

Can Vouchers Reduce Elite Capture of Local Development Projects? Experimental Evidence from the Solomon Islands

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Abstract

By reducing the scope for citizen oversight of public resource allocation, the external financing of public goods and services in developing countries can exacerbate elite capture and produce sub-optimal policy outcomes for the general population. In view of the muted effects of existing initiatives to increase citizen involvement in local public decision-making and project implementation, we present and experimentally test a novel mechanism that channels funding for local public goods via citizens. While control communities are offered a block grant to fund local public goods, households in randomly-assigned communities are provided with vouchers that they may contribute to a public good or redeem at a discount for private consumption. This voucher mechanism for public resource allocation increases community participation in allocation decisions and satisfaction in decision outcomes. It also increases the probability that allocation outcomes reflect the preferences of individuals who had not previously participated in local meetings when their preferences differ from those of leaders.

Keywords: public goods, decentralization, elite capture, private contributions

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1 Introduction

In works as far back as Montesquieu’s “Spirit of the Laws”,¹ social scientists have argued that the accountability of political authorities is influenced by the nature of public revenue. Specifically, where authorities derive revenues predominantly from resource rents instead of direct taxation, governance quality is adversely affected by the lesser inclination of citizens to monitor the quality of government expenditure (Brollo et al. (2013); Herb (2005)). Such theories of a “political resource curse” are frequently applied to explain the prevalence of poor governance and low levels of economic growth among oil-exporting states (Robinson, Torvik and Verdier (2006)). A number of works - including Tilly (1992) and Bates and Lien (1985) - further document how imperatives for revenue generation created by threats of war in medieval Europe resulted in the creation of direct taxation and, with it, the formation of bargains between elites and citizens that ultimately improved the quality of governance. Herb (2005) analogously argues that the lack of a similar war-induced process by which the introduction of direct taxation instigated citizen-elite bargaining underscores the poor governance in many African states.

A more recent literature argues that official development assistance (hereafter, aid) may also generate political resource curses by reducing the reliance of authorities on direct taxation to fund the provision of public goods (Djankov, Montalvo and Reynal-Querol (2008); Rajan and Subramanian (2007)). To minimize the potential for such negative impacts of aid on governance quality, practitioners have developed a number of innovative modalities to deliver development projects without adversely impacting the accountability of national and local leaders. Among such modalities are forms of decentralization of public good provision which, through initiatives such as community-driven development (hereafter, CDD) programs and social investment funds, aim to involve local communities in the selection and management of projects. This emphasis on local participation aims to achieve both the incorporation of local information on the marginal value of different public investments (Alatas et al. (2012)) and the engagement of community members in monitoring the performance of political authorities (Fung and Wright (2003)).

Evidence indicates, however, that local public decision-making is often subject to elite capture, which raises the specter that decentralization is merely localizing the political resource curse. Various studies, for instance, note that participants in

¹See “Book XIII. Of the Relation Which the Levying of Taxes and the Greatness of the Public Revenues Bear to Liberty”

local public decision-making are generally wealthier, more educated, hold higher social status and are more politically connected than non-participants (Mansuri and Rao (2013); Pradhan, Rao and Rosemberg (2010); Arcand and Fafchamps (2012); Mansuri (2012)). As a result, elites have generally captured the outcomes of local public decision-making (Fritzen (2007); Labonne and Chase (2009); Rao and Ibanez (2005)). While such elite capture is not necessarily detrimental to the general interest if it enables better-informed yet benevolent elites to exert heightened influence, Beath, Christia and Enikolopov (2017) observe that villagers' perceptions of the local economic and governance situation were adversely affected by elite influence over project selection decisions. In a related experimental study, Beath, Christia and Enikolopov (2013) identify a negative impact on the behavior of local leaders of the infusion of funds through a CDD program, a result which potentially is due to the distortion induced in accountability relationships between local leaders and the community.²

To date, adaptations of local public decision-making to strengthen accountability and reduce elite capture of externally-provided resources have met with limited success. Many CDD programs, for example, employ facilitators who guide communities through a needs identification and implementation process (Mansuri and Rao, 2013), although the presence of facilitators appears to shift the project choices toward the preferences of the facilitators themselves (Platteau and Gaspart, 2003). Olken (2010) studies another means of limiting elite capture - the use of referenda to enable villagers to select local projects from a menu provided by a CDD program - and finds that, while referenda improve citizen satisfaction, they do not change the type of projects that were selected.³ Beath, Christia and Enikolopov (2017) replicate the experiment in the context of a CDD program in Afghanistan and observe that referenda induce a small change in the influence of elites over project selection, but do not increase the effectiveness of implemented projects. Efforts to improve the accountability of local leaders and service providers through increasing community monitoring have similarly proved to be of limited effectiveness. (Olken, 2007), for instance, finds that community-based monitoring is less effective than traditional

²A related experimental literature shows the correlation between leaders preferences and community members behavior, from cooperation (Kosfeld and Rustagi (2015)) to contributions to public goods and private investment (Beekman, Bulte and Nillesen (2014), Jack and Recalde (2015))

³This result is consistent with evidence on individual valuation of decision processes, independently from decision outcomes (Guth and Weck-Hannemann (1997), Fehr, Herz and Wilkening (2013), Bartling, Fehr and Herz (2014), Owens, Grossman and Fackler (2014)), and on control aversion among individuals (Falk and Kosfeld (2006)).

top-down monitoring in reducing corruption of local public spending in Indonesia. Banerjee et al. (2010) also find that efforts to promote increased participation of beneficiaries in the monitoring of public services in India were generally ineffective in increasing community involvement or service outcomes.

In this paper, we present a novel mechanism to reduce elite capture of local public decision-making by channeling external resources to fund public goods through citizens. The mechanism provides households in villages covered by a CDD project with ‘vouchers’ which households may either contribute to the cost of a local development project proposed after discussion among village authorities and community members, or which may be redeemed at a discount for a private capital good. By providing villagers with the collective ability to de-fund a non-accountable local authority and by establishing the private opportunity cost of public expenditure, it is envisaged that this voucher-based CDD modality will increase incentives for local authorities to support publicly-beneficial projects and increases participation in project selection and monitoring, thereby resulting in higher quality projects and increased satisfaction with the choice.

To test the effects of vouchers on project selection, we administered a field experiment across 80 villages in the Solomon Islands, a country where local authorities have historically exercised substantial authority over local public decision-making. In each village, 20 randomly-selected adults were provided with 10 notes, which could be redeemed for either 10 Solomon Islander dollars (SBD 10) each (approximately USD 1.40) if contributed to a public fund, or SBD 5 each if retained for private consumption. In the control villages the maximum fund amount (SBD 2,000) was provided as a block grant with no individual contributions required and no possibility for households to retain any portion of the grant for private consumption. In both treatment and control communities, the public fund could be used to purchase items selected by participants from a pre-set menu of materials at a local hardware supplier. Importantly, participants made their decisions anonymously, thereby avoiding the potential for intimidation and retribution. Apart from the way in which the funds were distributed, all features of the process were the same across both treatment arms.

The results of the experiment indicate that the use of vouchers substantially alters both the selection process and outcomes, although the effects on project implementation outcomes and general welfare are ambiguous. Compared to discussions in control villages provided with block grants, the voucher scheme lengthens discus-

sions about project selection among participants and increases the average number of community members that speak during such discussions. The voucher scheme reduces the probability of selected projects matching the ex-ante preferences of village leaders and increases the probability of selected projects matching the preferences of individuals who had not participated in recent village meetings. Villagers also perceive project outcomes to be fairer under the voucher scheme. As expected, however, the voucher scheme reduces the volume of funding available to villages, with treatment villages receiving just 79.1% of funds available (58.1% of the total available in public good contributions, 20.9% in private cash). With available data, we are unable to assess whether this reduced flow of funds was associated with reduced welfare. Furthermore, with the limited data available, we do not observe differences between the control and treatment communities in the speed with which they obtain materials and implement selected projects.

The paper is divided into five sections: Section 2 describes the setting, experimental design and implementation, and outlines our hypotheses; Section 3 describes our sources of data and provides summary statistics on our sample; Section 4 presents results on discussion dynamics, project selection, participant perceptions, and project implementation; and Section 5 concludes.

2 Background

2.1 Sample Villages

The study occurred over June - August 2013 across 80 villages randomly sampled from the population of villages participating in the Solomon Islands Rural Development Program (RDP). Launched in 2008, RDP was implemented by the Solomon Islands' Ministry of Development and Planning and Aid Coordination (MDPAC) and was supported by AusAID, IFAD, and the World Bank. A CDD program, RDP financed investments identified by villagers through a participatory process. Existing local institutions (e.g., tribal councils and churches) planned and managed RDP activities at the community-level and supervised implementation of small works.

The 80 sample villages are small (average population of 488 people) and isolated. The average travel time to the respective provincial capitals is 12 hours and to the country's capital two and a half days. The main modes of transport are paddleboat or outboard canoe and the vast majority of villagers (82%) rely on subsistence fishing

and horticulture. Most villages do not have access to electricity, running water or sanitation. Approximately 80 percent of households use rainwater catchments for drinking water, only have access to solar lamps for lighting, and defecate in the sea or the bush. In this context, the financing provided by RDP offers a rare opportunity to improve local public facilities and services.

