Politics Without a Price: External service provision and democratic accountability

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Abstract

Democratic accountability assumes that voters reward or punish politicians for observed welfare, but public services are often delivered by non-state actors. When citizens can see benefits but not who provided them, politicians might be able to redirect state resources with little electoral cost. I argue that external provision can weaken democratic accountability by relaxing the electoral constraint that disciplines the use of public resources. I provide experimental evidence from the Philippines showing that a community-driven development program led politicians to reduce public service spending and direct procurement to political supporters. These effects are concentrated in municipalities with low political competition, where incumbents face weaker electoral pressure. I offer suggestive evidence that these political consequences attenuated the program's welfare gains. These results identify a limitation of democratic accountability: sharing service delivery between state and non-state actors can weaken constraints on both the level and distribution of public resources.

Keywords: democratic accountability; public goods; foreign aid; distributive politics

AI Disclosure: In developing and writing the paper, I used AI tools (Claude-Sonnet-5.5-2026 and GPT-5.5-2026) in a manner consistent with assistance from a research assistant. This included identifying and reviewing relevant literature, revising R code underlying the dataset construction and analysis, producing the tables and figures reported in the manuscript and supplemental appendix, and copyediting. I reviewed and verified all output these tools produced, and I assume full responsibility for the validity of the manuscript.

Democratic accountability assumes that voters reward or punish politicians for their performance, but this intuition depends on voters being able to observe not only whether welfare has improved, but who is responsible for producing it. Across much of the world, public services are delivered not only by governments, but also by aid agencies, non-governmental organizations (NGOs), churches, private donors, and community organizations that operate alongside the state and overlap with its mandates. I argue that shared service delivery creates a distinct limitation of democratic accountability. When attribution is clear, elections can both discipline incumbents and help voters distinguish more effective or responsive politicians from less effective ones. When voters can see benefits but not who provided them, politicians may be able to exploit public resources without paying the electoral price. External provision may thus weaken the accountability constraint that ordinarily disciplines the use of public resources.

This argument identifies a broader problem in the political economy of service delivery. Non-state provision is often justified as a way to improve welfare where governments are weak, corrupt, or ineffective. But bypassing the state may not bypass politics. Existing work suggests that externally provided goods can shape political credit and citizens' beliefs about the state (Cruz and Schneider 2017), while a growing empirical literature shows that weak electoral constraints can facilitate clientelism and dynastic persistence (Labonne 2016; Querubín 2016; Ravanilla, Haim, and Hicken 2022; Ravanilla and Hicken 2023; Weitz-Shapiro 2012). When external actors provide goods in domains for which citizens still hold politicians responsible, they may relax both fiscal and electoral constraints. Whether politicians can exploit that opportunity, however, depends crucially on electoral competition. In competitive settings, challengers have incentives to contest incumbent credit claims and expose the withdrawal of state effort, making external goods harder for incumbents to leverage.

I test this argument in the context of Kalahi-CIDSS (Kalahi), a World Bank-financed community-driven development program in the Philippines. Kalahi delivered block grants directly to citizen committees for small-scale development projects. Local politicians were neither financial intermediaries nor implementing agents (Beatty et al. 2018; DSWD 2024). I exploit the program's

randomized assignment across municipalities and merge impact evaluation data with administrative data on quarterly municipal spending, electoral returns, and procurement. This design provides rare causal leverage over a question that is typically studied only observationally: how does non-state service provision reshape the incentives and behavior of elected officials (Cook, Wright, and Andersson 2017; Cruz and Schneider 2017; Swaroop, Jha, and Rajkumar 2000)?

The Philippines is an especially revealing setting in which to study these dynamics. Municipal mayors exercise meaningful discretion over local public finance, giving them the capacity to alter both the level and distribution of public expenditure. Electoral competition also varies sharply across municipalities, while clientelist exchange and family-based political networks remain central to local politics (Cruz, Labonne, and Querubín 2017; Dulay and Go 2022; Labonne 2016; Querubín 2016). These features make the downstream consequences of shared service delivery particularly visible: if external goods weaken electoral accountability, Philippine mayors have both the incentive and the capacity to convert them into political capital.

I advance four central findings. First, I show that non-state service provision can crowd out public spending in overlapping domains. Kalahi significantly reduced local government expenditures in the same service sectors the program entered, and the decline was not offset by increases elsewhere in the budget. This finding demonstrates that non-state provision can reduce government effort even when financing bypasses municipal budgets entirely.

Second, I demonstrate that electoral competition conditions substitution. Where incumbents face meaningful contestation, challengers have incentives to expose retrenchment and voters have greater capacity to sanction it. Using municipal election returns, I show that crowd-out is concentrated in low-competition settings, while competitive municipalities maintain spending.

Third, I show that Kalahi increased procurement directed toward political supporters and the use of negotiated procurement. Like crowd-out, these responses were concentrated in low-competition municipalities. Together, the findings suggest that shared service delivery can weaken incentives to maintain broad public provision while giving incumbents greater latitude to prioritize politically favorable constituencies.

Fourth, I offer suggestive evidence on the welfare consequences of these responses. Reanalyzing survey data from the impact evaluation, I examine whether political competition shaped Kalahi's development outcomes. I show that Kalahi's welfare gains were significantly attenuated where mayors faced less competition. This pattern suggests that the political consequences of external service provision can reverberate back onto the effectiveness of the interventions themselves.

This paper contributes to the study of democratic accountability by identifying a limitation that arises when service provision is shared between states and non-state actors. I show that when citizens can observe welfare but not who provides it, government performance no longer maps cleanly onto electoral sanction, weakening constraints on the use of public resources. Furthermore, I link the fungibility literature to distributive politics by examining both whether public spending declines and how municipal resources are reallocated (Golden and Min 2013). External provision can reduce public service expenditure while encouraging more politically selective provision. This argument applies to settings beyond foreign aid, including those in which churches, charities, NGOs, and community organizations operate in domains that citizens associate with the state.

These findings also suggest that the effects of external provision depend not only on what is delivered, but on how these interventions reshape political incentives. Scholars often treat external provision as a technical or administrative substitute for weak governance, estimating impact while ignoring local politics (Beatty et al. 2018). The results here suggest that this dichotomy is false. External interventions do not operate in isolation: they can alter the political environment, and their effects can be shaped by it in turn.

The paper proceeds as follows. "Theory" develops a theoretical framework. "Data and Empirical Strategy" describes the Kalahi program and details the empirical strategy. "External Provision and Public Spending" reports the effects on municipal spending and examines how electoral competition conditions these responses. "External Provision and Distributive Politics" examines political targeting and discretion in municipal procurement. "Welfare Consequences" offers sug-

gestive evidence on how competition conditions Kalahi's welfare gains. The final section concludes.

Theory

Democratic accountability depends on a basic informational condition: voters must be able to observe changes in their welfare and determine whether elected officials are responsible for them (Fearon 1999; Ferejohn 1986; Przeworski, Stokes, and Manin 1999). When attribution is clear, elections can both discipline incumbents and help voters distinguish more responsive or competent politicians from less effective ones. When attribution is opaque, that informational link weakens. Citizens may continue to observe benefits while losing the ability to judge what their government actually contributed.

Shared service delivery introduces such a problem. Across much of the developing world, public goods are provided not only by governments, but also by aid agencies, NGOs, and community organizations. Villages receive water purification or electrification projects installed by a non-profit while those in other parts of the country enjoy clean drinking water and electricity provided by a government utilities system. Households benefit from cash transfers, literacy campaigns, or health interventions from international donors, while governments fund similar initiatives through local budgets. Citizens can observe that welfare has improved, but they cannot always tell whether those gains were produced by the state or by an external actor.

This shared domain of service delivery weakens the relationship between incumbent performance and electoral outcomes. When non-state actors supply goods, vote choice no longer maps cleanly onto public effort. Without the ability to attribute credit for the goods they receive, voters may reward incumbents who did not actually deliver them. Politicians can thus benefit from visible welfare without bearing responsibility for producing it, rendering poor performance less legible and easier for incumbents to survive electorally.

One implication is fiscal substitution. External provision frees incumbents from having to

finance visible welfare themselves, allowing them to pull resources from public services without paying the full electoral price. Prior work demonstrates that aid delivered to governments can crowd out public spending and substitute for local tax collection (Chatterjee, Giuliano, and Kaya 2012; Deserranno, Nansamba, and Qian 2025; Feyzioglu, Swaroop, and Zhu 1998).

A second implication concerns which constituencies benefit from municipal resources. As external provision sustains visible public benefits, incumbents may face less electoral pressure to maintain broad municipal provision and greater latitude to direct projects toward constituencies that sustain their political coalitions. In the Philippine context, where incumbents entrench power through clientelistic benefits, public goods can help maintain established coalitions when projects are directed toward political supporters (Cox and McCubbins 1986; Cruz, Labonne, and Querubín 2017).

