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Research Article

The Incumbent Tax: A Stochastic Assessment of Pre-Election Clientelism and Fiscal Insolvency in Philippine Local Government Units

Jemar John J. Lumingkit

Author Information:

Independent Researcher, Quezon City, Philippines

Corresponding Author:

Jemar John J. Lumingkit
Email: jjjlumingkit@gmail.com

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Abstract. Political budget cycles are well documented in developing democracies, yet the fiscal tail-risk of pre-election spending in decentralized settings remains unquantified. We combine a PSGC-keyed fiscal panel of 1,714 Philippine local government units (LGUs) covering fiscal years 1992–2024 with gubernatorial electoral returns for 82 provinces (1992–2025) and mayoral returns for 146 cities and 1,472 municipalities (2007–2025). Estimating a two-way fixed-effects voter elasticity model with the incumbent's own vote share as the outcome, the regression sample covers 192 province-cycles, 268 city-cycles, and 2,174 municipality-cycles. No sectoral spending category predicts the incumbent's vote share with statistical significance at any level of government. The one variable that reliably predicts electoral outcomes is the effective number of candidates: doubling the number of effective candidates is associated with a vote-share loss of roughly 42 percentage points, and this pattern holds at all three LGU levels. Feeding these elasticities into 10,000-draw Monte Carlo simulations calibrated separately at each level on constant 2018 Philippine pesos, a three-year pre-election spending surge (realized mean 22.5 percent above baseline in each spike year) raises the one-year insolvency probability from 56.8 to 71.8 percent in provinces, from 62.0 to 72.7 percent in cities, and from 62.5 to 72.3 percent in municipalities. Over a ten-year horizon, the surge adds roughly 0.4 to 0.5 additional deficit years and lifts the 95 percent Value-at-Risk of the cumulative deficit-to-income ratio from a baseline range of 0.35 to 0.57 to 0.47 to 0.70. A spending-neutral reallocation scenario produces fiscal outcomes identical to the surge because neither the level nor the composition of spending moves votes. These results support an "incumbent tax" interpretation that generalizes across all three LGU levels: incumbents absorb a consistent fiscal cost for no measurable electoral compensation. We propose pre-election discretionary spending ceilings and mandatory fiscal stress tests.

Keywords: *Clientelism; Fiscal risk; Incumbent tax; Monte Carlo; Philippine LGUs.*

Political budget cycles — the systematic manipulation of fiscal policy in election years — are well documented in developing democracies (Akhmedov & Zhuravskaya, 2004; Brender & Drazen, 2005; Shi & Svensson, 2006). Incumbents frequently raise public spending or strategically reallocate budgets toward visible sectoral programs and human capital investments before elections to signal competence or secure political support (Cantoni & Yuchtman, 2013; Drazen & Eslava, 2010; Keefer & Khemani, 2005). In decentralized settings, these mechanisms often function as patronage distribution (Brollo et al., 2013; Manacorda et al., 2011; Robinson & Verdier, 2013), with structural consequences for fiscal sustainability (Golden & Min, 2013). Recent evidence from Southeast Asia (Syam & Malikul, 2025) and the Philippines (Capuno, 2010, 2016; Punongbayan, 2025; Sacluti, Saturnino, & Punongbayan, 2026) confirms that local executives inflate expenditures on targeted programs ahead

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of elections.
A recurring policy concern is whether this pre-election spending yields a compensating electoral benefit. If incumbents can purchase vote shares cheaply with clientelistic transfers, the practice is individually rational even if socially wasteful. If, by contrast, the electoral return to spending is negligible, the fiscal cost becomes a deadweight burden — an 'incumbent tax' paid by the constituency in the form of degraded fiscal capacity and a persistent constraint on economic development (Peltzman, 1992; Brender & Drazen, 2008; Punongbayan, 2025).

Three gaps motivate this paper. First, most studies estimate static regressions treating local budgets as deterministic outcomes, ignoring the stochastic tail risk generated by pre-election overspending (Budzen & Wisniewski, 2024; Council of Europe, 2022). Second, the Philippine empirical record is fragmented—Capuno (2010, 2016), Punongbayan (2025), and Sacluti, Saturnino, and Punongbayan (2026) document that cycles exist, but the electoral-fiscal trade-off itself has not been quantified. Third, no study has tested whether the mechanism is specific to one administrative level — provinces, cities, or municipalities — or operates uniformly across the three, despite differences in spending authority, revenue structure, and electoral dynamics.

