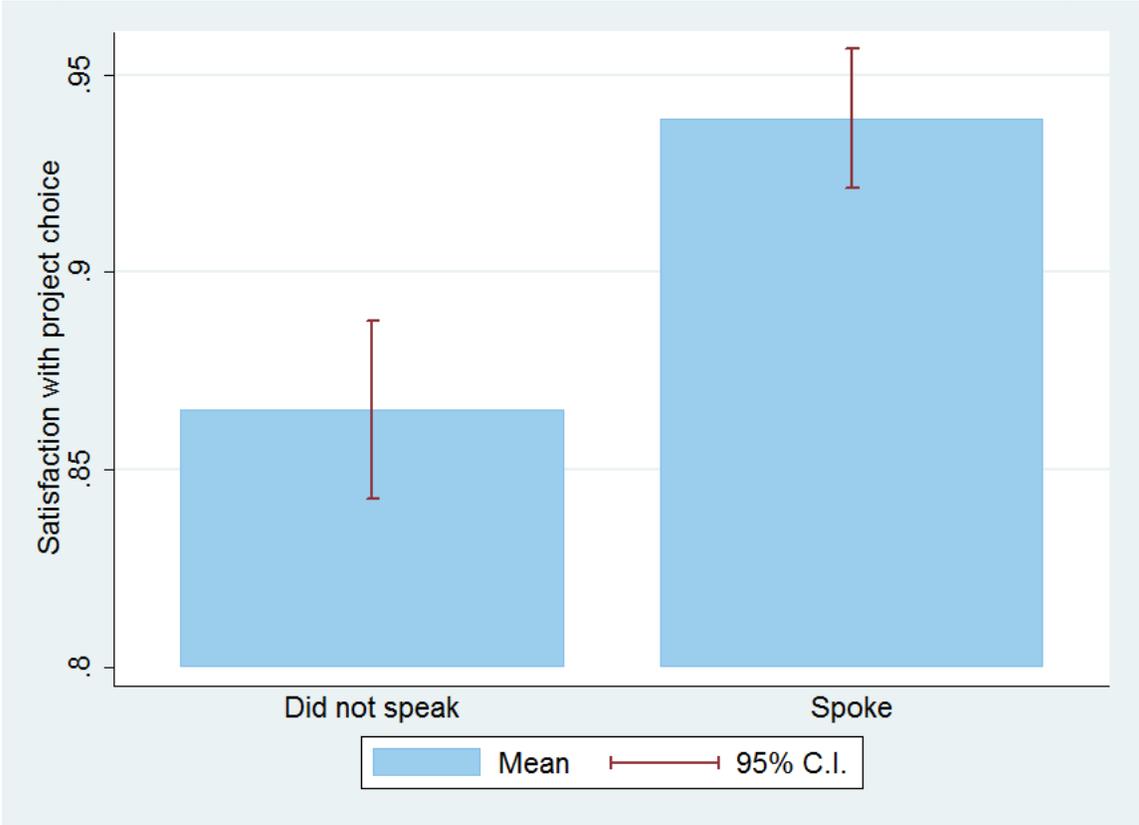


Table 1: Summary Statistics

	(1) Overall	(2) Voucher	(3) Control
SCA contribution (0-50)	28.60 (12.600)		
No. of 5 minute intervals spoken	0.75 (1.083)		
Probability of being a speaker (%)	0.45 (0.498)		
Agreement between project type and preferences (% village)	13.58 (0.345)		
Satisfied with project choice (%)	92.59 (0.264)		
Thinks project was chosen fairly (%)	95.06 (0.218)		
Female (%)	49.97 (0.012)	49.94 (0.018)	50 (0.018)
Young (%)	25.51 (0.011)	25.40 (0.015)	25.62 (0.015)
No assets (%)	17.53 (0.009)	17.5 (0.013)	17.56 (0.013)
Off-farm income(%)	11.44 (0.008)	12.12 (0.012)	10.75 (0.011)
Did not attend prior comm mtg (%)	39.64 (0.013)	42.95 (0.018)	36.08 (0.018)
Did not speak at prior comm mtg (%)	57.89 (0.012)	58.66 (0.017)	57.12 (0.017)
Altruism 1: willing to share with others without expecting anything in return (0-10)	8.33 (0.060)	8.34 (0.082)	8.33 (0.087)
Altruism 2: how much of SI\$500 would donate to someone who experienced a bad shock	191.87 (3.091)	207.69 (4.353)	174.97 (4.301)
Altruism 3: not understand why people do things not directly beneficial for them (0-10)	6 (0.087)	5.70 (0.120)	6.32 (0.124)
Reciprocity 1: willing to return a favor to a stranger (0-10)	8.336 (0.060)	8.264 (0.082)	8.413 (0.089)
Reciprocity 2: try hard to help someone who has helped me in the past (0-10)	8.32 (0.060)	8.35 (0.079)	8.29 (0.090)
Trust 1: always assume that people have only the best intentions (0-10)	6.92 (0.075)	6.76 (0.104)	7.10 (0.109)
Trust 2: others think I am too trusting (0-10)	6.68 (0.082)	6.67 (0.110)	6.70 (0.122)

