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

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Abstract

External financing of local public goods can potentially create 'political resource curses' by reducing citizen oversight, exacerbating elite capture, and producing policy outcomes that are sub-optimal for the general population. This paper experimentally tests a novel modality that seeks to mitigate elite capture of local development projects. Whereas control communities are provided with block grants to fund local public goods, households in treatment communities are provided with vouchers that they may either contribute to a public good or redeem at a discount for a private capital good. We find that the use of vouchers as a mechanism for aid delivery increases community participation in local public decision-making, changes the nature of allocation outcomes, and improves community satisfaction with allocation outcomes.

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. Where revenues are derived predominantly from resource rents instead of direct taxation, citizens are less likely to exert demands on their leaders, which in turn adversely affects the quality and quantity of public goods (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 conflict-induced imperatives for revenue generation in medieval Europe resulted in the imposition of direct taxation and, with it, the formation of bargains between elites and citizens that ultimately improved the quality of governance. Herbst (2000) analogously argues that low levels of direct taxation and the corresponding absence of citizen-elite bargains underscores poor governance in many African states.

As a number of economists have argued, 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)). In response to this problem, a number of development practitioners and researchers have developed modalities that seek to reduce the adverse effects that external financing of development projects may have on the accountability of local and nationals leaders. Among such modalities are community-driven development (hereafter, CDD) programs and social investment funds, which both involve local communities in the selection and management of projects. In addition to enabling the incorporation of local information on the marginal value of different public investments (Alatas et al. (2012)), the emphasis placed by such decentralization initiatives on local participation seeks to encourage community members to monitor the performance of political authorities in delivering the public goods funded by these programs (Fung and Wright (2003)).

Evidence indicates, however, that local public decision-making is often subject to capture by local elites. As such, decentralization initiatives may merely localize the political resource curse. Various studies, for instance, note that participants in local public decision-making are generally wealthier, more educated, hold higher

[1]See "Book XIII. Of the Relation Which the Levying of Taxes and the Greatness of the Public Revenues Bear to Liberty".

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, the outcomes of local public decision-making often align with the preferences of local elites (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) find that villagers perceive that they are worse off in cases where elites have more influence over project selection. In a related experimental study, Beath, Christia and Enikolopov (2013) find that a CDD program adversely affected the quality of decision-making by local leaders, a result that Brick (2008) suggests is caused by the effect of external financing on the accountability relationships between local leaders and the community.[2]

To date, innovations to reduce elite capture of externally-financed local public goods 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 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

[2] A 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))

[3] This 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)).

were generally ineffective in increasing community involvement or in improving the quality of services.

In this paper, we present a novel mechanism to reduce elite capture of local public decision-making by channelling 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 proposed local development project or which they may redeem 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, this 'voucher-based' modality seeks to encourage the formation of a 'fiscal social contract' between elites and villagers. As such, it is envisaged that the modality will increase the incentive for local authorities to propose and/or support publicly-beneficial projects and will increase villager participation in project selection and monitoring, thereby resulting in higher quality projects.

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 authority over local public decision-making. The field experiment was centred around structured community activities, or SCAs, as in (Casey, Glennerster and Miguel, 2012). In each village, 20 randomly-selected adults were provided with 10 notes, which could be redeemed for either 10 Solomon Islander dollars (SBD 10, approximatively USD 1.40) each 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, approximately USD 300) 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 allocation outcomes, although the effects on project implementation outcomes and general welfare are ambiguous. Compared to control villages, the voucher-based modality increases the duration of discussions about project selection among participants and the average number of times community members speak during such discussions. The voucher treatment changes the types of project that are selected by the group and, specifically, increases the probability of health-related interventions receiving funding. Villagers also perceive project outcomes to be fairer under the voucher scheme. The treatment effects on participation in the discussion and fairness perceptions are stronger for individuals who had not previously taken part in community decision-making. As expected, however, the voucher scheme reduces the volume of funding available to villages, with treatment villages receiving just 79 percent of funds available (58% of the total available in public good contributions, 21% 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 control and treatment communities in the speed with which they obtain materials and implement selected projects.

The paper is divided into seven sections: Section 2 describes the setting, experimental design and implementation; Section 3 outlines our hypotheses; Section 4 describes the sources of data and provides summary statistics for the sample; Section 5 presents the results of the experiment; Section 6 discusses the results; and Section 7 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.

As in other cases, our sample was limited by budgetary constraints, and cross-village treatment effects are thus limited in available precision for some outcomes. We discuss the minimum detectable effects implied by our estimates with each of our primary cross-village results.

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 it takes an average of two-and-a-half days to reach the capital. The vast majority of villagers (82%) rely on subsistence fishing and horticulture. Most villages do not have access to electricity, running water or sanitation. Four out of every five households use rainwater catchments for drinking water, only have access to solar lamps for lighting, and lack access to improved sanitation. In this context, the financing provided by RDP offers a vital opportunity to upgrade 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 villagers 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 villages, 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 the participants in the experiment.[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 had been allocated to fund the improvement of a local non-religious public facility, such as a

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

[5] It 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.

school, health center, market, toilet, road, or water system (i.e., a well or irrigation system).[6] Facilitators directed participants to discuss the type of project that they believed would most benefit the community, with the goal of reaching a consensus on which project to fund. In order to ensure the norms of discussion and decision-making adhered to those of the community, 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 passively 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.[7]

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.[8] 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.[9] 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.

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

[7] All activities were conducted in spaces protected from outsiders' intrusions, such as local schools or public buildings.

[8] Randomization was stratified within provinces.

[9] Thus, if a participant redeemed all vouchers for cash, they would receive SBD 50 (approximately USD 7.50), roughly equal to 5 percent of the average monthly income.

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 control villages, leaders preferring a particular project need only to convince a plurality of participants that the projects expected benefits exceed those of other potential projects. In treatment villages, however, participants may decide to withhold part or all of the funding for the respective project 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 funding for their preferred project must ensure the involvement of all participants in the decision-making process and 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. In this context, the use of vouchers 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, vouchers also should increase the extent to which non-leader participants announce their preferences over the menu of projects. In the event that such preferences generally differ from those of leaders and where uncertainty over other participants preferences otherwise exists, the discussion would thereby facilitate the aggregation of such preferences and 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 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, vouchers should increase 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:[10]

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 improve 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.