Given the isolation of the sample villages, formal government structures are of limited relevance. Most of the villages (85%) are governed by traditional village chiefs, with elected leaders (8%) and/or church leaders (13%) providing local governance services in a much smaller proportion of communities.⁴ All villages have one or more churches, which also serve as the community hall for meetings. Religion is an important part of daily life, with nearly all participants claiming a religious affiliation. In the sample villages, the predominant denominations are the United Church (28%), Seventh Day Adventist (27%), Catholic (25%), and South Seas Evangelical (22%).

2.2 Intervention

In the 80 sample villagers, leaders were asked to invite all available adults to a community meeting on a specified date. Attendees of this meeting represent the sampling frame for participants.⁵ From this frame, 18 villagers (9 male and 9 female) were selected via a random drawing of names. In addition, the two highest-ranking leaders (one male, one female) were selected from among those present at the meeting. The community meeting was then adjourned, with only the individuals selected to participate asked to remain.

In all villages, selected participants were informed that SBD 2,000 (approximately USD 300) had been allocated to fund the improvement of a local non-religious public facility, such as a school, health center, market, toilet, road, well, or irrigation system.⁶ Facilitators directed participants to discuss the type of project that they

⁴A number of villages have more than one type of village leader

⁵It is possible that leaders may have selectively invited villagers to the meeting and/or that villagers may have self-selected based on their needs and/or capacity to exercise voice. Using data concurrently provided by a random sample of 10 households in each village, we find that participants have slightly higher ownership of toilets than non-participants, but exhibit lower levels of access to primary schools and health clinics. There is also no difference between the correlation of project preferences of leaders and participants, and the correlation of the project preferences of leaders and non-participants. Overall, there is no evidence to indicate that the participant selection process was substantially affected by leaders and/or by self-selection

⁶The fund was provided as credit at a local hardware store and permitted the purchase of

believed would most benefit the community, with the goal of reaching a consensus on which project to fund. In order to ensure the discussion resembled a community meeting, no structure was imposed on the form of the discussion or on the method of selection of the project. Facilitators did not intervene in the discussion until an agreement was reached, but rather recorded who spoke and for how long. At the conclusion of the discussion, facilitators directed participants to complete a form identifying the type of project and which materials they intended to purchase. Following the completion of the form, participants in all sample villages were paid a small fee for participating in the activity. All activities were conducted in spaces protected from outsiders' intrusions, such as local schools or public buildings.

2.3 Description of Treatment

Sample villages were randomly allocated to either the control or treatment group, which differed in the mode of fund allocation.⁷ Any treatment-specific information was revealed to participants after they had been selected. In villages assigned to the control group, facilitators informed participants that a block grant of SBD 2,000 would be allocated to fund the community project. In the treatment group and prior to the discussion, facilitators issued each of the 20 selected participants with 10 paper vouchers and explained that each voucher could either be redeemed for cash or contributed to the fund for the community project. If redeemed, vouchers would be worth SBD 5 each, whereas vouchers contributed to the project would be worth SBD 10 each.⁸ Following the discussion and project selection, participants in the treatment group were asked by the facilitator to indicate privately how many of the vouchers they wished to redeem and how many they wished to contribute to the community project.

materials required for the work (such as paint, roofing iron, and/or cement). Villagers were required to provide labor and complementary materials for the selected project and one participant was selected to record community contributions and the use of allocated funds. Participants were also asked to nominate the person responsible for procuring materials from the hardware store.

⁷Randomization was stratified within provinces.

⁸Thus, if a participant redeemed all vouchers for cash, they would receive SBD 50 (approximately USD 7.50).

3 Hypotheses

The provision of vouchers to participants may affect selection processes and outcomes by changing the structure of incentives facing local leaders to build consensus. In treatment villages participants may (at least partially) withhold funding if they are not convinced that the benefits that will accrue to them from the selected project will exceed the redemption value of vouchers. As a result of this and the fact that participants' decisions over the use of the vouchers are made after project selection, leaders seeking to maximize project funding face an incentive to maximize the involvement of all participants in the decision-making process and to seek direct assurances from participants that they support the project choice. In contrast, during discussions in control villages, leaders face an incentive to minimize participation in order to reduce the probability of dissent over the relative benefits of project options.

The use of vouchers to select projects may also increase the willingness of villagers to participate in discussions about project selection by changing the framing of the selection process. As noted in Section 2.1 above, the sample villages generally adhere to a customary governance structure dominated by unelected village chiefs that derive their authority from their lineage and/or economic wealth. Decisions over the use of local public resources such as project selection are ordinarily the domain of these local elites, with social norms discouraging villagers from challenging the decisions of such elites in a public setting. The use of vouchers, by contrast, potentially creates a new frame for local public decision-making which provides individual participants with special authority over selection outcomes. As a result of this change of frame and the associated relaxation of social norms that govern local public decision-making, non-elite villagers may be more willing to actively participate in the selection process.

In so far as the use of vouchers to select projects increases active participation by community members, the use of vouchers should increase the extent to which non-leader participants announce their preferences over the menu of projects. In the event that such announced preferences generally differ from those of leaders and uncertainty over others' preferences otherwise exists, the discussion would thereby facilitate the aggregation of such preferences and thereby increase the probability of their realization. Furthermore, if leaders prefer the implementation of any project to no project, the use of vouchers may cause leaders to accept a selection outcome that they exclusively do not prefer in order to ensure that participants do not redeem vouchers and deprive the village of a project.

By increasing incentives for local leaders to encourage participation and to accept project outcomes that they do not necessarily prefer and by relaxing social norms that may otherwise inhibit participation, it is anticipated that the use of vouchers will result in increased participation by marginalized community members, improve the correspondence between participant preferences and selection outcomes, and improve satisfaction both with the process and the outcomes. In particular, the study tests the following hypotheses relating the treatment to various outcomes of interest:⁹

1. Vouchers increase participation in project selection, as measured by the duration and inclusiveness of discussions on project selection;
2. Vouchers increase the correspondence between selected projects and preferences of the median non-elite participant and preferences of marginalized participants who do not ordinarily participate in community decisions;
3. Vouchers increase the fairness of the project selection process as perceived by participants and satisfaction of participants with the selected project.

To account for multiple comparisons that arise because we test effects on multiple outcomes and across multiple subgroups, we follow Anderson (2008). Specifically, we create weighted indices when analyzing multiple outcomes in a single hypothesis (generally those presented in a single table) and sharpened q-values when analyzing multiple subgroups.

4 Data Sources and Descriptive Statistics

4.1 Data Sources

Data to estimate the effects of the treatment and to explore heterogeneity in treatment effects was collected across four stages in all sample villages:¹⁰

⁹These hypotheses were documented in a pre-analysis plan completed before the data collection. The pre-analysis plan also included hypotheses relating the treatment to the likelihood of projects being implemented and to the quality of projects. However, the available data does not allow us to test these hypotheses, as only a small number of communities had collected the material and started to work on the projects three months after the intervention. For this reason, we do not test any hypotheses pertaining to project implementation. However, the respective results are discussed briefly in Section 4.

¹⁰Voucher contributions are also observed for participants assigned to treatment villages.

1. Prior to the discussion, a short questionnaire was administered to all selected participants. Participants (including participating leaders) were asked to provide an ordinal ranking of the top three buildings that they believed should be improved with a hypothetical SI\$2000 grant. They were provided with the following nine options: kindergarten, primary school, health clinic, water supply (typically a community standpipe), sanitation, market, road / bridge / wharf, or another non-church community building.¹¹ Information on community characteristics was also collected by facilitators from a sample of key informants, such as village elders and other local leaders.
2. During the discussion in all sample villages, the facilitator recorded the number of speaking interventions by each participant per five minutes and the total length of the discussion.
3. Following the discussion, an additional short questionnaire was administered to participants. The survey collected information on demographic and socio-economic characteristics, prior experience with community organizations, social preferences, and satisfaction with the local leadership. The survey also collected information on participants willingness to share resources with others when nothing is expected in return.¹²
4. Approximately three months after the discussion, an enumerator returned to 65 communities to assess project progress as measured by the procurement and installation of materials funded by the intervention.

4.2 Participant Characteristics

Summary statistics are presented in Table 1. The first 6 columns report means and standard deviations for control villages (Columns 1-2), treatment villages (Columns 3-4) and the full sample (Columns 5-6), while the last column reports p-values for

¹¹An additional project type, church buildings, was overwhelmingly cited by respondents in the ‘other projects’ category, and so was assigned a separate category ex-post for the analysis.

¹²This question is shown to correlate strongly with choices in the dictator game, a behavioral game commonly used to capture altruism (Falk et al., 2012). These questions were asked after the discussion in order to avoid priming subjects to act cooperatively. Although this raises the possibility that the social preference measure is influenced by the treatment, we do not find evidence of a treatment effect on the altruism measure (see Table 1).

balance between the treatment and control groups across each variable.¹³

Panel A of Table 1 summarizes participant characteristics. Consistent with recruitment protocols, exactly half of the sample in both treatment and control communities is female. 26 percent of participants are under the age of 30, 18 percent report owning no fixed assets (such as a boat or a bicycle), and only 11 percent list a primary source of income other than farming or fishing.¹⁴ 55 percent of participants report that they either didn't attend any community meetings over the previous five years or did not speak at any of the meetings.¹⁵ Finally, the mean response to the altruism question is 8.3 on a scale from 1 to 10. Across all of the aforementioned characteristics, the sample is balanced between treatment and control groups.

