The Politics of Implementation: How Public Goods Policies Influence Incumbent Voting

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Abstract

Poor public services persist in many developing democracies. One explanation suggests that politicians invest too little in public services because voters fail to reward them. Some empirical studies indeed find that incumbents gain no electoral benefits or even suffer negative effects from public goods provision. However, does this imply citizens fail to hold leaders accountable? This study employs a natural field experiment in the Philippines, where municipalities were randomly selected to participate in a public goods program, and villages applied for projects. On average, village mayors experienced a 2-3 percentage point decline in reelection support. Further analysis, however, shows these electoral losses are concentrated in villages that were mobilized but did not secure funding. An instrumental variable analysis reveals that successful applications significantly increased reelection rates by 8 percentage points. These results suggest voters do value public goods but base their electoral support on whether politicians effectively implement programs, rather than rejecting public goods provision outright.

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1 Introduction

The under-provision of public services remains a major challenge in many developing economies. One explanation for this under-provision is that voters provide wrong incentives to politicians by not rewarding public service delivery. Indeed, despite widespread voter demand for improved services (Grossman and Slough, 2022),[1] empirical studies document null or negative effects of public service policies on incumbent electoral outcomes (De Kadt and Lieberman, 2017; Larreguy et al., 2018; Sandholtz, 2019; Goyal, 2019; Boas et al., 2021).[2] However, do negative or null effects necessarily imply that citizens fail to hold politicians accountable for service provision?

In this paper, I investigate the possibility that negative electoral effects of public-service programmes arise not because voters fail to reward public goods, but because they punish politicians for implementation failures. Politicians are often involved in the application, financing, coordination, or oversight of public goods policies—that is, their implementation. When these efforts are visible, each stage generates observable signals regarding the politician's competence, highlighting the incumbent's effort and its success or failure. If a politician successfully applies for and secures funding, voters reward the incumbent. If an application is rejected or progress stalls, that same visibility becomes a liability: voters infer incompetence and punish the incumbent. The average electoral effect of the policy is therefore a weighted sum of these opposing signals: more visible failures than successes can yield a net negative impact, even though voters do reward successful service delivery when it occurs.

Identifying the electoral consequences of public-goods provision has been challenging, as allocation is often endogenous to political support or targeted based on programmatic rules and need. In this article, I exploit a natural field experiment from the Philippines' KALAHI-CIDSS community-driven development program to overcome these challenges. Among 198 municipalities with intermediate poverty levels, 99 were randomly assigned to participate in KALAHI (treatment) and 99 served as controls.[3] Citizens democratically decided on necessary public goods and elected

[1]Appendix A provides evidence on voter preferences in the Philippines. Voters rank public service provision issues as the sixth most important problem.

[2]Importantly, this pattern holds for both national and local elections. See Boas et al. (2021) for evidence of negative effects on local incumbents.

[3]Specifically, the analysis relies on a randomized third-party experiment with probabilistic treatment assignment, known to the researcher but designed and controlled by a third party (Titiunik, 2020).

representatives to present their proposals. This policy, initiated by the president, co-funded by the central government and international donors, and implemented by government ministries, provides an ideal context for the research question for two reasons. First, participation in the policy was randomly assigned to some municipalities via a public lottery, allowing the estimation of the average causal effect of the policy on electoral outcomes. Second, local politicians played a critical role in implementation. In treated municipalities, village mayors (barangay captains) were encouraged to mobilize citizens and hold participatory assemblies. During these assemblies, citizens elected volunteer representatives, drafted project proposals, and sent representatives—often village captains—to municipal forums, where volunteers ranked and voted on projects until the budget was exhausted. Captains' visible roles in the mobilization process provided voters with a clear signal of how local leaders' efforts influenced funding outcomes.

The research design decomposes this causal chain into three identifiable effects, each addressing a distinct stage of the policy. First, because municipalities were randomly assigned to KALAHI by a public lottery, the Intention-to-Treat Effect (ITT) compares incumbent reelection rates across all villages in treated versus control municipalities, identifying the average electoral impact of merely offering the policy. However, the mobilisation and funding decisions themselves are endogenous: villages choosing to mobilise or receiving funding may differ systematically in unobserved ways. To overcome this, I exploit two embedded natural experiments. Second, I isolate the causal effect of mobilisation–defined as completing assemblies, electing volunteers, and submitting proposals–by using municipal lottery assignment as an encouragement instrument for village-level mobilisation. This instrumental variable (IV) approach identifies the Local Average Treatment Effect (LATE) of mobilisation among villages whose decision to mobilise is driven by random assignment. Third, within mobilised villages, funding allocation is competitive and based on rankings against a budget cutoff. By instrumenting the receipt of funding with an indicator for the signed distance from this cutoff, I isolate the local-average treatment effect of marginal funding on incumbent reelection. Together, these three estimates—the ITT of offering KALAHI, mobilisation LATE, and funding LATE—illustrate how the availability of the program, the effect of undergoing mobilisation, and the outcome of that mobilisation translate into changes in incumbent reelection rates.

The paper presents three sets of results. First, the ITT analysis shows that simply offering municipalities access to KALAHI-CIDSS reduces incumbent captains' reelection rates by roughly 2-3 percentage points. This effect operates almost entirely through the extensive margin: fewer captains choose to run again, while vote shares among those who do run remain unchanged. Second, using KALAHI eligibility as an instrument for mobilisation reveals that villages induced to mobilise experience a 3-4 percentage point drop in their captains' reelection probability. This decline also reflects incumbents opting out of reelection rather than losing electoral support. Third, conditional on mobilisation, winning funding reverses this penalty. Instrumental-variable estimates indicate that mobilised villages that fail to secure funding lose approximately 8 percentage points, whereas those just surpassing the municipal budget cutoff gain between 8 and 16–20 percentage points (as estimated by a fuzzy regression discontinuity design). In sum, voters reward public goods delivery only when visible mobilisation efforts result in funded projects and punish mobilised but unfunded efforts. While these results do not rule out other mechanisms, they strongly suggest that the visibility of politicians' efforts during policy implementation, as well as their success or failure, influence voters' electoral decisions.

The paper contributes to several literatures. Theoretically, it advances political accountability models in two ways. First, it extends the literature on the visibility of public-goods provision by demonstrating that citizens respond to signals during policy implementation, not just to public service outcomes. Starting from the assumption that voters reward or punish politicians based on observed performance (Ashworth, 2012), existing models predict politicians tend to over-invest in services whose outcomes are easily observable and clearly attributable to their actions (Mani and Mukand, 2007; Harding, 2015; Huet-Vaughn, 2019). This study advances this line of thought by showing that voters react not only to outcomes but also to visible efforts during implementation. If implementation steps themselves—door-to-door mobilisation, leading village assemblies, and funding decisions–are visible to voters, they reveal both how much effort the incumbent puts in and whether that effort pays off. Put differently, the implementation can function as a public competence test. When efforts are visible but the project fails, citizens infer incompetence and punish incumbents. When the same efforts succeeds voters reward incumbents. Thus accountability is triggered by the match (or mismatch) between visible effort and their success or failure, not merely by the realized outcome.

The argument is closely related to the literature on attribution. That literature suggests that differences in institutions and government cohesion–for example when a single party controls government or when politicians have complete power over policy–can shape the clarity of responsibility, which in turn affects how informative public goods outcomes are in signalling the quality of the incumbent (Powell Jr and Whitten, 1993; Tavits, 2007; Martin and Raffler, 2021). I add to the literature by clarifying the institutional conditions under which implementation signals matter for electoral outcomes. They require (i) politician involvement with discretionary effort, (ii) visibility of that effort to citizens, and (iii) clearly observable outcomes, such as funding notifications, allowing voters to assess success or failure accurately.

This paper is closely related to Cruz and Schneider (2017), who study an earlier iteration of KALAHI-CIDSS. The authors demonstrate that when voters face uncertainty about allocation—lacking clear information about who genuinely influenced project selection—municipal mayors can exploit this opacity to claim undeserved credit and boost their reelection prospects. My findings complement this insight. At the village level, I show that the logic is reversed: citizens directly observe who mobilizes community assemblies, who presents project proposals, and which projects ultimately receive funding. This visible implementation process allows voters to use politicians' efforts as indicators of competence. When villagers observe visible efforts that nevertheless fail to secure funding, they punish the incumbent; when the same visible effort successfully secures funding, they reward her.

More generally, the study adds causal evidence to a mixed empirical record on public goods and voting in low- and middle-income democracies (e.g. Harding, 2015; De Kadt and Lieberman, 2017; Larreguy et al., 2018; Goyal, 2019; Croke, 2021; Boas et al., 2021; Huet-Vaughn, 2019). Exploiting the random assignment of KALAHI to municipalities and two nested quasi-experiments, I disentangle three quantities that are often conflated: the intention-to-treat effect of being offered the programme, the local average treatment effect of taking the offer and going through mobilization, and–conditional in being mobalized–the local average treatment effect of receiving funding. The design therefore isolates the effect of the policy as a whole, the effect of mobilisation from the effects of project delivery. By contrast, most previous work relies on observational designs with single treatments that cannot separate these quantities.

2 Institutional Setting

I test the theory's central prediction using a natural experiment in the Philippines. In general, public goods provision in the Philippines is decentralized and responsibilities for implementation are shared between barangay (village), municipal, and provincial governments. While the latter provides services and infrastructure that include more than one municipality (hospitals, provincial roads), the municipality provides basic health care, education, social welfare programs, a substantive number of civil servants, public enterprises, and extension services (Atienza, 2004; Llanto et al., 2012; Sidel, 1999; Cruz and Schneider, 2017). At the lowest level of the political hierarchy are Punong Barangay (village captains), elected in officially non-partisan contests every three years with a 3 consecutive term limit. The Local Government Code (LGC) gives them a strong position in the provision of public services: the captain is the chief executive of the barangay and taked to ensure the delivery of basic services and facilities. Those services include primary health care through Barangay Health Workers, early-childhood nutrition and day-care, waste collection, village roads, communal water systems and small-scale irrigation. Public services are financed largely by a fiscal transfer from central government and, to a smaller extent, by local taxes.[4] About 20 % of fiscal transfer are reserved for the Barangay Development Fund, giving captains substantial discretion over both the scale and visibility of village services.

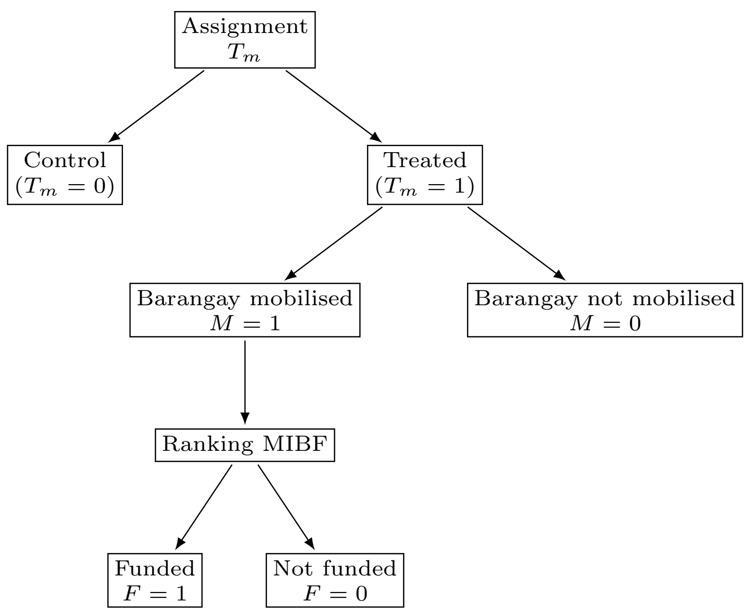
In general, politics in the Philippines tends to be clientelistic (Capuno, 2012; Hicken et al., 2018) and government resources are often allocated according to political considerations (Fafchamps and Labonne, 2016). Because of their role in providing local public services, village captains (barangay captains) are often used by municipal mayors as political brokers to distribute clientelistic goods and to foster their electoral success (Cruz et al., 2017; Ravanilla, 2017). Previous research has documented that voters are uncertain about the concrete responsibilities and influence of local politicians in relation to the allocation of public goods (Cruz et al., 2018) and often attribute municipal spending to the actions of mayors, as they are thought to have extensive influence over budget allocation (Capuno, 2012).

[4]The transfer is called Internal Revenue Allotment (IRA). The IRA is allocated 23% to provinces, 23% to cities, 34% to municipalities and 20% to barangays. It is funded by a fixed percentage of national tax revenues from three years prior, transferred directly from the national government, and constitutes about 85% of municipal government revenue. Comparatively, this transfer is rather large, and the municipal governments receive, on average, 49,905 million PHP (roughly one million US$) from IRA transfers (Troland, 2016).

2.1 The policy

KALAHI-CIDSS (KC)[5] is a community-driven development program that delivers local public goods to communities. The first phase (KC1) took place from 2003-2009. In 2011, the central government of the Philippines received a US$ 120 million grant from the United States government's Millennium Challenge Corporation Compact and a loan of US$59 million from the World Bank to continue the program (KC2). The rest of the funding came from the central government. Within each targeted province, municipalities with 70% or more poverty incidence automatically received the program while municipalities with less than 33% poverty incidence automatically did not receive the program.[6] For a sample of 198 municipalities with a poverty incidence between 33-69%, the program was randomly assigned to 99 treatment municipalities and not assigned to 99 control municipalities. These 198 municipalities constitute the population of this study.

Figure 1: KALAHI Implementation



The implementation of the KALAHI policy followed several steps. (I) Municipal mayors from eligible municipalities applied to participate. (II) Public lotteries, held from May 23 to June 30, 2011, randomly assigned treatment and control municipalities (Beatty et al., 2017).[7]

[5]Kapit Bisig Laban sa Kahirapan ("Linking Arms Against Poverty") – Comprehensive and Integrated Delivery of Social Services.

[6]For the poverty ranking, KC2 utilized the 2003 Small Area Estimates (SAE) published by the Philippine Statistics Authority (formerly National Statistical Coordination Board (NSCB)).

[7]Funding was only allocated to municipalities in provinces where guaranteed municipalities had not exhausted KC funding and if a municipal mayor or representative attended the lottery.

Municipal mayors did not influence project selection. (III) Village mayors (captains) and other village officials within selected municipalities were supposed to mobilize their communities to attend village meetings led by ministry officials.[8] A first village assembly was convened in every participating village to introduce KALAHI-CIDSS; during this meeting residents elected volunteer facilitators who would carry out a participatory situation analysis (PSA). The elected volunteers then led the PSA, guiding villagers through structured discussions to identify and rank local development issues, and produced a village action plan that named a single, top-priority sub-project for KALAHI-CIDSS funding. (IV) A second village assembly validated the PSA results and elected a Project Preparation Team (PPT) and a Barangay Representation Team (BRT) to prepare and to advocate for projects at the Municipal Inter-village Forum (MIBF). Village captains often represented the village as part of the BRT and presented project proposal at the MIBF. However, KALAHI-CIDSS required that at least 80% of a village's households attend the meetings that discuss proposed sub-projects. Not all villages complied with this requirement and therefore dropped out at this stage. (V) Mobilised villages competed for funds by presenting proposals at the MIBF. First, the ranking criteria for the municipal inter-barangay forum werre collectively decided by community volunteers, barangay leaders, LGU officials, and civil society. The criteria often included poverty focus, sustainability, relevance to community needs, and local contributions. Then each village presented its proposal, other villages questioned it, everyone scored against agreed criteria, and the consolidated scores produced a ranked list. In the process, captains persuaded other communities to vote for their projects. As one village official stated, *"It is up to us to make others understand and convince them to vote for us."* (VI) Once prioritized, a village's community account received direct funding from the Department of Social Welfare and Development (DSWD) to avoid political capture. In addition to the funding from funders and central government, municipalities and villages contributed through the Local Counterpart Contribution (LCC).[9]

Once the MIBF is concluded, funding decisions are made public–typically through press releases, Facebook posts, or website stories issued by LGUs or DSWD field offices–which local

[8] Village officials needed to go house-to-house to inform residents about the assemblies and convince them to attend.

[9] Municipal LCCs often drew from the Local Development Fund and could be in cash or in-kind (DSWD, 2015, 33-34). Contributions for Capacity Building and Implementation Support (CBIS) and subproject implementation (SPI) constituted at least 30% of total project costs.

newspapers often pick up.[10] The detailed ranking sheet is usually kept at the municipality where the MIBF was held and is accessible only to officials and community members who took part in the forum.

Up until the village election in 2013, KALAHI financed local public goods investments (called subprojects), stretching from social services (i.e., health, education, water), infrastructure (roads, bridges), facilities (community production, economic support, and common service facilities), to environmental protection and conservation. According to project data from the DSWD, 914 projects had been started and 538 completed within the study population by the time of the election.[11] On average participating municipalities received PHP 450,000 (approximately US$ 11,250) per village (Beatty et al., 2015). In an earlier evaluation of the economic impacts of KALAHI, Beatty et al. (2018) found that while specific sub-projects produced sizable welfare gains, those improvements did not translate into broad household-level growth. In particular, barangays that received KC funding experienced (i) shorter and cheaper trips to services and markets and lower crop-haulage costs after road investments, (ii) a rise in primary and secondary enrolment where school buildings were financed, and (iii) large reductions in both the time and money households spent obtaining water following water-system projects. Yet household consumption, assets and labour earnings remained flat, and rice yields fell in road-project barangays.

3 Data

The empirical section brings together four data sets: the random assignment status of municipalities obtained from the public registry, project-cycle monitoring files, village-level electoral returns, and the 2010 population census.

Random Assignment and treatment status

The starting point is the public registry compiled by Beatty et al. (2018), who were also responsible for the randomization. It lists all 198 municipalities that cleared the national poverty screen (poverty incidence between 33% and 69%) and indicates which member of each matched pair won the lottery ($T_m = 1$) and which one served as control ($T_m = 0$). The file also reproduces the four variables–

[10] For example, the local news site Boracay Island News reported six winning proposals in New Washington, Aklan, noting that they were selected through Municipal Development Council–Participatory Resource Allocation scoring, with barangay and LGU cash counterparts already pledged: https://www.boracayislandnews.com/kalahi-cidss-to-fund-six-community-projects-in-new-washington-aklan/.

[11] For details, see Appendix C.3. These data should be treated with caution, however, since not all projects listed as funded proposals appear in the project database (see the Data section for more). Consequently, the reader may interpret these figures only as rough estimates.

population, number of barangays, land area, and poverty incidence–that were used to form pairs. I will use these covariates later to increase precision.

Village Mobilization

I proxy mobilization with two binary indicators that capture the most visible activities. First, I collected data from the KALAHI-CIDSS Monitoring and Evaluation Unit on which village applied for a project until November 2013, how the project proposal was ranked in the Inter Barangay Forum as well as the date. The dataset also contains information on the total costs of the proposed project, the funding source(s), the ranking of the project, and if the project received funding, as well as the date of the decision at the MIBF. A barangay is coded as *mobilised* ($M_{im} = 1$) if it filed at least one proposal before 28 October 2013. Second, I collected data on the appointment of the Project Preparation Team (PPT) and the Barangay Representation Team (BRT). In particular, for each village and project cycle, I obtained data on whether a captain was elected to the Project Preparation Team (PPT) and/or the Village Representation Team (BRT). I created binary measures that take on the value of 1 if the village captain was elected into the BRT or PPT. In roughly 60 % of cases, a captain was active in the BRT, and in approximately 40 % of cases, a captain chaired the BRT.[12] The dataset also records the appointment date for each team. I restricted the sample to teams elected by the village election in late October 2013. Note that the two mobilization indicators don't fully coincide. Of the 2,212 projects flagged by at least one measure, 1,584 showed both rank-based and volunteer-based mobilization, 366 were identified only via the volunteer data, and 262 only via the ranking data. Some of this divergence reflects timing: volunteers are elected first, and rankings follow later, so a project may appear in the volunteer list but not yet have a rank. However, the 262 projects flagged solely by ranking suggest missing volunteer records for those cases.[13] I use the rank mobilization indicator: a village is considered mobilized if it received a ranking at an MIBF.