Electoral competition is one factor that influences whether politicians can exploit external public services. It can do so through two mechanisms. First, shared service delivery weakens accountability only if voters are left unable to distinguish external provision from government performance. In competitive settings, challengers have incentives to contest incumbent credit claims and expose the withdrawal of state effort, making responsibility for visible welfare harder both to appropriate and to obscure. Second, competition changes the coalition incumbents must maintain. Where electoral survival depends on broader and more cross-cutting support, the value of programmatic provision is higher and the return to narrow appropriation is lower (Burgess et al. 2015; Ferraz and Finan 2011; Harding and Stasavage 2014; Lindbeck and Weibull 1987; Lizzeri and Persico 2001). Where competition is weak, by contrast, that ambiguity is more likely to persist, and incumbents can rely more heavily on narrower political networks. The same external intervention can therefore generate different political equilibria: programmatic accommodation where competition is strong, political appropriation where it is weak. I demonstrate that competition conditions both fiscal substitution and political targeting, as well as Kalahi's impact on welfare.

This argument yields three testable expectations. First, non-state service provision should

crowd out public spending in overlapping domains by allowing incumbents to substitute away from programmatic provision. Second, external provision should shift the distribution of public resources toward particularistic benefits. Third, electoral competition should limit both fiscal substitution and political targeting. The analysis that follows evaluates these expectations using data from the randomized rollout of Kalahi in the Philippines.

Data and Empirical Strategy

Established in 2003, Kalahi aims to reduce poverty and increase local participatory governance in the Philippines. Kalahi delivers grants to qualifying municipalities for small-scale community development projects, such as farm-to-market roads, health stations, and irrigation and drainage systems.[1] Communities determine how to allocate funds democratically in order to meet their particular needs. In addition to financing local development projects, Kalahi trains community members in choosing, designing, and implementing projects.

Unlike intergovernmental transfer programs in which funds flow through municipal accounts, Kalahi delivered block grants directly to community committees at the village level. Municipal governments were neither financial intermediaries nor implementing agents. Facilitators from the Department of Social Welfare and Development (DSWD) supported communities in project selection and design, but disbursement bypassed the municipal budget entirely. This structure is common to community-driven development programs globally.

Although Kalahi bypassed municipal budgets and was implemented through citizen commit-

1. Municipal poverty rate is the predominant qualification for Kalahi. In the Philippines, municipalities are rated on a class scale of 1 (wealthiest) to 6 (poorest). In order to receive Kalahi, 4th-6th class municipalities must have a poverty incidence above the national average of 26.5%, based on the 2009 Small Area Estimates (SAE) of the National Statistical Coordination Board (NSCB). 1st-3rd class municipalities qualify if they have a poverty incidence of 40% or higher (DSWD 2024).

tees, beneficiaries associated the projects with local government provision. In Macatunao, Iloilo, Kalahi helped replace a dangerous route used by schoolchildren with a foot trail and bridge. Residents who had once watched children climb rocks and cross the shoreline to reach school expressed gratitude that "the government" had transformed their lives (DSWD 2013). In Kapatagan, Lanao del Norte, residents were similarly reported as "thankful to the government" for Kalahi projects, including solar street lights that made nighttime movement safer and a concreted access road that helped farmers transport coconut and corn (Antonio 2022). In Barangay Busog, an isolated Iraynon Bukidnon community in Antique, Kalahi financed an 820-meter pathway and 100-meter hanging footbridge that connected residents to markets and basic services. A community volunteer thanked "the government" for bringing a project to a village that had long been difficult for public agencies to reach (Griño 2017). These accounts demonstrate an attributional problem: when citizens benefited from external public services, they frequently credited them to the government.

Reporting also suggests that public officials understood Kalahi as a substitute for government services. In Maramag, the vice mayor reported that initial resistance gave way to appreciation once the municipality saw that Kalahi projects became "projects that we have" (Soldevilla 2022). In Calayan, Mayor Joseph Llopis described the program as "very timely" because municipalities "could not cater to all the needs of the people" (Palattao 2021). Cruz and Schneider show that Kalahi's political value was also salient to mayors: despite lacking formal control over program funds, some local politicians recognized their electoral benefits and publicly claimed credit (Cruz and Schneider 2017). These accounts reveal that political actors did indeed recognize external provision as both salient and substitutable with municipal responsibilities. Taken together, the qualitative record increases confidence that my results stem from the mechanism I theorize. Mayors, aware that Kalahi could sustain visible welfare and cognizant that voters may credit them regardless, could reduce broad service spending while directing procurement toward political supporters.

Beginning in 2011, Innovations for Poverty Action (IPA) conducted an impact evaluation of

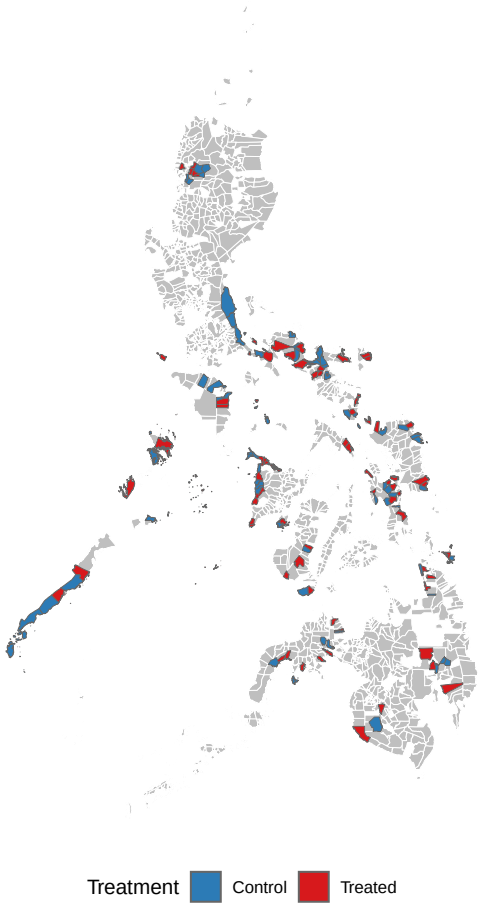
Kalahi aiming to determine the effects of the program on key socioeconomic and governance outcomes via a randomized controlled trial (RCT). The impact evaluation spanned 198 municipalities across all three of the main island groups of the Philippines (Luzon, the Visayas, and Mindanao). Treatment was assigned via a series of public lotteries conducted within provinces, with municipalities matched into pairs immediately prior to each lottery on four pre-treatment characteristics: poverty incidence, population, land area, and number of barangays (villages), the smallest administrative unit in the Philippines. Within each matched pair, one municipality was randomly assigned to treatment and one to control, yielding 99 treatment-control pairs across 26 provinces (Beatty et al. 2018). All specifications exploit this structure directly, using matched pair fixed effects to absorb time-invariant across-pair heterogeneity consistent with the randomization design.

Kalahi was implemented through repeated Community Empowerment Activity Cycles (CEACs), each of which generally took nine to twelve months and encompassed social preparation, project selection, funding, and implementation. The first cycle began with social preparation in 2012, with funded subprojects beginning implementation in December of that year. Subsequent cycles continued through the study period, although the exact timing and number of cycles varied across municipalities. In each cycle, the funding allocation for each municipality was calculated as PhP 450,000 times its number of barangays, the level at which citizen committees were organized. While all barangays in a participating municipality were eligible to propose projects, funds were allotted to prioritized subprojects until the municipal allocation was exhausted. As a result, roughly one-third of barangays received funded subprojects in a given year, with some receiving multiple projects across cycles and others receiving none.[2] Figure 1 displays a map of

2. For a detailed review of selection criteria for the IPA impact evaluation, see Beatty et al. 2018. More information on Kalahi projects can be found on the website of the Department of Social Welfare and Development (DSWD) of the Philippines, the national agency that now administers Kalahi (DSWD 2024).

the location of treated and control municipalities across the Philippines.

Figure 1: Kalahi Evaluation Sample, Philippines



Kalahi was found to substantially improve welfare outcomes in the original evaluation. Infrastructure projects reduced the time to key services and the cost to get farm products to market, water-related projects reduced the time and cost to obtain it, and education initiatives increased school enrollments (Beatty et al. 2018). I ask whether even a successful program that bypassed municipal politicians nonetheless came at a cost by altering the incentives under which they governed.