Panel B of Table 1 summarizes *ex-ante* project preferences of participants. More than a third of participants ranked the local kindergarten as their most preferred option, while sanitation (15%) and water supply (12%) were the second and third most popular first choices respectively (and most frequently ranked as second choices). In 59 percent of villages, kindergarten was the most frequently reported top ranked preference among all participants, while sanitation was the most frequently reported top ranked preference in 15 percent of villages. Participants preferences over project types are balanced across treatment and control villages, with the exception of those over health centers ($p = 0.018$), while leader preferences are imbalanced over water and sanitation projects.¹⁶ When looking at the aggregate distribution of project preferences across treatment and control villages, a chi-square test does not reject the hypothesis that both overall preferences and leaders preferences are drawn from the same distribution ($p = .878$ and $p = .114$, respectively).

Across the sample, leaders preferences diverge from those of other community members, particularly for health, sanitation, and school projects (Figure 1). Specifically, 12.5 percent of leaders prioritize the improvement of health facilities, compared to 8.5 percent of participants overall (two-sided t-test, $p = .093$). Similarly, sanitation is prioritized by 20.6 percent of leaders versus 15.4 percent of all participants ($p = .038$). Conversely, primary schools are preferred by 9.6 percent of participants

¹³P-values are calculated by regressing each variable on a treatment dummy and province fixed-effects, with standard errors clustered at the village level when the outcome variable is at the individual level, and robust standard errors otherwise.

¹⁴In the analysis, the latter two measures are used to proxy for respondents' income.

¹⁵In the analysis, this measure is used to proxy for marginalization. This measure is significantly negatively correlated with leader status and wealth and significantly positively correlated with being female and being aged under 30 (Table A1).

¹⁶Table A2 reports balance tests for preferences over projects by other relevant sub-groups.

generally, but just 5.6 percent of leaders ($p = .026$). A chi-square test rejects the hypothesis that the distributions of preferences of these two groups are the same ($p = .024$).

4.3 Voucher Contributions

In communities assigned to the treatment group, contributions by participants averaged SBD 29 (Table 2) and ranged between the feasible minimum of SBD 0 and the feasible maximum of SBD 50, with a mode at SBD 25 (Figure 2). Contributions generally decrease monotonically on both sides of the mode, with the exception that 13.3 percent of participants contributed the full amount possible. Only 0.6 percent of participants contributed zero. These contribution levels are higher than those typically observed in laboratory experiments using one-shot games, where contribution rates are often approximately 40 percent.

Voucher contributions are correlated with individual characteristics (Table 3). Column 1 reports results of the regression of individuals' voucher contributions on demographic characteristics and province fixed effects. Purposively-selected leader participants contribute about SBD 2.7 more than the average participant. An effect of similar magnitude is observed for participants with off-farm income, which serves as a proxy for high socioeconomic status. Participants under 30 years of age contribute SBD 2 dollars less. Differences in contributions by gender, asset level, and history of participation are insignificant. Column 2 further indicates that contributions are positively and significantly correlated with participants' altruism (as gauged by survey responses).¹⁷ The coefficients on leader status, age, off-farm income and altruism retain statistical significance in the combined regression (Column 3). These results thereby indicate that more powerful, older, wealthier, and more altruistic individuals contribute larger absolute amounts.

Voucher contributions are correlated with active participation in the discussion, perceptions of fairness in process and outcomes, and match between the selected project and individual preferences. Subjects who spoke during the discussion contributed on average SBD 30.1, while those who did not contributed SBD 27.2 ($p = .001$). Participants who perceive the selection process as fair and are satisfied with the project choice each contribute SBD 2 more than those who do not, although these differences are not statistically significant ($p = .385$ and $p = .106$, respectively).

¹⁷Other survey measures of trust and reciprocity are not significantly correlated with contribution levels.

5 Results

The following subsections report results for the tests of the aforementioned hypotheses. In particular, the subsections report estimates of the effect of the treatment on the selection process; on selection outcomes; on perceptions of and satisfaction with the process; and on implementation outcomes.

5.1 Effect on Selection Process

5.1.1 Treatment Effect

Discussions last an average of 12.7 minutes in control communities and 15.4 minutes in treatment communities, with the difference significant at the 5 percent level (Table 2). The effect of the treatment on discussion duration appears to arise from a shifting of the right tail of the distribution: only 15 percent of control communities have discussions that last 20 minutes or longer, while 38 percent of treatment communities do so. The increase in the duration of discussions in treatment villages is due to an increase in individual participation, both on the extensive and intensive margins. The treatment increases the share of participants speaking during the discussion by 4.1 percentage points over the control group level of 39.5 percent. The treatment also causes participants to speak more: the average number of five minute intervals during which a participant speaks is 0.67 in control villages, compared to 0.83 in treatment villages.¹⁸ These differences are statistically significant at the 5 and 10 percent levels for duration and number of interventions, respectively (Table 2).

Table 4 report the results of regressions of the treatment on participation in the discussion. Columns 1, 3 and 5 estimate the following regression:

$$y_{ip} = \beta_1 Treat_{ip} + Province_p + u_{ip}$$

Where y_{ip} is the probability of a participant speaking (Column 1), the number of the number of five minute intervals during which a subject spoke (Column 3), and a summary index of these two outcome variables (Column 5).¹⁹ Column 5 shows that the voucher treatment has a positive effect that is statistically significant at

¹⁸Given that the probability of speaking in the meeting was affected by treatment, the analysis of speaking intervals does not censor this outcome at participation equals one, but always considers the unconditional outcome. We thank an anonymous referee for pointing this out.

¹⁹This addresses the multiple comparisons problem. The methodology follows Anderson (2008).

the 10 percent level. Vouchers thus appear to generally increase participation in the selection process. While it is feasible that this increase may arise as a result of coordination among participants on contribution strategies, facilitators reported that such discussions happened in only a few villages.²⁰

5.1.2 Effect Heterogeneity

Columns 2, 4, and 6 of Table 4 provide estimates of heterogeneity in treatment effects, according to the following regression:

$$y_{ip} = \beta_1 Treat_{ip} + \beta_2 IndChar_{ip} + \beta_3 Treat_{ip}XIndChar_{ip} + Province_p + u_{ip}$$

Where $IndChar_{ip}$ is a vector of individual characteristics: being a leader, female, young, reporting off-farm income sources or no asset ownership. Outcome variables are thus regressed on the treatment, individual characteristics, and their interaction. The analysis adjusts for the increased potential for false positives due to multiple comparisons by adjusting the p-values for the false discovery rate (Anderson, 2008) and reporting sharpened q-values.²¹ The heterogeneity analysis reveals that leaders and those with off-farm income are more likely to speak, and women and persons under 30 are less likely to contribute to the discussion. However, there is no evidence that indicates any heterogeneity in treatment effects over any of the respective sub-groups.²²

Of particular interest is the effect of the treatment on ‘marginalized individuals who had not actively participated in previous community meetings (see Section 4.2). While such participants were 32 percentage points less likely than non-marginalized participants to speak during the discussion in control villages (p = .000), the treatment significantly increases the probability of them becoming involved in the discussion. Figure 3 shows the effect of the treatment on the probability of speaking (left) and the number of interventions (right). While the general effect on both outcomes is positive overall, the effect for marginalized participants is larger and attains a

²⁰The relatively wide distribution of contributions within villages suggest that, if any such coordination took place, it was not particularly effective. Specifically, the difference between the smallest and the largest contributions is greater than SBD 30 in 60 percent of the villages.

²¹This adjustment is performed in all regressions with multiple interaction terms.

²²These results are robust to controlling for unbalanced preferences over project types overall and by leaders, women, and younger participants.

higher level of statistical significance (two-sided t-test $p = .001$ and $p = .0001$ for the probability of speaking and number of interventions, respectively).²³

5.2 Effects on Selection Outcomes

5.2.1 Treatment Effect

Figure 1 compares selection outcomes in the treatment and control villages with *ex-ante* primary preferences of participant villagers and participant leaders.²⁴ Sanitation projects were selected less frequently in treatment (2.5%) than control communities (17.5%; $p = .025$), while health projects were selected more frequently (15% vs. 2.5%; $p = .049$). To further explore the overall effect of treatment on project choice we turn to regression analysis. Since choices between project types are related, Table 5 shows SUR estimation results (a la Chattopadhyay and Dulfo (2004)). As noted above, we observe unbalances in baseline preferences over projects, namely health projects were more strongly preferred in the treatment group, overall and by women and young people, and sanitation projects were more strongly preferred by leaders in the treatment group, which confounds estimation of treatment effects. Regressions thus control for all unbalanced baseline preferences, overall and by sub-groups, as well as for province fixed-effects:

$$y_{vp} = \beta_1 Treat_{vp} + \beta_2 Pref_{vp} + \beta_3 Province_p + u_{vp}$$

Where y_{vp} are indicator variables for whether each project type was selected in village v in province p , and $Pref_{vp}$ is a vector of baseline preferences over project types. The regression results (Table 5) confirm that the voucher treatment had a positive effect on the likelihood that health projects were selected, and a negative one on the likelihood that sanitation projects were selected (both effects are statistically significant at the 5 percent level). A Wald test of joint significance of the treatment coefficients shows that vouchers significantly affected selection outcomes ($p = .073$).