Subproject Ranking and Funding For data on project funding, I also rely on records from the KALAHI-CIDSS Monitoring and Evaluation Unit. To test weather funding influence voters evaluations of politicians, I collected on which village applied for which project until November 2013, how the project proposal was ranked in the Inter Barangay Forum.[14] I only include data

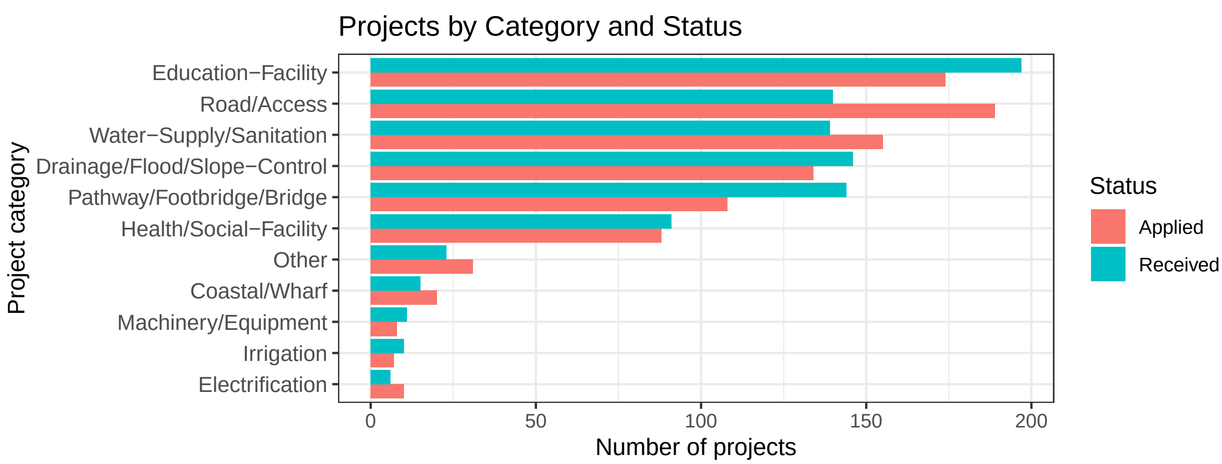
[12] For details, see Appendix C.4.

[13] For details, please refer to Appendix C.1.

[14] First, note that if two or more villages applied together for a project they receive the same rank. Second, note that there are some cases of missing ranks in the data. For details, please see Appendix C.2.

for projects that were ranked until November 2013, the data of the village elections. For every proposal ranked, I code the inverse rank r_{im} (1 = top). Given the municipality-specific budget ceiling, the last fundable rank is $\bar{r}_m$; funding is granted if $r_{im} \le \bar{r}_m$. Additionally, the dataset includes the short titles of all ranked projects. I leveraged those titles in an R script to perform bulk natural-language classification–using the OpenAI GPT-4o-mini API with a few-shot prompt–to assign each of the 1,700 project names to one of our predefined infrastructure categories (e.g., Water-Supply/Sanitation, Irrigation, Road/Access). Figure 2 presents the results.

Figure 2: Subprojects started in the 99 treatment municipalities between May 2011 and November 2013



Notes: Projects financed by Millennium Challenge Corporation (MCC) and Additional Financing (AF) from the World Bank in the 99 treatment municipalities until November 2013. Source: Department of Social Welfare and Development, Republic of the Philippines.

Electoral Outcomes

To measure the electoral performance outcomes, I collected electoral results at the village level for 99 treatment and 99 control municipalities ($N = 4850$) from the Commission on Elections (COMELEC). Local offices are elected in first-past-the-post elections every three years, with village elections typically held in October.[15] The dataset includes the names of all candidates who ran for the office of village captain, as well as the votes obtained by each. Because contests are

[15]The Philippines was severely affected by Typhoon Yolanda, which devastated the country in November 2013. However, the village elections were held in October 2013, prior to the disaster, ensuring that the results were not influenced by it.

non-partisan, I identified incumbents by an exact match on last and middle names.[16]. To measure electoral performance, I use five outcomes: a re-election indicator ($Y_{im} = 1$ if the 2010 incumbent won again in 2013), an indicator for whether the incumbent ran, the incumbent's vote share when running, and the win margin (vote share of the incumbent minus that of the runner-up), and the total number of candidates running in the 2013 election.

Covariates: Village characteristics

For all villages within treatment and control municipalities, I collected pre-treatment data on village characteristics from the 2010 Census. Variables include the total population, average education, average age, percentage catholic, urbanization, ethnicity, citizenship, and marital status. Second, I include the incumbent vote margin, win margin, and number of candidates from the previous election (2010).

4 Empirical Strategy

The paper is interested in two questions. First, it seeks to test whether the provision of public services affects the electoral outcomes of incumbent village mayors. Second, it asks to what extent these average effects are driven by success or failure during the implementation, in particular funding of projects. The ideal experiment would randomise three components. First, randomise programme eligibility across municipalities. Second, within treated municipalities, randomise whether each village is compelled to complete the full mobilisation protocol of assemblies, committee formation, and proposal writing. Third, among mobilised villages, randomise which proposals actually receive the block grant. In practice only the first is random to answer the first question. Mobilisation is voluntary and funding is allocated competitively by rank until the budget is exhausted. The empirical strategy nests two quasi-experiments inside the original KALAHI lottery. First, I compute an intention-to-treat estimate: a simple difference in means between treated and control municipalities yields the average electoral impact of merely offering the CDD programme. Second, I exploit the assignment lottery as a random encouragement for mobilisation. Instrumenting each village's decision to complete the mobilisation process running a participatory assembly, selecting project, organize counterpart contribution, selecting volunteers, applying for funding) with that encouragement, we can identify the Local Average Treatment Effect for the subset of villages whose

[16]The coding of incumbents using a combination of last and middle names was necessary because electoral documents lacked coherent spelling or notation rules. The variation in spelling and notation was most pronounced for first names, which sometimes also included candidates' nicknames

mobilisation status is changed by programme eligibility. Third, conditional on having mobilised, I take advantage of the sharp budget cut-off in the municipal ranking. Proposals lying just above and just below the final funded rank constitute a quasi-experiment, and a fuzzy regression-discontinuity design that instruments funding with an "inside-cutoff" indicator delivers the causal effect of actually receiving the grant for marginal projects. Lastly, because the IV and RD estimates apply only to their respective complier groups, I profile compliers and non-compliers on baseline characteristics to assess external validity. Taken together, the trilogy of estimates–ITT for assignment, the mobilisation LATE, and the funding LATE–maps the political payoff (or penalty) at each stage of the CDD process.

4.1 Effects of the KALAHI Policy on Electoral Outcomes (ITTs)

First, I test whether assignment to the KALAHI–CIDSS community–driven development (CDD) program affects the re-election probability of incumbent barangay captains in the village elections in October 2013.[17] I rely on a clustered matched-pair design in which 198 municipalities were first paired based on covariates (resulting in 99 pairs). In particular, municipalities within a region were matched on four variables: *poverty incidence*, *population*, *land area*, and *number of villages* within each municipality. The 198 municipalities were then paired using nearest neighbour matching within each province based on a composite measure of all four measures.[18] The program was *randomly* allocated to 99 of the 198 municipalities whose poverty incidence lay between 33% and 69%. My unit of analysis is the village within municipality ($N = 4850$). Villages are indexed by i and municipalities by m. The binary programme–assignment indicator T_m equals one in the 99 municipalities randomly allocated to KALAHI. The outcome of interest, Y_{im}, equals one if the incumbent captain is re-elected in October 2013 and zero otherwise. Accordingly, our first estimand is the intention-to-treat (ITT) effect of municipal assignment to KALAHI–CIDSS:

$$\text{ITT} = \underbrace{\mathbb{E}[Y_{im} \mid T_m = 1]}_{\text{treated}} - \underbrace{\mathbb{E}[Y_{im} \mid T_m = 0]}_{\text{control}}, \tag{4.1}$$

where Y_{im} equals 1 if the incumbent captain of village i in municipality m is re-elected and 0 otherwise, and T_m denotes treatment status. Because treatment was randomly assigned, equation (4.1)

[17] Captains serve a three–year term; the 2013 election is therefore the first contest after KC2 implementation began.

[18] As we can see in the Appendix, the distribution across treatment and control groups is equal in our sample.

can be estimated consistently with the following reduced-form specification:

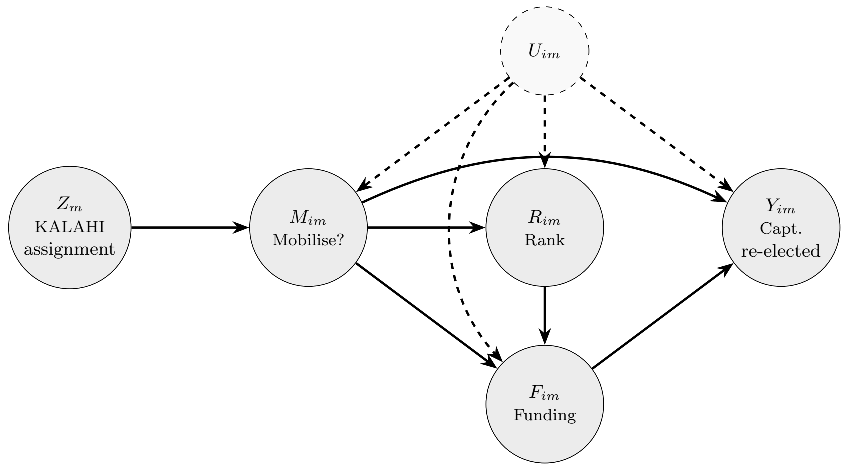
$$Y_{im} = \alpha + \beta T_m + \mathbf{X}'_{im}\gamma + \mu_p + \varepsilon_{im}, \tag{4.2}$$

where $\mathbf{X}_{im}$ includes pre-treatment covariates (e.g. baseline poverty), μ_p are pair fixed effects, and ε_{im} is an idiosyncratic error term. Standard errors are clustered at the municipality level to reflect the unit of randomisation. To account for the pair randomisation, I include pair fixed effects in my preferred specifications.

4.2 Effect of Mobilisation and Funding

Recall that our second research question asks whether any electoral gains from KALAHI–CIDSS are channelled through the *success* in securing a funded sub-project. The ITT captures every pathway through which randomised programme assignment can influence re-election rate. In practice, however, there at least are two sequential and potentially endogenous steps that could drive any electoral reward: first, a village may or may not *mobilise*. Participation required substantial community mobilisation: residents had to attend barangay assemblies, elect project committees, draft proposals, select a representation team, and defend the proposal in a competitive municipal forum. Let M_{im} be a binary indicator equal to 1 if village i in municipality m successfully completed this mobilisation protocol, and 0 otherwise. Second, if a village mobilised, its proposal may or may not obtain *funding*. Conditional on $M_{im} = 1$, proposals are ranked in the municipal forum and funded while resources last. Denote by R_{im} the (inverse) rank, where lower values are better; the municipality-specific funding cut-off is $\bar{r}_m$. A proposal is funded if and only if it is both mobilised and scored above the cut-off ($F_{im} = 1\{M_{im} = 1 \text{ and } R_{im} \leq \bar{r}_m\}$).

Figure 3: Directed-acyclic graph (DAG) of the mobilisation–funding process



Notes: Z_m is the randomised assignment of municipality m to KALAHI. M_{im} indicates whether village i mobilises; R_{im} is its proposal rank (lower = better); F_{im} is an indicator for funding; Y_{im} is captain re-election. U_{im} captures latent attributes—leadership quality, social capital–that may affect mobilisation, ranking of proposals, funding decision and incumbent re-election. Solid arrows are hypothesised causal relations. Rank and funding are defined only when $M_{im} = 1$; the arrow $M_{im} \rightarrow F_{im}$ encodes this prerequisite.

Figure 3 summarises the causal structure. Random assignment Z_m shifts the probability that a village mobilises but, by design, cannot affect ranks, funding, or votes except through mobilisation and its descendants. Mobilisation is required before a proposal can be ranked; rank, together with the budget line, determines funding. Funding and mobilisation may each influence electoral outcomes through distinct channels—visible efforts during mobilization versus tangible delivery of projects. Finally, unobserved village or leader traits U_{im} can raise both the propensity to mobilise, to receive funding, and the likelihood of re-election.The DAG makes clear why separate identification of the mobilisation effect (M_{im}) and the funding effect (F_{im}) is non-trivial. The following two subsections describe two natural experiments to recover these causal effects.

4.2.1 Effect of Mobilisation (CATE)

If voters reward leaders their efforts during KALAHI implementation, we would expect incumbent barangay captains in villages that *mobilise* to have different electoral outcomes from those who did not mobablize. Testing this hypothesis requires isolating the causal impact of mobilisation.

However, the decision to mobilise is endogenous: energetic captains are more likely to mobilise their village, certain villages might be more likely to mobilise in the first place. Both factor might also influence electoral outcomes. To isolate the causal effect of mobilisation on incumbent re-election , I rely on an instrumental variable approach to identify the Complier-Average Causal Effect (Angrist et al., 1996). In particular, I use the random assignment of municipalities to the KALAHI policy T_m as an instrument for the mobilisation of a village. I estimate a 2SLS:

$$Y_{im} = \alpha_M + \theta_M M_{im} + \mathbf{X}'_{im}\gamma_M + \mu_p + \varepsilon_{im}, \tag{4.3}$$

$$M_{im} = \pi_0 + \pi_1 Z_m + \mathbf{X}'_{im}\eta_M + \mu_p + \nu_{im}, \tag{4.4}$$

where Z_m is the randomised KALAHI assignment and $\mathbf{X}_{im}$ the same pre-treatment controls as in the ITT regression, M_{im} indicates if a village i in municipality m completed the mobilisation protocol, and Y_{im} denotes captain-re-election. Equation (4.3) is estimated by 2SLS, instrumenting M_{im} with Z_m. The instrumental variable approach makes several assumptions.[19] The randomisation of Z_m ensures (i) *relevance* ($\pi_1 > 0$) and unconfoundness (ii)): Z_m shifts M_{im} but, conditional on the matched-pair fixed effects μ_p, is orthogonal to unobserved determinants of the vote. Monotonicity (iv) is satisfied by design: assignment can only raise, never lower, the probability of mobilisation. The exclusion restriction is also likely fulfilled because the lottery assignment to KALAHI should have no direct effect on re-election. Under these conditions $\hat{\theta}_M$ is the Local Average Treatment Effect (LATE) of mobilisation on the sub-population of villages whose decision to mobilise is induced by programme assignment[20]:

$$\text{LATE}_{\text{mob}} = \mathbb{E}\big[Y_i(1) - Y_i(0) \;\big|\; D_i(1) > D_i(0)\big].$$

[19]Following Felton and Stewart (2024), I consider four main IV assumptions. (1) *Relevance* implies that the instrument has a non-zero average causal effect on treatment uptake. (2) *Unconfoundedness* assumes that the instrument shares no common causes with the treatment or outcome. (3) The *exclusion restriction* assumes that the instrument has only an effect on the outcome through the treatment. (4) *Monotonicity* assumes that there are no defiers the our sample.

[20]See Appendix D for details.

4.2.2 Effect of Funding (CATE)

A second channel through which incumbents may earn electoral credit is delivering a concrete, financed project. To test this we must isolate the incremental impact of *receiving funds conditional on the village having already mobilised*. However, within the group of mobilised villages, funding is not randomly assigned: stronger proposals (or better-connected captains) may rank higher in the municipal forum and therefore have a greater chance of crossing the budget cut-off. A naive comparison of funded and unfunded villages would thus mingle the causal effect of the grant with unobserved proposal/proposer quality.

To account for the non-random selection of funding, I rely on two strategies. First, I use the fact that project proposal were ranked and only a subset received funding. In particular, at municipal forums, every subproject proposal was ranked on an ordinal list and only the top $\bar{r}_m$ ranks were financed. A lower rank number therefore raised the funding probability. I treat the rank, r_{im}, as an instrument for funding F_{im}. If all assumption[21] are met, we can use the two-stage least-squares coefficient to identify the Local Average Treatment Effect of receiving KALAHI funds for the subset of compliers, that is *marginal* villages whose financing status flips with small rank changes.[22] Empirically, I restrict the sample to mobilised villages ($M_{im} = 1$) and estimate the two-stage least-squares regressions:

$$F_{im} = \pi_0 + \pi_1 \tilde{r}_{im} + \pi_2 \tilde{r}_{im}^2 + \lambda_m + u_{im},$$

$$Y_{im} = \beta_0 + \theta_F \widehat{F}_{im} + \pi_2 \tilde{r}_{im}^2 + \lambda_m + \varepsilon_{im},$$

where Y_{im} is the outcome of interest, λ_m are municipality–cycle fixed effects, and $\tilde{r}_{im}^2$ absorbs any smooth selection on rank. Under these assumptions, $\hat{\theta}_F$ is the Local Average Treatment Effect of receiving KALAHI funds for the subset of compliers.

I also employ an alternative research design and exploit the fact that the municipal budget cut-off creates a fuzzy regression discontinuity (RD). I use the signed distance between a project's rank and the last rank financed, $Z_{im} = r_{im} - \bar{r}_m$, as a running variable.[23] Because compliance

[21] Relevance, Unconfoundness, Monotonicity, and Exclusion Restriction.

[22] For details, please refer to Appendix E.

[23] Similarly Jacob and Lefgren (2011) and De Benedetto et al. (2025) who use score- or rank-based funding thresholds

is imperfect–some proposals just inside the window are not funded and a project just outside are funded–crossing the cut-off only *increases* the probability of funding. I estimate the Local Average Treatment Effect for the *marginal villages* whose funding status is flipped by the budget boundary. I estimate this effect with `rdrobust` Calonico et al. (2017), using an Epanechnikov kernel, the MSE-optimal bandwidth, quadratic bias correction ($p = 1, q = 2$), cycle fixed effects, municipality and village covariates, and standard errors clustered at the municipality level.

5 Results

5.1 The Average Effects of the KALAHI Policy on Electoral Outcomes

First, I present estimates of the average effects of the KALAHI policy on various electoral outcomes. Table 1 presents OLS regression results from estimating several variants to equation 4.2. Raw (1) asks whether simple assignment to KALAHI altered the probability that a captain was re-elected into office. The specification in the first column estimates the effects of the KALAHI policy on the likelihood that an captain mayor is reelected. Because treatment assignment was done using pair-randomization, column (2) includes matched-pair fixed effects. From column (3) onward I cluster standard errors at the municipality level to account for the level of treatment assignment. Columns (4) adds the four characteristics that entered the pair-matching algorithm–population, number of villages, poverty index, and land area–to increase precision. Following De Chaisemartin and Ramirez-Cuellar (2024), column (5) included both fixed effects at the pair level and clustered standard errors at the pair level.[24]

The ITT coefficients for the re-election outcome in the top panel of Table 1 are remarkably stable across specifications: the point estimate fluctuates between −0.02 and −0.03 and becomes statistically significant (p ¡ 0.05) once we cluster at the municipality level and absorb the baseline covariates (columns 3–5). The next row shows that a similar-sized penalty operates one step earlier in the electoral process: treated captains are around 2-3 percentage points less likely to *stand* for re-election. This negative effect is statistically significant in every fully specified regression. Conditional on running, however, the program has–at best–a muted impact on intensive-margin

as quasi-random instruments.

[24] Appendix H also provides results using the simple difference-in-means estimator. The point estimate remain unchanged, but the confidence interval are slightly larger. This is not surprising as Bai et al. (2024) show that both differences-in-means estimator and t-tests constructed from linear regressions are typically conservative in cluster randomized trials with matched pairs.

Table 1: Intention-To-Treat Effects of KALAHI on Re-election

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
Pr(reelection)	-0.03*	-0.02	-0.02**	-0.03**	-0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pr(run)	-0.02*	-0.03*	-0.03**	-0.02**	-0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Vote Share	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Win Margin	0.01**	0.01**	0.01	0.01	0.01
	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
No. Candidates	-0.03	-0.02	-0.02	0.01	0.01
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)
Fixed Effects		✓	✓	✓	✓
Clustered Standard Errors			✓	✓	✓
Mun. Cov				✓	✓
Num.Obs.	4829	4829	4829	4829	4829
Num.Clusters			196	196	98

[a] Notes. This table reports the effects of the KALAHI policy on various electoral outcomes. Each column presents the results of an OLS regression of the dependent variables listed in that column on an indicator variable for whether the municipality was assigned to treatment or control. Regressions in columns (4) and (5) include municipal characteristics that were also used for pair matching: population, number of villages, poverty index, and land area. The sample in rows (1) and (2) includes all mayors who were eligible for reelection. The samples in rows (3)–(5) include only the mayors who chose to run for reelection. Robust standard errors are displayed in brackets. Significantly different from zero at 99 (***), 95 (*), 90 (*) % confidence.

performance. In rows 3–5, the coefficients on vote share, win margin, and number of candidates are all near zero, and only the win-margin coefficient achieves statistical significance–and only in the model that fails to account for clustered treatment. Taken together, the ITT results suggest that on average participation in KALAHI has a small but negative effect on incumbent electoral outcomes. The program reduces incumbent survival mainly through the extensive margin: some captains choose not to run for re-election. The intensive-margin performance of those who do run show no difference. Taken together, the results suggest that KALAHI mainly affected the *extensive* margin—whether captains chose to run and whether they ultimately retained office–rather than the electoral performance of those who contested.

6 Mechanisms

The previous section demonstrated that KALAHI policy had a small average negative effect on the the re-elections rates among village politicians within treatment municipalities. On the first look, this might seem counterintuitive as villages within treatment municipalities received public goods like roads, schools, or communities buildings. Why then did the delivery of public goods lead to political losses for the incumbent? If the negative effects are driven by policy implementation we would expect different patterns in outcomes between those villages that successfully mobabilized and went through the implementation process versus those who did not.