Table 2: Voucher Contributions

	(1)	(2)
	SCA contributions	
Leader	2.912**	
	(1.245)	
Female	0.792	
	(0.910)	
Young	-2.227**	
	(1.045)	
No assets	-0.986	
	(1.399)	
Off-farm incomes	2.556*	
	(1.332)	
Worried about people in the village trying to cheat or steal		-1.119
		(0.768)
Altruism 1: willing to share with others without expecting anything in return		0.543**
		(0.223)
Altruism 2: how much of SI\$500 would donate to someone who experienced a bad shock		0.0225***
		(0.00501)
Altruism 3: not understand why people do things not directly beneficial for them		0.0829
		(0.175)
Reciprocity 1: willing to return a favor to a stranger		0.210
		(0.289)
Reciprocity 2: try hard to help someone who has helped me in the past		-0.236
		(0.232)
Trust 1: always assume that people have only the best intentions		-0.121
		(0.197)
Trust 2: others think I am too trusting		0.155
		(0.225)
Observations	787	710
R-squared	0.033	0.084

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered by community.  
Province fixed effects included in each regression.

Table 3: Impacts on SCA Discussion

	(1)	(2)	(3)
Dependent variable:	Probability of speaking in SCA	Total 5 minute interventions	Length of discussion
Unit of analysis:	Individual	Individual	Village
Control	0.420 (0.0265)	0.668 (0.0504)	12.69 -0.646
Treatment	0.478 (0.0302)	0.837 (0.0696)	15.38 -1.027
Observations	1,603	1,551	78
p-value on T = C	0.147	0.0496	0.0265

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered by community. Province fixed effects included in each regression.

Table 4: Impacts on SCA Discussion

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
		Probability of being a speaker				Number of five minute intervals spoken
Treatment	0.0473 (0.0496)	0.0176 (0.0400)	0.0245 (0.0336)	-0.0238 (0.0427)	-0.0505 (0.0652)	0.320** (0.157)
Leader	0.337*** (0.0471)				0.275*** (0.0462)	
Female	0.201*** (0.0280)				0.124*** (0.0322)	
Young	-0.207*** (0.0340)				-0.177*** (0.0343)	
No Assets	-0.0281 (0.0398)				-0.0105 (0.0373)	
Off-Farm Income	0.114** (0.0519)				0.0812 (0.0541)	
Treatment x Leader	-0.0196 (0.0624)				0.00466 (0.0643)	
Treatment x Female	-0.00120 (0.0396)				0.0441 (0.0443)	
Treatment x Young	0.0160 (0.0531)				0.00550 (0.0535)	
Treatment x No Assets	-0.0320 (0.0571)				-0.0481 (0.0556)	
Treatment x Off-farm income	-0.0308 (0.0740)				-0.00958 (0.0750)	
Did not attend prior comm mtg		-0.196*** (0.0330)		-0.342*** (0.0398)	-0.270*** (0.0462)	-0.450*** (0.118)
Treatment x Did not attend		0.0858* (0.0498)		0.127** (0.0536)	0.161*** (0.0582)	0.000759 (0.178)
Did not speak at prior comm mtg			-0.252*** (0.0367)	-0.381*** (0.0402)	-0.273*** (0.0449)	-0.270 (0.165)
Treatment x Did not speak			0.0622 (0.0537)	0.114** (0.0544)	0.110* (0.0589)	-0.275 (0.200)
Observations	1,600	1,600	1,600	1,600	1,600	665
R-squared	0.168	0.0533	0.075	0.129	0.208	0.140

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered by community. Province fixed effects included in each regression.

Table 5: Impacts on Project Selection

	(1)	(2)	(3)
Dependent variables:	Agreement between project type and preferences of All	Leaders	Did not speak at prior mtg
Treatment	0.0500 (0.0975)	-0.125 (0.0907)	0.0755 (0.0941)
Constant	0.250*** (0.0650)	0.275*** (0.0681)	0.250*** (0.0618)
R-squared	0.107	0.078	0.204
Observations	80	80	80

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.01$ . Standard errors clustered by community. Province fixed effects included in each regression.

Table 6: Sensitivity Checks

	(1)	(2)	(3)	(4)
	Agreement between project type and preferences of Leaders		Did not speak at prior mtgs	
Treatment	-0.100 (0.0730)	-0.251 (0.468)	-0.0500 (0.0814)	0.161 (0.519)
Leader altruism		0.0112 (0.0329)		0.0331 (0.0366)
Treatment x leader altruism		0.0175 (0.0529)		-0.0230 (0.0587)
Share of no speakers				-0.409 (0.321)
Observations	80	80	80	80
R-squared	0.086	0.093	0.025	0.055

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.01$ . Standard errors clustered by community. Province fixed effects included in each regression.