[10]These 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 5.

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:[11]

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 system, sanitation, market, road / bridge / wharf, or another non-church community building.[12]

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, perception of the decision process and outcome of the SCA, 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, a proxy of altruism.[13]

4. Information on community characteristics was collected by facilitators from a sample of key informants, such as village elders and other local leaders.

[11] Voucher contributions are also observed for participants assigned to treatment villages.

[12] An 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.

[13] This question is shown to correlate strongly with choices in the dictator game, a behavioral game commonly used to capture altruism (Falk et al., 2013). This question was asked after the discussion in order to avoid priming subjects to act cooperatively, as evidence shows how focusing individual attention on social norms affects behavior in subsequent experimental tasks (Krupka and Weber, 2009). Although this raises the possibility that the altruism measure is influenced by the treatment, we do not find evidence of a treatment effect on it (see Table 1).

5. 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.[14]

4.2 Descriptive Statistics

4.2.1 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 balance between the treatment and control groups across each variable.[15]

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.[16] 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.[17]. 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

[14] Data was not collected from 25 communities due to inaccessibility.

[15] P-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.

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

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

Table 1: Descriptive Statistics

	Control		Treatment		Total		P-value
	Mean	**Sd**	**Mean**	**Sd**	**Mean**	**Sd**	
Panel A: Participant Characteristics							
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
Panel B: Primary Project Preference							
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.

and sanitation projects.[18] 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 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).

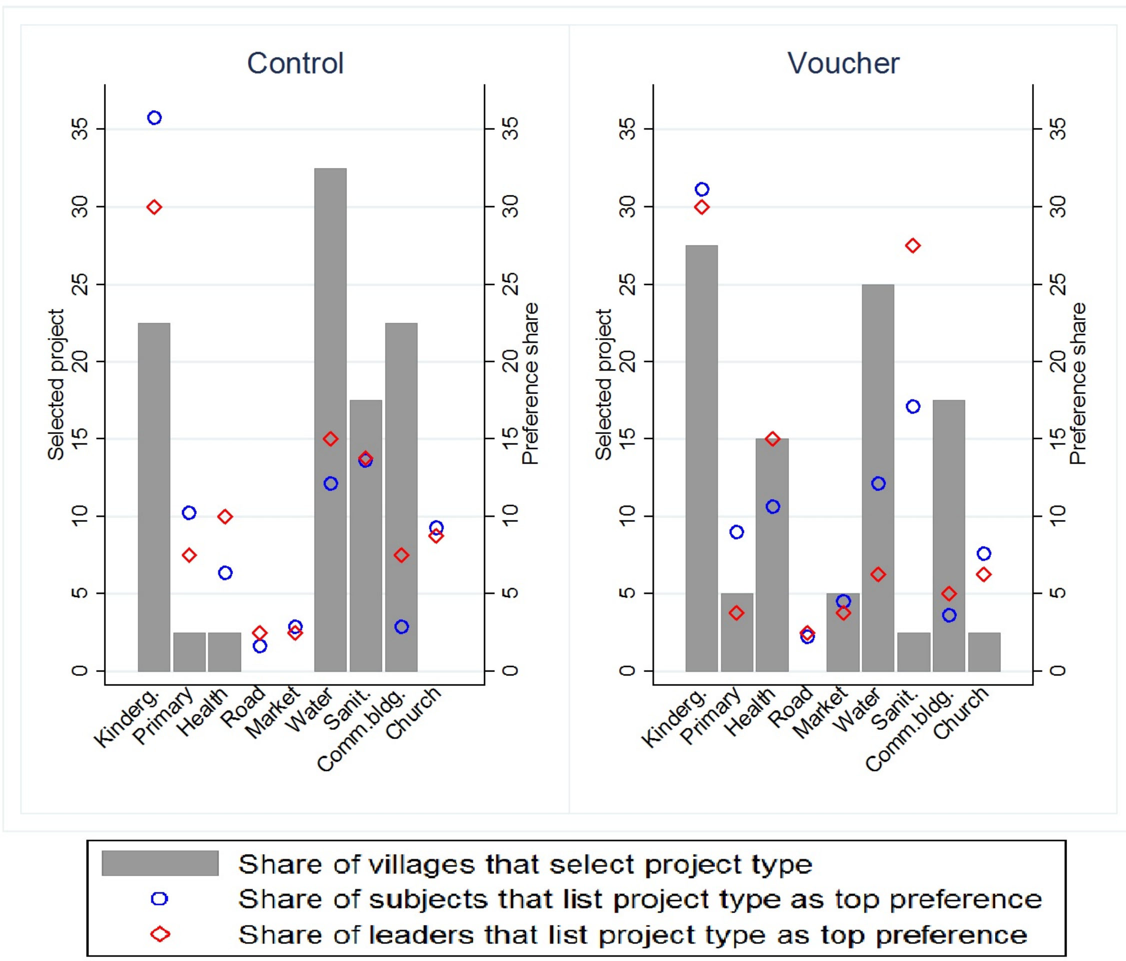
Among other sub-groups, women have stronger preferences for kindergarten and lower preferences for sanitation projects than men. Sanitation is also less popular among under-30, poor and marginalised participants. Young respondents are also more likely to rank primary education projects at the top.[19] These results are reassuring, as they show how preferences align with the types of public goods that individuals are likely to need most (Chattopadhyay and Duflo, 2004).

4.2.2 Selection Process

Discussions among participants on the selection 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. These differences are

[18] Table A2 reports balance tests for preferences over projects by other relevant sub-groups.
[19] Results available upon request.

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



statistically significant at the 5 and 10 percent levels for duration of the discussion and number of interventions, respectively (Table 2).