6.1 The Effect of Mobilization

Table 2 represents the results Local Average Treatment Effect (LATE) of mobilising on electoral outcomes. Throughout, the instrument is the random KALAHI assignment. The coefficient of interest, θ_M, is therefore the LATE for villages that mobilise only because they were randomly encouraged. The estimate therefore answers the question: "Among those villages, how does the captain's re-election probability change relative to what it would have been had they not mobilised?"

Table 2: Complier Average Treatment Effect of Mobilization on Incumbent Re-election

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
Pr(reelection)	-0.04*	-0.03	-0.03**	-0.04**	-0.04**
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Pr(run)	-0.04*	-0.04*	-0.04**	-0.03**	-0.03**
	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
Pr(share)	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Pair fixed effects		✓	✓	✓	✓
Clustered Standard Errors			✓	✓	✓
Census Covariates				✓	✓
Political Covariates					✓

[a] Notes. Panel A reports the complier average treatment effect of mobilization on incumbent re-election; Panel B reports the effect on the probability the incumbent runs again. Models add pair fixed effects (2–5), cluster SEs by municipality (3–5), and include census (4–5) and political covariates (5). Robust standard errors in parentheses. Significantly different from zero at 99 (***), 95 (*), 90 (*) % confidence.

Table 2 estimates equation (4.3) and each column and reports separate 2SLS regressions that instrument the village-level mobilisation indicator with the random assignment of the programme. The quantity of interest, θ_M, is therefore the LATE for villages that mobilise only because they were randomly encouraged. The columns add controls cumulatively: the raw specification in column (1) contains no controls, column (2) include matched-pair fixed effects, column (3) clusters the standard errors at the municipality level–the unit of randomisation of the encouragement–while columns (4) and (5) add, successively, the census and pre-campaign political covariates.

Across all five specifications the point estimate varies between -0.03 and -0.04 and becomes statistically significant ($p < 0.05$) once we account for the clustered design (columns 3-5). Because the instrument identifies the Local Average Treatment Effect, this effect applies to the *complier* villages–those that mobilised only because KALAHI eligibility encouraged them. The second row shows that mobilisation lowers the probability that the incumbent to run for re-election by roughly the same four percentage points. The results are significant in every specification once we cluster standard errors. Rows 3 looks at intensive-margin outcomes conditional on running: vote share. The coefficient is small (0.01) and never reaches conventional significance levels. Taken together, this suggests that the process of community mobilization deter a subset of captains from re-entering the race rather than reducing support among those who do run. As mentioned earlier, two IV assumptions are met by design as the instrument was randomly assigned and is thus unlikely to confounded. As argued earlier, it also plausible that the exclusion restriction holds because the fact a municipality won the KALAHI lottery should not influence electoral outcomes other than through the KALAHI policy.

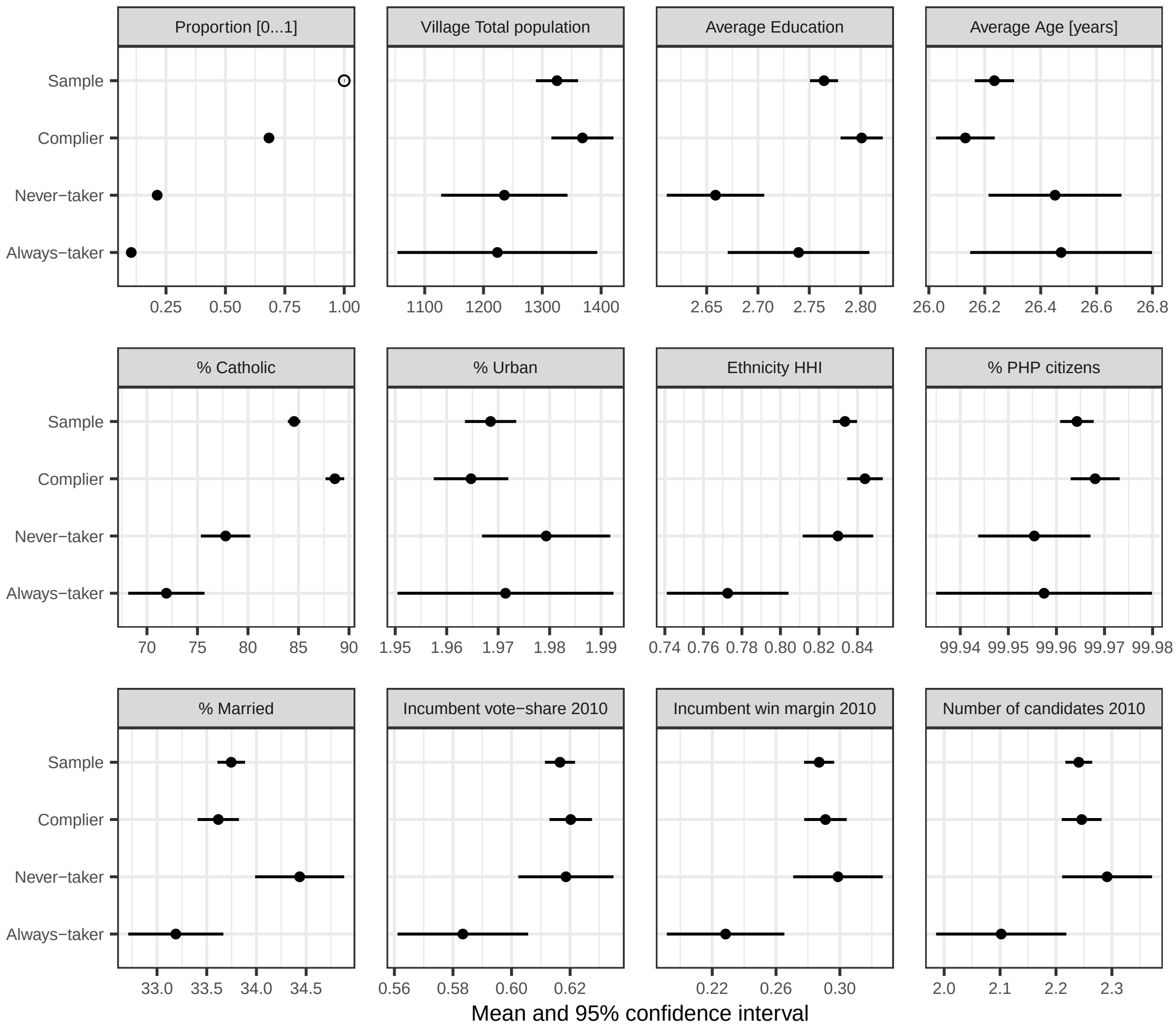
6.1.1 Profiling Compliers and Noncompliers

While the ITT effect answers the straightforward question "What is the overall impact of rolling out this encouragement program to everyone, on average?", the interpretation of LATE of mobilization and funding is less clear. In particular, the LATE isolates the causal effect only among the "complier" villages–those that mobilize because they were encouraged. Because these compliers may differ systematically from (i) always-takers, who would have mobilized regardless of encouragement, and (ii) never-takers, who never mobilize, it is crucial to profile each group on their pre-treatment characteristics. Therefore, I apply the profiling estimator by Marbach and Hangartner (2020) to characterize compliers, always-takers, and never-takers.

Figure 4 displays the overall sample mean alongside the subgroup means and their 95% bootstrap confidence intervals for each of the twelve pre-treatment village and political covariates.[25] We find that 68% of observations are compliers, 10% are always-takers, and 21% are never-takers. Although the three strata are broadly similar, compliers differ on a few dimensions. Complier villages average 1 368 residents–larger than always-taker (1 224) and never-taker (1 235) villages–and feature slightly higher average education, greater Catholic concentration, and ethnic homogeneity (HHI = 0.84 vs. 0.77 and 0.83). Differences in age, urbanization, citizenship, marital status, and 2010 vote-share, margin, or number of candidates are minimal. These patterns imply that the LATE we estimate for compliers pertains to villages that are somewhat larger, more educated, and more religiously and ethnically uniform. Generalizing this effect to more diverse or smaller villages should therefore be done with caution. Put differently, the communities whose decision to mobilise is shifted by the KALAHI encouragement are not the smallest or least educated (and therefore perhaps poorest) places in the sample. If anything, they look slightly more advantaged. Because the IV estimate of -0.04 p.p. is identified only for compliers, it is less likely to be attributed to extreme deprivation. That supports the interpretation that the electoral losses are caused by the mobilisation itself (its costs, conflicts, or heightened scrutiny), rather than from disadvantages of complier villages.

[25]Numerical values are presented in Appendix I.2.

Figure 4: Profiles of Complier and Noncomplier Subpopulations, Mobilzation IV



Notes: Descriptive statistics (mean and 95% bootstrap confidence intervals) for the complier and noncomplier subpopulations in the mobilization assigment. Subgroup shares appear in the first panel; subsequent panels show village population, education, age, religious and urbanization measures, ethnicity concentration, citizenship and marital rates, and 2010 electoral characteristics..

6.1.2 Summary

Taken together, what do we learn from the IV estimates? Offering KALAHI reduces the incumbent's re-election probability by 2–3 percentage points on average. For the subset of villages that responded to the offer by mobilizing, the negative effect is almost identical (about 4 p.p.). The fact that conditional vote shares remain flat while candidacy rates decline points to an extensive-margin channel: mobilization primarily determines who appears on the ballot, not how the remaining candidates perform. At least two interpretations are consistent with this pattern. First, the mobilization process may expose voters to low-quality incumbents. Local politicians are highly visible during mobilization—they persuade citizens to attend assemblies and often lead those meetings—so any shortcomings in their performance become apparent. Scrutiny from ministry officials or engineers may also expose weak barangay captains. Incumbents may anticipate this potential backlash and choose not to run again. Second, the negative effect may be driven by the funding outcome. All mobilized villages are a mix of eventually funded and eventually unfunded villages. The average effect could simply indicate that unfunded villages punish incumbents more than funded villages reward them. In particular, the mobilization process imposes real costs on citizens–time spent in assemblies and possible volunteer labour–and if project proposals are rejected, voters may feel they bore costs without any payoff and punish the incumbent. Note that both mechanisms could operate simultaneously. The next section studies the funding mechanism and asks whether this electoral penalty is reversed once a village's proposal crosses the funding threshold and a tangible project breaks ground.

6.2 The Effect of Project Funding

The results from the instrumental variable design are presented in Table 3. The sample is restricted to villages that did mobilize and received a rank for their project before the elections. Column (1) reports the bivariate 2SLS estimate, instrumenting the binary funding indicator with the proposal's inverse rank centred at the budget cut-off. The point estimate of 0.09 implies that, for villages whose funding status is shifted by a one–rank nudge at the margin, receiving the grant raises the captain's re-election probability by nine percentage points. The identification assumptions for IV require that on average, projects with a better (numerically lower) rank are more likely to cross the budget line and receive funds (relevance). Empirically, I estimate first–stage and use F-statistic as an indicator for relevance. Results in Appendix J.1 show an effective F-statistic of 29, indicating there is no problem of relevance. The remaining columns further probe the robustness of the effect.

Column (2) includes cycle fixed effects, thereby taking into account that very few municipality already started their second cycle. The point estimate remains unchanged and significant at

Table 3: Complier Average Treatment Effect of Funding on Incumbent Re-election

	Model1	Model2	Model3	Model4	Model5
Funding	0.09***	0.09***	0.09**	0.09**	0.08**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Cycle fixed effects		✓	✓	✓	✓
Clustered SEs			✓	✓	✓
Census Cov.				✓	✓
Political Cov.					✓
Num. obs.	1824	1824	1824	1824	1824
N Clusters			89	89	89

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Model 1 is the bivariate IV; Model 2 adds cycle fixed effects; Model 3 additionally clusters SEs by municipality; Model 4 further controls for census-level covariates (population, education, age, religion, urbanization, ethnicity, citizenship, marital status, municipal fiscal and land variables); Model 5 augments Model 4 with 2010 election characteristics (winner vote-share, margin, and number of candidates). Robust standard errors in parentheses.

$p < 0.05$. Clustering standard errors at the municipality–level in column (3) increases precision. The credibility of the research design requires that no omitted variable jointly affects a project's rank and funding decisions (instrument-treatment confounding), and/or project's rank and election outcomes (instrument-outcome confounding). One concern is that villages with stronger volunteer teams or proposals may rank higher and perform better electorally. To address this, I control for village quality using average education levels and incumbent quality using the captain's previous election (2010) vote share. Columns (4) includes a set of twelve pre-treatment covariates from the Census 2010, including average levels of education. The point estimate is stable and remains statistical significant.

Another potential issue is coordination among volunteer teams (sometimes including captains) in the MIBF to push allied projects up the ranking to receive funding (instrument-treatment confounding) and–in the case of captains– also helping each other to get re-elected (instrument-outcome confounding). Semi-structured interviews with citizens and officials (Appendix M) yield mixed evidence: some deny vote-trading, citing KALAHI's rules-based process, while others acknowledge informal conversations. However, for these possible bargains to influence funding, officials would have to coordinate around the budget-cutoff before ranking. An observable implication would be an unusual distribution of funded and unfunded projects around the cut-off.[26] To test this, I apply the density test proposed by Cattaneo et al. (2020) (Appendix K.1.1), finding no statistically significant evidence of systematic manipulation. Lastly, municipal mayors could influence project ranking and election outcomes for favored villages (instrument-outcome confounding). While municipal mayors were not allowed to vote in the MIBF, I include the captain's prior margin of victory as a proxy for informal mayoral support. Column (5) includes the vote share as wells as the vote margin of the incumbent captain in 2010 as electoral controls. Reassuringly, the point estimate is stable at 0.08, and the null of no effect is rejected at conventional significance levels. To test how much unobserved confounding would be required to overturn the reduced-form link between project rank and incumbent re-election, I apply the robustness-value analysis of Cinelli and Hazlett (2025).[27] The result implies that a hidden factor would have to explain about 3.3% of the residual variation in both Z and Y to make the reduced-form coefficient non-significant at the 5 % level.

Next, the *exclusion restriction* assumes that, apart from its influence on funding, the rank has no independent causal effect on the outcome. This would be violated if the ranking of proposals directly influences re-election chances in ways unrelated to project receipt (e.g., publicity or prestige associated with high-ranking proposals). The assumption, however, is plausible in this case because villagers are unlikely to observe the list of funded or unfunded projects and only observe whether their proposal was funded. The notion that citizens have limited knowledge about the KALAHI process apart from the assemblies and funding is supported by qualitative interviews.[28] However, following the sensitivity approach of **?**, I re–estimate the 2SLS model after deducting a hypothetical direct effect of the rank instrument on incumbent re-election outcome. The results show that statistical significance of the LATE disappears once we introduce a $-0.16pp$ direct effect per one–unit increase in the ranking. Hence, the IV result is moderately sensitive: its credibility depends on whether a direct effect is deemed plausible in the context (see Appendix

[26]For example, suppose in Forum A a small group of captains collude to reduce their project budgets or to mobilize extra counterpart funding, in order to push marginal proposals that would be ranked below the fixed municipal cut-off to get funded, thereby reducing the number of unfunded villages around the cut-off. In contrast, Forum B–with no such collusion–retains the baseline mix of funded and unfunded cases.

[27]Recall that the IV estimator can be calculated by dividing the reduced form estimate by the first stage estimate. The robustness value is the smallest partial R^2 that an unobserved variable must have simultaneously with the instrument and with the outcome–conditional on the controls–to drive the reduced-form t-statistic down to the two-sided 5 % threshold, undermining the IV estimate built from it.

[28]For details, please see Appendix M.

J.3.2 for full details and plots).

Finally, the *monotonicity* assumption rules out "defiers". So we exclude the possibility that village would lose funding if rank improved (ruled out). We can not check this assumption using the data. However, given the near deterministic funding rule that all entries above $\bar{r}_m$ are funded and all below are not, every project's probability of funding is should be weakly increasing as its rank improves.

6.2.1 Robustness and additional analysis

The results from instrumental variable estimation suggest that villages whose proposal is pushed just far enough up the MIBF list to clear the budget line are 8–9 percentage-points more likely to vote for their incumbent captain. However, one concern is that a linear first stage of the IV may impose to much structure. In particular, the centred-rank IV assumes a linear first stage and treats every proposal–no matter how far from the cut-off–as an incremental "encouragement" toward funding, so identification leans on the assumed global relationship between rank and both treatment uptake and the outcome.[29]

Fuzzy Regression Discontinuity Design Columns (1)–(5) of Table 4 therefore use a Fuzzy Regression Discontinuity Design (RDD) and re–estimate the effect non-parametrically using the `rdrobust` estimator. The different rows report the conventional, bias-corrected, and robust point estimates and their corresponding standard errors. We can observe two patterns. First, across specifications, the estimated effect lies between 0.16 and 0.20, twice as large as the 2SLS estimates. The difference is not surprising, because the two designs answer slightly different questions. The rank-IV recovers the complier LATE for those whose funding status is shifted by an incremental improvement anywhere along the rank distribution. The fuzzy RDD recovers a local LATE at the municipal budget line: the effect for proposals that happen to fall just above versus just below the cut-off. Second, while the RDD produces larger point estimates, they are estimated with wider confidence intervals and are significant only at the 10 per cent level for most models. Neither the

[29]The relationship is "global" in the sense that proposals far above or below the cut-off contribute just as much to the first-stage slope as proposals near it. Note, however, that the IV design does not assume the causal effect of funding on the outcome is linear. It only assumes that the first-stage conditional mean can be approximated by a line.

Table 4: Fuzzy RDD Estimates of KALAHI Funding on Re-election Rates

	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
Conventional	0.188*	0.169*	0.160*	0.178*	0.181*
	(0.096)	(0.086)	(0.069)	(0.051)	(0.063)
Bias-Corrected	0.207*	0.183*	0.175**	0.195**	0.195**
	(0.067)	(0.064)	(0.046)	(0.033)	(0.045)
Robust	0.207	0.183	0.175*	0.195*	0.195*
	(0.121)	(0.117)	(0.091)	(0.071)	(0.094)
N	1824	1824	1824	1824	1824
N.effective	1307	1307	1365	1307	1243
Cycle fixed effects		✓	✓	✓	✓
Clustered SEs			✓	✓	✓
Census Controls				✓	✓
Political Controls					✓

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

[a] Notes. This table reports the results of recieving KALAHI funding on re-election rates. Each column presents the results from from a Fuzzy RDD analysis. The running variable is the village's MIBF rank centred at the municipality-specific budget cut-off. "Conventional" is the raw local-linear estimate with its usual SE; "Bias-corr." adjusts the point estimate for small-sample bias but keeps the same SE; "Robust" combines the bias-corrected estimate with heteroskedasticity-robust, nearest-neighbour SEs. Column 4 additionally controls for municipality and village characteristics. Column 5 additionally controls for political village characteristics. Municipal: poverty-incidence index (PI), number of barangays, land area (squared), log total population. Village: average education (years), average age, share urban, ethnicity HHI, % Philippine citizenship, % married, All specifications use an Epanechnikov kernel with MSE-optimal bandwidth selector (msesum). Political: incumbent vote share and margn in previous election (2010) and number of candidates running. Standard errors are heteroskedastic; columns 3–5 are clustered at the municipality level.

inclusion of cycle fixed effects, nor clustering at the municipality level, nor the battery of census and political covariates significantly changes the uncertainty or magnititude of the effects. This is likely because the effective sample shrinks to barely two-thirds of the IV sample. Nevertheless, we can conclude that the positive effects from receiving funding on a KALAHI sub-project lends some support that the effect of the IV is not an artefact of functional-form choices.

Next, I examine the robustness of these RDD to a number of potential issues. First, identification rests on two assumptions: 1) there should be no manipulation of funding decision around the cutoff and (2) covariates potentially correlated with the funding (treatment) and the probability of re-election (outcome) must vary smoothly around the cutoff. Using the density test by Cattaneo et al. (2020), I find no statistically significant evidence for manipulation (see Appendix K.1.1). Appendix K.1.2 checks the covariate balance around cut-off. I find all that there is no discontinuity of covariates at the cutoff, suggesting that villages are similar except their treatment status. Likewise, the density test in Appendix K.1.1 finds no manipulation of the running variable, supporting the validity of the fuzzy RD design.

Variation around the Cut-Off Next, I exclude cycles where all projects were funded or no projects were funded and re-estimate both the IV and fuzzy RDD results (see Table A13 and 4 in the Appenidx). The 2SLS coefficient varies between 0.08 and 0.09 across all five specifications. It remains significant at the 1 % level in the bivariate specification, at the 5 % level once municipality clustering is introduced, and at the 10 % level after all census and electoral covariates are included. The first–stage F-statistic stays above 25, so the relevance condition is also met. For the Fuzzy RD design the effective bandwidth now relies on roughly 900 observations. Conventional and bias-corrected estimates centre between 0.18 and 0.27–slightly larger than those obtained in the full sample– .Yet the reduced effective N widens the robust confidence intervals so that significance falls to the 10 per cent threshold once clustering is imposed. In sum, excluding cycles with no within-municipality variation barely moves the IV estimate and leaves the RDD estimate of similar order of magnitude, albeit with wider confidence bands.