The treatment had no statistically significant effect on the probability of correspondence between the selected project and preferences of general participants, leaders, or participants who were *ex-ante* marginalized, or on a weighted index of all

²³As shown in Table A3, the results on the probability of speaking and on the weighted index retain statistical significance when adding controls for individual characteristics, their interaction with treatment and unbalanced preferences over project types.

²⁴Across all villages, water systems were the most frequently selected project (28.7%), followed by kindergartens (25%) and community buildings (20%).

three, as indicated in Table 6).²⁵ The coefficients are nonetheless qualitatively large and generally exhibit the anticipated sign.²⁶

5.2.2 Effect Heterogeneity

Differences in leader and villager preferences may arise as a result of differences in interests or of differences in information on the relative benefits of projects (Kosfeld and Rustagi, 2015). Where information asymmetries cause differences in preferences, the resolution of the asymmetry in the course of the discussion may cause villagers to adopt leaders preferences, resulting in minimal elite capture despite the appearance of such given the structure of *ex-ante* preferences. To isolate the effect of the treatment on 'interest-driven' elite capture, the treatment is interacted with measures of the altruism of participating leaders:²⁷

$$y_{vp} = \beta_1 Treat_{vp} + \beta_2 AltrLead_{vp} + \beta_3 Treat_{vp} \times AltrLead_{vp} + \beta_4 Pref_{vp} + \beta_5 Province_p + u_{vp}$$

Column 1 of Table 7 indicates that, as predicted, altruism among leaders weakens the effect of the treatment in improving the correspondence between participants preferences and selection outcomes.²⁸ Columns 3, 5 and 7 estimate the interaction effects on correspondence with leader preferences, marginalized villager preferences, and a weighted index of the three outcome variables. While the coefficients generally assume the expected signs, all are insignificant at conventional levels.

In order to identify the effect of the treatment in cases where there is a divergence in the preferences of leaders and marginalized participants,²⁹ the treatment is interacted with a binary measure indicating disagreement between the preferences of

²⁵A community-level dummy variable was generated to indicate whether the project type that was selected matched any of the modal priority preferences of all participants, leaders, or marginalized participants. In the event of a tie in preferences, correspondence was checked with either of the respective preferences. Regressions of this variable are run at the community level on a treatment indicator, province fixed effects and controls for unbalanced preferences over project types overall and by leaders and marginalized individuals.

²⁶The exception to this is the coefficient on leader preferences, which assumes a positive sign.

²⁷The approach assumes that the selection discussion enables villagers to discover whether leaders preferences are driven by information or interest and that, in the case of the former, that villagers will accede to the leaders preferences, weakening the treatment effect.

²⁸The coefficient on the un-interacted treatment effect (which indicates what happens where leaders are not altruistic) has the expected sign, but is insignificant.

²⁹As above, marginalized participants are defined as those who had not spoken in or attended prior community meetings.

leaders and marginalized individuals.³⁰ The even columns of Table 7 indicate that, while neither the treatment nor the interaction have a statistically significant effects on the correspondence between selection outcomes and group preferences when entered individually, the interacted treatment effect is statistically significant at the 5 percent level when preferences are combined in a weighted index. Specifically, the treatment increases the correspondence between the selected project and the preferences of marginalized and non-leader participants in those cases where marginalized and leader participants have distinct preferences.

Table 8 explores interactions between the treatment and community characteristics that may affect cooperation between villagers, such as the size of the community, number of tribal factions, nature of local economic activity,³¹ distance to the provincial center, and average level of altruism:

$$y_{vp} = \beta_1 Treat_{vp} + \beta_2 VillChar_{vp} + \beta_3 Treat_{vp} X VillChar_{vp} + \beta_4 Pref_{vp} + \beta_5 Province_p + u_{vp}$$

As indicated in Columns 1 and 6,³² the treatment induces a statistically significant increase in the probability of correspondence between selection outcomes and leader preferences in large communities. This effect is, however, at least partially offset by the statistically significant reduction in the probability of correspondence induced by village size. In villages that are located far from the provincial center, the treatment reduces the probability of correspondence between leader preferences and selection outcomes, although this effect is also offset by the un-interacted effect of distance on the probability of correspondence.

5.3 Effects on Perceptions of and Satisfaction with Process

Table 9 reports estimates of the effect of the treatment and individual characteristics on participants perceptions of and satisfaction with the selection process.³³

³⁰We thus run the same specification as above, only replacing leader altruism with the disagreement indicator.

³¹Off-farm income is positively and significantly correlated with voucher contributions and positively correlated with asset ownership.

³²In addition to including all covariates, Column 6 reports multiple inference adjusted q-values so as to correct for multiple testing.

³³Data is provided by participants responses to the questions: “Do you think the project was chosen in an equitable and fair way?” and “Are you personally satisfied with the project that was selected today?”.

Perceived fairness (87% in control villages) and satisfaction (87% in control villages) are generally high (see Table 2). Leaders are 7.4 percentage points more likely to perceive the process as fair and 3.9 percentage points more likely to be satisfied with the outcome. The opposite holds for marginalized individuals, who report lower satisfaction with the decision process and outcome, by 4.7 and 6 percentage points respectively. The treatment increases perceived fairness by 5.4 percentage points and satisfaction by 6.0 percentage points, with these effects significant at the 5 percent and 1 percent level, respectively.

Columns 2 and 4 of Table 9 examine interactions between the treatment and leader and marginalized status, according to the following specification:

$$y_{ip} = \beta_1 Treat_{ip} + \beta_2 Leader_{ip} + \beta_3 Treat_{ip} \times Leader_{ip} + \beta_4 Marg_{ip} + \beta_5 Treat_{ip} \times Marg_{ip} + \beta_6 IndChar_{ip} + \beta_7 Province_p + u_{ip}$$

While the treatment does not significantly affect leaders perceptions or satisfaction with the process, it does significantly increase both outcomes for marginalized individuals, a result that is significant at the 1 percent level. All these findings hold for the weighted index of these outcomes (Columns 5 and 6).³⁴ It is feasible that these effects are driven by the effect of redeemable vouchers, rather than by improving the quality of the discussion and selection process *per se*. While we cannot discount this directly, we find a positive and statistically significant correlation between participation in the discussion and perceptions and satisfaction, as well as higher voucher contributions by participants that spoke during the discussion. Given the result that the treatment increases the quality of participation in the discussion, there thus exists *prima facie* evidence to indicate that the improvement in perceptions and satisfaction is driven by the increase in the quality of discussion rather than the mere provision of redeemable vouchers.

5.4 Effects on Project Implementation

Enumerators returned to 65 of the 80 communities approximately three months after the discussion to collect data on project implementation progress.³⁵ This data

³⁴We further test the robustness of these results by controlling for unbalanced preferences over projects and separately interacting the treatment with individual characteristics and find qualitatively consistent results.

³⁵Data was not collected from 25 communities due to inaccessibility.

indicated that, as a result of the remoteness and the infrequency of transport services, only a quarter of communities had been able to obtain the necessary materials from the hardware suppliers to which the credits were issued. As indicated in Table 10, there are no significant differences between treatment and control communities in the probability of materials being obtained at follow-up, even if the treatment effect is interacted with a binary variable indicating whether the community lies farther than the median travel time from the respective provincial center.

6 Discussion

Of all the hypotheses that we set out to test, we find weak empirical support for the ability of the voucher treatment to shift project selection outcomes away from leaders' preferences and towards median and marginalised individuals' preferences. However, such lack of evidence is not necessarily proof that vouchers did not improve project selection outcomes. In this section, we thus seek to provide further evidence on the quality of selected projects.

First, we focus on treatment communities and examine the relationship between the type of project selected and voucher contributions. If the decision process itself led to broader consensus for the selected projects, beyond individual initial preferences over project types, we do not expect individuals to contribute larger amounts to them on the basis of their baseline top-ranked project type. We regress individual voucher contribution on an indicator for the match between selected and top-ranked project type, controlling for individual characteristics, unbalanced baseline preferences over project types and province fixed-effects, and find that participants, whose top-ranked project type was selected to be implemented, do not contribute significantly more.³⁶ Moreover, while certain project types are much more frequently preferred than others,³⁷ voucher contributions do not appear to be systematically affected by the type of project that is selected (Figure 4). Average contributions by the type of selected project range between SBD 26.8 for community buildings and SBD 37 for markets.³⁸ It thus appears that discussing over which project to fund leads to support for the selected project beyond participants' initial preferences over

³⁶The regression coefficient indicates that contributions are 1.997 SBD higher on average when there is a match (s.e. = 1.306; $p = .134$). Results available upon request.

³⁷The share of top-ranked preferences among participants range from 33.4% for kindergartens to 1.9% for roads.