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

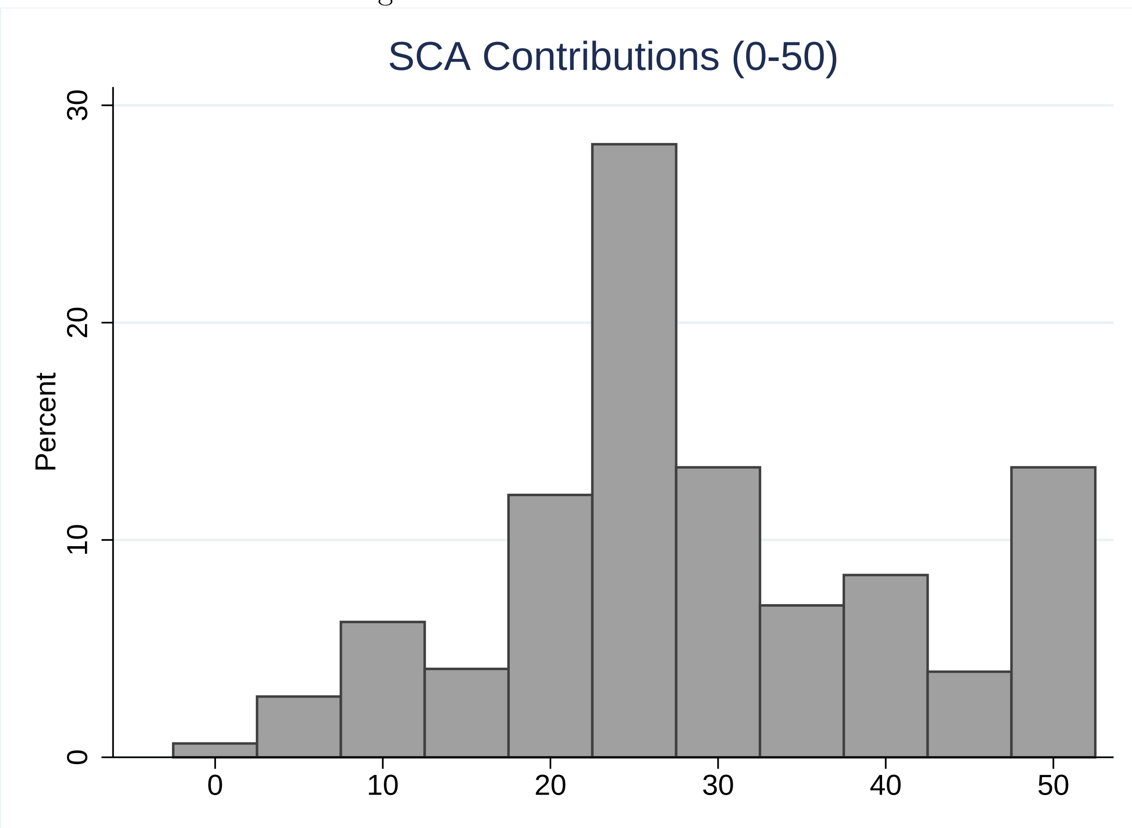
Note: p-values from regressions of outcome on treatment and province fixed-effects. "Speaker" is equal to 1 if a subject spoke during the discussion. "Interventions" represents the number of five minute intervals during which a subject spoke. "Match All" indicates a correspondence between the selected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

4.2.3 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. However, not owning any assets is not

Figure 2: Voucher Contributions



significantly correlated with lower contributions.[20] Participants under 30 years of age contribute SBD 2 dollars less. Differences in contributions by gender 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).[21] 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, wealthy, and more altruistic individuals contribute larger absolute amounts.

Table 3: Individual Correlates of Voucher Contributions

	Voucher contribution		
	(1)	**(2)**	**(3)**
Leader	2.607**		2.309*
	(1.224)		(1.269)
Female	-0.547		-0.560
	(0.887)		(0.908)
Young	-2.068*		-2.357**
	(1.091)		(1.089)
Off-Farm Income	2.514*		2.350*
	(1.336)		(1.392)
No Assets	-0.970		-1.567
	(1.383)		(1.367)
Marginalized	-1.073		-0.967
	(1.043)		(1.028)
Altruism		0.580***	0.549**
		(0.204)	(0.205)
Constant	29.97***	24.61***	26.08***
	(2.750)	(3.167)	(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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Voucher contributions are also correlated with active participation in the discus-

[20]At the village level, the share of better off and poor participants is not significantly correlated with total contribution levels in the SCA.

[21]Other survey measures of trust and reciprocity are not significantly correlated with contribution levels.

sion and perceptions of fairness in process and outcomes. 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).[22]

4.2.4 Project Implementation

Three months after the selection process, only a quarter of communities had been able to obtain the necessary materials from the hardware suppliers for which the credits were issued. Qualitative evidence garnered by enumerators indicated that most of the communities that had not obtained the materials had been constrained from doing so by remoteness and the infrequency of transport services.

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. For each set of results, we both estimate the treatment effect and examine heterogeneity in treatment effects.

5.1 Effect on Selection Process

5.1.1 Treatment Effect

In order to estimate the effect of the treatment on the selection process, we fit the following participant-level equation:

$$y_{iv} = \beta_1 Treat_{iv} + \beta_2 Province_v + u_{iv}$$

where y_{iv} is the probability of participant i in community v speaking (Column 1), the number of of five minute intervals during which participant i in community v

[22] In Section 6 we discuss the relationship between contributions, baseline preferences and selection outcomes.

spoke (Column 3),[23] and a summary index of these two outcome variables (Column 5).[24] $Treat$ is a binary variable indicating whether community v was assigned to the treatment or control group; and $Province$ represents a matrix of binary variables for each province.