Profiling Compliers and Noncompliers Next, I repeat the profiling exercise (Marbach and Hangartner, 2020) for the effects of funding on incumbent re-election. Note that the sample is already subset to observable mobilizers, which consists of the union of always-takers and compliers (in the treated arm).[30] Appendix K.3 reports the results. We can see that only 3% of villages are always-takers (prioritized regardless of cutoff), virtually none are never-takers, and 97 % are compliers (those whose prioritization status changes at the cutoff). Therefore, complier villages closely mirror the observable mobilizers sample on every dimension. The exceptionally

[30]Note that Marbach and Hangartner (2020) developed their profiling method under the binary-instrument framework. Therefore, I treat my binary indicator for observation inside or outside the cut-off as a binary instrument.

high compliance rate (97 %) implies that the LATE is numerically almost identical to the ITT–so our complier-specific estimate effectively captures the policy's average effect across all mobilizing villages.

Taken together with the negative mobilisation LATE in Table 2, the results suggest that *starting* the CDD process reduces the incumbent re-election probability about 4%, but completing the implementation and securing funding compensates, increasing the incumbents re-elections probability. by 8-9 %. Going back to the DAG in Figure 3, voters appear to punish captains who impose participation costs without a visible return, yet reward those who deliver a funded project.

7 Discussion and Limitations

Taken together, what do these three sets of results suggest? First, the ITT shows that offering the KALAHI policy to a municipality reduces the incumbent re-election by roughly two to three percentage points. The penalty operates almost entirely through the extensive margin–captains decide not to run–while the vote share of those who do run remains unchanged. Second, among the villages whose mobilisation decision is induced by KALAHI eligibility, going through the mobilisation process–regardless of whether the proposal is eventually funded or not–reduces incumbent re-election probability by about 3-4 percentage points. Third, conditional on having mobilised, winning a KALAHI grant reverses the pattern. The centred-rank IV estimates imply an eight-to-nine-point boost in the captain's re-election chances, and a fuzzy RDD that leans solely on projects lying just above versus just below the municipal budget line yields local effects of 16–20 points—larger, but statistically weaker once clustering is imposed. Because 97 percent of mobilised villages are "compliers" with respect to funding, the IV and RDD estimates speak to almost the entire set of mobilisers. Taken together, these estimates imply that mobilising a barangay is politically risky unless it culminates in a funded project.

7.1 External Validity

Does the implementation failure-reward mechanism generalise beyond CDD? I argue that the logic should travel to setting satisfying three conditions. First, politicians must play an observable role during implementation. For example, they may have to mobilise community meetings, draft proposals, co-finance projects, supervise works, or clear audits–tasks that voters can observe and automatically associate with the incumbent. Second, voters must receive performance signals, whether positive or negative, before elections. KALAHI's competitive ranking makes winners and losers clearly visible, but other institutions can transmit equally sharp signals. For example, public audit releases or unfinished worksites could be equally useful for citizens. Crucially, these signals have map into real effort. Where a project materialises automatically regardless of actions of politcians, the mechanism weakens. Third, attribution matters: citizens punish or reward only when they can plausibly hold the incumbent responsible, rather than blaming higher-tier bureaucrats or sheer bad luck. Recent findings by Martin and Raffler (2021) suggest that informing voters of the bureaucracy's role in delivery can diffuse electoral punishment of the incumbent. Note also that the competitive design of KALAHI likely helped. Because barangay captains had to compete against each other for the same pool of grants, the scheme generated *benchmarked* information that helped voters judge relative performance. Prior work demonstrates that such benchmarking makes accountability signals particularly important for voters (Kayser and Peress, 2012; Bhandari et al., 2023). In short, wherever local leaders are visibly "on the hook" for clearing a competitive or procedural hurdle and citizens learn, in time, whether they have succeeded, one should expect the same pattern: mobilisation without success leads to electoral losses, while successful delivery earns an electoral returns.

7.2 Limitations

I acknowledge several limitations. First, future research should improve the current research design by including a random assignment of eligible projects, possible also via public lottery. Second, future research should rule out the possibility that different expectations for different offices drive the results. The ideal research design would randomly assign different implementation tasks to political offices. Here, it would be essential to explore if politicians have the same baseline ability to claim credit, conditional on different roles during implementation.

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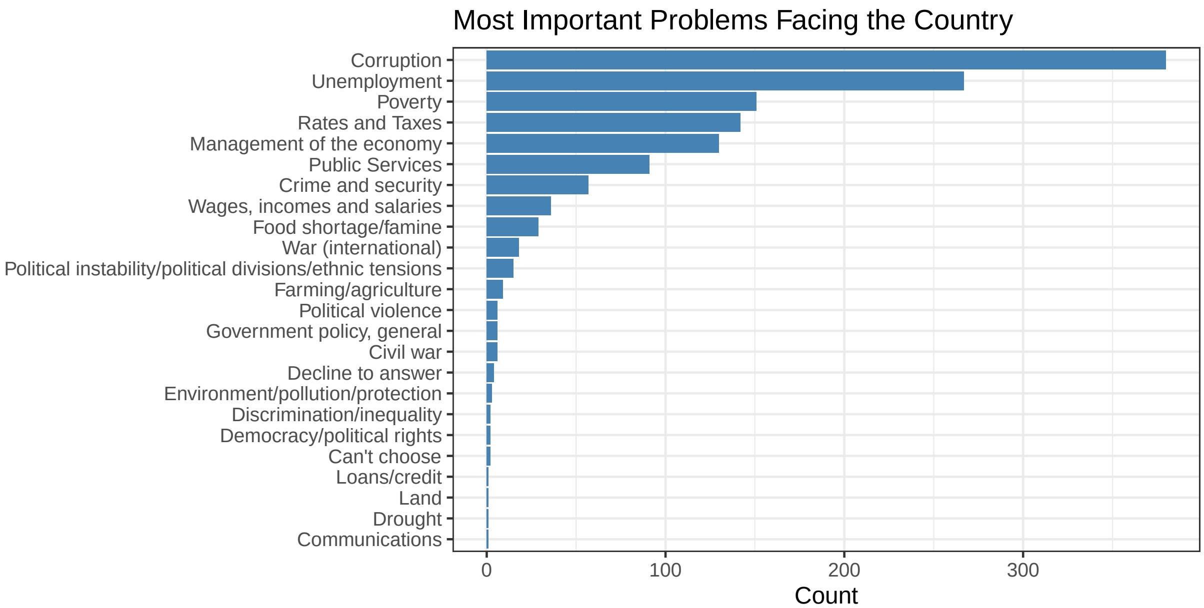
Appendices

A Background: Philippines

Figure A1: Philippines Public Opinion: Most Important Problems



Notes: Data from Asiabarometer Wave 4.

B Background: Kalahi
B.1 Sample

Figure A2: KALAHI Sample

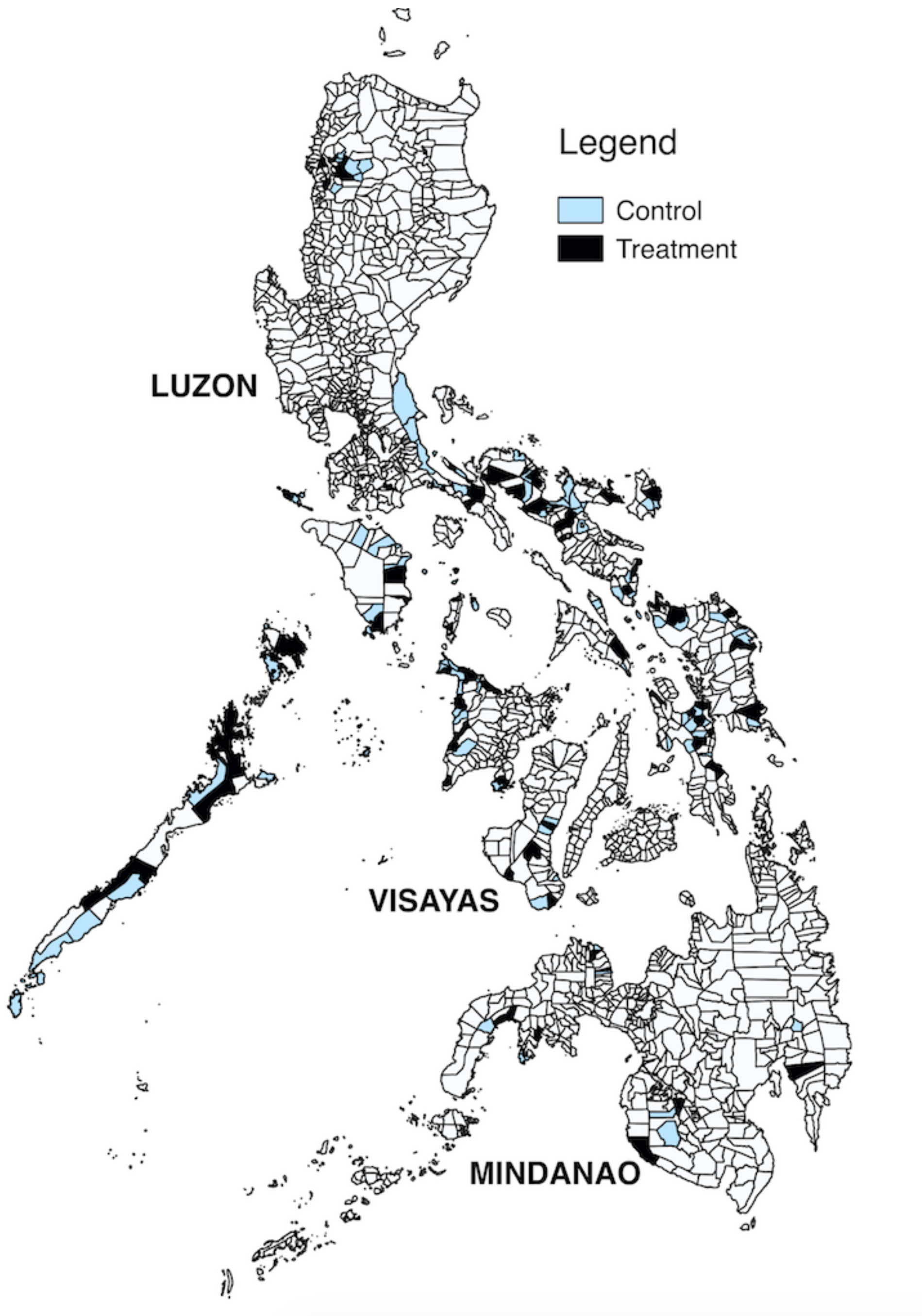
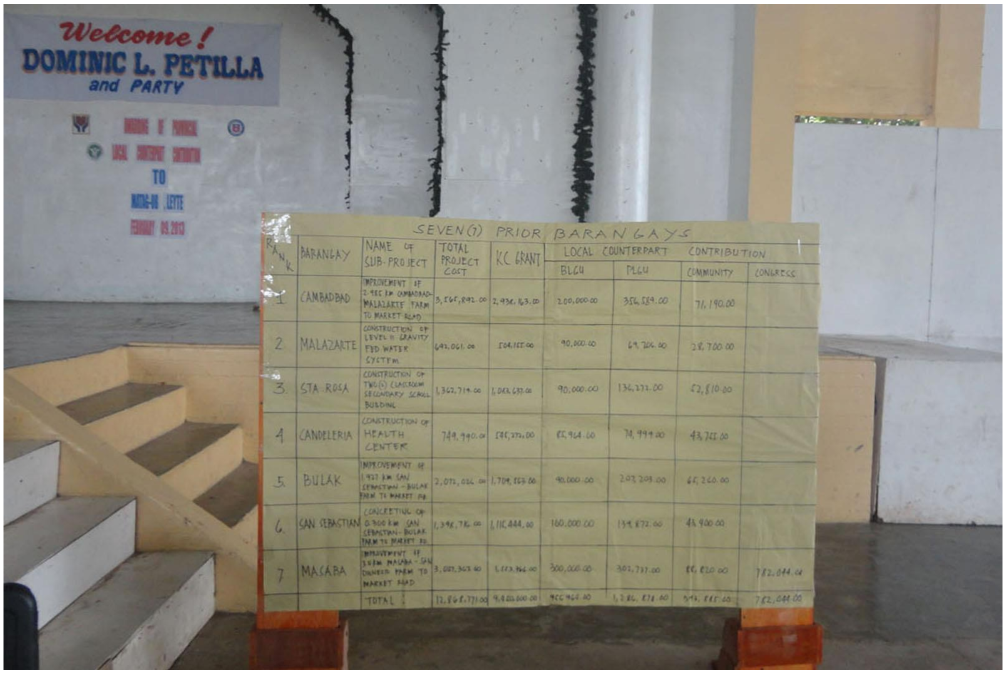


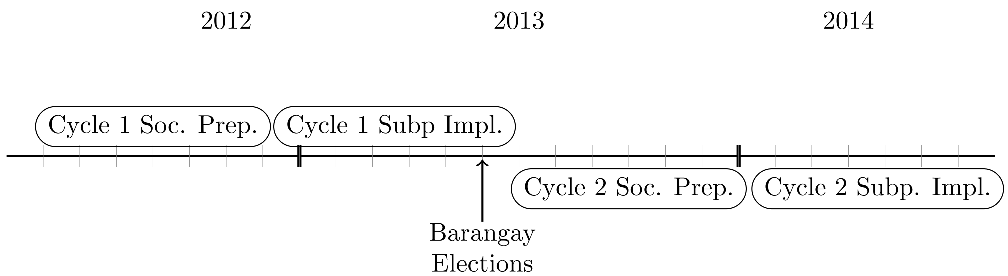
Figure A3: Example:Counterpart Contribution



B.2 Timeline

By the time of the village election in 2013, all villages went through the social perpetration stage and a substantial number of village had projects started and completed (see below.)

Figure A4: KC Timeline and Local Elections



B.3 Compliance

The table below shoes the municipalities that experierened issues of non-compliance according to Beatty et al. (2015). The two Abra drop-outs Peñarrubia and Pidigan were not included in the data because they never reached baseline.

Table A1: Dropout Municipalities in KC Evaluation (Beatty et al.)

Province	**Municipality**	**Assignment**	**Reason for Dropout or Non-Compliance**
Dropouts and Replacement			
Abra	Peñarrubia	Treatment	Unable to provide required counterpart funding.
Abra	Pidigan	Control	Dropped with paired treatment municipality (Peñarrubia).
Palawan	Taytay	Treatment	Replacement for Peñarrubia.
Palawan	San Vicente	Control	Replacement for Pidigan.

Table A2 lists every municipality that violated its random-assignment status in the original KC experiment and spells out the specific mechanism. I distinguish four analytically separate groups:

- treatment-assigned municipalities that never managed to launch KC
- control municipalities that kept their control status but lost their treated partner when that partner dropped out
- control municipalities that crossed over and received KC funds
- pairs in which irregular reallocations left *both* municipalities treated.

Table A2: Non-Compliant Municipalities in the KC Impact Evaluation

Province	Municipality	RCT Assignment	Detailed Reason for Non-Compliance
Treatment-assigned Municipalities that Never Implemented KC			
Leyte	Calubian	Treatment	Failed to mobilise counterpart funds.
Sultan Kudarat	Palimbang	Treatment	Security conflict halted assemblies.
Sultan Kudarat	Lambayong	Treatment	Governance problems prevented Stage 1 mobilisation.
Abra	Lagangilang	Treatment	Withdrew after missing counterpart-funding deadlines.
Control Municipalities that Lost Their Treatment Match (Still KC-Free)			
Leyte	Santa Fe	Control	Lost treatment partner when Calubian exited.
Sultan Kudarat	Bagumbayan	Control	Lost treatment partner when Palimbang exited.
Sultan Kudarat	Esperanza	Control	Lost treatment partner when Lambayong exited.
Abra	Villaviciosa	Control	Lost treatment partner when Lagangilang exited.
Control Municipalities that Crossed Over (Received KC)			
Sorsogon	Santa Magdalena	Control	Successfully appealed for KC; funded in Cycle 1.
Oriental Mindoro	Pinamalayan	Control	Successfully appealed for KC;.
Abra	Luba	Control	Received KC funds after Lagangilang exited.
Abra	Malibcong	Control	Funded when DSWD replaced a non-sample drop-out.
Pairs with Irregular Funding – Both Municipalities Received KC			
Sorsogon	Irosin	Treatment	Funded, but its control (Santa Magdalena) also funded.
Oriental Mindoro	Roxas	Treatment	Funded, but its control (Pinamalayan) also funded.
Abra	La Paz	Treatment	Funded, but its control (Luba) also funded.
Abra	Langiden	Treatment	Funded, but its control (Malibcong) also funded.

Table A3: Treatment Municipalities that Never Implemented KC

Municipality	Brgys (N)	Brgys with BRT	Brgys with Application	Share BRT	Share Application
CALUBIAN	53	0	0	0	0
LAGANGILANG	17	0	0	0	0
LAMBAYONG	26	0	0	0	0
PALIMBANG	40	0	0	0	0

Table A4: Control Municipalities that Lost Their Treatment Match (KC–Free)

Municipality	Brgys (N)	Brgys with BRT	Brgys with Application	Share BRT	Share Application
BAGUMBAYAN	19	19	19	1	1
ESPERANZA	39	0	0	0	0
SANTA FE	20	0	0	0	0
VILLA VICIOSA	8	0	0	0	0

Table A5: Control Municipalities that Crossed Over (Received KC)

Municipality	Brgys (N)	Brgys with BRT	Brgys with Application	Share BRT	Share Applica
LUBA	8	8	8	1	
MALIBCONG	12	12	12	1	
PINAMALAYAN	37	0	0	0	
SANTA MAGDALENA	14	0	0	0	

Table A6: Treatment Municipalities were Control Pair also Treated

Municipality	Brgys (N)	Brgys with BRT	Brgys with Application	Share BRT	Share Application
IROSIN	28	28	27	1.000	0.964
LA PAZ	13	13	13	1.000	1.000
LANGIDEN	6	6	4	1.000	0.667
ROXAS	51	19	3	0.373	0.059

B.3.1 Non-Compliance, Control, beyond Beatty et al.

Next, I check if there are cases

B.3.2 Compliance, Data on Baranagay Level

The tables below present the compliance status at the village or municipality level as of November 2013. A municipality is coded 1 for each category if at least one village within the municipality fall into the category. For example, when at least on village within a treatment municipality received a project it is coded “Treatment: applied, received”. Only if villages applied and none within a municipality received a project the municipality if coded ”Treatment: applied, not received”.

Table A7: Distribution of Villages by Compliance Status

Compliance Status	Count	Percentage (%)
Control: never Applied	2117	43.84
Control: applied, not received	59	1.22
Control: applied, received	104	2.15
Treatment: never Applied	532	11.02
Treatment: applied, not received	829	17.17
Treatment: applied, received	797	16.50
Total	4829	100.00
	391	8.10

Note:

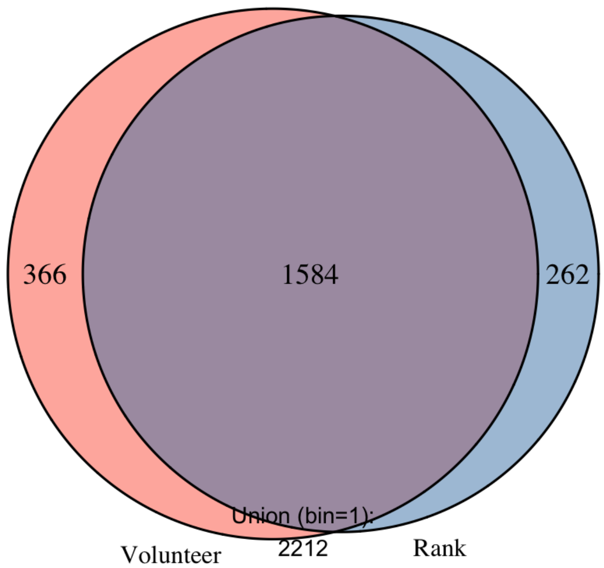
This table summarizes the compliance status of villages, categorized by control and treatment groups.

C Data

C.1 Data: Mobilization

The Venn diagram illustrates the overlap between the two mobilization indicators: of the 2,212 projects flagged as mobilized by at least one criterion, 1,584 projects experienced both rank-based and volunteer-based mobilization, while 366 projects were mobilized only by volunteers and 262 only by rank.

Figure A5: Venn diagram, Mobilization Indicators



C.2 Data: Ranking

The ranking data include cycles where all villages received funding (possible), but also rankings that have some missing ranks, and few rankings where villages that crossed the cut-off receive funding. The table below displays the issues.