³⁸Roads were never selected to be funded.

projects.

Second, we investigate whether satisfaction with the selection outcome is influenced by the match between the selected project and baseline preferences over project types differently in treatment and control villages. If the decision process, shaped by the treatment, facilitated information sharing and updates of beliefs on the value of different project types, we would expect satisfaction in treatment villages not to differ significantly depending on whether the selected project type was the individual's top-ranked one. We thus run the following specification:

$$y_{ip} = \beta_1 Treat_{ip} + \beta_2 Match_{ip} + \beta_3 Treat_{ip} \times Match_{ip} + \beta_4 Pref_{s_{ip}} + \beta_5 Province_p + u_{ip}$$

where y_{ip} is satisfaction with selection outcome. We find that participants, whose top-ranked project type was selected to be implemented, are more satisfied with the decision outcome in control villages, but not in treatment villages (see Column 1 of Appendix Table A4).³⁹

Third, we attempt to test whether the treatment led to the selection of projects objectively more deserving of funding. The available data limits our ability to do so, as we do not have objective measures of local public good quality, but we exploit survey data on community members perception of improvements in public goods quality over the previous 5 years. The rationale for these tests is that having been improved prior to the experiment would make a public good in less need of further improvement. We run village-level SUR of the likelihood that a specific project type is selected on the interaction between treatment and an indicator of perceived past improvements, controlling for baseline project preferences and province fixed-effects. When testing the joint significance of the interaction terms, we find no statistically significant effect of treatment on the likelihood that previously not improved projects were selected ($p = .239$).

Finally, we exploit information on the local public goods improved with RDP funding. We expect that the limited funding provided through the experiment could be put to better use for incremental improvements or maintenance activities of existing public goods, rather than for constructing new ones. We run village-level SUR

³⁹This claim is confirmed by a Wald test of the sum of treatment and interaction term coefficient ($p = .330$). Perceived fairness of the selection process does not instead appear to be influenced by the match between selection and ex-ante preferences neither in treatment nor in control villages (Column 2 of Table A4).

of the likelihood that a specific project type is selected on the interaction between treatment and an indicator of perceived past improvements, controlling for baseline project preferences and province fixed-effects. Again, we do not find any overall statistically significant effect of treatment on the likelihood that the experiment and RDP-selected project types match ($p = .210$). However, we obtain significant results when we look at the correlation between the distribution of projects chosen during the experiment and as part of the RDP program: chi-square tests cannot reject the hypothesis that projects selected within the experiment and RDP are drawn from the same distribution in treated villages ($\chi^2 = 91.8$; $p = .000$), while it rejects the hypothesis for control communities ($\chi^2 = 34.981$; $p = .770$).⁴⁰ Figure 5 shows the frequency of experiment and RDP-selected projects in treated and control villages and confirms the closer correspondence between the two variables in the former than in the latter set of villages.

Overall, these results suggest that the improvement in the decision process fostered by the voucher treatment encourages information sharing among participant, which improves the perceived quality of the decision outcome, regardless of ex-ante preferences. Moreover, there appears to be a closer correspondence between the experimental and RDP selection outcomes in treated villages, which can be interpreted as evidence of sensible use of the resources made available through the experiment, since they were intended for the improvement of existing public goods.

7 Conclusions

In order to both improve the correspondence between development projects and local preferences and to encourage increased citizen participation in project implementation, development programming has become increasingly decentralized over recent decades. However, the equity and efficiency of such programs has been hampered by the propensity of local elites to exercise undue influence over project selection and by limited participation among community members. While various initiatives such as community fora, referenda, and community participatory monitoring - have been devised to constrain elite capture and increase community involvement, these have experienced limited success. This paper presents an alternative scheme which seeks to reduce elite capture and increase community engagement by providing vouchers to community members participating in a local resource allocation exercise.

⁴⁰The distribution of RDP-selected projects does not differ between treated and control villages.

The effects of the voucher scheme on the nature of selection processes, selection outcomes, and implementation outcomes are tested using a randomized controlled trial covering 80 villages in the Solomon Islands. All sample villages were provided with SBD 2,000 to allocate to a community project to be selected by a group of 18 villagers and 2 village leaders. In 40 of these villages, members of the group were provided with vouchers that could either be redeemed for a private good or allocated to the community project. In the other 40 villages, project selection followed a conventional process.

Estimates from the experiment indicate that vouchers increase the duration of the discussion and the probability of otherwise-marginalized individuals participating in the discussion. There is also evidence that vouchers change the type of projects selected and, while there is no general evidence that vouchers alter the probability of selected projects corresponding with participant preferences, there is some evidence that vouchers increase the probability of marginalized individuals realizing their preferences when those preferences disagree with those of leaders. In addition, vouchers improve the perceived fairness of and satisfaction with the selection process among both participants generally marginalized participants specifically.

Potentially due to the limited progress in project implementation observed across the sample in the three months after the study, the available data does not provide any indication that vouchers affect project implementation outcomes. Nonetheless, the improvement that vouchers induce in participant involvement and community satisfaction provides reason to suggest that, over a longer time period, vouchers may potentially increase the involvement of community members in project monitoring and, in so doing, improve the quality of implemented projects. On the other hand, however, as redeemable vouchers also generally reduce the funding that is provided to public works projects, it is feasible the vouchers may constrain the scope of funded project and thereby the benefits offered by such projects. Exploring the longer-term effects on project outcomes of the use of vouchers in community project selection is an important avenue for subsequent research.

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Tables and Figures

Table 1: Descriptive Statistics

	Control		Treatment		Total		P-value
	Mean	Sd	Mean	Sd	Mean	Sd	
<i>Panel A: Participant Characteristics</i>							
Female	0.500	(0.500)	0.500	(0.500)	0.500	(0.500)	.
Under-30	0.256	(0.437)	0.255	(0.436)	0.256	(0.436)	0.967
Own Assets	0.175	(0.380)	0.176	(0.381)	0.176	(0.381)	0.969
Off-Farm Income	0.108	(0.310)	0.121	(0.327)	0.114	(0.318)	0.493
Limited Participation	0.537	(0.499)	0.560	(0.497)	0.549	(0.498)	0.416
Altruism	8.331	(2.324)	8.335	(2.266)	8.333	(2.294)	0.968
<i>Panel B: Primary Project Preference</i>							
Kindergarten	0.357	(0.480)	0.311	(0.463)	0.334	(0.472)	0.256
Primary School	0.102	(0.303)	0.090	(0.286)	0.096	(0.295)	0.518
Health Center	0.064	(0.244)	0.106	(0.308)	0.085	(0.279)	0.018
Roads	0.016	(0.127)	0.022	(0.148)	0.019	(0.138)	0.384
Market	0.029	(0.167)	0.045	(0.207)	0.037	(0.189)	0.115
Water	0.121	(0.327)	0.121	(0.327)	0.121	(0.327)	1.000
Sanitation	0.136	(0.343)	0.171	(0.377)	0.154	(0.361)	0.354
Community Bldg.	0.029	(0.167)	0.036	(0.187)	0.032	(0.177)	0.483
Church	0.092	(0.290)	0.076	(0.266)	0.084	(0.278)	0.541

Note: p-values from regressions of outcome on treatment and province fixed-effects.

Table 2: Descriptive Statistics of Outcome variables

	Control		Treatment		Total		P-value
	Mean	Sd	Mean	Sd	Mean	Sd	
Voucher Contributions			29.085	(12.169)			
Speaker	0.395	(0.489)	0.436	(0.496)	0.416	(0.493)	0.216
Interventions (a)	0.668	(1.000)	0.835	(1.157)	0.751	(1.083)	0.063
Discussion Duration	12.69	(5.110)	15.38	(6.726)	14.04	(6.087)	0.030
Match All (b)	0.250	(0.439)	0.350	(0.483)	0.300	(0.461)	0.302
Match Leader	0.400	(0.496)	0.375	(0.490)	0.388	(0.490)	0.813
Satisfaction	0.869	(0.338)	0.927	(0.259)	0.898	(0.303)	0.005
Fair Process	0.859	(0.348)	0.912	(0.283)	0.886	(0.318)	0.020

Note: p-values from regressions of outcome on treatment and province fixed-effects. "Speaker" is equal to 1 if a subject spoke during the discussion. "Interventions" represents the number of five minute intervals during which a subject spoke. "Match All" indicates a correspondence between the selected project and the modal priority of participants.

Table 3: Individual Correlates of Voucher Contributions

	Voucher contribution		
	(1)	(2)	(3)
Leader	2.607** (1.224)		2.309* (1.269)
Female	-0.547 (0.887)		-0.560 (0.908)
Young	-2.068* (1.091)		-2.357** (1.089)
Off-Farm Income	2.514* (1.336)		2.350* (1.392)
No Assets	-0.970 (1.383)		-1.567 (1.367)
Marinalized	-1.073 (1.043)		-0.967 (1.028)
Altruism		0.580*** (0.204)	0.549** (0.205)
Constant	29.97*** (2.750)	24.61*** (3.167)	26.08*** (2.915)
N	787	755	755
Adj. R-sq	0.023	0.016	0.033

Note: All regressions include province fixed-effects.