Columns 1, 3 and 5 of Table 4 report the results. Column 5 indicates that, per the aggregate outcome measure, the treatment has a positive effect on participation and that the effect is statistically significant at the 10 percent level. Vouchers thus generally increase participation in the selection process by 0.13 standard deviation units (the minimum effect detectable in our sample at the 10% level is approximately 0.12 SD units; at the 5% level it is 0.15 SD units). 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.[25]

5.1.2 Effect Heterogeneity

In order to estimate how the treatment affects different types of participants, we fit the following participant-level equation:

$$y_{iv} = \beta_1 Treat_{iv} + \beta_2 IndChar_{iv} + \beta_3 Treat_{iv} \cdot IndChar_{iv} + Province_v + u_{iv}$$

where $IndChar_{iv}$ is a vector of one the following characteristics of participant i: leader; female; young (under 30); off-farm income source; no assets. 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.[26]

Columns 2, 4, and 6 of Table 4 report regression results. While the results show that leaders and subjects with off-farm income are more likely to speak and women and young persons are less likely to do so, there is no evidence of that treatment

[23] Given that the probability of speaking in the meeting was affected by the 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 the suggestion.

[24] This addresses the multiple comparisons problem. The methodology follows Anderson (2008).

[25] The 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.

[26] This adjustment is performed in all regressions with multiple interaction terms.

Table 4: Effect on Selection Process

	Prob. Speaking		No. Interventions		Weighted Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0413	0.0461	0.162*	0.233*	0.130*	0.175*
	(0.0331)	(0.0430)	(0.0858)	(0.126)	(0.0728)	(0.0962)
Leader		0.337***		0.699***		0.710***
		(0.0471)		(0.0968)		(0.0842)
Female		-0.201***		-0.466***		-0.446***
		(0.0280)		(0.0664)		(0.0525)
Young		-0.207***		-0.383***		-0.409***
		(0.0340)		(0.0779)		(0.0701)
Off-Farm Income		0.114**		0.203**		0.220**
		(0.0519)		(0.0994)		(0.0985)
No Assets		-0.0281		-0.112		-0.0853
		(0.0398)		(0.0769)		(0.0752)
Treatment x Leader		-0.0196		0.147		0.0392
		(0.0624)		(0.147)		(0.122)
		[1.00]		[1.00]		[1.00]
Treatment x Female		0.00120		-0.136		-0.0703
		(0.0396)		(0.107)		(0.0831)
		[1.00]		[1.00]		[1.00]
Treatment x Young		0.0160		-0.0837		-0.0333
		(0.0531)		(0.105)		(0.102)
		[1.00]		[1.00]		[1.00]
Treatment x Off-Farm		-0.0308		-0.0743		-0.0699
		(0.0740)		(0.177)		(0.157)
		[1.00]		[1.00]		[1.00]
Treatment x No Assets		-0.0320		0.0478		-0.0109
		(0.0571)		(0.140)		(0.123)
		[1.00]		[1.00]		[1.00]
Constant	0.419***	0.538***	0.684***	0.956***	-0.0467	0.214***
	(0.0334)	(0.0361)	(0.0896)	(0.101)	(0.0757)	(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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

changes the nature of participation by any of the respective sub-groups.[27]

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 their involvement 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 higher level of statistical significance (two-sided t-test p = .001 and p = .0001 for the probability of speaking and number of interventions, respectively).[28]

5.2 Effects on Selection Outcomes

5.2.1 Treatment Effect

Figure 1 compares selection outcomes in the treatment and control villages with the *ex-ante* primary preferences of participant villagers and participant leaders. 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 identify the overall effect of the treatment on the pattern of selection outcomes, we follow Chattopadhyay and Dulfo (2004) in estimating a series of community-level seemingly unrelated regressions (SUR). As there is an imbalance between treatment and control groups in baseline preferences over projects,[29] we include controls for all imbalanced baseline preferences in the regression equation:

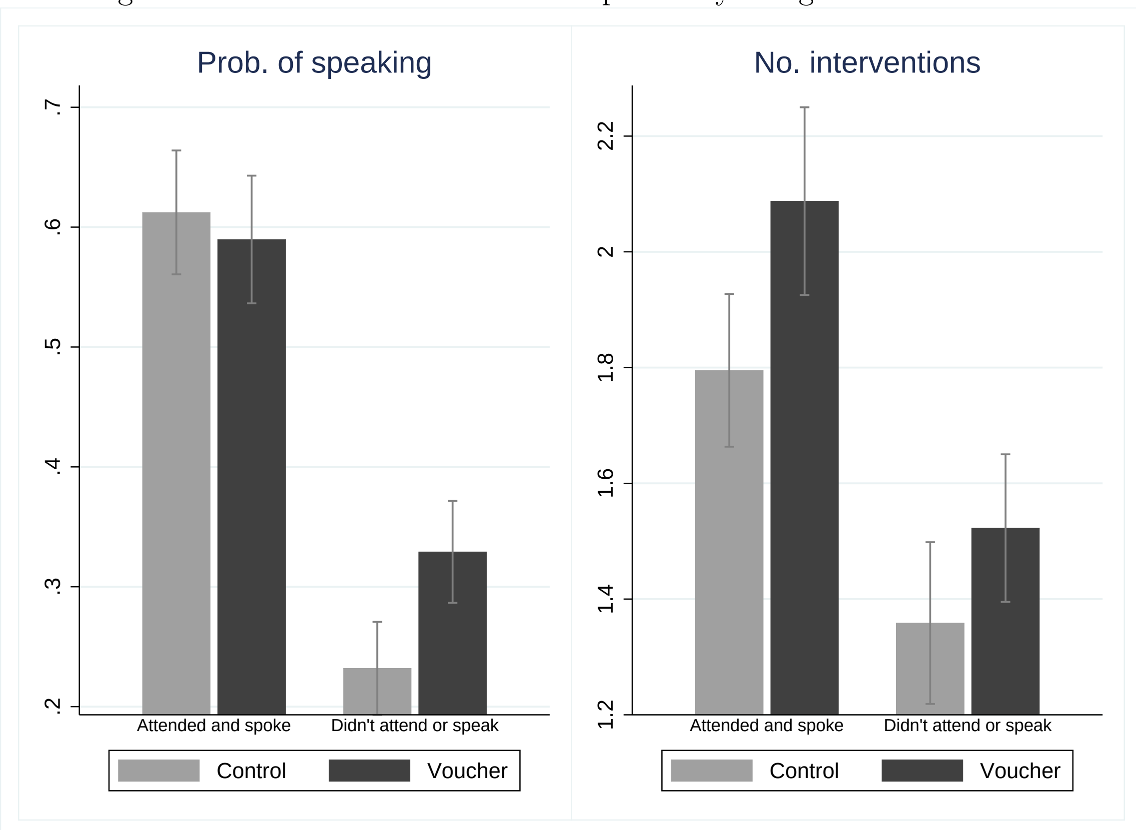
$$y_v = \beta_1 Treat_v + \beta_2 Pref_v + \beta_3 Province_v + u_{iv}$$

[27]These results are robust to controlling for imbalanced preferences over project types overall and by leaders, women, and younger participants.