To evaluate whether municipal governments exploited Kalahi under these conditions, I construct an original municipality-quarter panel that combines local public finance data with elec-

toral returns and procurement data.[3] I estimate intent-to-treat effects among the IPA impact evaluation sample for the years in which grants were given to all treated municipalities (2013 through 2015), retaining all 198 municipalities under their original assignment regardless of compliance status, consistent with the evaluation's own analytical approach (Beatty et al. 2018). The core outcome for the spending analysis is per capita public service spending in Philippine pesos, which captures whether municipal governments maintained their own effort in the same broad domains entered by Kalahi.

On November 8, 2013, midway through program implementation, Typhoon Yolanda (international name Haiyan) struck the Philippines. One of the most powerful tropical cyclones ever recorded, Yolanda killed at least 6,300 Filipinos and is estimated to have cost US$13 billion in damages (World Bank 2017). Given the impact on public spending, I code a dichotomous variable for heavy damage from Yolanda, as determined by the Asian Development Bank (Asian Development Bank 2013). As the Philippine government continued recovery efforts for years after Yolanda, this variable takes the value of 1 for high-impact municipalities for all quarters in my sample following the storm.

A further consideration bearing on the analysis window concerns the KC-National CDD Pro-

3. Data on local government spending are obtained from the Bureau of Local Government Finance of the Philippines. Data on the IPA impact evaluation of Kalahi are publicly available through the Millennium Challenge Corporation Evidence Platform of the ICPSR (IPA 2019). I present all budget categories and my classification scheme for spending outcomes in the appendix. Grant allocation years and other qualitative information on implementation of the randomized controlled trial are derived from the third round impact evaluation report (Beatty et al. 2018). Data on municipalities included in the impact evaluation affected by Typhoon Yolanda are obtained from the Kalahi supplementary documentation from the Asian Development Bank (Asian Development Bank 2013). I merge these data with 2013 municipal election returns published by the Commission on Elections of the Philippines (COMELEC).

gram (KC-NCDDP), a nationwide scale-up of Kalahi formally launched in June 2014. By the time of the endline survey in 2015, 79 of 99 control municipalities had been enrolled in KC-NCDDP (Beatty et al. 2018). Because control municipalities receiving a program functionally identical to treatment face the same opportunity for substitution, KC-NCDDP enrollment should attenuate the treatment-control spending difference from mid-2014 onward, biasing estimates toward zero. Spending and procurement results using the full period should therefore be interpreted as conservative lower bounds on the true effect. To confirm this, I re-estimate spending specifications restricting the sample to 2013 through the first half of 2014, prior to KC-NCDDP's first disbursement in July 2014, and present results in the appendix. Estimates are stable in the pre-contamination window, consistent with conservative bias in the full-period results.

Treated and control groups are balanced across pre-treatment covariates and spending outcomes. I summarize pretreatment balance using a conventional balance table and assess joint balance with randomization inference. Results are reported in the appendix, along with summary statistics for all main variables.[4]

External Provision and Public Spending

I estimate the causal effect of Kalahi on public service spending with the following equation:

$$y_{it} = \tau D_i + X'_{ipt}\beta + \alpha_p + \delta_t + \epsilon_{it}$$

where y_{it} represents per capita spending in municipality i at quarter t and D_i is an indicator for Kalahi treatment. The coefficient τ captures the intent-to-treat effect on municipal spending,

4. Randomization inference was conducted by simulating 1,000 random assignments consistent with the procedure employed in the impact evaluation to compute randomization-based p-values for overall and individual covariate balance. The overall p-value is not statistically significant at conventional levels.

Table 1: External provision reduces municipal public service spending

	Public service spending	Total spending	IRA	Public service spending
Kalahi treatment	-90.082***	-88.053**	-87149.629	
	(26.318)	(32.597)	(225088.449)	
Peace Corps				16.786
				(31.106)
Matched-pair FE	X	X	X	X
Quarter FE	X	X	X	X
Specification	Baseline	Baseline	Placebo	Placebo
Control mean	659.87	862.65	18851603.47	620.68
Num. Obs.	2,361	2,361	2,367	2,361

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Spending outcomes are measured in per capita Philippine pesos (PhP), except IRA, which is measured in levels. All models include matched pair and quarter fixed effects with standard errors clustered at the municipality level.

while X_{ipt} denotes a vector of unit-specific controls. α_p and δ_t are matched pair and time fixed effects, respectively. Standard errors are clustered at the municipality level, the unit of treatment assignment. The remaining term accounts for error.

Table 1 shows that Kalahi significantly reduced municipal public service spending in the same domains targeted by the program. The estimated effect is about PhP 90 per resident per quarter. For a municipality at the sample median population, this corresponds to roughly PhP 9.5 million ($234,000) less public service spending per year.[5] This is a substantively large fiscal response: rather than simply adding externally financed projects on top of existing municipal provision, Kalahi induced local governments to withdraw a meaningful amount of their own spending from overlapping service sectors.

The estimated effect on total municipal spending is also negative, suggesting that the decline in program-relevant spending was not fully offset by observable increases elsewhere in the bud-

5. The median municipal population in the Kalahi impact evaluation sample is about 26,396. I convert Philippine pesos to dollars using the exchange rate at the beginning of the first full year in which Kalahi was implemented (2013), which was approximately 40.6 PhP per dollar.

get. While this treatment effect is smaller, a stacked-outcome Wald test fails to reject equality between it and the treatment effect on public service spending, indicating that the two estimates are not statistically distinguishable from one another. This pattern is consistent with fiscal substitution rather than simple relabeling across standard expenditure categories. Subsequent procurement results examine whether external provision also altered the political allocation of municipal resources.

A potential concern in program evaluation is that control municipalities bordering treated areas may indirectly benefit from spillovers, such as through interjurisdictional learning, shared infrastructure, or political signaling effects. I therefore re-estimate the baseline model on a subsample that excludes control municipalities bordering treated ones, leaving treated municipalities and non-border control municipalities only. I also re-estimate the same model on a subsample that excludes all matched pairs with control municipalities bordering treated ones. Appendix Table A4 shows that the crowd-out result persists in this restricted sample, suggesting that the main finding does not appear to be driven by contamination from regional spillovers.

As a placebo test, I examine the Internal Revenue Allotment (IRA), the constitutionally mandated intergovernmental transfer that finances roughly 80 percent of municipal operating income. The IRA is determined by a fixed formula based on population, land area, and income class. Because IRA allocations are computed centrally and municipalities exercise no discretion over them, treatment should have no effect on IRA receipts.[6] The estimated treatment effect is small and statistically insignificant, consistent with the placebo prediction.

6. IRA is reported in levels rather than per capita because the placebo concerns whether Kalahi affected the transfer itself, which is determined centrally by a statutory formula. A per-capita transformation is less appropriate here: the IRA formula depends on official population benchmarks and other components, including land area and income class, so dividing by a population measure would introduce variation from the denominator rather than cleanly testing whether treatment changed the transfer.

As an additional placebo test, I estimate the same specification using Peace Corps Philippines volunteer assignments as the treatment variable. Peace Corps volunteers work in overlapping service domains but do not finance community projects at a scale comparable to Kalahi. Volunteer assignments have no statistically significant effect on municipal public service spending. This null is consistent with the interpretation that crowd-out responds to external provision that can substitute for municipal services, rather than to the mere presence of an outside development program.

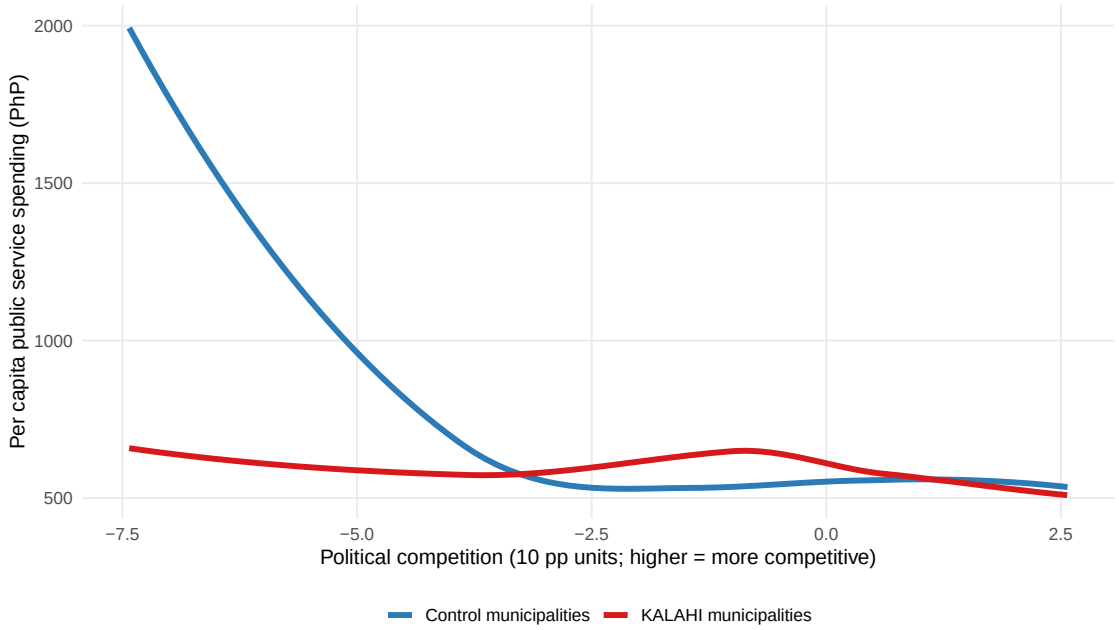
External Provision and Electoral Incentives

The preceding results reveal an average crowd-out effect, but the theory predicts that this response should depend on whether blurred responsibility remains politically exploitable. If politicians in competitive municipalities face stronger incentives to sustain visible public provision and stronger risks that retrenchment will be exposed, then crowd-out should be less pronounced where electoral competition is high. Here I test whether electoral competition conditions how mayors responded to shared service delivery.