Standard errors clustered at the village

parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effect on Selection Process

	Speaker		No.interventions		Weighted index	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0413 (0.0331)	0.0461 (0.0430)	0.162* (0.0858)	0.233* (0.126)	0.130* (0.0728)	0.175* (0.0962)
Leader		0.337*** (0.0471)		0.699*** (0.0968)		0.710*** (0.0842)
Female		-0.201*** (0.0280)		-0.466*** (0.0664)		-0.446*** (0.0525)
Young		-0.207*** (0.0340)		-0.383*** (0.0779)		-0.409*** (0.0701)
Off-Farm Income		0.114** (0.0519)		0.203** (0.0994)		0.220** (0.0985)
No Assets		-0.0281 (0.0398)		-0.112 (0.0769)		-0.0853 (0.0752)
Treatment x Leader		-0.0196 (0.0624)		0.147 (0.147)		0.0392 (0.122)
		[1.00]		[1.00]		[1.00]
Treatment x Female		0.00120 (0.0396)		-0.136 (0.107)		-0.0703 (0.0831)
		[1.00]		[1.00]		[1.00]
Treatment x Young		0.0160 (0.0531)		-0.0837 (0.105)		-0.0333 (0.102)
		[1.00]		[1.00]		[1.00]
Treatment x Off-Farm		-0.0308 (0.0740)		-0.0743 (0.177)		-0.0699 (0.157)
		[1.00]		[1.00]		[1.00]
Treatment x No Assets		-0.0320 (0.0571)		0.0478 (0.140)		-0.0109 (0.123)
Constant	0.419*** (0.0334)	0.538*** (0.0361)	0.684*** (0.0896)	0.956*** (0.101)	-0.0467 (0.0757)	0.214*** (0.0801)
N	1600	1600	1548	1548	1548	1548
Adj. R-sq	0.032	0.161	0.055	0.209	0.041	0.201

Note: All regressions include province fixed-effects. Standard errors clustered at the village level in parentheses parentheses, FDR-adjusted q-values in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effect of Vouchers on Project Choice

	Treatment coeff. (s.e.)
<i>Dependent variable: selected project is</i>	
Kindergarten	0.0500 (0.0944)
Primary School	0.0250 (0.0414)
Health center	0.126** (0.0598)
Market building	0.0500 (0.0340)
Water system	-0.0729 (0.0984)
Sanitation	-0.145** (0.0635)
Community building	-0.0500 (0.0861)
Church building	0.0250 (0.0242)
<i>N</i>	80

Note: SUR estimation. All regressions include province f.e. and unbalanced baseline preferences over project types. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effect of Vouchers on Correspondence between Selected Project and Participant Preferences

	Correspondence btw. project and preferences			Index
	All (1)	Leader (2)	Marg (3)	(4)
Treatment	0.0805 (0.103)	0.0594 (0.109)	0.130 (0.106)	0.0745 (0.236)
Constant	0.00202 (0.125)	0.117 (0.132)	0.0565 (0.129)	-0.0962 (0.286)
N	80	80	80	80
Adj. R-sq	0.139	0.159	0.110	0.048

Note: All regressions include province f.e. and unbalanced baseline preferences over project types. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Heterogeneity of Treatment Effects on Project Selection

	Correspondence btw project and preferences						Index	
	All (1)	(2)	Leader (3)	(4)	Marg. (5)	(6)	(7)	(8)
Treat.	0.850 (0.631)	-0.251 (0.202)	0.126 (0.702)	0.0848 (0.222)	0.588 (0.676)	-0.0833 (0.213)	2.132 (1.489)	-0.423 (0.474)
Altruistic leader	0.0463 (0.0445)		-0.0149 (0.0487)		0.0379 (0.0470)		0.157 (0.106)	
Treat x Altruistic	-0.122* (0.0724)		-0.0110 (0.0802)		-0.0725 (0.0777)		-0.265 (0.172)	
Disagreement		-0.116 (0.140)		-0.150 (0.165)		-0.193 (0.148)		-0.325 (0.330)
Treat x Disagreement		0.263 (0.208)		-0.0120 (0.232)		0.201 (0.219)		0.914* (0.486)
Constant	-0.311 (0.426)	0.140 (0.147)	0.276 (0.458)	0.177 (0.154)	-0.198 (0.445)	0.196 (0.156)	-1.547 (1.017)	0.0149 (0.359)
N	80	80	80	80	80	80	80	80
Adj. R-sq	0.183	0.169	0.119	0.139	0.084	0.094	0.034	0.044

Note: All regressions include province x treatment fixed-effects and control for unbalanced preferences over project types. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Heterogeneity of Treatment Effects on Project Selection by Village Characteristics

	Match between project choice and leader preferences					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.05 (0.117)	-0.056 (0.197)	0.035 (0.172)	0.287* (0.169)	-0.024 (1.035)	-1.027 (1.064)
Large	-0.395** (0.186)					-0.449* (0.226)
Treat x Large	0.578** (0.256)					0.944*** (0.325)
No. Tribal Groups		-0.011 (0.025)				0.002 (0.025)
Treatment x Tribal Groups		0.026 (0.034)				-0.04 (0.038)
Off-Farm			-0.547 (0.970)			0.733 (0.986)
Treatment x Off-Farm			0.262 (1.193)			-1.479 (1.274)
Remote				0.201 (0.148)		0.379** (0.161)
Treatment x Remote				-0.328 (0.216)		-0.492** (0.232)
Altruism					-0.034 (0.099)	-0.211** (0.104)
Treatment x altruism					0.01 (0.124)	0.201 (0.132)
Constant	0.125 (0.137)	0.091 (0.167)	0.176 (0.172)	-0.109 (0.166)	0.380 (0.813)	1.469* (0.808)
N	78	76	80	72	80	70
Adj. R-sq.	0.320	0.277	0.246	0.365	0.243	0.472

Note: all regressions include province fixed-effects and unbalanced baseline preferences over project types. Robust standard errors in parentheses, FDR-adjusted q-values in brackets.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Effect of Voucher on Satisfaction with Decision Process and Outcome

	Fair process		Satisfied		Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0549** (0.0227)	-0.0138 (0.0292)	0.0601*** (0.0202)	0.0122 (0.0235)	0.204*** (0.0706)	-0.00179 (0.0833)
Leader	0.0582*** (0.0215)	0.0357 (0.0323)	0.0191 (0.0217)	0.0224 (0.0305)	0.135* (0.0715)	0.102 (0.108)
Marg.	-0.0486** (0.0228)	-0.107*** (0.0289)	-0.0624*** (0.0204)	-0.106*** (0.0272)	-0.197*** (0.0707)	-0.377*** (0.0874)
Treat x Leader		0.0466 (0.0420)		-0.00571 (0.0425)		0.0701 (0.141)
		[0.157]		[0.808]		[0.452]
Treat x Marg.		0.117*** (0.0386)		0.0884*** (0.0328)		0.362*** (0.115)
		[0.007]		[0.018]		[0.005]
Female	-0.0393* (0.0218)	-0.0384* (0.0220)	-0.0355 (0.0217)	-0.0347 (0.0219)	-0.132* (0.0739)	-0.129* (0.0746)
Young	0.0742*** (0.0185)	0.0726*** (0.0178)	0.0633*** (0.0190)	0.0616*** (0.0185)	0.243*** (0.0609)	0.237*** (0.0587)
No asset	-0.0103 (0.0262)	-0.00727 (0.0259)	-0.0103 (0.0256)	-0.00753 (0.0254)	-0.0364 (0.0859)	-0.0262 (0.0850)
Off farm	0.00327 (0.0240)	0.000620 (0.0240)	0.0126 (0.0197)	0.0107 (0.0200)	0.0285 (0.0615)	0.0206 (0.0619)
Constant	0.935*** (0.0274)	0.971*** (0.0275)	0.954*** (0.0243)	0.979*** (0.0228)	0.186** (0.0873)	0.293*** (0.0819)
N	1600	1600	1600	1600	1600	1600
Adj. R-sq	0.053	0.059	0.054	0.058	0.064	0.070

Note: All regressions include province fixed-effects. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Effect on implementation

	Picked up materials	
	(1)	(2)
Treatment	-0.0122 (0.111)	0.262 (0.181)
Far from prov.ctr.		0.241 (0.170)
Treatment x far		-0.290 (0.246)
Constant	0.156 (0.114)	-0.0697 (0.169)
N	65	58
Adj. R-sq	0.031	0.058

Note: All regressions include province fixed-effects.