[28]As shown in Table A3, the results on the probability of speaking retain statistical significance at the 10% level when adding controls for individual characteristics, their interaction with treatment and imbalanced preferences over project types. The difference in impacts on the weighted index is also large (0.19 SD) but loses statistical significance after adjusting for the multiple subgroup testing.

[29]Specifically, health projects were more 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

Figure 3: Treatment Effect on Participation by Marginalization Status



where y_v is a binary variable that assumes a value of one if the respective project type was selected in community v and $Pref_v$ is a vector of baseline preferences over project types.

Table 5 reports the results of the estimation, which confirm that the treatment increased the probability that health projects were selected and reduced the probability of sanitation projects being 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 minimum effect detectable in our sample at the 5% level varies by project type, from roughly 4.7 pp for church buildings to 19.7 pp for water systems.

Table 5: Effect of Vouchers on Project Choice

	Treatment coeff. (s.e.)
Dependent variable: selected project is	
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)
N	80

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

In order to determine whether the treatment resulted in the selection of projects that were more aligned with the preferences of leaders, general participants, or those who were *ex-ante* marginalized, we regress, on the treatment, a binary variable that indicates whether the selected project corresponded with the modal priority preferences of the respective group. In the event of a tie in preferences, correspondence is checked with either of the respective preferences: for instance, in the case of leaders, a project is defined as matching leaders' preferences if it was either the male or the female leader's top-ranked project type.[30] Regressions of this variable are run at the community level on a treatment indicator, province fixed effects and controls for imbalanced preferences over project types overall and by leaders and marginalized individuals.

Table 6 reports the results of the estimation, which indicates that the treatment has no statistically significant effect on the probability of the selected project corresponding with the preferences of general participants, leaders, or participants who were *ex-ante* marginalized, or on a weighted index of all three.[31] The minimum effect on the weighted index detectable at the 5% level in our sample is 0.47 SD units. This constrains what we can conclude from these cross-village average impact estimates on the correspondence of preferences and project choices. However, we do find that within sets of villages with varying levels of initial agreement and leader altruism, more precise impact estimates are possible, as discussed below.

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. To isolate the effect of the treatment on 'interest-driven' elite capture (as opposed to more benign forms of 'information-driven' elite capture), the treatment is interacted with measures of the altruism of participating leaders:[32]

[30]Out of 31 instances of selected projects matching leaders' preferences, 7 correspond to cases in which male and female leaders preferences are aligned. Of the remaining instances, male leaders' preferences are matched by project choice in 16 cases, and female leaders' in 8 cases.

[31]No statistically significant effects of treatment are observed if we examine agreement with male and female leaders separately. Results available upon request.

[32]The 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.

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

	Correspondence btw. project and preferences			**Index**
	All	**Leader**	**Marg**	
	(1)	**(2)**	**(3)**	**(4)**
Treatment	0.0805	0.0594	0.130	0.0745
	(0.103)	(0.109)	(0.106)	(0.236)
Constant	0.00202	0.117	0.0565	-0.0962
	(0.125)	(0.132)	(0.129)	(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 imbalanced baseline preferences over project types. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

$$y_v = \beta_1 Treat_v + \beta_2 AltrLead_v + \beta_3 Treat_v \cdot AltrLead_v + \beta_4 Pref_v + \beta_5 Province_v + u_v$$

where y_v represents correspondence between the selected project and the preferences of the respective group and $AltrLead_v$ represents the willingness of the group's leaders to share resources with others when nothing is expected in return (see Section 4.1).

The corresponding estimates are reported in Column 1 of Table 7 and indicate that the treatment differentially improves the correspondence between participants' preferences and selection outcomes where the leader is less altruistic.[33] Columns 3, 5 and 7 estimate the interaction effects on the correspondence between the selected project and leader preferences, marginalized villager preferences, and a weighted index of the three outcome variables, respectively. The coefficients are statistically insignificant at conventional levels.

In order to identify how the effects of the treatment are conditioned by the correspondence of preferences of leaders and marginalized participants, the treatment is interacted with a binary measure indicating disagreement between the preferences

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

Table 7: Heterogeneity of Treatment Effects on Project Selection

	Correspondence btw project and preferences						**Index**	
	All		**Leader**		**Marg.**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Treat.	0.850	-0.251	0.126	0.0848	0.588	-0.0833	2.132	-0.423
	(0.631)	(0.202)	(0.702)	(0.222)	(0.676)	(0.213)	(1.489)	(0.474)
Altruistic leader	0.0463		-0.0149		0.0379		0.157	
	(0.0445)		(0.0487)		(0.0470)		(0.106)	
Treat x Altruistic	-0.122*		-0.0110		-0.0725		-0.265	
	(0.0724)		(0.0802)		(0.0777)		(0.172)	
Disagreement		-0.116		-0.150		-0.193		-0.325
		(0.140)		(0.165)		(0.148)		(0.330)
Treat x Disagreement		0.263		-0.0120		0.201		0.914*
		(0.208)		(0.232)		(0.219)		(0.486)
Constant	-0.311	0.140	0.276	0.177	-0.198	0.196	-1.547	0.0149
	(0.426)	(0.147)	(0.458)	(0.154)	(0.445)	(0.156)	(1.017)	(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 imbalanced preferences over project types. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

of these two groups.[34] The even Columns of Table 7 indicate that, while neither the treatment nor the interaction have a statistically significant effect on the correspondence between selection outcomes and group preferences when entered individually, the interacted treatment effect is statistically significant at the 10% level when correspondences with the different populations' preferences are combined into 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.[35]

As the treatment may be conditioned by social capital, we also explore interactions between the treatment and community characteristics that may affect cooperation between villagers, including the size of the community, number of tribal factions, nature of local economic activity,[36] distance to the provincial center, and average level of altruism. Accordingly, we estimate the following equation:

$$y_v = \beta_1 Treat_v + \beta_2 VillChar_v + \beta_3 Treat_v \cdot VillChar_v + \beta_4 Pref_v + \beta_5 Province_v + u_v$$

where $VillChar_v$ represents the respective characteristic of community v.