I measure political competition using the incumbent mayor's margin of victory in 2013, immediately preceding the main period of municipal fiscal response, with narrower margins interpreted as indicating more intense electoral competition. I reorient the measure so that higher values correspond to greater competition, mean-center it, and rescale it so that a one-unit increase corresponds to a 10 percentage-point increase in competition. Figure 2 illustrates this moderating relationship descriptively by plotting public service spending against political competition by treatment assignment, with fitted LOESS regressions overlaid.

As a formal test of this difference, I estimate interaction models that include the treatment indicator and municipal political competition. Results are presented in Table 2. The coefficient on the interaction term is positive and statistically significant, indicating that Kalahi reduced public spending primarily in low-competition municipalities. The spending decline weakens sharply as competition rises, consistent with the argument that electoral contestation limits the political

Figure 2: Competition Conditions the Spending Response to Kalahi



Note: Reductions in spending are concentrated where competition is weak.

usability of external provision. Where challengers are better positioned to contest incumbent claims, mayors appear less willing or less able to substitute away from public service provision.

In substantive terms, the marginal effect is concentrated in electorally secure municipalities. At the 25th percentile of the competition distribution, Kalahi reduced public service spending by roughly PhP 157 per capita. By the median level of competition, the implied effect is essen-

Table 2: Competition constrains the fiscal response to external provision

	Public service spending	Public service spending	Public service spending	Public service spending
Kalahi treatment	-82.347***	-77.683***	-30.039	-111.298**
	(22.527)	(21.815)	(37.361)	(35.721)
Kalahi x competition	72.778**	60.670**	2.787	119.866**
	(24.012)	(20.999)	(25.410)	(38.159)
Matched-pair FE	X	X	X	X
Quarter FE	X	X	X	X
Specification	Baseline	Full controls	Term-limited sample	Re-election eligible sample
Control mean	659.87	662.16	710.12	646.97
Num. Obs.	2,361	2,301	552	1,809

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: Spending is measured per capita in Philippine pesos. Models include matched pair and quarter-level fixed effects with standard errors clustered at the municipality level.

tially zero, and by the 75th percentile it is no longer statistically distinguishable from zero. The fiscal crowd-out effect is therefore concentrated where incumbents face the weakest electoral constraints.

These results are robust to alternative measures of competition. In the appendix, I re-estimate the main interaction using several alternative constructions of electoral competition, including a standardized version of the same underlying margin measure, a within-sample rank transformation, and binary measures of competitive elections. I also report common-support and binned-effects diagnostics, as suggested by Hainmueller, Mummolo, and Xu 2019, alongside a continuous marginal-effects plot that overlays observed support and a placebo check using pre-treatment annual spending changes. Together, these diagnostics show substantial overlap in the moderator across treatment status, indicate that the crowd-out result is concentrated in the least competitive municipalities rather than in highly competitive settings, and provide no evidence that the interaction is driven by differential pre-treatment fiscal trends. Across these specifications, the treatment-by-competition interaction remains substantively similar, indicating that the main result is not driven by any single coding choice for electoral competition.

The crowd-out result is also robust to winsorization of the spending outcome, though more aggressive tail compression attenuates the estimates, consistent with some influence of the upper spending tail. Appendix Figure A5 presents the raw, 1/99, and 2.5/97.5 winsorized estimates for both the simple and interaction specifications. The crowd-out and competition moderation results also survive Lin-style adjustment, which interacts treatment with all demeaned covariates (Lin 2013). I present these robustness results in the appendix.

Given that political competition is not randomly assigned, I investigate additional plausible explanations for these findings to probe whether the results derive from political dynamics correlated with competition. In particular, I include a dummy indicator for political dynasties, a salient feature of Philippine politics known to hamper competition. I also include controls for long-run political structure, including political competition from the 2010 and 2007 municipal elections. Results are presented in column 2 of Table 2 (“Full controls”). The treatment–competition in-

teraction remains large and precisely estimated after adjusting for dynasty status and historical political structure. Crowd-out is consistent with contemporaneous electoral incentives rather than persistent political dominance.

To further investigate the mechanism through which competition constrains substitution, I explore the fiscal behavior of term-limited mayors who lack reelection incentives. In the Philippines, mayors are constitutionally limited to three consecutive terms. Term-limited mayors in 2013, having already served two prior terms, could not seek reelection and thus faced weaker electoral accountability. If competition disciplines incumbents primarily through reelection concerns, then the moderating effect of competition should disappear when incumbents are term-limited.

To test this, I estimate the baseline model on subsamples of term-limited and reelection-eligible mayors. The results support the hypothesized mechanism. Among incumbents eligible for reelection, competition strongly constrains fiscal substitution. Among term-limited mayors, that effect largely disappears. This pattern suggests that reelection incentives are the channel through which electoral competition disciplines fiscal behavior under external provision.

External Provision and Distributive Politics

The crowd-out result establishes that Kalahi reduced municipal public service spending. A remaining question is whether external provision also changed how municipal resources were allocated. If shared service delivery weakens accountability, incumbents may face less pressure to maintain broad service provision and greater latitude to politicize resources. In this section, I use municipal procurement data to examine whether Kalahi allowed mayors to target public goods to political supporters.

In the Philippine context, procurement is an especially visible and politically useful domain of such allocation (Golden and Picci 2008; Hicken 2011; Hutchcroft 2012; McCoy 1993; Stokes et al. 2013). Procurement is a particularly useful domain for observing political targeting because

it converts municipal resources into geographically traceable contracts. Unlike aggregate budget categories, procurement records identify the value and location of municipal projects. Philippine law requires local governments to publish procurement opportunities and award information (Republic of the Philippines 2003), and the resulting records include project classification, contract value, and the village of implementation, where applicable. This allows me to examine not only whether Kalahi changed the level of municipal spending, but whether it altered the spatial distribution of public contracts. I exploit this geographic granularity to test whether Kalahi caused municipalities to shift procurement toward villages that were politically supportive of the incumbent. In the Philippines, contracts in supportive villages can sustain the established local networks that mobilize voters for incumbents, offering more predictable political returns than attempts to win swing constituencies (Cruz, Labonne, and Querubín 2017).

I code outcomes that capture the political orientation of villages receiving municipal contracts and the extent of discretionary contracting. I analyze procurement awards at the contract level and construct a measure of the destination village's percentile in the within-municipality distribution of incumbent mayoral support. I use this within-municipality rank rather than raw vote share so the estimates capture political targeting relative to the mayor's own local support distribution, net of differences across municipalities in the incumbent's overall popularity. I also construct a discretionary procurement outcome indicating whether each award was made through negotiated procurement, a less competitive procurement mode that affords officials greater discretion over the award process. Together, these outcomes distinguish whether treated mayors directed contracts toward more politically supportive villages and whether they relied more heavily on negotiated procurement.

Results are presented in Table 3. Treatment is positive and significant for the political support of villages receiving municipal contracts, while the interaction between treatment and competition is negative and significant. The same pattern appears for negotiated procurement: Kalahi increased the likelihood that contracts were awarded through negotiated procedures, although the negative interaction with competition is only marginally significant. Treated mayors in low-

Table 3: External provision shifts procurement toward political supporters

	Support percentile	Support percentile	Negotiated procurement
Kalahi treatment	25.156***	18.740**	0.235***
	(6.434)	(6.773)	(0.068)
Kalahi x competition	-9.388***	-9.324***	-0.062+
	(2.321)	(2.451)	(0.032)
Matched-pair FE	X	X	X
Quarter FE	X	X	X
Specification	Baseline	Full controls	Baseline
Num. Obs.	543	543	4,281

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: All columns include pair and quarter fixed effects with standard errors clustered at the municipality level. Support percentile outcomes are defined only for contracts delivered to a particular village, while negotiated procurement is observed for all municipal contracts.

competition municipalities directed procurement toward villages that were relatively supportive of the incumbent and used negotiated procurement more frequently. In more competitive municipalities, each of these patterns is attenuated. These procurement responses are concentrated in the same low-competition settings as the spending reductions. Competition thus conditions both the fiscal and distributional effects of external assistance.