Robust standard errors in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Relative Preferences over Project Types and Effect on Type of Selected Project

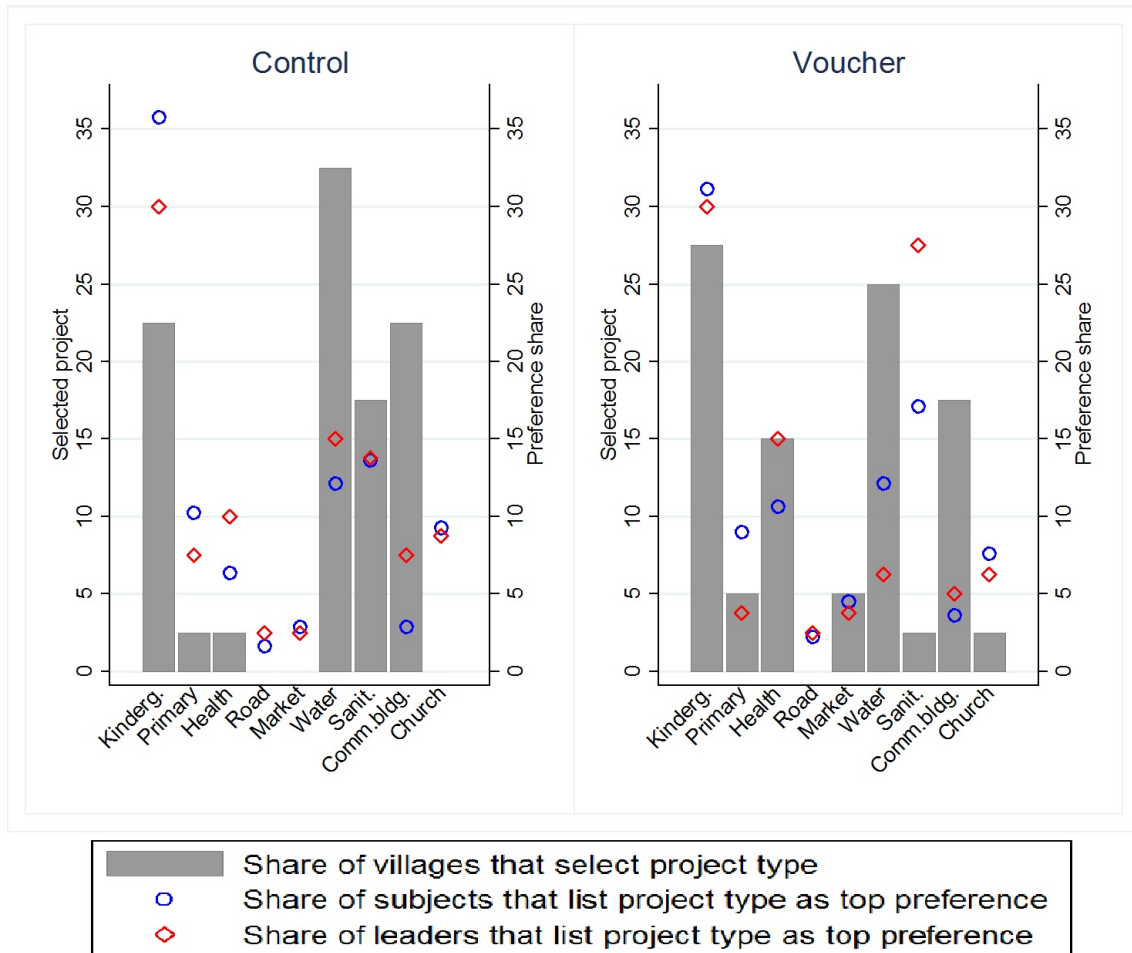


Figure 2: Voucher Contributions

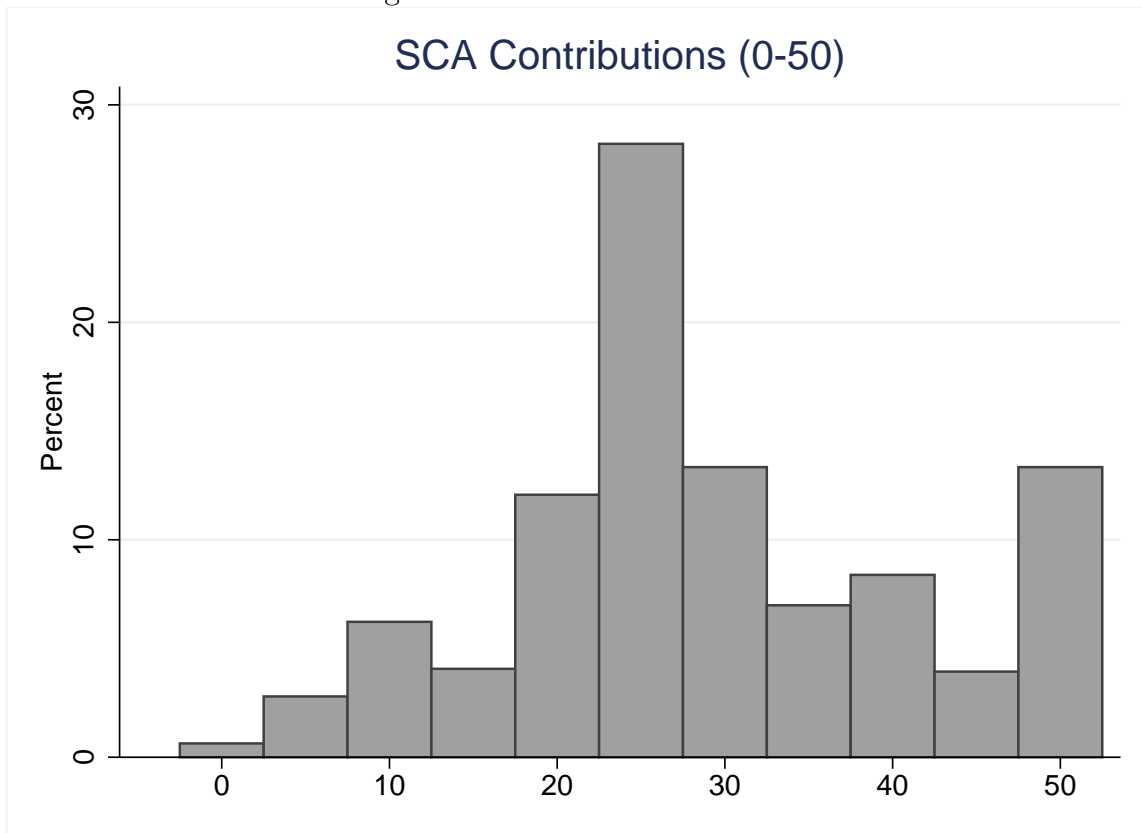


Figure 3: Treatment Effect on Participation by Marginalization Status

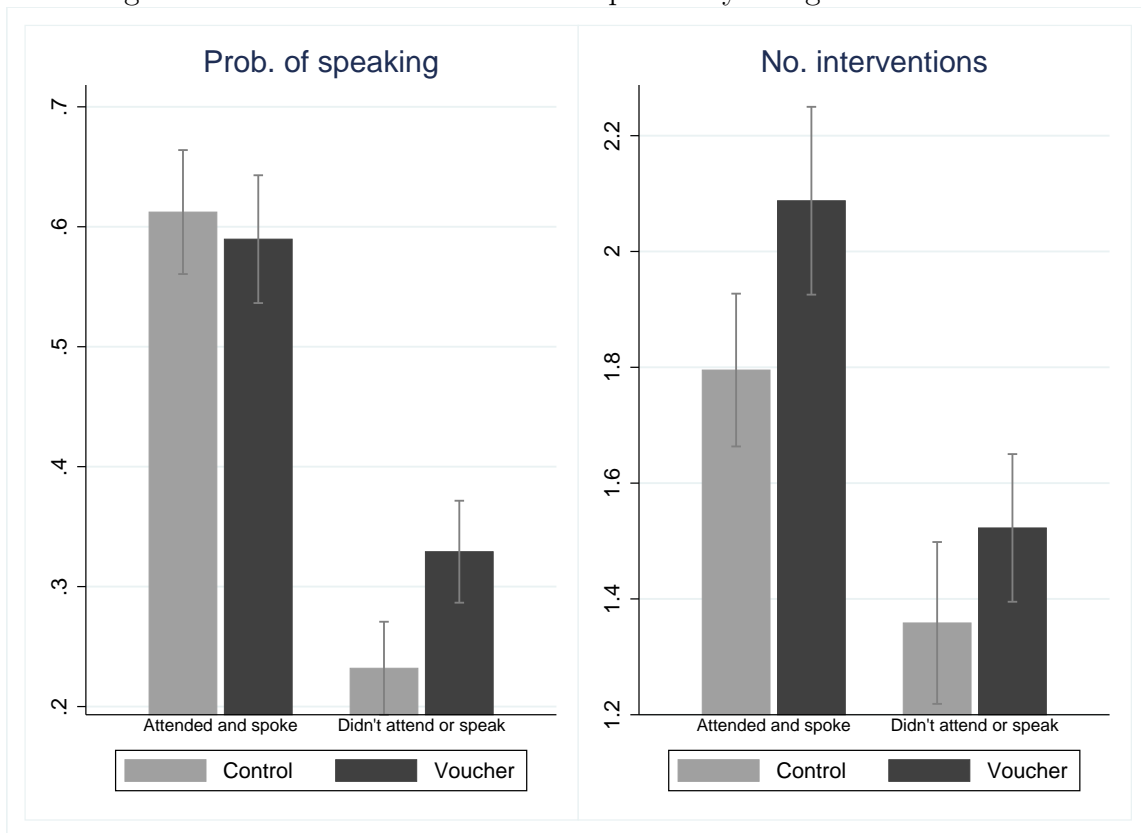


Figure 4: Preferences over Projects and Voucher Contributions by Type of Selected Project