Columns 1 and 6 of Table 8 show that the treatment induces a statistically significant increase in the probability of correspondence between selection outcomes and leader preferences in large communities.[37] 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.

[34] We thus run the same specification as above, only replacing leader altruism with a binary indicator for divergent preferences.

[35] The minimum difference in effects across these preference disagreement types that is detectable in our data at the 5% confidence level is 0.97. Our estimated difference is 0.91.

[36] Off-farm income is positively and significantly correlated with voucher contributions and positively correlated with asset ownership.

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

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	-0.056	0.035	0.287*	-0.024	-1.027
	(0.117)	(0.197)	(0.172)	(0.169)	(1.035)	(1.064)
Large	-0.395**					-0.449*
	(0.186)					(0.226)
Treat x Large	0.578**					0.944***
	(0.256)					(0.325)
						[0.029]
No. Tribal Groups		-0.011				0.002
		(0.025)				(0.025)
Treatment x Tribal Groups		0.026				-0.04
		(0.034)				(0.038)
						[0.213]
Off-Farm			-0.547			0.733
			(0.970)			(0.986)
Treatment x Off-Farm			0.262			-1.479
			(1.193)			(1.274)
						[0.213]
Remote				0.201		0.379**
				(0.148)		(0.161)
Treatment x Remote				-0.328		-0.492**
				(0.216)		(0.232)
						[0.084]
Altruism					-0.034	-0.211**
					(0.099)	(0.104)
Treatment x altruism					0.01	0.201
					(0.124)	(0.132)
						[0.157]
Constant	0.125	0.091	0.176	-0.109	0.380	1.469*
	(0.137)	(0.167)	(0.172)	(0.166)	(0.813)	(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 imbalanced baseline preferences over project types. Robust standard errors in parentheses, FDR-adjusted q-values in brackets.

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

5.3 Effects on Perceptions of and Satisfaction with Process

5.3.1 Treatment Effect

In order to estimate the effect of the treatment on participants' perceptions of and satisfaction with the selection process, we estimate the following participant-level equation:

$$y_{iv} = \beta_1 Treat_v + \beta_2 Leader_i + \beta_3 Marg_i + \beta_4 Female_i + \beta_5 Young_i + \beta_6 No.Asset_i \\ + \beta_7 OffFarm_i + \beta_8 Province_v + u_{iv}$$

where y_{iv} represents a binary measure of whether participant i in community v perceived the selection process was fair or a measure of whether said participant was satisfied with the selection outcome;[38] $Leader_i$ is a binary variable that assumes a value of one if participant i is a leader; $Marg_i$ is a binary variable that assumes a value of one if participant i is marginalized; $Female_i$ is a binary variable that assumes a value of one if participant i is female; $Young_i$ is a binary variable that assumes a value of one if participant i is under 30; $No.Asset_i$ is a binary variable that assumes a value of one if participant i has no assets; and $OffFarm_i$ is a binary variable that assumes a value of one if participant i has off-farm income.

Table 9 reports the respective estimates. Perceived fairness (86% in control villages) and satisfaction (87% in control villages) are generally high (see Table 2). The treatment increases perceived fairness by 5.5 percentage points and satisfaction by 6.0 percentage points, with these effects significant at the 5 percent and 1 percent level, respectively.[39] The effect on the weighted index is estimated to be 0.20 SD units, a substantively large effect (the minimum detectable effect in our sample is 0.14 SD units). The results also indicate that leaders are more likely to perceive the process as fair, while marginalized individuals are less likely to perceive the process

[38] Data 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?"*.

[39] While we cannot completely discount the possibility that these effects are driven by the direct effect of granting vouchers to participants rather than through the indirect effect of improving the quality of the selection process *per se*, there exists a positive and statistically significant correlation between participation in the discussion and both perceptions and satisfaction. In addition, participants that spoke during the discussion made higher voucher contributions, on average. 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 vouchers.

as fair and are less likely to be satisfied with the outcome.

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**	-0.0138	0.0601***	0.0122	0.204***	-0.00179
	(0.0227)	(0.0292)	(0.0202)	(0.0235)	(0.0706)	(0.0833)
Leader	0.0582***	0.0357	0.0191	0.0224	0.135*	0.102
	(0.0215)	(0.0323)	(0.0217)	(0.0305)	(0.0715)	(0.108)
Marg.	-0.0486**	-0.107***	-0.0624***	-0.106***	-0.197***	-0.377***
	(0.0228)	(0.0289)	(0.0204)	(0.0272)	(0.0707)	(0.0874)
Treat x Leader		0.0466		-0.00571		0.0701
		(0.0420)		(0.0425)		(0.141)
		[0.157]		[0.808]		[0.452]
Treat x Marg.		0.117***		0.0884***		0.362***
		(0.0386)		(0.0328)		(0.115)
		[0.007]		[0.018]		[0.005]
Female	-0.0393*	-0.0384*	-0.0355	-0.0347	-0.132*	-0.129*
	(0.0218)	(0.0220)	(0.0217)	(0.0219)	(0.0739)	(0.0746)
Young	0.0742***	0.0726***	0.0633***	0.0616***	0.243***	0.237***
	(0.0185)	(0.0178)	(0.0190)	(0.0185)	(0.0609)	(0.0587)
No asset	-0.0103	-0.00727	-0.0103	-0.00753	-0.0364	-0.0262
	(0.0262)	(0.0259)	(0.0256)	(0.0254)	(0.0859)	(0.0850)
Off farm	0.00327	0.000620	0.0126	0.0107	0.0285	0.0206
	(0.0240)	(0.0240)	(0.0197)	(0.0200)	(0.0615)	(0.0619)
Constant	0.935***	0.971***	0.954***	0.979***	0.186**	0.293***
	(0.0274)	(0.0275)	(0.0243)	(0.0228)	(0.0873)	(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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.3.2 Effect Heterogeneity