At mean competition, Kalahi increased the support percentile of villages receiving municipal contracts by roughly 25 points in the baseline specification. A ten-percentage-point increase in competition reduces this effect by roughly 9 points. The negotiated procurement result is similar in direction: at mean competition, Kalahi increased the likelihood of negotiated procurement by roughly 24 percentage points, while a ten-percentage-point increase in competition attenuates this effect by about 6 percentage points, although the interaction is only marginally significant. Political targeting is concentrated in the least competitive municipalities and attenuates sharply as electoral constraints strengthen.

Because Kalahi was randomly assigned at the municipality level, I can infer that differences in procurement behavior across treatment arms derive from randomized municipal assignment. The remaining inferential challenge is why procurement changed. External provision could jus-

tify reducing expenditure on overlapping services and undertaking smaller maintenance or complementary projects. The competition interaction weighs against a purely technical explanation, but does not by itself rule out efficient adaptation. Electorally supportive villages may also differ in need, centrality, economic activity, or ease of service delivery.

I address these differences by adding a battery of pre-treatment village-level covariates to the contract-level support-percentile specification, including log population, log land area, population density, distance to the municipal center, and a binary indicator for villages that are the municipal center. These controls help rule out the possibility that contracts flow to incumbent-supporting villages simply because those are larger, denser, more central, more urbanized, more economically active, or easier to serve. The adjusted estimate, presented in Table 3, remains positive and similarly attenuated by competition. These effects are also robust to alternative outcome constructions and winsorization, changing little under either 1/99 or 2.5/97.5 winsorization. I present alternative specifications and winsorized results in the appendix.

Taken together, these results reveal fiscal and distributional consequences of external provision. In low-competition municipalities, Kalahi reduced public service expenditure. The procurement findings identify a distributional consequence beyond aggregate fiscal withdrawal: external provision changed the political geography of municipal projects. These responses are consistent with shared service delivery weakening electoral constraints on both municipal spending and resource allocation.

These results carry a broader implication for the development literature. Aid programs, especially community-driven development programs such as Kalahi, are frequently justified not only on welfare grounds but as investments in state capacity: interventions that, by demonstrating the value of public goods provision, might strengthen the programmatic relationship between citizens and the state (Casey, Glennerster, and Miguel 2012; Mansuri and Rao 2013). The results here suggest that external provision can also reinforce particularistic allocation where accountability is weak. In low-competition municipalities, where mayors are entrenched and programmatic politics is already precarious (Hicken 2011; Keefer 2007), Kalahi reduced municipal public service

expenditure and increased procurement directed toward political supporters. Targeted contracts may reinforce incumbent networks and localized loyalty (Cruz, Labonne, and Querubín 2017; Hutchcroft 2012), even as external programs sustain visible public services.

The broader implication is that external assistance may reproduce weak political institutions by changing the incentives under which officials govern. Where responsibility for service delivery is hard to observe and electoral constraints are weak, external provision can allow governments to leverage public resources to entrench their power.

Welfare Consequences

How do political responses to external public goods shape the returns from these interventions? In this section, I re-analyze data from the Kalahi impact evaluation to examine whether these mechanisms reduced Kalahi's impact on welfare. If mayors withdraw expenditure from services in the same domains as external provision, the gains from external intervention may be attenuated.

The preceding analyses show that municipal spending and procurement responses vary with electoral competition. Where competition is weak, mayors have greater latitude to withdraw broadly distributed services and exploit public resources. Where competition is stronger, electoral pressure should constrain these responses. I therefore examine whether Kalahi's welfare returns were greater in more competitive municipalities, using the same competition measure employed throughout the paper. Shared service delivery should be most distortionary where politicians face weaker electoral pressure to maintain municipal provision.

I focus on the endline results for two reasons. First, it is the more policy-relevant horizon because it captures welfare after implementation had more time to unfold. Second, by endline most control municipalities had been enrolled in the national scale-up, so treatment assignment no longer corresponds cleanly to actual exposure and demands a different estimand. Following Beatty et al., who led the impact evaluation, I instrument realized treatment intensity with origi-

nal random assignment (Beatty et al. 2018). Specifically, I instrument treatment intensity and its interaction with competition using assignment and its interaction with competition. Intensity measures whether a village completed any subproject for a given substantive service area. Under instrumental variable assumptions, these specifications use assignment-induced variation in exposure to account for convergence between the original treatment and control groups.

I replicate the empirical strategy used in the impact evaluation, instrumenting realized treatment intensity with original assignment, and extend it by adding a competition moderator and the municipal covariates employed in the preceding analyses. The central estimand is not simply the average program effect, but whether treatment gains were greater where mayors faced stronger electoral competition. Conditioning on municipal characteristics accounts for observed differences associated with competition while retaining the assignment-based source of variation in treatment intensity. I retain matched-pair fixed effects and cluster standard errors at the municipality level.

The original evaluators divide outcomes into several substantive categories, including service access, agricultural productivity, school provision, and household economic welfare. They construct an index for each category combining relevant survey outcomes. I reconstruct these indices from the public-use impact evaluation files. I standardize component outcomes relative to the control group and average them within each index, recoding variables such that positive values indicate improved welfare. Effects are expressed in component standard deviation units.

Formally, I estimate each outcome separately:

$$welfare_{jip} = \beta_1 \widehat{D}_{ji} + \beta_2 (\widehat{D_{ji} C_i}) + \theta C_i + X'_{ji}\gamma + \alpha_p + \epsilon_{jip}$$

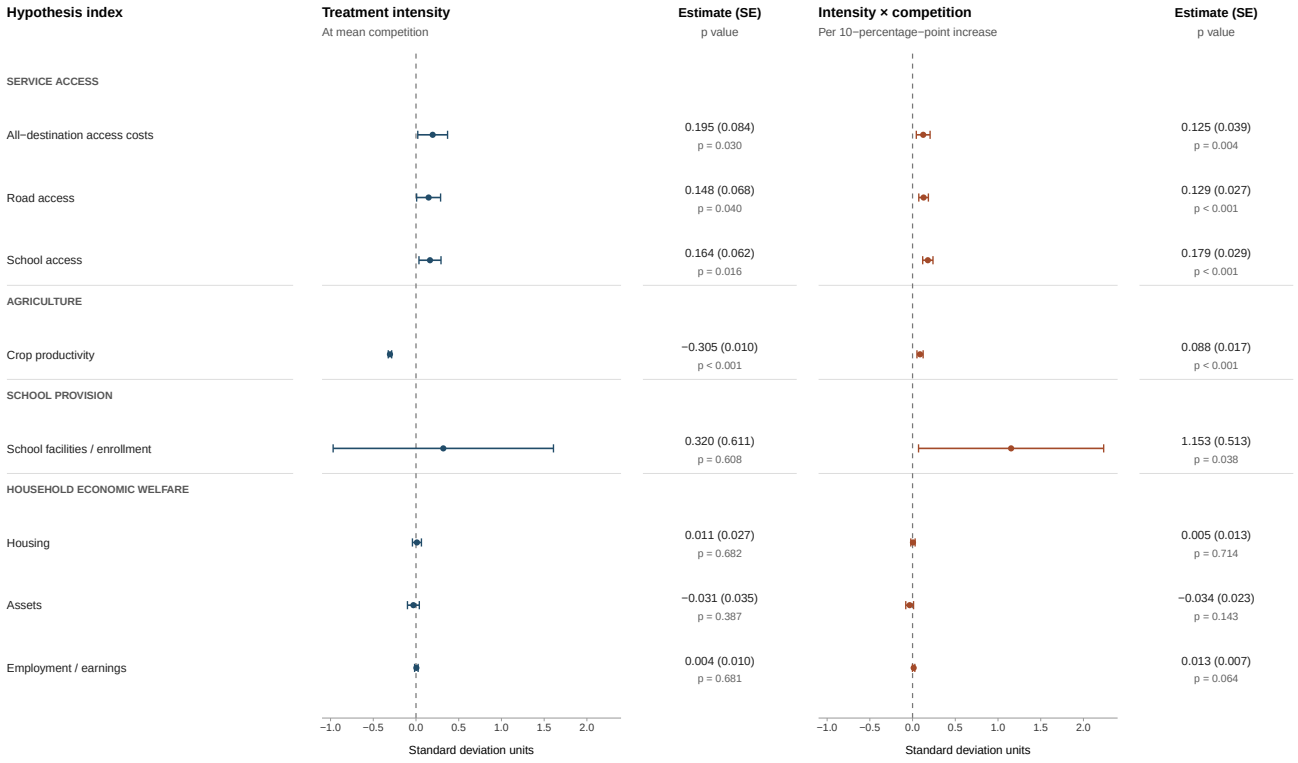
$$D_{ji} = \pi_1 Treat_i + \pi_2 (Treat_i C_i) + \pi_3 C_i + X'_{ji}\delta + \alpha_p + \nu_{jip}$$

$$D_{ji} C_i = \rho_1 Treat_i + \rho_2 (Treat_i C_i) + \rho_3 C_i + X'_{ji}\lambda + \alpha_p + \eta_{jip}.$$

Here, j indexes survey observations in municipality i and matched pair p. Outcomes are measured at the household level, except for school provision, which uses village-level observations.