[para]

Note:

Any municipality/cycle with missing or out-of-order rank(s).

Table A8: Rank-Sequence Issues by Municipality

Province	Municipality	pme_Fundsource	pme_Cycle	min_rank	max_rank	has_gaps	Missing_Ranks_Text
ABRA	BUCAY	KC-MCC	1	1	20	FALSE	—
ABRA	LA PAZ	KC-MCC	1	1	12	FALSE	—
ABRA	PILAR	KC-MCC	1	1	18	FALSE	—
ABRA	SALLAPADAN	KC-MCC	1	1	9	FALSE	—
AGUSAN DEL NORTE	KITCHARAO	KC-AF	1	2	11	TRUE	1
QUEZON	AGDANGAN	KC-AF	1	2	11	TRUE	1
QUEZON	ALABAT	KC-AF	1	1	1	FALSE	—
QUEZON	GENERAL LUNA	KC-AF	1	1	26	FALSE	—
QUEZON	LOPEZ	KC-AF	1	1	86	TRUE	20, 35
OCCIDENTAL MINDORO	LUBANG	KC-MCC	1	1	10	FALSE	—
PALAWAN	ABORLAN	KC-MCC	1	1	18	TRUE	2, 15
PALAWAN	CAGAYANCILLO	KC-MCC	1	3	11	TRUE	1, 2
PALAWAN	CORON	KC-MCC	1	6	19	TRUE	1, 2, 3, 4, 5
PALAWAN	CUYO	KC-MCC	1	1	15	FALSE	—
PALAWAN	NARRA	KC-MCC	1	1	23	TRUE	17, 18
ROMBLON	CALATRAVA	KC-MCC	1	5	5	TRUE	1, 2, 3, 4
ZAMBOANGA DEL SUR	DUMALINAO	KC-AF	1	2	2	TRUE	1
CAMARINES NORTE	BASUD	KC-MCC	1	1	18	FALSE	—
CAMARINES NORTE	SAN VICENTE	KC-MCC	1	1	9	TRUE	4
CAMARINES SUR	BAAO	KC-MCC	1	1	26	TRUE	18, 19
CAMARINES SUR	CARAMOAN	KC-MCC	1	1	49	TRUE	11, 23
CAMARINES SUR	LIBMANAN	KC-MCC	1	1	70	TRUE	14, 16, 17, 26, 31, 35, 39, 40, 42, 43, 44, 46, 47, 48, 51, 52, 54, 55, 56, 57, 58, 59, 60, 61, 62, 64, 65, 66, 67, 68
MASBATE	CATAINGAN	KC-AF	1	1	34	FALSE	—
MASBATE	PALANAS	KC-AF	1	8	22	TRUE	1, 2, 3, 4, 5, 6, 7
SORSOGON	IROSIN	KC-AF	1	1	27	TRUE	10
SORSOGON	PRIETO DIAZ	KC-AF	1	1	22	TRUE	21
AKLAN	BATAN	KC-MCC	1	1	16	TRUE	9, 10, 11, 12
ANTIQUE	LAUA-AN	KC-MCC	1	1	24	TRUE	3, 12
ANTIQUE	PANDAN	KC-MCC	1	1	32	TRUE	11, 12, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 27, 28, 29, 30, 31
ANTIQUE	SAN REMIGIO	KC-MCC	1	2	24	TRUE	1
GUIMARAS	SIBUNAG	KC-MCC	1	1	5	FALSE	—
EASTERN SAMAR	DOLORES	KC-AF	1	1	34	FALSE	—
EASTERN SAMAR	LLORENTE	KC-AF	1	1	28	FALSE	—
LEYTE	ABUYOG	KC-MCC	1	1	39	FALSE	—
LEYTE	ALANGALANG	KC-MCC	1	35	54	TRUE	1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 32, 33, 34, 36, 37, 38, 39, 40, 41, 42, 43, 44, 45, 46, 47, 48, 49, 50, 51, 52,
LEYTE	BARUGO	KC-MCC	1	4	35	TRUE	1, 2, 3
LEYTE	BURAUEN	KC-MCC	1	1	77	TRUE	68, 70
LEYTE	TANAUAN	KC-MCC	1	3	27	TRUE	1, 2
NORTHERN SAMAR	CATARMAN	KC-AF	1	42	42	TRUE	1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 32, 33, 34, 35, 36, 37, 38, 39, 40, 41
MISAMIS OCCIDENTAL	CALAMBA	KC-AF	1	1	18	FALSE	—

C.3 Funded Project Stated and Completed

The dataset, supplied by DSWD, contains every sub-project that ultimately received KC funding and records two dates for each one: the *construction start* and the *completion* date. To align the datset with the barangay-level BRT data and the MIBF–ranking data I apply a timing rule based on KC implementation standards. Field logs in Beatty *et al.* (2018, p. 11) show that *most* funded sub-projects were built in fewer than six months after ranking, and the entire CEAC—from the first barangay assembly to hand-over—rarely exceeded nine to twelve months. The KC *Operations Manual* formalises this practice: the civil-works phase is supposed to finish within six months of the first tranche release, and *may never exceed twelve months*.[31] Accordingly, I classify a sub-project as *on-cycle* if its completion date lies ≤ 365 days after the last include MIBF-ranking date (October 28, 2013).

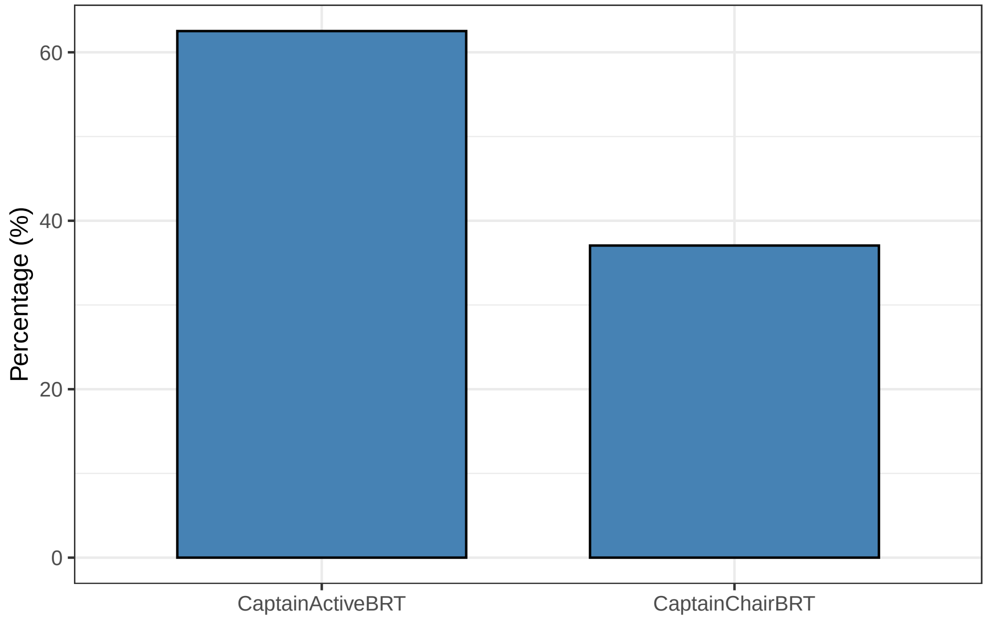
Figure A6: Funded Project Stated and Completed

Notes: Source: DSWD. Vertical represent the data of the village election in 2013. The data includes projects started at least one year after the village elections (October 24th 2014).

[31] KC Operations Manual v1.3, §4.3.2(c): "The physical implementation of any sub-project shall be completed within six (6) months from the date of first tranche release to the community account. In exceptional and fully documented cases the RPMO may grant a single extension; total duration shall in no case exceed twelve (12) months."

C.4 Volunteer Data

Figure A7: Funded Project Stated and Completed



Notes: Source: DSWD Monitoring and Evaluation Unit. The plot shows the percentage of captains who were present and chaired the Village Representation Team, expressed as a proportion of all project teams.

D Research Design: Effect of Mobilzation

Define:

$$M_{im}(z) \in \{0, 1\}, \quad Y_{im}(d) \in \mathbb{R},$$

where

- $M_{im}(z)$ is the mobilization status village i in municipality m would take if $Z_m = z$.
- $Y_{im}(d)$ is the electoral outcome (e.g. incumbent re-election) if mobilization status is d.

Under monotonicity ($M_{im}(1) \geq M_{im}(0)$), there are three compliance types:

$$\begin{aligned} &\textbf{Compliers:} && M_{im}(0) = 0,\ M_{im}(1) = 1, \\ &\textbf{Never-takers:} && M_{im}(0) = 0,\ M_{im}(1) = 0, \\ &\textbf{Always-takers:} && M_{im}(0) = 1,\ M_{im}(1) = 1. \end{aligned}$$

Type	$M_{im}(0)$	$M_{im}(1)$	**Interpretation**
Complier	0	1	Villages that mobilize if and only if assigned ($Z_m = 1$)
Never-taker	0	0	Villages that never mobilize, regardless of assignment
Always-taker	1	1	Villages that always mobilize, regardless of assignment
Defier	1	0	Villages that Mobilize only when $M_i = 0$; (Ruled out by monotonicity)

The 2SLS IV estimand converges to the Local Average Treatment Effect for compliers:

$$\frac{(Y_{im}, Z_m)}{(M_{im}, Z_m)} \longrightarrow \mathbb{E}\big[Y_{im}(1) - Y_{im}(0) \bigm| M_{im}(1) - M_{im}(0) = 1\big] = \text{LATE}_{\text{mob}}.$$

E Research Design: Effect of Funding

E.1 Rank IV on Funding

I use the (centred) ordinal rank $\tilde{r}_{im} \in \mathbb{R}$ as an instrument. For a minimal change in ranks ε, we can define

$$F_{im}^{-} = F_{im}(\tilde{r}_{im} - \varepsilon), \quad F_{im}^{+} = F_{im}(\tilde{r}_{im} + \varepsilon),$$

where $F_{im}(r)$ is the indicator that village i in forum m would be funded at rank r. We then can partition villages into:

Type	F_{im}^{-}	F_{im}^{+}	**Interpretation**
Complier	0	1	Would gain funding from at small rank improvement.
Never-taker	0	0	Remains unfunded under any small rank change.
Always-taker	1	1	Remains funded under any small rank change.
Defier	1	0	Would lose funding if rank improved (ruled out).

Put differently, never-takers are those villages where slight improvement in their rank does not lead them to cross the funding line and always-takers are those villages that would receive funding even if they would receive a worse rank.

E.2 Fuzzy RDD Funding

The fuzzy-RDD LATE at the cut-off is the ratio of the jump in the expected outcome as the centred rank $\tilde{r}_{im}$ approaches the cut-off c from below versus above to the jump in funding probability:

$$\text{LATE}_{\text{RDD}} = \frac{\lim_{x \downarrow c} \mathbb{E}[Y_{im} \mid \tilde{r}_{im} = x] - \lim_{x \uparrow c} \mathbb{E}[Y_{im} \mid \tilde{r}_{im} = x]}{\lim_{x \downarrow c} \Pr\big(F_{im} = 1 \mid \tilde{r}_{im} = x\big) - \lim_{x \uparrow c} \Pr\big(F_{im} = 1 \mid \tilde{r}_{im} = x\big)}$$

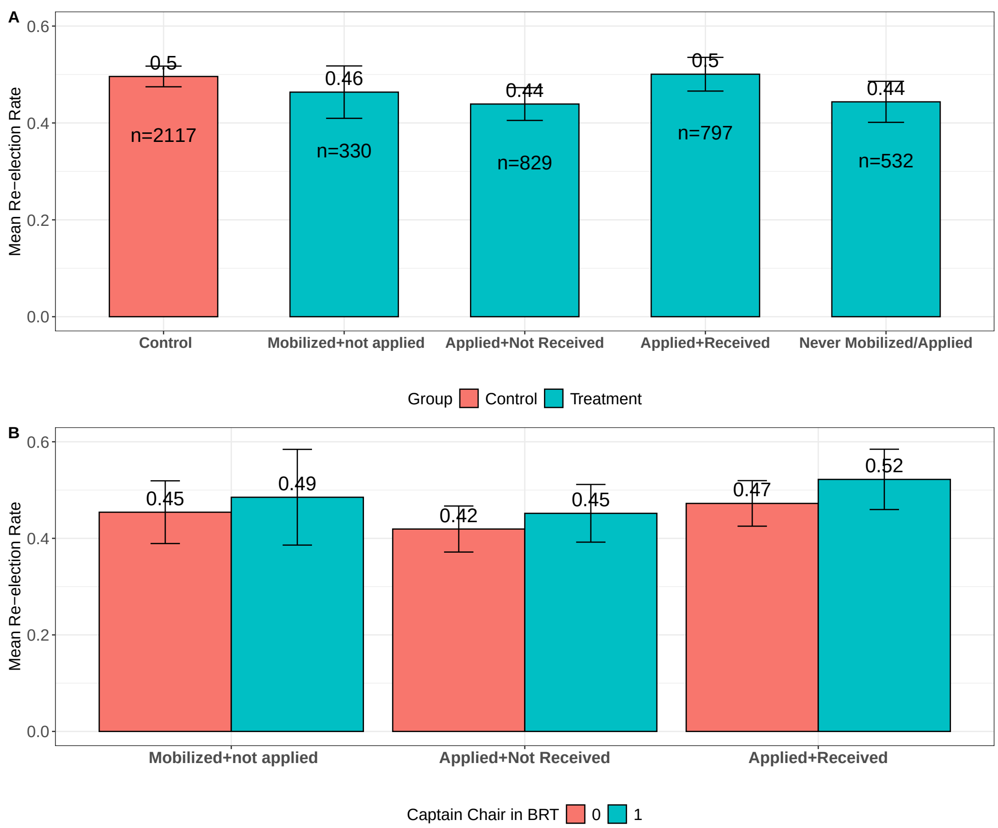
Put differently, among villages whose funding status changes right at the municipal-cycle budget cut-off, how much does being funded versus unfunded boost the incumbent's re-election probability?

F Summary Statistics

G Results: Descriptive

The upper panel of Figure A8 displays the re-election rates for incumbent incumbent village captains subset to those villages in control municipalities (Control), villages in treatment municipalities that did not apply for a project (Not Applied), villages in treatment municipalities that did apply but never received a project (Applied), and villages that did apply and received at least one project (Recieved).[32] However, the reader should not that the subgroup differences are not causally identified.

Figure A8: Descriptive evidence: project receipt and incumbent involvement



Notes: The figure reports group means and two-sided 95% confidence intervals for village captain re-election rates (Y) across within treatment compliance groups (X). "Control" refers to those villages that were not mobilised, i.e. complied with their treatment assignment. Mean values are displayed at the bottom of each sub-group. The data includes all villages in treatment municippalities.

I find that only villages that received local public goods see a positive difference compared to the control group, albeit this is not statistically significant. Incumbent village captains have the lowest re-election rate in villages that did not apply for project funding in the first place. Though not statistically significantly different from the control group, the effect indicates that voters punish village captains in villages that had the chance to apply for local public goods but did not do so.

[32]For most cases, villages only applied once up until the time of the election. There are a few cases in which villages applied twice.

However, we can see significant differences between villages that applied and did not receive a project and those who applied and received it. The average negative effect indicates that the group of punishing villages is larger than those rewarding the incumbent captain. This interpretation is supported by the fact that out of roughly 2,500 villages eligible to participate, only 800 received a transfer and started a project.

Next, I present descriptive evidence on the role of incumbent efforts during implementation by leveraging variation between more or less active village captains during the implementation of sub-projects and successful outcomes (did the village receive the project). To do so, I subset the data to include all the cases in which villages applied for subproject funding and merge them. with information about the involvement of village politicians in the village representation team.[33] Whenever a village captain volunteered and was appointed at any point until November 2013 to the village representation team, a village is coded 1 and 0 otherwise.[34] The lower panel of Figure A8 displays the results. On average, voters only rewarded those village captains who presented the subproject and received funding. In line with the performance mechanism, I find evidence that effort is a necessary but not sufficient condition to increase incumbent support.

[33]The data about the involvement of village politicians was obtained from the Monitoring and Evaluation Unit.

[34]The reader should note that this coding of funded project and appointed volunteers has slightly different time markers.

H Results: ITT

Table A9: Design-based Difference-in-Means ITT Estimates Across Outcomes

	Model (1)	Model (2)	Model (3)
Pr(reelection)	-0.027*	-0.023	-0.023
	(0.014)	(0.015)	(0.014)
Pr(run)	-0.024*	-0.023*	-0.023*
	(0.014)	(0.014)	(0.014)
Pr(share)	0.006	0.011	0.011
	(0.007)	(0.007)	(0.012)
Winner vote-share	0.011**	0.012**	0.012
	(0.005)	(0.005)	(0.009)
No. candidates	-0.027	-0.028	-0.028
	(0.023)	(0.022)	(0.038)
Fixed effects		✓	✓
Clustered Standard Errors			✓

[a] Notes. Each panel shows the difference in means (and standard errors in parentheses) of the outcome on treatment assignment under three designs: naive, matched-pair (block), and matched-pair plus cluster. Blocks = matched pairs; clusters = municipalities. Significantly different from zero at 99 (***), 95 (*), 90 (*) % confidence.

Table A10: **Intention-To-Treat Effect Incumbent Re-election**

	M 1	M 2	M 3	M 4	M 5	M 6	M 7
Treatment	-0.02^{*}	-0.03^{*}	-0.03^{**}	-0.02^{**}	-0.03^{**}	-0.03^{**}	-0.03^{*}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Voteshare 2010				0.18^{***}		0.18^{***}	0.18^{***}
				(0.05)		(0.05)	(0.05)
Population				-0.00^{*}		-0.00	-0.00
				(0.00)		(0.00)	(0.00)
Urban				-0.06		-0.06	-0.06
				(0.05)		(0.05)	(0.06)
Education				-0.07^{***}		-0.07^{***}	-0.07^{***}
				(0.02)		(0.02)	(0.02)
Age				0.03^{***}		0.03^{***}	0.03^{***}
				(0.01)		(0.01)	(0.01)
Female				-0.00		-0.00	-0.00
				(0.00)		(0.00)	(0.00)
Catholic				0.00		0.00	0.00
				(0.00)		(0.00)	(0.00)
Married				0.00		0.00	0.00
				(0.00)		(0.00)	(0.00)
Ethnicity HHI				0.02		0.03	0.03
				(0.05)		(0.05)	(0.06)
Migration HHI				-0.50^{***}		-0.50^{***}	-0.50^{***}
				(0.14)		(0.14)	(0.15)
N Barangays					0.00^{**}	0.00	0.00
					(0.00)	(0.00)	(0.00)
Population					-0.00^{*}	-0.00	-0.00
					(0.00)	(0.00)	(0.00)
Land Area					0.00^{*}	0.00^{***}	0.00^{**}
					(0.00)	(0.00)	(0.00)
Poverty					-0.00	-0.00	-0.00
					(0.00)	(0.00)	(0.00)
Fixed Effects		✓	✓	✓	✓	✓	✓
Clustered SEs			✓	✓	✓	✓	✓
Vill. Cov				✓		✓	✓
Mun. Cov					✓	✓	✓
Num. obs.	4829	4829	4829	4792	4829	4792	4792
N Clusters			196	196	196	196	98

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table A11: **Intention-To-Treat Effect on the number of candidates**

	M 1	M 2	M 3	M 4	M 5	M 6	M 7
Treatment	−0.03	−0.02	−0.02	−0.02	−0.01	−0.02	−0.02
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)
Voteshare 2010				-1.11^{***}		-1.10^{***}	-1.10^{***}
				(0.08)		(0.08)	(0.09)
Population				0.00^{***}		0.00^{***}	0.00^{***}
				(0.00)		(0.00)	(0.00)
Urban				−0.06		−0.05	−0.05
				(0.11)		(0.11)	(0.11)
Education				0.02		0.02	0.02
				(0.03)		(0.04)	(0.03)
Age				−0.02		−0.01	−0.01
				(0.02)		(0.02)	(0.02)
Female				0.01^{*}		0.01^{*}	0.01^{*}
				(0.01)		(0.01)	(0.01)
Catholic				-0.00^{*}		-0.00^{*}	−0.00
				(0.00)		(0.00)	(0.00)
Married				-0.01^{**}		-0.01^{**}	-0.01^{**}
				(0.00)		(0.00)	(0.00)
Ethnicity HHI				0.02		0.03	0.03
				(0.09)		(0.08)	(0.08)
Migration HHI				−0.09		−0.09	−0.09
				(0.23)		(0.23)	(0.24)
N Barangays					-0.01^{***}	-0.01^{***}	-0.01^{**}
					(0.00)	(0.00)	(0.00)
Population					0.00^{***}	0.00^{***}	0.00^{***}
					(0.00)	(0.00)	(0.00)
Land Area					0.00	−0.00	−0.00
					(0.00)	(0.00)	(0.00)
Poverty					0.01^{**}	0.01^{**}	0.01^{*}
					(0.00)	(0.00)	(0.00)
Fixed Effects		✓	✓	✓	✓	✓	✓
Clustered SEs			✓	✓	✓	✓	✓
Vill. Cov				✓		✓	✓
Mun. Cov					✓	✓	✓
Num. obs.	4829	4829	4829	4792	4829	4792	4792
N Clusters			196	196	196	196	98