In addition to the overall treatment effect, we are also interested in whether the treatment affects the perceptions and satisfaction of leaders and marginalized participants differently than other types of participants. Accordingly, we estimate the following participant-level equation:

$$
\begin{aligned}
y_{iv} = \beta_1 Treat_v + \beta_2 Leader_i + \beta_3 Marg_i + \beta_4 Treat_v \cdot Leader_i \\
+ \beta_5 Treat_v \cdot Marg_i + \beta_6 Female_i + \beta_7 Young_i + \beta_8 No.Asset_i \\
+ \beta_9 OffFarm_i + \beta_{10} Province_v + u_{iv}
\end{aligned}
$$

The results are reported in Columns 2 and 4 of Table 9. While the treatment does not significantly affect leaders perceptions or satisfaction with the process, it significantly increases both outcomes for marginalized individuals. All these findings hold for the weighted index of these outcomes (Columns 5 and 6).[40]

5.4 Effects on Project Implementation

5.4.1 Treatment Effect

Notwithstanding the constrained sample and the lack of variation in the outcome indicator relating to project implementation (Section 4.2.4), we estimate the effect of the treatment on the project implementation via the following community-level regression:

$$y_v = \beta_1 Treat_v + \beta_2 Province_v + u_v$$

Column 1 in Table 10 shows how the available data indicates that the treatment had no effect on the probability of materials being picked-up in the three months following the meeting.

5.4.2 Effect Heterogeneity

We also examine effect heterogeneity by interacting the treatment with a binary variable indicating whether the community lies farther than the median travel time from the respective provincial center. The following community-level equation is fitted:

$$y_v = \beta_1 Treat_v + \beta_2 Far_v + \beta_3 Treat_v \cdot Far_v + \beta_5 Province_v + u_v$$

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

Table 10: Effect on implementation

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

Note: All regressions include province fixed-effects.
Robust standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

As with the basic regression, we find no evidence to indicate that the treatment affected the probability of the participants retrieving materials to complete the project, regardless of whether the community was closer to or farther away from the respective provincial center.

6 Discussion

While vouchers significantly change the pattern of projects that are selected, the modality does not reduce the probability of correspondence between the selected project and the *ex-ante* preferences of leaders and does not increase the probability of correspondence between the selected project and the *ex-ante* preferences of marginalized and/or non-leader participants. *Prima facie*, such results suggest that vouchers have no impact on elite capture. However, vouchers do increase the duration of discussions and the number of participants that contribute to discussions and it is feasible that, with these improvements in the quality of discussions, participants may update their preferences. Accordingly, the correspondence between the selected project and participants' *ex-ante* preferences may not be informative as a true measure of elite capture. In order to better understand whether the change in the pattern of selected projects that is induced by vouchers is likely to be beneficial to non-leader participants, we further examine the absolute and relative characteristics of projects selected in treatment communities.

First, we examine the relationship between selected and *ex-ante* preferred projects and voucher contributions in treatment communities (Figure 4). If participants' preferences are not significantly affected by the discussion, we would expect participants to contribute more where the selected project corresponds with their *ex-ante* preferred project. However, when we compare vouchers contributions and baseline project preferences, we observe little correlation between the two distributions. This visual impression is confirmed when we regress participant's voucher contribution on a binary variable that denotes the correspondence between selected projects and the same participant's and top-ranked project:[41] we find that participants do not contribute significantly more when their *ex-ante* preferred project is selected.[42] Accordingly, there is indicative evidence that discussions alter participants' preferences.

Second, we examine whether the treatment led to the selection of projects that are in greater need of funding, as proxied by survey data on community members perceptions of improvements in the quality of different public facilities over the previous five years. Specifically, we run village-level SURs of the probability 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. However, we find no statistically significant effect of the treatment on the likelihood that previously un-improved projects were selected ($p = .239$).

Finally, we further examine whether the treatment led to the selection of projects which complemented on-going projects funded by RDP. The assumption is 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 SURs of the likelihood that a specific project type is selected on the interaction between the treatment and an indicator of RDP-selected project type, controlling for baseline project preferences and province fixed-effects. Again, we do not find any statistically significant effect of the treatment on the likelihood that the experiment and RDP-selected project types match ($p = .210$). However, there exists a correlation between the distribution of

[41] The regression specification also controls for individual characteristics, imbalanced baseline preferences over project types, and province fixed-effects.

[42] The 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.