D_{ji} denotes realized treatment intensity, measured at the village level and assigned to each survey observation in that village. C_i is the centered 2013 competition measure employed in prior analyses, scaled so that a one-unit increase represents ten percentage points. $Treat_i$ denotes original randomized treatment assignment. The first-stage equations jointly instrument realized intensity and its interaction with competition using original assignment and its interaction with competition. The hats denote fitted values from these separate first stages. All equations include the competition main effect and matched-pair fixed effects. The coefficient of interest is β_2, which captures whether the welfare returns to realized Kalahi exposure were greater in more competitive municipalities. X_{ji} includes municipal covariates together with the baseline outcome where included in the corresponding impact evaluation specification. I retain outcome-specific sample restrictions and module sampling weights where applicable.

Figure 3: Competition conditions welfare effects



Note: Effects of Kalahi treatment intensity on welfare and moderation by electoral competition. The left panel reports treatment effects at mean competition. The right panel reports changes in these effects for a ten-percentage-point increase in competition. Points show 2SLS estimates. Horizontal bars indicate 95% confidence intervals. Municipality-clustered standard errors appear in parentheses.

Results, presented in Figure 3, show that Kalahi's service-access gains were greater in municipalities with stronger electoral competition. At mean competition, treatment intensity improves the cumulative service access index by 0.195 standard deviation units, road access by 0.148, and school access by 0.164. A ten-percentage-point increase in competition increases these treatment effects by 0.125, 0.129, and 0.179, respectively. All main and interaction effects on service access are statistically significant at the five-percent level. The pattern is consistent: Kalahi generated larger improvements where mayors faced stronger electoral pressure.

These differences are substantively meaningful. In a municipality with competition ten percentage points below the mean, Kalahi's estimated reduction in average travel costs is approximately PhP 9. At ten percentage points above the mean, the estimated reduction is approximately PhP 45. These amounts capture the average cost of reaching the everyday destinations, including schools, health facilities, and markets. The findings suggest that the political responses documented above may have consequences for the everyday affordability of public services. Where electoral pressure is stronger, Kalahi produces larger reductions in the financial barriers households face in reaching those services. Where it is weaker, its impact is blunted.

The remaining outcomes present a more mixed picture. The school provision interaction is also positive and statistically significant, but I find no statistically significant moderation of housing, assets, or employment and earnings. The competition interaction is positive and significant for crop productivity, but the treatment effect at mean competition is negative. Thus, stronger competition attenuates an estimated loss rather than amplifying a positive effect at the mean.

The pattern across outcomes is consistent with municipal substitution offsetting the gains from external provision. Access to roads, schools, and other services depends directly on public provision in domains financed by both Kalahi and municipal governments. If mayors withdraw their own expenditure in these sectors, Kalahi projects may partly replace municipal provision, reducing the net improvement in access. Housing and asset holdings are less direct measures of municipal service provision, so any effects of substitution on these outcomes may operate through additional channels. The clearer evidence of moderation in service access therefore aligns with

the outcomes for which municipal withdrawal should most directly attenuate Kalahi's returns.

Substantively, these results suggest that Kalahi's welfare gains may have been partially offset by political responses from local governments. The findings are consistent with the spending and procurement results: where electoral pressure is weaker, government responses may attenuate the returns from external provision. The welfare analysis does not, however, identify crowd-out or procurement targeting as the cause of this heterogeneity. Competition is not randomly assigned, and the specifications do not rule out differences in implementation associated with competition. The findings nevertheless highlight the importance of considering the political context in evaluating development programs. Even well-designed community-driven programs may yield weaker gains where shared service delivery allows officials to reshape municipal provision with limited electoral consequences.

Conclusion

Democratic accountability depends on voters being able to connect observed welfare to political responsibility. This paper shows how that link can weaken when states share service delivery with external actors. When non-state programs provide goods that citizens ordinarily associate with government, welfare remains visible even as responsibility for producing it becomes harder to discern. Under these conditions, politicians may be able to benefit from the appearance of provision without bearing its full cost.

Leveraging a randomized evaluation of the World Bank-financed Kalahi program in the Philippines, I show that non-state service provision reduced municipal public spending in overlapping domains. In municipalities with weak political competition, mayors did not simply spend less. They also directed more procurement toward supportive villages and relied more heavily on negotiated procurement than on competitive bidding procedures. Where competition was stronger, these patterns were substantially attenuated. Shared service delivery thus produced different political equilibria across local democratic settings.

These findings clarify the conditions under which external provision is most likely to distort governance. The mechanism should travel most readily to settings in which non-state actors provide goods that overlap with government mandates, voters cannot easily distinguish external provision from public effort, and local officials control resources that can be redirected for political use. It should be less relevant where service provision is clearly attributable or electoral competition forces politicians to sustain broad-based provision. External provision does not necessarily erode the state. Its political consequences depend on the institutional environments into which it is introduced.

The paper also carries a broader implication for how scholars study external interventions. Such programs are often evaluated as technical responses to weak governance: they are assessed in terms of outputs, targeting, and welfare effects, while local politics remains in the background. The results here suggest that this separation is incomplete. External provision does not simply operate within political environments; it can reshape them, and its effects are in turn conditioned by them. Analyses of aid, NGOs, and community-based interventions should therefore attend not only to what these programs deliver, but to how shared service delivery alters public incentives and accountability.

These conclusions also bear on policy. Bypassing government channels may insulate aid from direct capture, but it does not prevent it from shifting the incentives of politicians. Donors and implementing organizations cannot infer success from outputs alone. They must also ask whether electoral conditions make substitution politically sustainable, whether governments maintain effort in overlapping sectors, and whether local officials possess the discretion to manipulate public resources. Where these conditions hold, external interventions may deliver impactful goods while simultaneously weakening the accountability relationship on which durable public provision depends.

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Politics Without a Price: Appendix

Table A1: Summary statistics for main variables

Variable	N	Mean	SD	Min	Max
A. Spending: municipality-quarter					
Public spending (PHP per resident)	2361	608.65	545.81	0.00	7058.59
Total spending (PHP per resident)	2361	813.08	721.65	0.00	11159.93
Yolanda exposure	2361	0.63	0.48	0.00	1.00
B. Procurement: awarded tender					
Destination support percentile	543	47.14	29.08	1.00	100.00
Negotiated procurement	4281	0.34	0.47	0.00	1.00
C. Welfare: endline outcome-specific samples					
All-destination access costs	682	0.10	0.39	-3.69	0.42
Road access	719	0.04	0.57	-6.38	1.12
School access	531	0.29	0.79	-1.56	3.24
Crop productivity	314	0.05	0.75	-0.50	6.46
School facilities / enrollment (village)	18	0.33	1.16	-1.10	4.06
Housing	1537	0.01	0.53	-1.93	1.88
Assets	2331	0.13	0.65	-0.50	15.19
Employment / earnings	3310	0.20	0.36	-1.19	9.51
D. Treatment, competition and core covariates: municipality					
Kalahi assignment	198	0.50	0.50	0.00	1.00
Competition (10pp units)	198	-0.03	2.81	-7.43	2.57
Assignment × competition	198	-0.00	2.05	-7.43	2.57
Dynasty status	198	0.10	0.30	0.00	1.00
Log population (2007)	198	10.10	0.74	7.56	11.69
Poverty incidence	198	47.30	9.40	33.67	69.09
Land area (sq. km)	198	218.06	251.65	4.68	1462.94
Number of barangays	198	24.68	14.98	4.00	95.00

Note: Outcomes use their corresponding main text estimation samples.