Table 7: Impacts on Fairness and Satisfaction

	(1)	(2)	(3)	(4)	(5)
Panel A					
Dependent variable:	Project was chosen in fair & equitable way				
Treatment	0.0538** (0.0227)	0.0569** (0.0241)	-0.0174 (0.0264)	0.0453 (0.0827)	0.00902 (0.0438)
Leader		0.0875*** (0.0328)			
Treatment x Leader		-0.0319 (0.0401)			
Non-speaker			-0.119*** (0.0242)	-0.0174 (0.0181)	-0.116*** (0.0313)
Treatment x Non-speaker			0.124*** (0.0356)	0.0547** (0.0265)	0.120*** (0.0404)
R-squared	0.035	0.040	0.052	0.038	0.065
Control mean	0.859	0.938	0.807	0.859	0.859
Panel B					
Dependent variable:	Satisfied with project chosen				
Treatment	0.0588*** (0.0205)	0.0653*** (0.0216)	-0.00536 (0.0213)	-0.110 (0.0941)	0.00834 (0.0385)
Leader		0.0764** (0.0319)			
Treatment x Leader		-0.0653 (0.0417)			
Non-speaker			-0.123*** (0.0227)	-0.0199 (0.0167)	-0.120*** (0.0297)
Treatment x Non-speaker			0.113*** (0.0311)	0.0458* (0.0234)	0.111*** (0.0360)
R-squared	0.036	0.039	0.056	0.034	0.067
Control mean	0.869	0.938	0.814	0.869	0.869
Province FE	Y	Y	Y	Y	Y
Treatment x Social preferences				Y	
Treatment x Demographics					Y
Observations	1,600	1,600	1,600	1,600	1,600

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.01$ . Standard errors clustered by community. Province fixed effects included in each regression.

Table 8: Impacts on Project Selection, by Leader Preferences

Dependent variables:	(1)	(2)	(3)	(4)
	Agreement between All	Agreement between Leaders	project type and Did not speak at prior mtg	preferences of Did not speak at prior mtg
Treatment	0.865 (0.646)	-0.642 (0.620)	1.117* (0.629)	-0.335 (0.253)
Leader altruism	0.0299 (0.0452)	-0.0393 (0.0434)	0.0343 (0.0440)	
Treatment x Leader altruism	-0.118 (0.0744)	0.0763 (0.0714)	-0.137* (0.0725)	
Disagreement btw prefs of leader and non-speakers				-0.231 (0.149)
Treatment x Disagreement				0.423** (0.211)
Treatment x Province	Y	Y	Y	Y
Constant	0.250*** (0.0618)	0.275*** (0.0681)	0.250*** (0.0618)	0.250*** (0.0618)
R-squared	0.134	0.087	0.240	0.240
Observations	80	80	80	80

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ . Standard errors clustered by community. Province fixed effects included in each regression.

Table 9: Community-level heterogeneous impacts

Dependent variable:	(1)	(2)	(3)	(4)	(5)
	Agreement between project type and leaders' preferences				
Treatment	-0.199*	-0.238	-0.339**	-0.179	0.105
	(0.103)	(0.175)	(0.145)	(0.157)	(0.906)
No. of HHs > 100	-0.286				
	(0.172)				
Treatment X No. of HHs > 100	0.452*				
	(0.237)				
No. of tribal groups		-0.0157			
		(0.0224)			
Treatment x No. of tribal groups		0.0306			
		(0.0309)			
Share of participants with off-farm income			-0.837		
			(0.842)		
Treatment x Share off-farm			1.863*		
			(1.013)		
Travel time to province capital > median				0.0860	
				(0.143)	
Treatment X Travel time				0.0604	
				(0.207)	
Participants' mean altruism					0.0147
					(0.0864)
Treatment x Participants' mean altruism					-0.0277
					(0.108)
Observations	80	80	80	80	80
Mean of heterogeneous variable	0.192	4.763	0.114	0.597	8.328
Inter-quartile range of heterogeneous variable	0	5	0.100	1	1.247
R-squared	0.125	0.084	0.126	0.121	0.079

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.01$ . Standard errors clustered by community. Province fixed effects included in each regression.

Table 10: Implementation

	(1)	(2)
	Picked up materials	
Treatment	-0.0122 (0.111)	0.262 (0.181)
Travel time to province center > median		0.241 (0.170)
Treatment x Travel time		-0.290 (0.246)
Observations	65	65
R-squared	0.091	0.158

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered by community. Province fixed effects included in each regression.