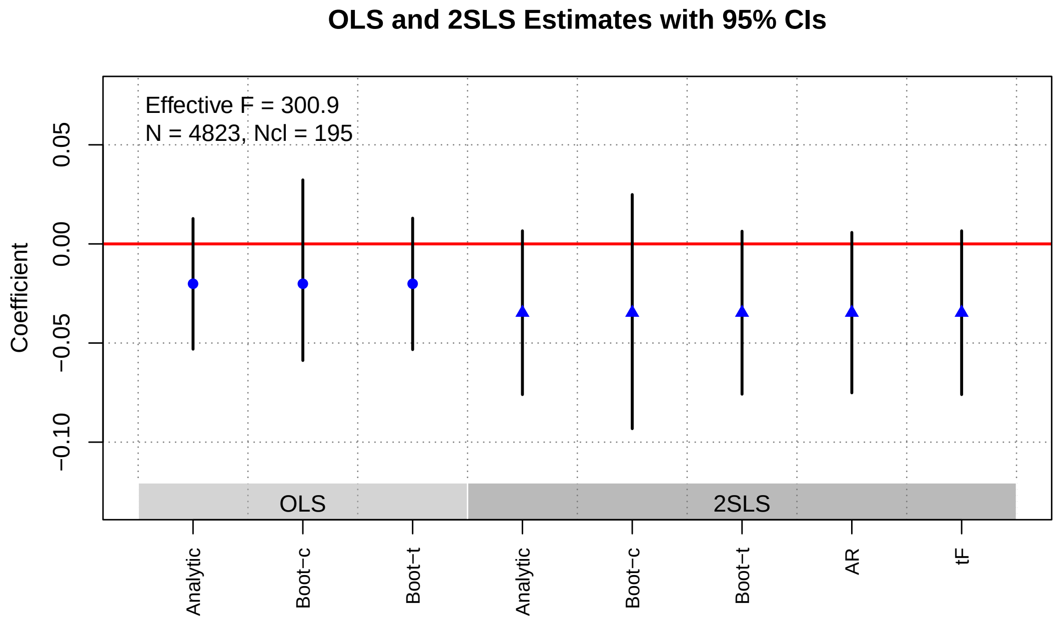
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

H.1 Heterogeneous treatment effects

I Results: Effect of Mobilisation

I.1 Robustness: Mobilisation IV

Figure A9: Effect of Mobilisation on Re-election



Notes: Points mark the point estimates and horizontal bars the 95 percent confidence intervals for both ordinary least squares (OLS) and two–stage least squares (2SLS) estimates of the effect of D on Y, instrumented by Z. Each OLS estimate is accompanied by three inference methods—analytic cluster–robust standard errors and coefficient- and t-ratio block bootstraps—whereas the 2SLS estimate appears with five: those three plus Anderson–Rubin inversion intervals Chernozhukov and Hansen (2008) and the tF procedure of Lee et al. (2022), whose critical value is adjusted by the effective first–stage F. The inset reports the effective Kleibergen–Paap first–stage F statistic, the numbers of observations and clusters, and the Anderson–Rubin p-value. All specifications include the pair fixed effects, standard errors are clustered at the municipality level, sociodemographic and political controls. Computed using `ivDiag` (Lal et al., 2024).

I.2 Profiling Compliers and Noncompliers

Table A12: Means (and 95% bootstrap SEs) of covariates by compliance strata

Variable	Always-taker	Never-taker	Complier	Sample
Proportion [0–1]	0.10 (0.01)	0.21 (0.01)	0.68 (0.01)	1.00 (0.00)
Village Total population	1223.63 (86.65)	1235.44 (54.84)	1368.27 (26.94)	1324.99 (18.30)
Average Education	2.74 (0.04)	2.66 (0.02)	2.80 (0.01)	2.76 (0.01)
Average Age [years]	26.47 (0.17)	26.45 (0.12)	26.13 (0.05)	26.23 (0.04)
% Catholic	71.92 (1.93)	77.78 (1.25)	88.60 (0.47)	84.57 (0.32)
% Urban	1.97 (0.01)	1.98 (0.01)	1.96 (0.00)	1.97 (0.00)
Ethnicity HHI	0.77 (0.02)	0.83 (0.01)	0.84 (0.00)	0.83 (0.00)
% PHP citizens	99.96 (0.01)	99.96 (0.01)	99.97 (0.00)	99.96 (0.00)
% Married	33.19 (0.24)	34.44 (0.23)	33.62 (0.11)	33.75 (0.07)
Incumbent vote-share 2010	0.58 (0.01)	0.62 (0.01)	0.62 (0.00)	0.62 (0.00)
Incumbent win margin 2010	0.23 (0.02)	0.30 (0.01)	0.29 (0.01)	0.29 (0.00)
Number of candidates 2010	2.10 (0.06)	2.29 (0.04)	2.25 (0.02)	2.24 (0.01)

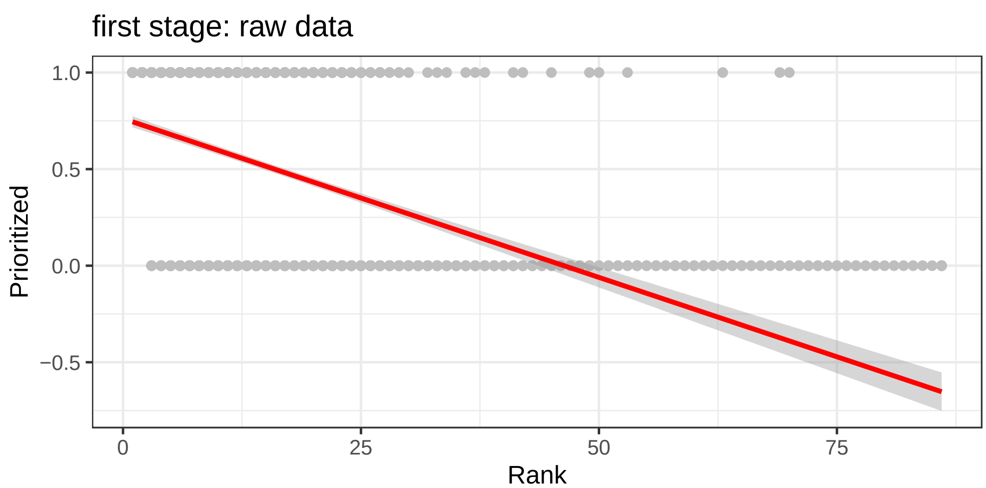
J Results: Effect of Funding (Rank Instrument)

J.1 Checking Assumption

J.1.1 First Stage

Figure A10 plots the first stage of the ”raw” rank (X-axis) against the probability of re-election (Y-axis). We can see that lower ranks are associated with a higher probability of receiving funding for a project.

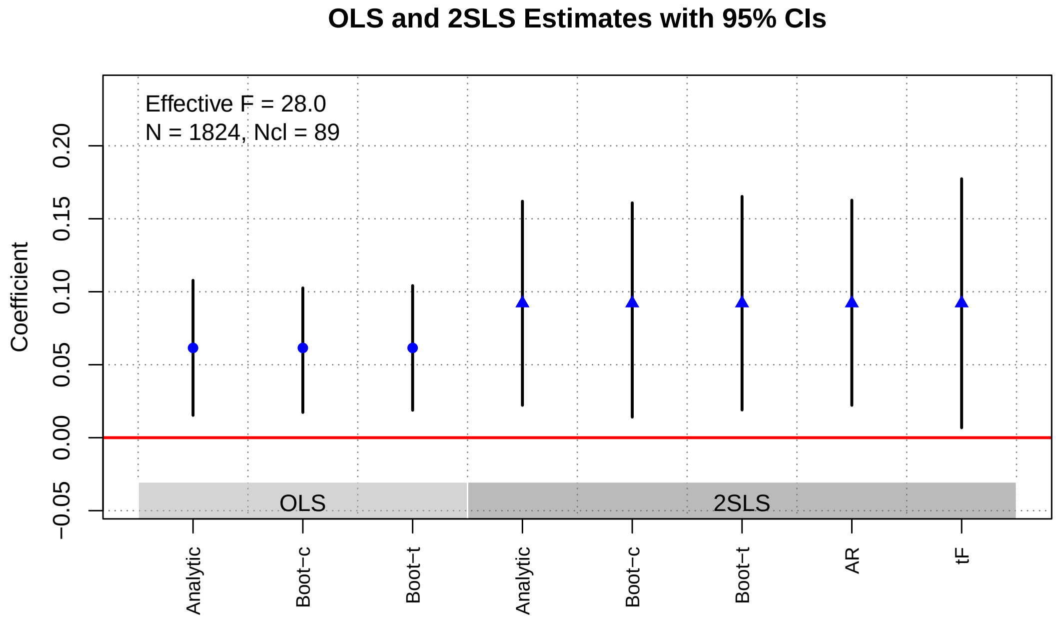
Figure A10: First Stage: ”Raw” Rank Instrument



J.2 Robustness

To check the robustness, I use the omnibus function ‘ivDiag‘ in R from Lal et al. (2023) which conducts both OLS and 2SLS estimation and quantify uncertainties using multiple inferential methods. It also output relevant information such as the first-stage *F*-statistics and results from the AR test. I find that the positive effects remain significant. Voters seem to reward project receipt when comparing incumbents that did apply and not received a project. However, somewhat worrying, the point estimates from the 2SLS are consistently larger than the OLS estimates, a trend that is observed in observational studies but not in experimental ones.

Figure A11: Electoral Effect of Project Funding



Notes: Points mark the point estimates and horizontal bars the 95 percent confidence intervals for both ordinary least squares (OLS) and two–stage least squares (2SLS) estimates of the effect of D on Y, instrumented by Z. Each OLS estimate is accompanied by three inference methods—analytic cluster–robust standard errors and coefficient- and t-ratio block bootstraps—whereas the 2SLS estimate appears with five: those three plus Anderson–Rubin inversion intervals Chernozhukov and Hansen (2008) and the tF procedure of Lee et al. (2022), whose critical value is adjusted by the effective first–stage F. The inset reports the effective Kleibergen–Paap first–stage F statistic, the numbers of observations and clusters, and the Anderson–Rubin p-value. All specifications include the cycle fixed effects, standard errors are clustered at the municipality level, sociodemographic and political controls. Computed using `ivDiag` (Lal et al., 2024).

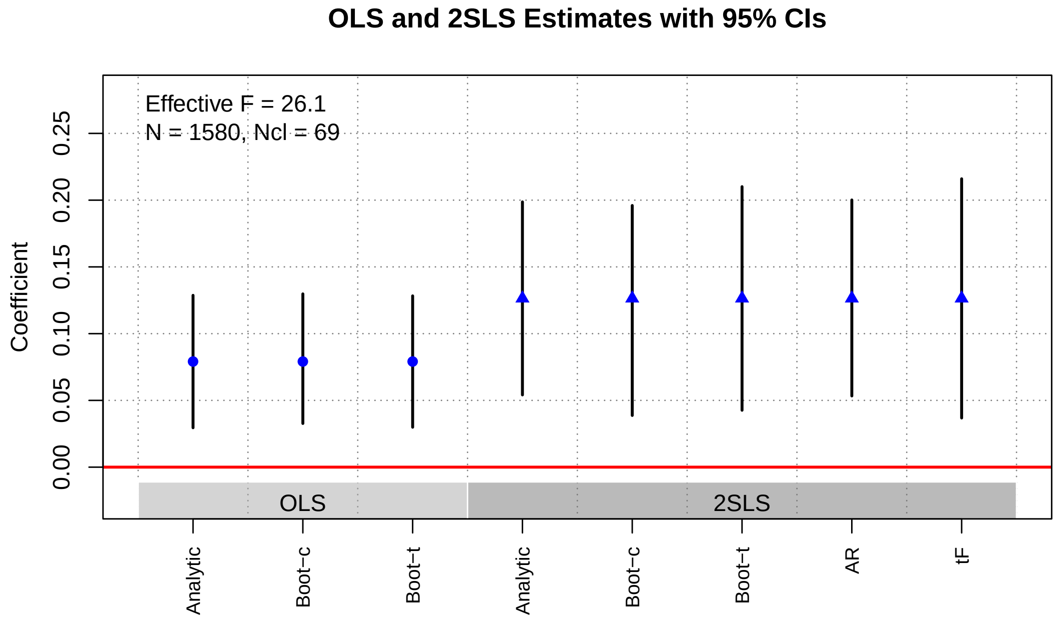
J.3 Results IV: Valid Cut-off

Table A13: Complier Average Treatment Effect of Funding on Incumbent Re-election

	Model1	Model2	Model3	Model4	Model5
Funding	0.09^{***}	0.09^{***}	0.09^{**}	0.08^{**}	0.08^{*}
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Pair fixed effects		✓	✓	✓	✓
Clustered SEs			✓	✓	✓
Census Cov.				✓	✓
Political Cov.					✓
Num. obs.	1580	1580	1580	1580	1580
N Clusters			69	69	69

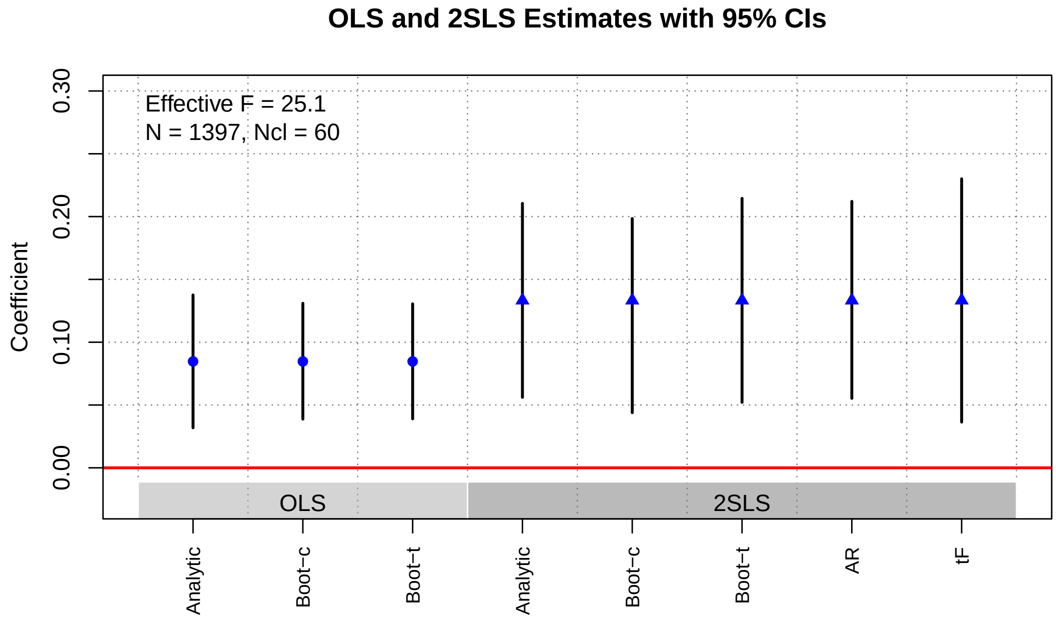
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Model 1 is the bivariate IV; Model 2 adds pair fixed effects; Model 3 additionally clusters SEs by municipality; Model 4 further controls for census-level covariates (population, education, age, religion, urbanization, ethnicity, citizenship, marital status, municipal fiscal and land variables); Model 5 augments Model 4 with 2010 election characteristics (winner vote-share, margin, and number of candidates). Robust standard errors in parentheses.

Figure A12: Electoral Effect of Project Funding, Valid Cut-off



Notes: Points mark the point estimates and horizontal bars the 95 percent confidence intervals for both ordinary least squares (OLS) and two–stage least squares (2SLS) estimates of the effect of D on Y, instrumented by Z. Each OLS estimate is accompanied by three inference methods—analytic cluster–robust standard errors and coefficient- and t-ratio block bootstraps—whereas the 2SLS estimate appears with five: those three plus Anderson–Rubin inversion intervals Chernozhukov and Hansen (2008) and the tF procedure of Lee et al. (2022), whose critical value is adjusted by the effective first–stage F. The inset reports the effective Kleibergen–Paap first–stage F statistic, the numbers of observations and clusters, and the Anderson–Rubin p-value. All specifications include the cycle fixed effects, standard errors are clustered at the municipality level, sociodemographic and political controls. Computed using `ivDiag` (Lal et al., 2024).

Figure A13: Electoral Effect of Project Funding, Valid Cut-off



Notes: Points mark the point estimates and horizontal bars the 95 percent confidence intervals for both ordinary least squares (OLS) and two–stage least squares (2SLS) estimates of the effect of D on Y, instrumented by Z. Each OLS estimate is accompanied by three inference methods—analytic cluster–robust standard errors and coefficient- and t-ratio block bootstraps—whereas the 2SLS estimate appears with five: those three plus Anderson–Rubin inversion intervals Chernozhukov and Hansen (2008) and the tF procedure of Lee et al. (2022), whose critical value is adjusted by the effective first–stage F. The inset reports the effective Kleibergen–Paap first–stage F statistic, the numbers of observations and clusters, and the Anderson–Rubin p-value. All specifications include the cycle fixed effects, standard errors are clustered at the municipality level, sociodemographic and political controls. Computed using `ivDiag` (Lal et al., 2024).

J.3.1 Check IV: Bias Analysis for Unmeasured Confounding

I follow Cinelli and Hazlett (2022) and probe the sensitivity of the IV estimate's by looking at the reduced form and first stage. After we partial out the controls, a large of the variability in each variable (instrument and outcome) is still noise. If an unmeasured confounder (for example, a village trait like administrative quality) were correlated enough with *ranking* that it alone accounted for 3.3% of that residual variance, and also accounted for 3.3% of the residual variance in re-election, then including it in the regression would change the rank–outcome coefficient to a point where it is no longer statistically significant. I use the strongest predictor (2010 vote share) as a benchmark.

Table A14: Robustness values (for reduced form and first stage regressions.

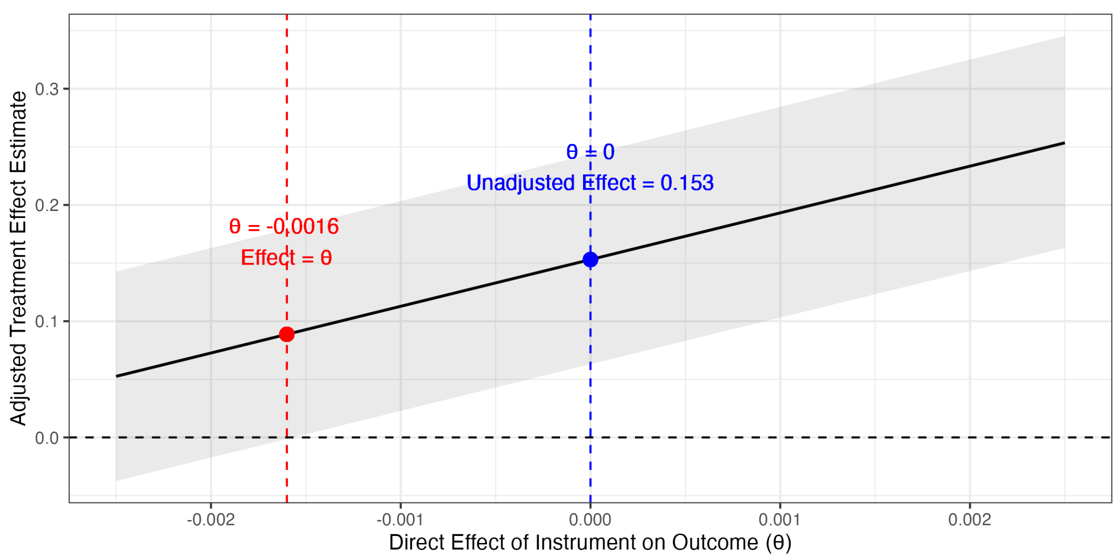
Benchmark covariate	Robustness value – reduced form (%)	Robustness value – first stage (%)
winner_voteshare_2010	3.26	50.29

An unobserved factor would have to explain at least 3.3% of the residual variation in both the instrument and the outcome to nullify the reduced-form relationship, but it would need to explain a much larger 50.3% of that variation to overturn the first-stage link.