Table A2: Pretreatment covariate balance

Variable	Control mean (SD)	Treatment mean (SD)	Difference	Paired p	RI p
Public service spending pc	255.23 (208.43)	220.88 (120.71)	-34.34	0.063	0.065
Total spending pc	1757.68 (1309.92)	1601.99 (955.72)	-155.69	0.130	0.155
Poverty incidence	47.16 (9.57)	47.45 (9.27)	0.28	0.712	0.704
Number of villages	24.10 (13.39)	25.25 (16.46)	1.15	0.436	0.449
Population	30159.99 (21226.01)	31250.75 (19916.13)	1090.76	0.607	0.601
Land area	215.94 (259.56)	220.17 (244.79)	4.24	0.862	0.848
Joint balance	–	–	–	–	0.635

Note: Spending measures average available annual observations from 2001–2010, as treatment assignment was announced in 2011. Means and standard deviations use the same 198 municipalities in 99 complete treatment-control pairs. Conventional p-values use paired t-tests. Two-sided individual and joint randomization-inference p-values use 1,000 within-pair swaps.

Table A3: Budget categories and construction of spending outcomes

Budget category	Public	Total	Excluded
A. Current operating expenditure			
General public services	Yes	Yes	No
Education, culture, sports and manpower	Yes	Yes	No
Health, nutrition and population control	Yes	Yes	No
Housing and community development	Yes	Yes	No
Social services and social welfare	Yes	Yes	No
Labor and employment	No	Yes	No
Economic services	No	Yes	No
Debt interest and other charges	No	Yes	No
B. Non-operating expenditure			
Property, plant and equipment / capital outlay	No	Yes	No
Purchases of other entities' debt securities	No	Yes	No
Loans granted to other entities	No	Yes	No
Loan amortization	No	Yes	No
Bond / debt-security redemption	No	Yes	No
Other non-operating expenditure	No	Yes	No
C. Items outside the spending aggregates			
Prior-year accounts payable payments	No	No	Yes
Continuing appropriations	No	No	Yes
Non-income receipts (financing and asset receipts)	No	No	Yes

Table A4: Spending robustness to spillover effects

	Baseline	Standardized	Drop control units	Drop pairs
Kalahi treatment	-90.082***	-0.165***	-73.415**	-73.042**
	(26.318)	(0.048)	(25.233)	(25.180)
Matched-pair FE	X	X	X	X
Quarter FE	X	X	X	X
Observations	2,361	2,361	1,921	1,478

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses on the line below each coefficient.

Table A5: Spending results before the KC-NCDDP scale-up

	Public spending	Total spending	Competition	Competition
Specification	Baseline	Baseline	Baseline	Full controls
A. Full period				
Kalahi treatment	-90.082***	-88.053**	-82.347***	-77.683***
	(26.318)	(32.597)	(22.527)	(21.815)
Kalahi × competition			72.778**	60.670**
			(24.012)	(20.999)
Observations	2,361	2,361	2,361	2,301
B. Before scale-up				
Kalahi treatment	-80.926**	-88.554**	-72.454**	-66.460**
	(26.258)	(29.492)	(22.659)	(21.422)
Kalahi × competition			71.209**	58.385**
			(26.100)	(22.078)
Observations	1,174	1,174	1,174	1,144
Matched-pair FE	X	X	X	X
Quarter FE	X	X	X	X

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses on the line below each coefficient.
Note: Full period regressions use observations from 2013–2015. Early period regressions use observations through the second quarter of 2014.

Table A6: Robustness of spending results to alternative definitions of competition

	Margin (10pp)	Standardized	Rank	Close race	Top third
Kalahi treatment	-82.347***	-84.275***	-328.908**	-136.286**	-129.852**
	(22.527)	(22.951)	(114.781)	(42.638)	(40.433)
Kalahi x competition	72.778**	204.866**	493.979*	147.606*	142.650*
	(24.012)	(67.593)	(193.941)	(65.985)	(62.934)
Matched-pair FE	X	X	X	X	X
Quarter FE	X	X	X	X	X
Observations	2,361	2,361	2,361	2,361	2,361

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses on the line below each coefficient.

Figure A1: Distribution of electoral competition by treatment assignment

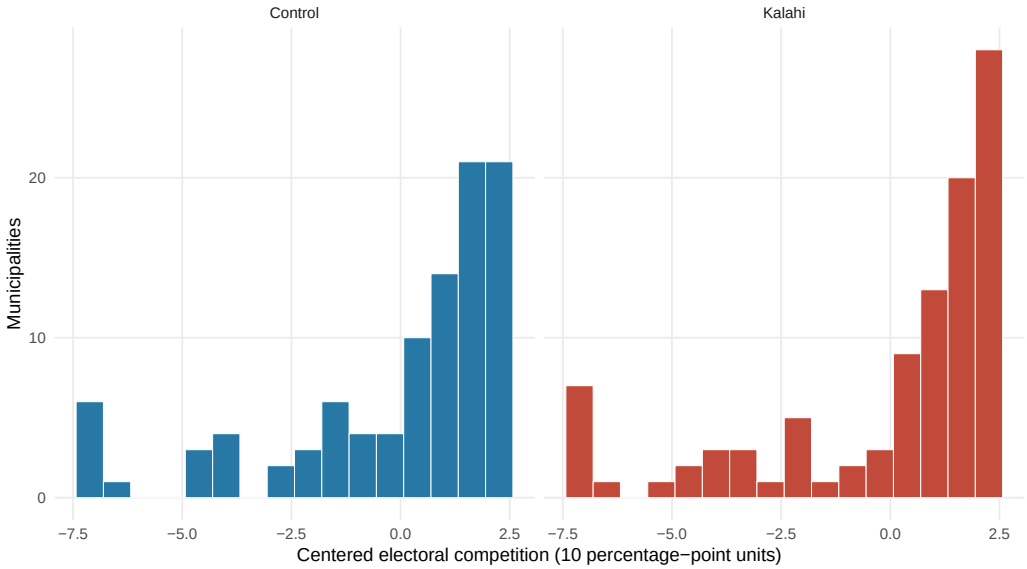
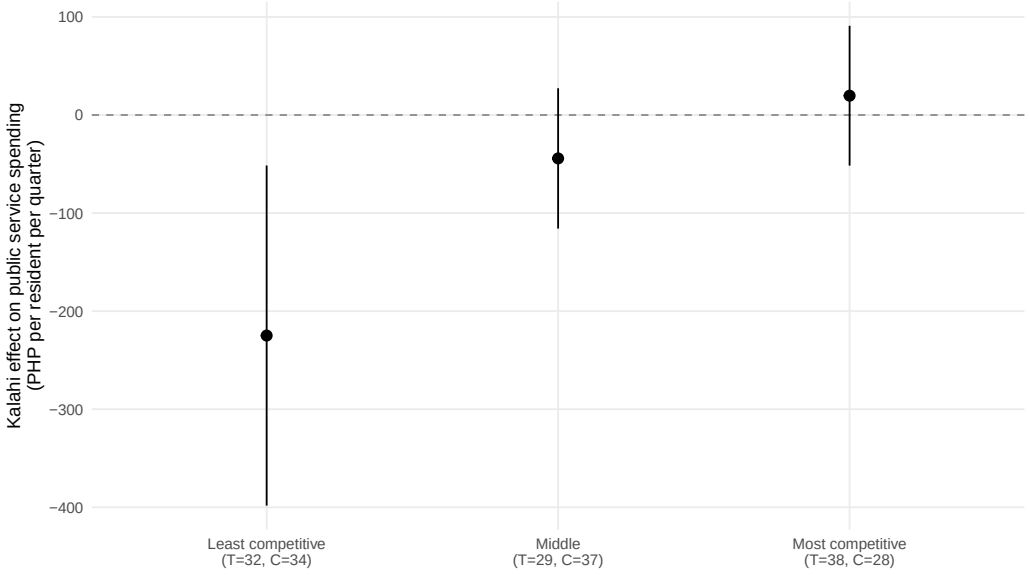
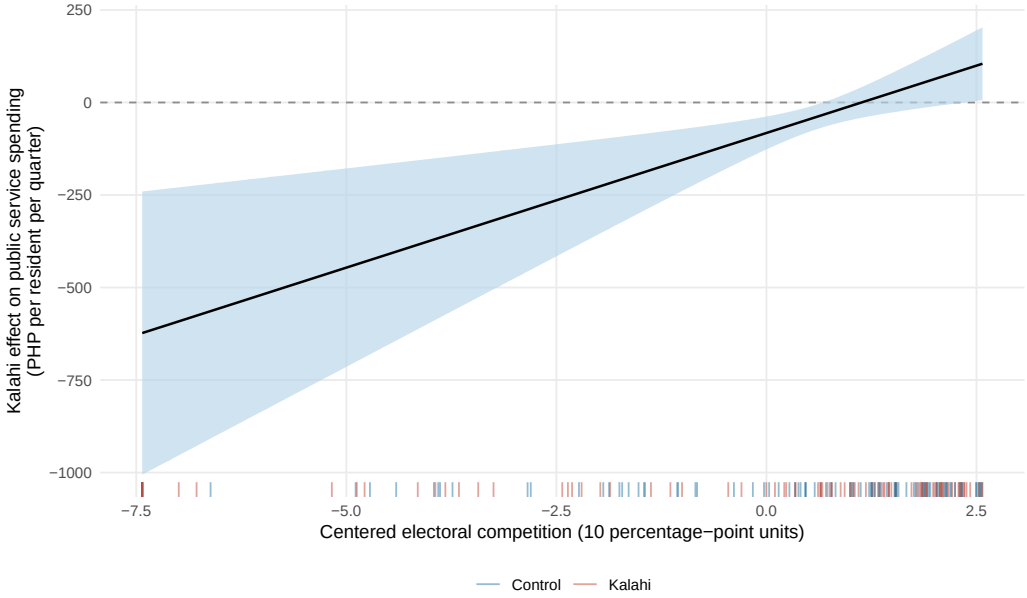