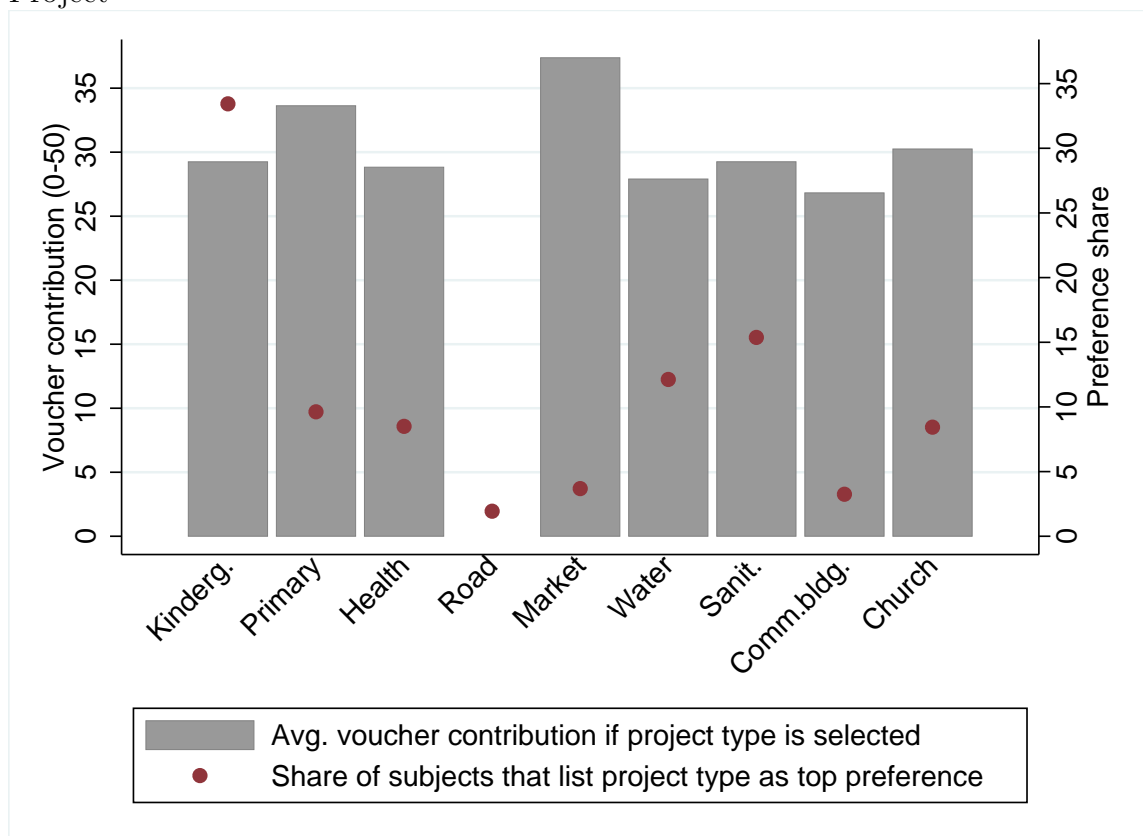
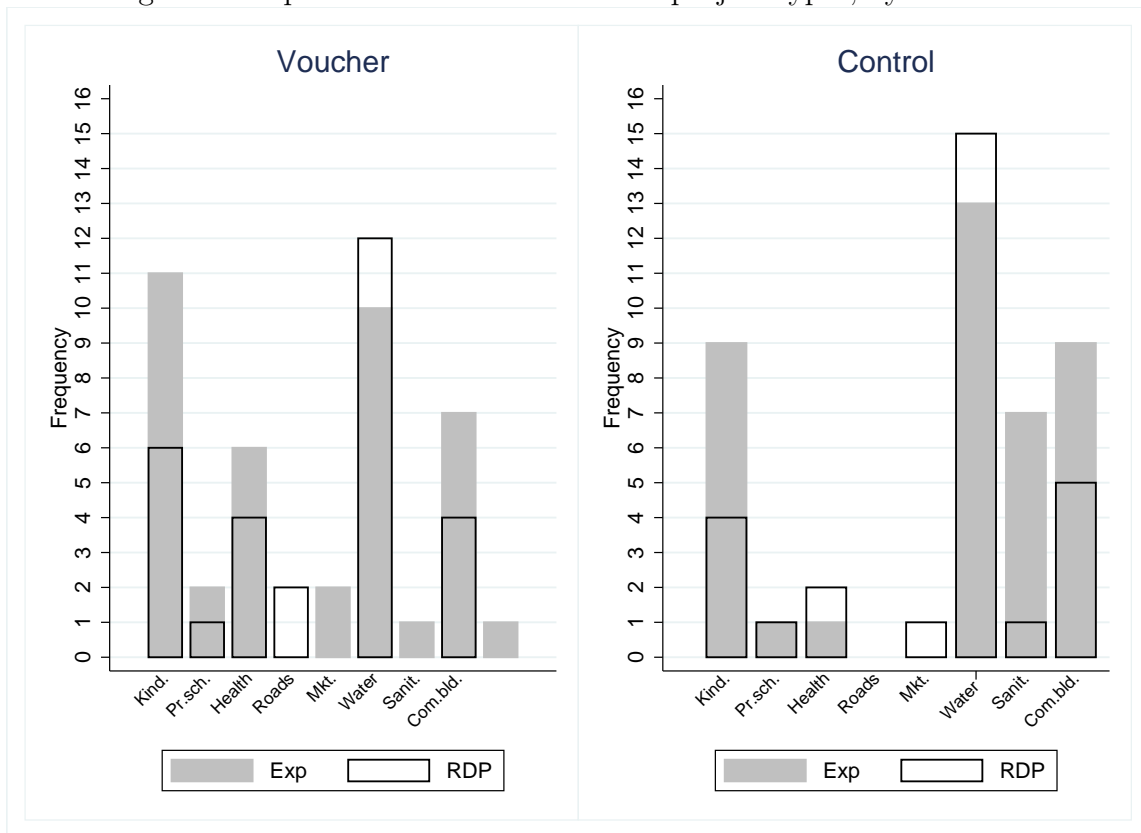


Figure 5: Experiment and RDP-selected project types, by treatment



Appendix

Table A1: Participation in Previous Community Meetings

Attended and Spoke (1)	
Leader	0.263*** (0.036)
Female	-0.261*** (0.027)
Young	-0.125*** (0.028)
No assets	-0.035 (0.034)
Off farm	0.085** (0.037)
Constant	0.531*** (0.036)
N	1600
Adj. R-sq	0.141

Note: All regressions include province fixed-effects.

Standard errors clustered at the village level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Preferences over project types by sub-group

	Kindy (1)	Pr.sch. (2)	Health (3)	Roads (4)	Mkt. (5)	Water (6)	Sanit. (7)	Comm. (8)	Church (9)
All	0.256	0.518	0.018	0.384	0.115	1.000	0.354	0.483	0.541
Leader	1.000	0.333	0.356	1.000	0.631	0.075	0.024	0.507	0.567
Female	0.297	0.844	0.085	0.649	0.041	0.879	0.353	0.851	0.154
Young	0.361	0.296	0.086	0.041	0.891	0.564	0.958	0.978	0.401
No asset	0.689	0.181	0.135	0.604	0.190	0.955	0.858	0.218	0.983
Off farm	0.401	0.465	0.600	0.888	0.217	0.685	0.996	0.470	0.276
Marg. (a)	0.241	0.987	0.027	0.174	0.542	0.993	0.527	0.439	0.973

Note: p-values from regressions of outcome on treatment and province fixed-effects. Standard errors clustered at the village level. (a) didn't speak or attend prior community meetings.

Table A3: Heterogeneous Treatment Effect on Selection Process

	Speaker (1)	No. Interv. (2)	Index (3)
Treatment	-0.00964 (0.0473)	0.182 (0.149)	0.0976 (0.112)
Marginalised	-0.287*** (0.0436)	-0.526*** (0.0903)	-0.556*** (0.0859)
Treat x Marg.	0.124** (0.0533)	0.132 (0.121)	0.191* (0.107)
	[0.097]	[0.871]	[0.525]
Constant	0.673*** (0.0407)	1.200*** (0.111)	0.472*** (0.0887)
N	1600	1548	1548
Adj. R-sq	0.201	0.244	0.242

Note: All regressions include individual controls (leader, female, young, no assets, off-farm income), their interaction with treatment, unbalanced baseline preferences over projects and province fixed-effects. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Satisfaction and Fairness as Outcomes of Project Preferences Match

	Satisfied	Fair process	Index
	(1)	(2)	(3)
Treatment	0.0663*** (0.0243)	0.0579** (0.0268)	0.220** (0.0847)
Match	0.0712* (0.0409)	0.0492 (0.0419)	0.214 (0.142)
Treatment x Match	-0.0292 (0.0462)	-0.0121 (0.0499)	-0.0739 (0.163)
Constant	0.934*** (0.0287)	0.922*** (0.0326)	0.128 (0.104)
N	1600	1600	1600
adj. R-sq	0.059	0.054	0.067

Note: All regressions include individual controls (leader, female, young, no assets, off-farm income), unbalanced baseline preferences over projects and province fixed-effects. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$