J.3.2 Check IV: Bias Analysis for Exclusion-restriction

Second, probe the sensitivity of the exclusion restriction by observing how the estimated treatment effects change under potential violations of the exclusion restriction assumption. Following Conley et al. (2012); Wang et al. (2018); Felton and Stewart (2024), I adjust the confidence intervals for the treatment effect estimates by incorporating hypothetical direct effects of the instrument on the outcome. Specifically, I specify a range of θ values representing the potential direct effect of the instrument (ranking of projects) on the outcome (incumbent re-election) independent of the treatment. For each value of θ, I adjust the outcome by subtracting $\theta \times$ Rank and re-estimate the treatment effects using 2SLS, reporting 95% Anderson–Rubin confidence intervals. Figure A14visualises the analysis. We can see that a θ value of -0.16 percentage (or $\theta = -0.0016$) would render the treatment effect insignificant. At this value the lower bound of the confidence interval for the adjusted treatment effect first includes zero. At $\theta = -0.0016$, the adjusted treatment effect estimate is 0.0929, meaning the effect is still positive. This result shows that your treatment effect is robust to small violations of the exclusion restriction assumption, up until a direct effect of $\theta = -0.0016$. Beyond this point, the statistical significance of the treatment effect would be lost. In practical terms, this means that if the direct effect of the instrument on the outcome (outside of its effect through the treatment) is as small as -0.0016, your estimated treatment effect could be nullified in terms of statistical significance.

Figure A14: Conley et al. (2012) Bias analysis plot

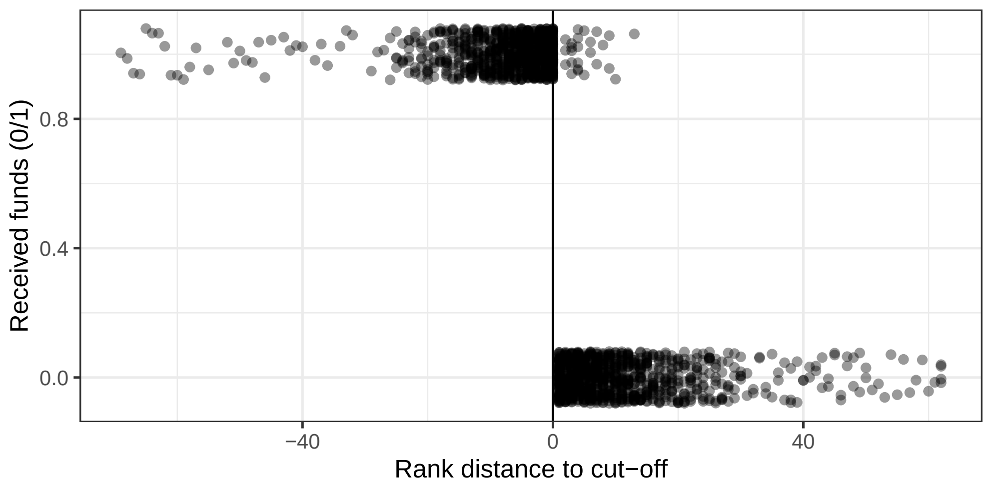


Notes: The plot shows adjusted treatment effect estimates for each value of θ with 95% Anderson–Rubin confidence intervals. θ represents the direct effect of the instrument (ranking of KALAHI project proposal) on the outcome () that does not occur through the treatment. An effect of -0.08 percentage points would render the treatment effect statistically insignificant at the $\alpha = 0.05$ level.

K Results: Effect of Funding (Fuzzy RDD)

Figure A15 plots the centered rank (around the funding cut-off) against the probability of receiving funding. The funding cut-off was calculated from the ordinal rank, $1 \ldots n$, and takes on the value of 0 once a project was not funded. We can see that there a few cases where projects still received funding even though there were projects with a lower rank that were not funded.

Figure A15: Plot: Centered Rank, Cut-Off and Funding Decision

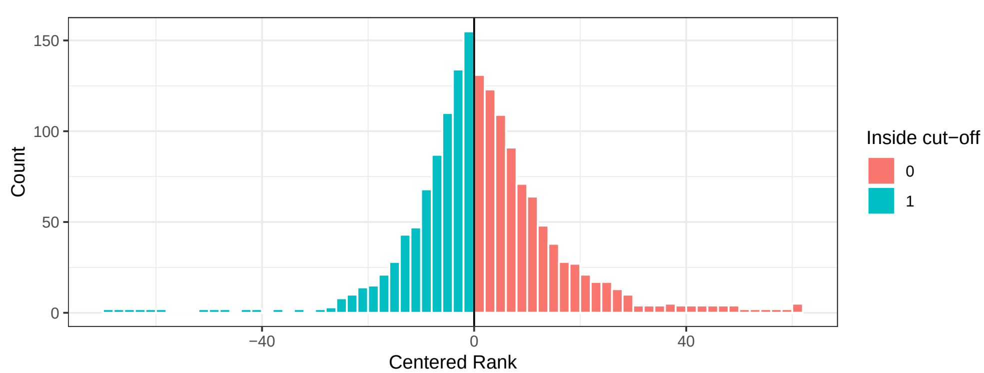


K.1 Checking Assumption

K.1.1 Density test

First, I inspect the distribution of the running variable. Figure A16 displays the histogram of running variable. From visual inspection, we can see that the distribution looks overall symmetric, but that there is a somewhat bigger mass at the left of the cutoff.

Figure A16: Histogram Running Variable, Full Sample.



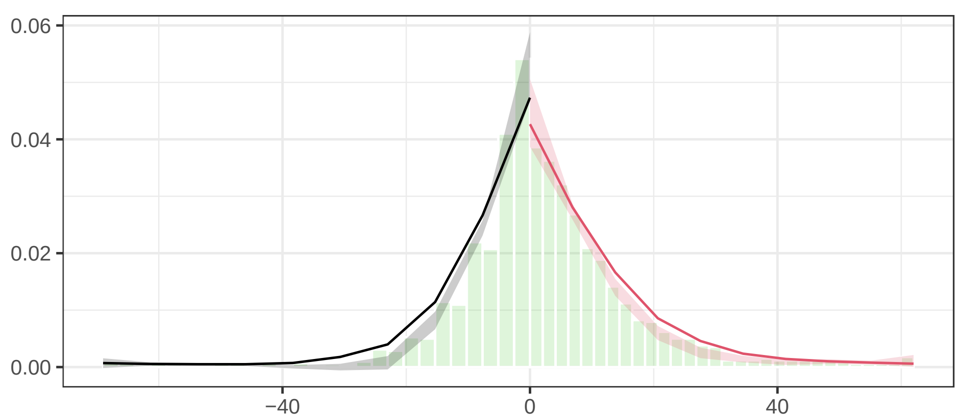
To test this notion more formally, Table reports the discrete-adjusted density-continuity test of Cattaneo et al. (2020) for the centred rank Z_{im}. For each bandwidth–either estimated by the MSE rule or fixed at ±5, ±10, and ±20 rank points- the table displays the total effective sample size (sum of the "effective" observations on either side of the cut-off), the jackknife-robust test statistic, and the associated p-value. In every case $p > 0.05$, providing no evidence of a discontinuity in the density of Z_{im}. However, we see that global p-value is just above $p < 0.1$, giving some evidence for manipulation.

Table A15: Density-discontinuity tests for Z_{im} at the funding cut-off

Window	Bandwidth	Effective N	T-stat	p-value
Global (h est.)	23	1492	-1.433	0.152
Fixed ±5	5	709	0.571	0.568
Fixed ±10	10	1103	-1.095	0.273
Fixed ±20	20	1445	-1.450	0.147

Figure A17 shows the estimated density of the centred project rank, $\tilde{r}_{im}$, using a local–polynomial density estimator (epanechnikov kernel, jackknife VCE). The solid lines on either side of the cut-off at $\tilde{r} = 0$ represent separate quadratic fits to the histogram bars. The near-perfect overlap at the cut-off indicates no evidence of systematic bunching or manipulation of the rank around the funding threshold.

Figure A17: Discrete-adjusted density test for rank at the funding cut-off.



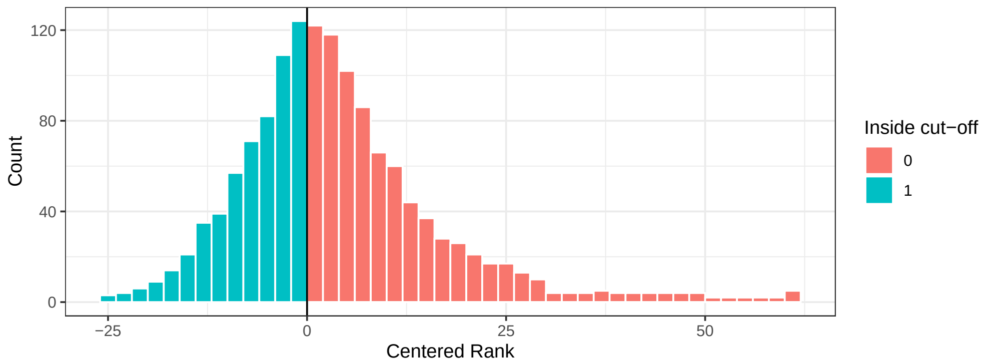
The test above included cycles where all projects were funded (see Appendix C.2), which could lead to bunching. Table A15 reports the discrete-adjusted density-continuity test of Cattaneo et al. (2020) for the centred rank Z_{im}, limited to municipal forum–cycles that include at least one funded ($\tilde{r}_{im} \leq 0$) and one unfunded ($\tilde{r}_{im} > 0$) village. I present the MSE-optimal bandwidth, the total effective sample size (sum of effective observations on each side of the cut-off), the jackknife-robust test statistic, and its p-value. The high $p = 0.849$ confirms no evidence of density discontinuity–even among those cycles capable of identifying a jump–thereby reinforcing the unconfoundedness of the rank instrument. Appendix **??** reports the results of the Fuzzy RDD for this sub-sample.

Table A16: Density-discontinuity test for Z_{im} in cycles with both funded and unfunded villages

Subset	Bandwidth	Effective N	T-stat	p-value
Valid cycles only	23	1290	0.148	0.882

Figure A18 displays the histogram of running variable. We can also detect no sign for manipulation around the cutoff.

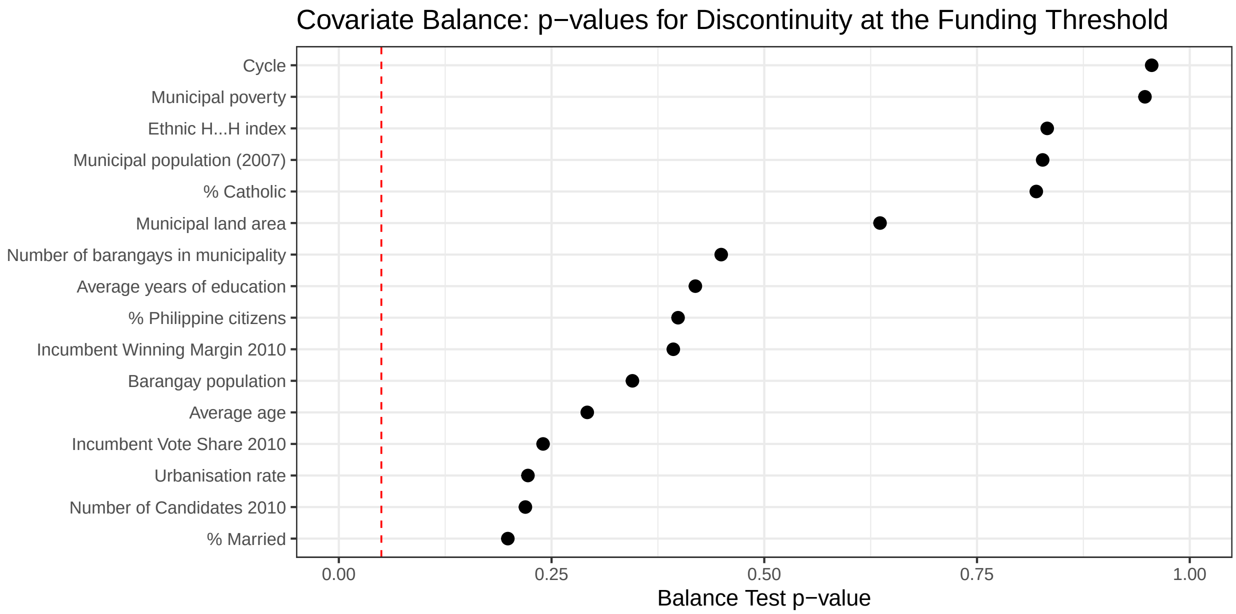
Figure A18: Histogram Running Variable, Valid Cut-Off Sample.



K.1.2 Covariate Balance P-Values for Discontinuity

Figure A19 plots the p-values from sharp-RD continuity tests for fourteen pre-treatment village- and municipality-level covariates. All p-values exceed 0.05 (the smallest is 0.113 for percentage married), indicating that none of these baseline characteristics exhibits a statistically significant jump at the cut-off. This supports the assumption that there is no confounding.

Figure A19: Covariate Balance P-Values.



K.2 Results: Valid Cut-off

The results below are based on cycles that included both funded and unfunded projects (N=1580). The point estimates are somewhat larger as well as the confidence intervals. However, there is not substantive change in the results.

Table A17: Fuzzy RDD Estimates of KALAHI Funding on Re-election Rates, Valid Cut-Off

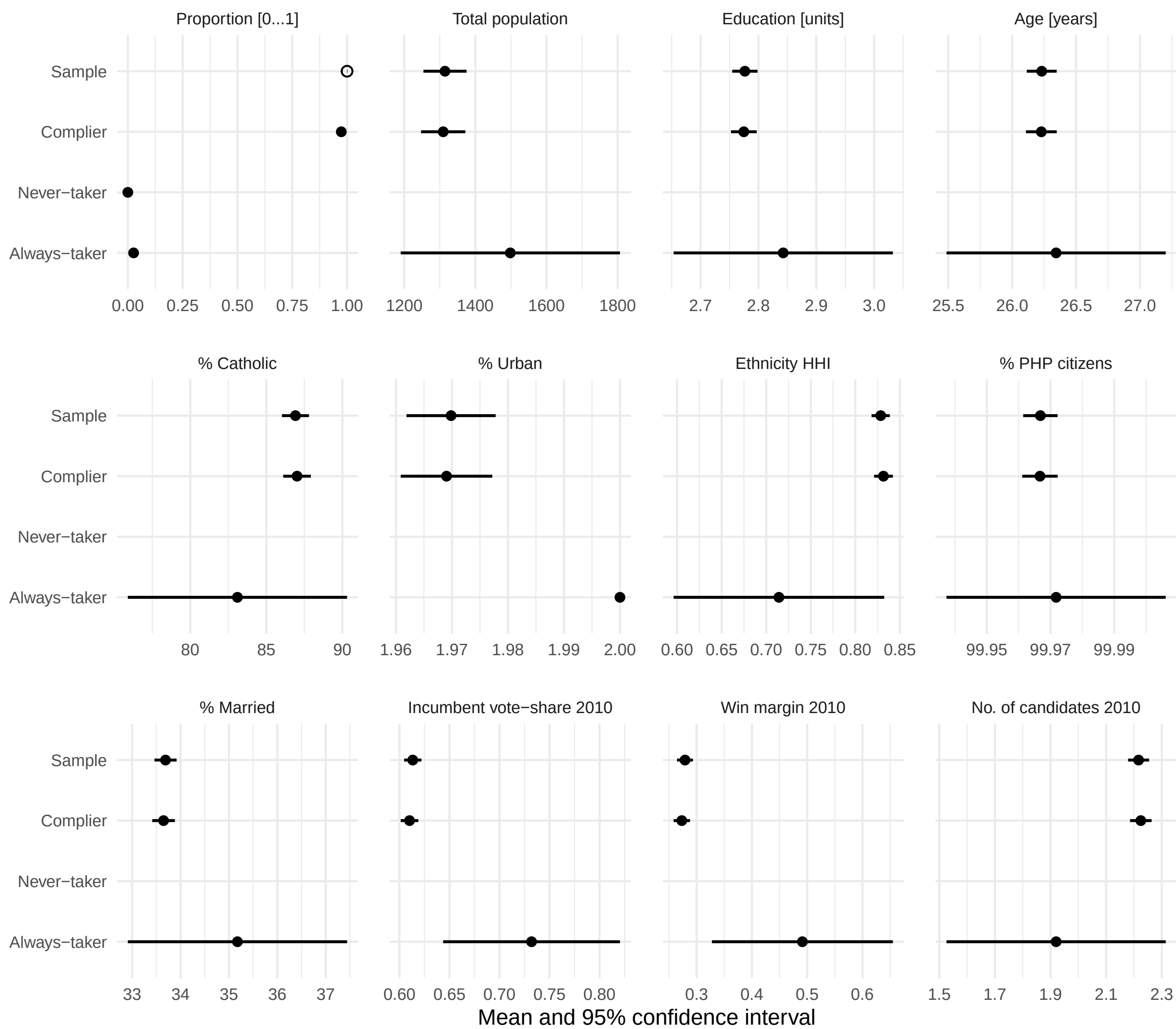
	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)
Conventional	0.188	0.192	0.204	0.239*	0.233*
	(0.233)	(0.221)	(0.167)	(0.082)	(0.086)
Bias-Corrected	0.198	0.202	0.224	0.273**	0.267**
	(0.209)	(0.200)	(0.130)	(0.048)	(0.050)
Robust	0.198	0.202	0.224	0.273	0.267
	(0.311)	(0.301)	(0.225)	(0.109)	(0.109)
N	1580	1580	1580	1580	1580
N.effective	895	895	895	895	973
Cycle fixed effects		✓	✓	✓	✓
Clustered SE			✓	✓	✓
Census Controls				✓	✓
Election Control					✓

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

[a] Notes. Sample is subset to ranking cycles with funded and unfunded projects. This table reports the results of recieving KALAHI funding on re-election rates. Each column presents the results from from a Fuzzy RDD analysis. The running variable is the village's MIBF rank centred at the municipality-specific budget cut-off. "Conventional" is the raw local-linear estimate with its usual SE; "Bias-corr." adjusts the point estimate for small-sample bias but keeps the same SE; "Robust" combines the bias-corrected estimate with heteroskedasticity-robust, nearest-neighbour SEs. Column 3 adds cycle fixed effects. Column 4 additionally controls for municipality and village characteristics. Column 5 additionally controls for political village characteristics. Municipal: poverty-incidence index (PI), number of barangays, land area (squared), log total population. Village: average education (years), average age, share urban, ethnicity HHI, % Philippine citizenship, % married. All specifications use an Epanechnikov kernel with MSE-optimal bandwidth selector (msesum). Political: incumbent vote share and margn in previous election (2010) and number of candidates running. Standard errors are heteroskedastic; columns 3–5 are clustered at the municipality level.

K.3 Profiling Compliers and Noncompliers

Figure A20: Profiles of Complier and Noncomplier Subpopulations, Funding IV



Notes: Descriptive statistics (mean and 95% bootstrap confidence intervals) for the complier and noncomplier subpopulations in the funding assigment. Subgroup shares appear in the first panel; subsequent panels show village population, education, age, religious and urbanization measures, ethnicity concentration, citizenship and marital rates, and 2010 electoral characteristics..

K.3.1 Checking Assumptions

Table A18: Means (and 95% bootstrap SEs) of covariates by compliance strata (funding IV)

Variable	Always-taker	Never-taker	Complier	Sample
Proportion [0–1]	0.03 (0.01)	0.00 (0.00)	0.97 (0.01)	1.00 (0.00)
Total population	1498.32 (157.25)	NaN (NA)	1309.63 (31.81)	1314.60 (30.95)
Education [units]	2.84 (0.10)	NaN (NA)	2.77 (0.01)	2.78 (0.01)
Age [years]	26.34 (0.44)	NaN (NA)	26.23 (0.06)	26.23 (0.06)
% Catholic	83.10 (3.68)	NaN (NA)	87.02 (0.46)	86.92 (0.45)
% Urban	2.00 (0.00)	NaN (NA)	1.97 (0.00)	1.97 (0.00)
Ethnicity HHI	0.71 (0.06)	NaN (NA)	0.83 (0.01)	0.83 (0.01)
% PHP citizens	99.97 (0.02)	NaN (NA)	99.97 (0.00)	99.97 (0.00)
% Married	35.18 (1.16)	NaN (NA)	33.65 (0.12)	33.69 (0.12)
Incumbent vote-share 2010	0.73 (0.05)	NaN (NA)	0.61 (0.00)	0.61 (0.00)
Win margin 2010	0.49 (0.08)	NaN (NA)	0.27 (0.01)	0.28 (0.01)
No. of candidates 2010	1.92 (0.20)	NaN (NA)	2.22 (0.02)	2.22 (0.02)

L Further Results

L.1 Alternative Outcome Measure

M Field Evidence

To complement the quantitative analysis, I conducted semi-structured interviews in October 2019 with officials and residents from five *out-of-sample* barangays. Although the conversations post-date the focus of the study (2010-2013), the institutional mechanism of KALAHI–CIDSS (mobilisation assemblies, inter-barangay ranking, municipal sign-off) have remained the same across cycles and municipalities. Therefore, the interviews are informative about the decision rules and incentives that governed the study villages. In the notes below, Q refers to questions but the researcher and A refers to answers. The following topics are discussed:

- The role of village captain during mobilsation
- Deals among villages and project funding
- Citizen Knowledge About KALAHI
- Citizens perceived responsibility for subproject is selection

M.1 The role of village captain during mobilsation

M.1.1 Interview with village captain

Q: [Bilang kapitan ho, ano ho yung tungkulin niyo doon sa Kalahi?] As captain, what is your role in KALAHI?