Figure A2: Treatment effects by electoral-competition tercile



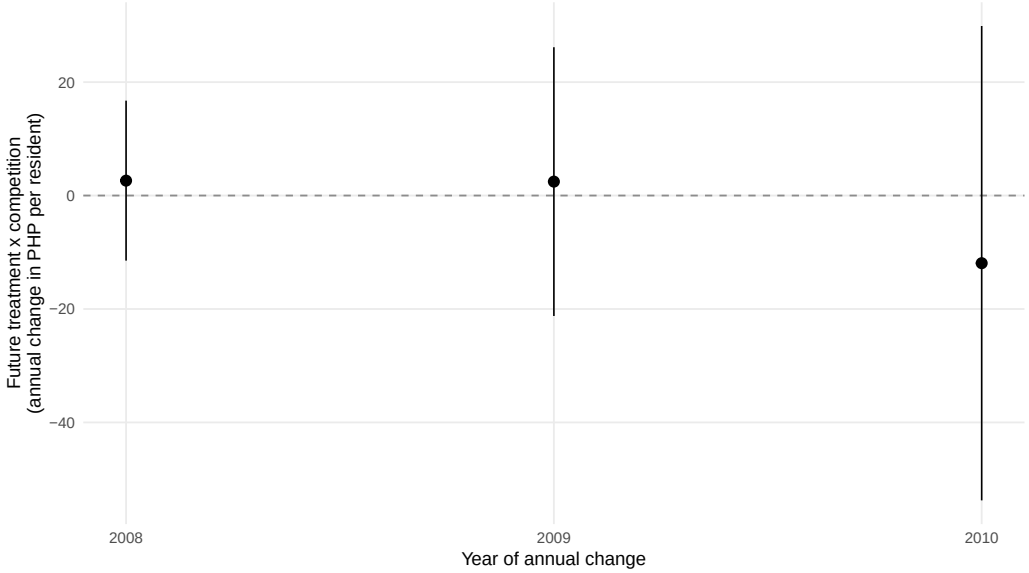
Note: Points and 95 percent confidence intervals are linear combinations from a pooled model interacting treatment with competition terciles. Standard errors are clustered by municipality.

Figure A3: Marginal effect of Kalahi across electoral competition



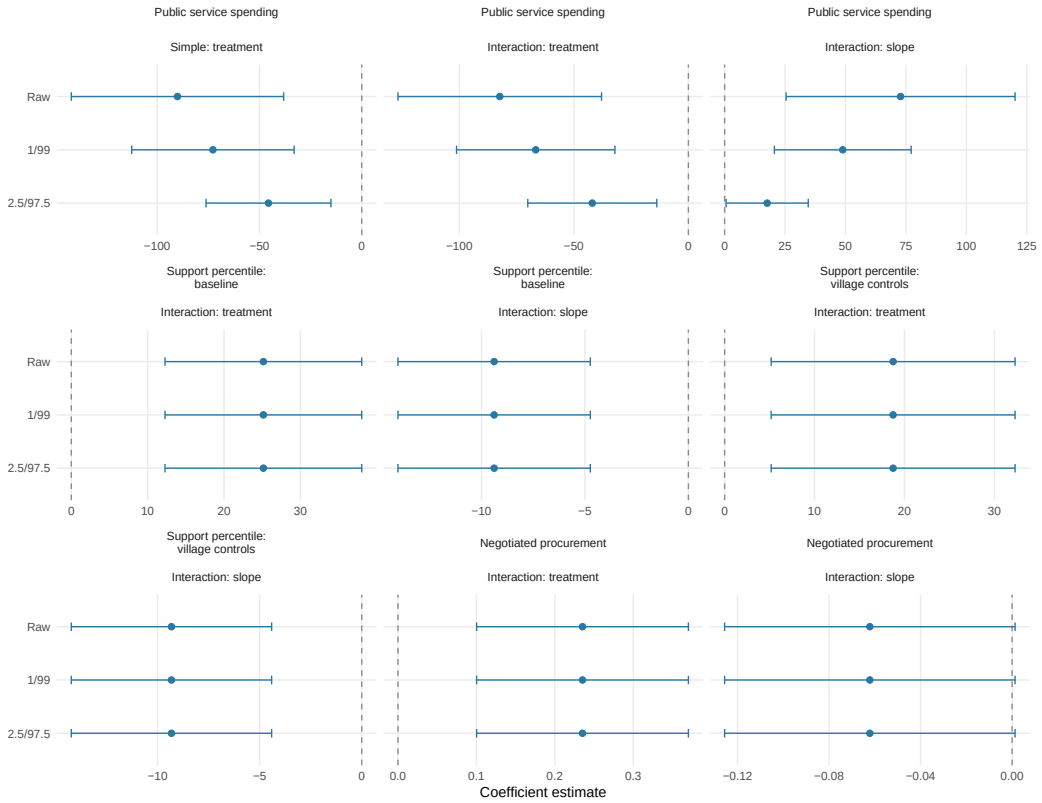
Note: The line shows the estimated treatment effect from the baseline linear interaction model. Shading indicates pointwise 95 percent confidence intervals using municipality-clustered standard errors. Rug marks show observed competition once per municipality, separately by treatment assignment.

Figure A4: Placebo moderation in pretreatment spending changes



Note: Year-specific regressions relate consecutive annual changes in public-service spending to future treatment, 2013 electoral competition, and their interaction. Only spending years strictly before the 2011 treatment assignment are included. Bars show pointwise 95 percent confidence intervals using municipality-clustered standard errors.

Figure A5: Winsorized spending and procurement results



Note: Points and 95 percent confidence intervals compare raw outcomes with winsorization at the 1st/99th and 2.5th/97.5th percentiles. Cutoffs are pooled across treatment arms on each raw model's fixed estimation sample. Models retain their main controls, matched-pair and quarter fixed effects, and municipality-clustered inference.

Table A7: Lin adjustment for spending results

	Public spending	Total spending	Competition	Competition
Specification	Baseline	Baseline	Baseline	Full controls
A. Main specification (recentered)				
Kalahi treatment	-90.082***	-88.053**	-84.275***	-78.292***
	(26.318)	(32.597)	(22.951)	(21.938)
Kalahi × competition			72.778**	60.670**
			(24.012)	(20.999)
B. Lin adjustment				
Kalahi treatment	-90.301***	-88.555**	-86.519***	-86.672***
	(24.601)	(30.991)	(22.277)	(21.584)
Kalahi × competition			63.139**	61.754**
			(19.102)	(19.429)
Matched-pair FE	X	X	X	X
Quarter FE	X	X	X	X
Observations	2,361	2,361	2,361	2,301

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses on the line below each coefficient.

Note: Estimates with municipality-clustered standard errors in parentheses. Current and adjusted specifications use identical samples and equal-municipality covariate centering. Lin adjustment interacts treatment with every demeaned covariate.

Table A8: Robustness to procurement outcome constructions

	Top quartile	Top decile
A. Baseline controls		
Kalahi treatment	0.475***	0.510***
	(0.113)	(0.089)
Kalahi × competition	-0.263***	-0.248***
	(0.044)	(0.038)
B. Village controls		
Kalahi treatment	0.393**	0.465***
	(0.113)	(0.088)
Kalahi × competition	-0.262***	-0.249***
	(0.042)	(0.035)
Matched-pair FE	X	X
Quarter FE	X	X
Observations	543	543

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Standard errors in parentheses on the line below each coefficient.
Note: Estimates with municipality-clustered standard errors in parentheses. Each outcome indicates whether an awarded tender goes to a village in the specified upper support group. Outcome units are probabilities.