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

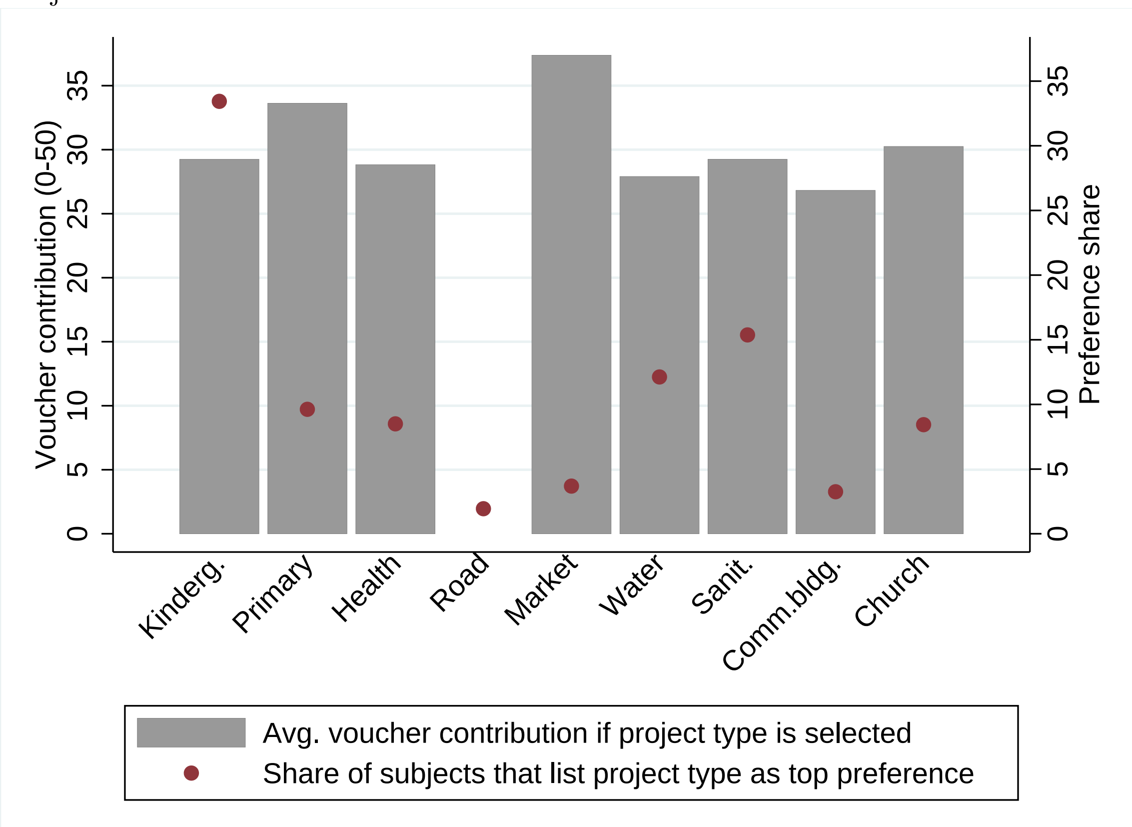
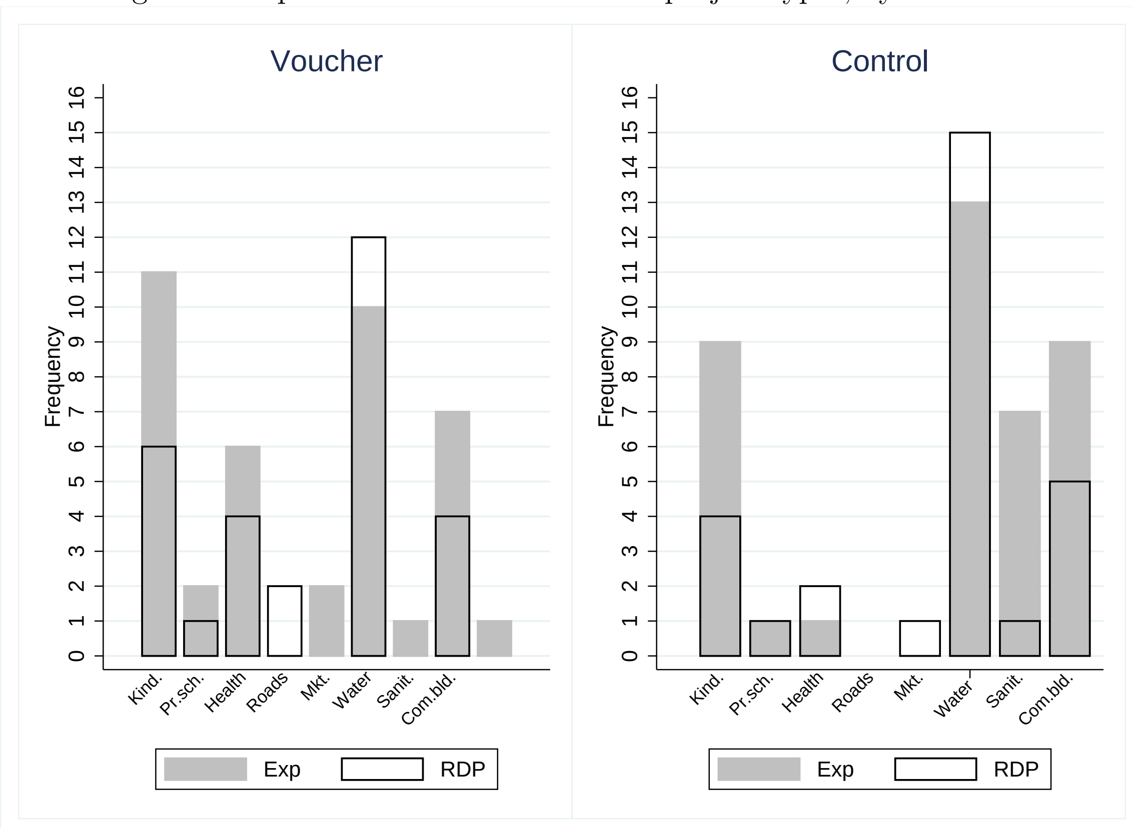


Figure 5: Experiment and RDP-selected project types, by treatment



projects chosen during the experiment and RDP projects. Specifically, a chi-square test cannot reject the hypothesis that projects selected by the experiment and RDP are drawn from the same distribution in treated villages (chi2 = 91.8; p = .000), but does rejects the hypothesis for control communities (chi2 = 34.981; p = .770).[43] Figure 5 shows the frequency of projects selected by the experiment and by RDP in treated and control villages and confirms the closer correspondence between the two variables in the former set of villages.

Overall, these results suggest that the increase in the quality of discussion induced by the voucher-based modality changes participant preferences and promotes a more rational allocation of resources to improve existing public goods.

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

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 limited participation among community members and by the propensity of local elites to exercise undue influence over project selection. 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.

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 funding was provided through standard block grants.

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, and 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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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	0.182	0.0976
	(0.0473)	(0.149)	(0.112)
Marginalised	-0.287***	-0.526***	-0.556***
	(0.0436)	(0.0903)	(0.0859)
Treat x Marg.	0.124**	0.132	0.191*
	(0.0533)	(0.121)	(0.107)
	[0.097]	[0.871]	[0.525]
Constant	0.673***	1.200***	0.472***
	(0.0407)	(0.111)	(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, imbalanced baseline preferences over projects and province fixed-effects. Standard errors clustered at the village level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$