A: [Ay ako kasi, nung unang una, nung first cycle pa lang na ipinakikilala yung Kalahi, talagang kasi yung barangay namin napakaliit ng IRA saka talagang mahirap yung barangay namin. Sabi ko, talagang ito kailangan naming tutukan to kasi eto yung pagkakataon, baka ito yung makatulong sa amin kaya talagang nagpursigi ako. Ako talaga umiikot ako sa barangay, nagpapaliwanag ako na iaccept natin tong programa na to. Kaya hindi naman kami nabigo. Sa totoo lang, yung mga barangay assembly na hinihinging 80% eh mas mataas pa kami dun. May barangay assembly pa kami na umabot ng 96%] I decided to participate because to begin with, our IRA is really small and our barangay is poor. I really gave much effort and focus because this was an opportunity that would be helpful to the barangay. I really went around the barangay, convincing people to accept this program. And we weren't disappointed. The barangay assemblies require 80% attendance, and we easily exceed that. Sometimes we even reach 96% attendance.

Q: [Wow, nagbabahay-bahay po talaga kayo?] So you really went house-to-house?

A: [Talagang nagbabahay-bahay kami. Pagka eto na, may cycle na naman na dadating at may 1st barangay assembly, hindi ako napapagod, inaaya ko yung aking mga konsehal, nagbabahay bahay kami, iniikot namin: may meeting tayo the other day, kailangan umattend kayo at napakahalaga nito para satin] Yes, we really visit houses. I make it a point not to get tired, after the barangay assembly we still visit the houses and tell them that there are more meetings and that they have to attend because this is really important.

M.2 Deals among villages and project funding

M.2.1 Interview with village captain

Q: [Yung Kalahi po kasi parang ilang taon na siyang sinasagawa. Sa tingin niyo po ba yung mga barangay, gumagawa ng kasunduan na parang, "suportahan namin yung San Manuel ngayong taon, basta next year kami naman suportahan niyo?"] KALAHI has been implemented for many years. In your opinion, do barangays make bargains and say, "I will support your barangay this year, but support our barangay next year?"

A: [Wala namang ganun. Dinadaan namin sa magandang proseso, yung talagang Kalahi] That doesn't happen. We go through a process, the one prescribed by KALAHI.

Q: [Kasi yun nga po, may mga nakapanayam kami ng iba] Yes, because we interviewed others...
A: [Parang naiinggit ganun?] What, others are envious?

Q: [Yung iba raw nakarami na, sila wala pa] Some got many, some none.

A: [Eh kasi ako, wala namang problema sa ganun. Eh pano kung hindi naman kaya nung halimbawa, kami nga nun, nanalo kami sa, anong pangalan nun, sa lineup kasi merong ganun yun] Personally, I don't have a problem with that. What if a barangay doesn't have capacity? For us, we got into the lineup because we could do it.

Q: [Priority po?] You mean you were prioritized?

A: [Priority, priority kami. Ngayon dun sa papers namin, merong mga hindi naming kayang i-submit ganon kaya kung malintikan (??) nawawala yung proyekto namin, napupunta sa ibang barangay.] Yes, we were prioritized. On the documents, if we aren't able to submit them, it's possible that we lose the project to other barangays.

Q: [Wala naman hong?] So nothing fishy goes on?
A: [Wala naman. Eh hindi nga namin kayang i-produce yung... kasi yung project naming, e di gagawa kami ng project, maaprubahan ng Kalahi. Ngayon gagawa kami ng papel para yung, konting papers, matuloy na yung kwan. Kung nagkakaron ng medyo... yung dito sa amin, kagaya yung itatayo na lang, kailangan yung right of way, mga ganun ganun, doon nangyayari yun kaya nawawala.] None. If you can't produce the documents.... Like for our project, we have to do that to get approved by KALAHI. We will fix the papers so it will push through. Sometimes there are issues, like you just have to build it but there are right-of-way issues, things like that. That's why others don't get projects.

M.2.2 Interview with village councilor

Q: [Sa tingin niyo ba, nag-uusap-usap yung mga barangay pag namimili ng mga proyektong ipa-priority? Halimbawa sabihin ng barangay, ”osige suportahan kita Tandoc, next year kami naman.” Nangyayari ho ba yun sa tingin niyo?] Do you think barangays make bargains? For example, they will support your barangay this year in exchange for your support next year?

A: [siguro sir, kasi... sa mga kapitan din yata sir may botohan din yata sila, ganun din yata yung nangyayari] It's possible because captains also vote on it. That's a possible event.

Q: [Pero posible ho yun na mag-usap yung mga barangay na ”o, tulungan kita tapos ako naman tulungan mo”?] So it's possible for barangays to make deals and help each other?

A: [Siguro sir. Kasi pag nagmimeeting yung mga kapitan, di namin... Sila-sila lang.] Yes, it's possible. When captains meet, it's just among them.

M.2.3 Interview with Secretary of Brgy. Calisitan

Q: [Pero isa ho ito sa mga pinakahuling mga tanong ko: Hindi ho ba nagkakasundo yung mga barangay na parang, o sinuportahan ka namin nung nakaraang taon, kami naman ngayon?] I'm on my last few questions. Do barangays make deals, saying “I supported you last year, it's our turn now”?

A: [Meron sir. Bago magkaroon po ng botohan, nag-uusap-usap po yung mga barangay. Gaya po namin, yung mga barangay na nakita naman namin na kailangang kailangan nila yung proyekto na yon... Nag- usap usap po kami sir, kaming mga PPT noong araw. Eh kapitan hindi naman pwedeng bumoto, yung mga volunteers lang tsaka (??). Tapos nag-usap usap pa kaming mga barangay captain ngayon na] Yes, it happens. Before the vote, barangays talk to one another. We talk, all of us representatives, we talk about the projects we think certain barangays really need. The captain doesn't really vote, just the volunteers and they make uhh....

Q: [Ganto yung diskarte?] So that's how they bargain?

A: [Na ganito yung diskarte namin.] Yeah, we discuss that it's how we'll do things.

M.2.4 Interview with Barangay Secretary

Q: [Kasi ho yung Kalahi ang implementation niya, maraming taon. Parang 2014 pa lang nagsubok na kayo. Sa tingin niyo ho ba, merong mga barangay na nakikipag-kasundo sa isat isa, tipong, priority kayo this year; next year kayo naman?] Give KALAHI has been implemented across many years, do you think barangays make deals with one another such that you are a priority one year and they are prioritized next year?

A: [Wala naman po siguro. Sa amin ah, kasi siguro kung ganon yung takbo ng programa, siguro hindi lang 1 yung proyektong mapupunta sa amin kung sakali. Kung ano man siguro yung andun, napaghati-hatian. Siguro tinitingnan talaga yung mas higit na may pangangailangan, higit na mas matutugunan yung aim ng Kalahi na maiangat yung antas ng kabuhayan ng mga tao na binibigyan ng sub-project.] I don't think that happens. Because if that were the case, we would be getting more than one project and we'd just divide the projects amongst ourselves. I think they really evaluate based on need, and that it will accomplish the aim of KALAHI to raise the standards of living of the project beneficiaries.

Q:[Kaya ang batayan ho talaga ay kung gaano siya makakatulong dun sa mga tao dun?] So the basis is really how it would help the people there?

A: Yes, yes.

M.2.5 Interview with Village Captain

Q: [Yung Kalahi po di ba pinapatupad sa maraming taon. Sa Municipal inter barangay forum, meron bang mga... gumagawa ng kasunduan, parang: ngayon suportahan natin Sto Cristo, sa isang taon kami naman?] KALAHI is implemented across many years. In the Municipal Inter Barangay Forum, do barangays strike agreements or deals? Like I support you this year, next year it's our turn?

A: [Hindi ho, wala ho. Patas po kami. Kung sino po yung... ika nga magtatampuhan yung mga kapitan, hindi po, patas po. Nagtatawanan pa nga kami pagka nagbobotohan na. Talagang ganun e] No, we do it fairly. The captains don't really have bad sentiments, and we even laugh about it. It's a fair process.

M.3 Citizen Knowledge About KALAHI

This section of the interview asked citizens about their knowledge about KALAHI.

M.3.1 Interview with Citizen

Q: [Ma'am baka pwede ho kayo mainterview sandali. Narinig niyo na po ba yung Kalahi-CIDSS?] Maam, can I interview you for a while? Have you hear of KALAHI-CIDSS?

A: Kalahi-CIDSS?

Q: [Narinig... pamilyar ho kayo?] Are you familiar or have you heard of it?

A: [Pamilyar kami, may ginawa dito sa amin.] Yes, we are familiar. They constructed something like that in our place.

Q: [Sa pagkakaalam niyo, kaninong programa yung Kalahi-CIDSS?] As far as you know, whose program is KALAHI-CIDSS?

A: [Hindi ko alam kung kaninong programa yung Kalahi dito.] I don't know whose program it is.

Q: [Di niyo kabisado?] You don't know details?

A: [Di ko alam ata] I don't know.

Q: [Pero sa pagkakaalam niyo po, may tungkulin ho ba si kapitan sa Kalahi-CIDSS?] But to your knowledge, does the captain have a role?

A: [Wala ata] None.

Q: [So alam niyo po konsehal lang?] So you think it's just the councilors?

A: [Tsaka yung mga volunteer] Yes, and volunteers.

Q: [Pero ano ho yung proyekto ng Kalahi sa inyo? Ito po bang kalsada?] But what is the project of KALAHI in here? Is it that road?

A: [Yung kalsada, yung diretso. Ayan tsaka ayun] That road, that one straight ahead.

Q: [Ah ito ho, yung pagkaka konkreto nitong...] I see, so the concreting of this road.

A: [Di naman ito, dati na ito eh. Yung palabas dun] Not this one exactly, but the one on the way out there.

Q: [Ahh, sa dulo ho? Pero yung Kalahi po ba, inaasahan niyo sa kapitan niyo yun, yung mga proyektong yun?] Ahhh, the one at the end? But as for KALAHI, do you expect it from your captain? As in the projects.

A: [Hindi, sila lang ata ang lumalakad nun] Not really, they're just the ones facilitating it.

Q: [Pero di niyo naman po inaasahan na makakagbigay sila ng proyektong ganun?] But you don't expect that they will give you projects?

A: [Hindi naman namin pag-aasahan ng ganun, na may darating na ganyan] Not really. We don't expect any to come.

Q: [Pero hindi niyo po alam kung kanino kayo dapat magpasalamat o kung sinong may gawa ng proyektong ito?] But would you know who to thank for projects like this?

A: [hindi namin alam kung sino ba may...] We don't really know the...

Q: [Pakana po? Opo] Whose idea?

A: [Basta Kalahi, yun ang alam namin. Kalahi.] KALAHI, that's what we know. KALAHI.

M.3.2 Interview with Citizen

Q: [Magtatanong lang po ako kung alam niyo yung Kalahi-CIDSS?] I'm just gonna ask if you know KALAHI- CIDSS?

A1: Kalahi?

Q: [Opo. Narinig niyo na po ba yun?] Yes, have you heard of it? A1: [Opo.] Yes.

Q: [sa pagkakaalam niyo po... mabilis lang ho, wala namang maling sagot. Sa pagkakaalam niyo po, sino po yung responsible sa Kalahi, kaninong proyekto yun sa pagkakaalam niyo?] As far as you know... and this will only be quick. As far as you know, who is responsible for KALAHI?

Whose project is it?

A1: [Di ko alam sir eh.] I don't know, Sir.

Q: [Narinig niyo na po ba yung Kalahi?] Have you heard of KALAHI? A2: [Narinig ko na] Yes, I've heard.

Q: [Sa pagkakaalam niyo po, sino po yung may pakana ng Kalahi?] As far as you know, whose idea is KALAHI?

A: DSWD

Q: [sa pagkakaalam niyo po, may tungkulin ba yung kapitan sa Kalahi?] Do you know if the captain has any role in KALAHI?

A: [Meron din po, kasama po rin siyang tumutulong] Yes, the captain also helps.

Q: [nakapunta na rin po ba kayo sa... nakasama na rin po ba kayo sa mga meeting sa Kalahi?] Have you attended any KALAHI meetings?

A: [Dito lang po samin.] Yes, just near our place.

Q: [Sa pagkakaalam niyo po, paano napipili yung proyekto na pinopondohan sa Kalahi?] How do you think the projects to be funded are chosen?

A: [Nilalaban kasi yan sir, nagbobotohan sila kung mas maganda yung... nagbobotohan sila sa proyekto na kung sinong maaprubahan] That is defended and they vote on which one's are best. They vote on which projects will be approved.

Q: [Tapos sinusuportahan din po ng munisipyo itong proyektong ito?] Does the municipality support the project?

A: [Opo] Yes

Q: [Pero ang aktwal na gumagawa ng proyekto yung munisipyo ba o barangay po?] But the actual work, is that the municipality or the baragany?

A: [parehas po] Both.

Q: [Inaasahan niyo po ba yung Kalahi sa kapitan niyo – parang, sana mabigyan niya kami ng proyekto? Parang inaasahan niyo ba sa kapitan niyo yun?] Do you expect KALAHI projects from your captain?

A: [Opo] Yes

Q: [Dahil?] Because?

A: [para gumanda yung barangay namin.] So our barangay becomes beautiful.

Q: [Sa tingin niyo po, sinong dapat pasalamatan para sa Kalahi? Di ba nakakuha ho kayo ng project dito, sino ho kayang dapat pasalamatan?] Who do you think should you thank for KALAHI? You've gotten a project, so who do you think should you be grateful to?

A: [yung... (inaudible) nilalapit-lapit yan sa gobyerno...] Umm... we go to the government for uh...

Q: [Sa munisipyo po ba, si kapitan po ba? Tingin niyo po sino?] Is it the municipality or is it the captain, or what?

A: [Sila pong dalawa] Both.

Q: [Parehas sila] So both.

A: [Tsaka yung mga istaff ng DSWD... na nasa Kalahi] And the DSWD staff for KALAHI.

Q: [Nakita niyo naman po yung kahalagahan, kumbaga naging masaya yung mga tao nung nagawa yung proyekto?] Did you see the importance of the project? Did the people appreciate the project?

A: [Parang, medyo guminhawa po yung mga dadaanan ng mga motor.] Life became easier, especially for the ones who pass by with motorcycles.

Q: [Naging masaya ho ba yung mga tao sa performance nung kapitan dati? Yung kapitan nung sinasagawa yung proyekto?] Were the people satisfied with the performance of the captain? I mean, especially when the project was being implemented.

A: [Okay naman po.] Yes, he was okay.

Q: [Kumbaga bilib kayo sa kanya?] So you believe in him?

A: [Opo.] Yes.

M.3.3 Interview with Citizen

Q: (not in the recording: Have you heard of KALAHI?)

A: [hindi ako kuwan diyan, nagda-dancing ako ah.] I'm not uhhh... Look, I am dancing (Zumba).

Q: [Hindi niyo pa ho narinig?] So you haven't heard?

A: [Naririnig ko pero (laughs)] I've heard but uhh... *laughs

Q: [Ok lang po, wala namang maling sagot. Magtatanong lang po ako ng mga tao kasi kanina po, kinausap ko na yung kapitan kaya maghahanap sana ako ng mga tigadito. Sa pagkakaalam niyo po, sino ang responsible sa programang ito – kaninong programa ito, sa pagkakaalam niyo lang?] No worries, there are no wrong answers. I talked to your captain earlier, so I'm now asking residents. To your knowledge, who is responsible for this program?

A: [Malay ko] I do not know.

Q: [Sa pagkakaalam niyo ho, may katungkulan ba yung kapitan sa Kalahi-CIDSS?] But would you know if the captain has any responsibility in KALAHI-CIDSS?

A: [Meron po siguro] I think, yes.

Q: [Meron. Alam niyo ho ba kung paano pinipili yung mga proyektong pinopondohan sa Kalahi? May alam ho ba kayo?] Yes, okay. Would you know how projects to be funded are chosen?

A: [Wala] No, I don't know.

Q: [Hindi po? Bale hindi niyo pa narinig masyado?] So you haven't heard of it much? A: [Hindi pa.] Not much.

M.3.4 Interview with Citizen

Q: [Ma'am magandang hapon po, mag-iinterview lang po sana ako tungkol sa Kalahi-CIDSS sana. Mabilis lang po. Narinig niyo na po ba yung Kalahi-CIDSS?] Good afternoon, can I just interview you about KALAHI-CIDSS? It will only be quick. Have you heard of KALAHI-CIDSS?

A:[Kalahi? Oo.] KALAHI? Yes.

Q: [Sa pagkakaalam niyo po, kaninong programa yun?] As far as you know, whose program is it?

A: [Sa amin kasi doon sa Dumaguete, Kalahi, yung ginagawa naman yung day care center.] In our place in Dumaguete, they did a bakers' center.

Q: [Sa pagkakaalam niyo po, kaninong programa yun?] So whose program you think it is? A: [Dito sa amin foreigner ang nag-ano] Well, in our place it was foreigners.

Q: [foreigner po sa pagkakaalam niyo. Kilala niyo po ba sino yung respons... yung mga taong may responsibilidad sa programa na yun – si kapitan ba, si mayor ba, si ano ba, Sa pagkakaalam niyo po?] But would you know who is responsible? Is it the captain, the mayor, or what?

A: [Parang hindi ko masyadong... (inaudible) parang syempre dadaan ng mayor yun bago...] Well, I'm not sure, but I think they go through the mayor first.

Q: [Sa tingin niyo po si mayor po?] So you think the mayor?

A: [Hindi naman po didiretso dito kundi dumaan doon sa bayan] I don't think it'll get here if it didn't go through the municipal hall.

Q: [Pero yung aktwal na pagsasagawa ng proyekto, alam niyo po ba kung sino, sino ang namumuno?] But the actual implementation, who heads it?

A: [Hindi namin alam. Ewan ko kung nag-umpisa yung... yun ba yun, yung parang ginagawa diyan?] I don't know. All the works there, I don't know.

Q: [Alam niyo ho ba kung sino yung mamimili kung sinong munisipyo o sinong barangay ang makakakuha ng proyekto, pamilyar ho ba kayo?] Would you know who chooses the eligible municipalities, and which barangays get the projects?

A: [Hindi, narinig ko lang yung napili eh ibang lugar eh] No. I just hear about those that were selected.

Q: [Pero di niyo ho alam pano napipili?] But you don't know who gets chosen?

A: [Opo, naririnig lang naming] Yeah, we just here who gets chosen.

Q: [Pero yung sa lugar niyo po, inaasahan niyo yung proyekto ng Kalahi sa kapitan niyo?] But do you expect KALAHI from your captain?

A: [Samin kasi ang kinukunan ng kwan, lupa namin eh, dinodonate lang namin. Kaya na-kwan siya, donation namin yung lupa.] In our place, we just donate the land.

Q: [Pero sa kapitan niyo ho ba, inaasahan niyo na kapag ikaw yung kapitan kailangan makakuha ka ng proyekto para samin? Hindi naman?] But do you expect your captain to get you projects?

A: [kwan na lang yun, kumbaga... foreigner kasi ang nag-kwan sa amin dun eh. Lugar naming ang binibigyan ng...] Well, in our place, foreigners handled it. We got a project.

Q: [Huling tanong lang ho, sa tingin niyo sino dapat yung pasalamatan para sa mga Kalahi projects? Kagaya diyan may ginagawa na Kalahi daw sabi ni secretary. Sa pagkakaalam niyo po, sinong dapat pasalamatan diyan?] Last question, who do you think should we thank for KALAHI projects? Like for the project there, your barangay secretary told me it's from KALAHI. To your knowledge, who should we thank?

A: [Yung Kalahi kasi sila ang...] KALAHI, because they are uh...

Q: [Pero kilala niyo po ba sinong nasa likod ng Kalahi?] But do you know who is behind KALAHI?

A: [Ahh hindi.] Ahhh, no.

M.4 Citizens perceived responsibility for subproject is selection

This evidence is corroborated by survey evidence documenting that citizens ascribed foremost influence on which *subproject is selected* foremost to village captains (79%), followed by other village officials (64%) and ordinary citizens (61%). Notably, only 24% of respondents named the municipal mayor, 10% named other officials (more than one category could be chosen) (ADB, 2012, 25).

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