This matters because the Mandanas-Garcia Supreme Court ruling (G.R. Nos. 199802 and 208488, 2019) expanded the internal revenue allotment (IRA) base from 2022 onward, increasing the fiscal headroom available to LGUs at all three levels. Larger transfers amplify the fiscal consequences of unconstrained pre-election spending, making it necessary to establish whether the incumbent-tax mechanism is provincial-specific or general.

This paper introduces a stochastic simulation framework that quantifies the trade-off between electoral survival and fiscal sustainability at all three Philippine LGU levels. We ask three questions. (1) Does pre-election budget expansion yield electoral gains in Philippine local elections? (2) What is the estimated fiscal cost of clientelistic spending in terms of expected deficits, insolvency probability, and tail risk? (3) Does the incumbent tax operate uniformly across provinces, cities, and municipalities, or is it level-specific?

The design proceeds in three stages. First, we estimate voter elasticities of sectoral spending using a two-way fixed-effects model, separately for provinces, cities, and municipalities. Second, we calibrate the joint distribution of local revenue and IRA as a bivariate lognormal separately at each level, with a Cholesky-induced correlation estimated from the data, and simulate 10,000 forward revenue–expenditure paths over ten years. Third, we compare three counterfactual scenarios: Scenario A (clientelistic surge), Scenario B (normative human-capital portfolio at a lower fiscal level), and Scenario C (spending-neutral reallocation at Scenario A's fiscal level but Scenario B's composition).

Construct Operationalization
We use public welfare expenditure as a proxy for clientelistic spending. This is defensible but imperfect. Public welfare spending in Philippine LGU budgets is dominated by locally funded aid programs — the Assistance to Individuals in Crisis Situation (AICS) program, senior citizen assistance, persons-with-disability transfers — that are frequently disbursed in the months preceding elections. However, welfare expenditure also includes legitimate poverty-reduction transfers that are not clientelistic. The proxy therefore likely overstates the true extent of clientelism. The limitation applies to the interpretation of the point estimates, not to the simulation's fiscal conclusions, which depend only on expenditure totals rather than their motivation.

The Incumbent Tax
We define the incumbent tax formally as the net fiscal burden that a pre-election spending surge imposes on the incumbent's jurisdiction, measured as the difference between scenario-simulated deficits (or insolvency risk) and the counterfactual, minus any electoral compensation. Grounded in empirical evidence that voters do not systematically reward pre-election deficits or spending expansions (Peltzman, 1992; Brender & Drazen, 2008) and that opportunistic spending serves as a persistent constraint on development (Punongbayan, 2025), the electoral compensation parameter in this study is estimated to be zero at every LGU level. Consequently, the incumbent tax equals the fiscal cost of the surge.

Methodology

Research Design

The study uses a quantitative, computational design that combines two-way fixed-effects panel estimation (Wooldridge, 2010) with forward-looking Monte Carlo simulation (Glasserman, 2003), applied at three administrative levels. The panel-elasticity stage identifies how sectoral spending covaries with the incumbent's vote share, holding unit and election-cycle characteristics fixed. The simulation stage propagates these elasticities through 10,000 stochastic revenue paths to characterize the distribution of fiscal outcomes under alternative spending policies.

Data Sources and Sample Construction

Fiscal panel. Fiscal variables are drawn from the Bureau of Local Government Finance (BLGF) Statement of Receipts and Expenditures (SRE), the annual financial report for Philippine LGUs, accessed through the BLGF timeseries repository. The panel covers 1,714 LGUs (82 provinces, 149 cities, 1,483 municipalities) over fiscal years 1992–2024, yielding 55,904 LGU-year observations. Rows are restricted to SRE-verified records; fund types (general fund, Special Education Fund, other special funds) are aggregated to LGU-year totals.

Deflation. All fiscal values are converted to constant 2018 Philippine pesos. The deflator is the Philippines Consumer Price Index (base 2018 = 100), reconstructed from the World Bank Consumer price index (2010 = 100) series (indicator FP.CPI.TOTL, sourced from IMF International Financial Statistics) and rebased to 2018 = 100. The reconstructed index ranges from 29.26 (1992) to 126.12 (2024). We do not apply per-capita adjustments; the regression uses per-LGU totals, and the LGU fixed effect absorbs permanent differences in population.

Electoral panel. Vote-share data for governorships combine two sources. For 2007–2025, we use OpenHalalan (Leung et al., 2026), an open-source dataset of Philippine election results (ODbL v1.0) providing candidate-level vote counts. For 1992–2004, the period preceding OpenHalalan's coverage, we retain COMELEC-derived vote shares. The combined province panel covers 82 provinces across twelve cycles (1992–2025), yielding 937 province-cycle observations. For the mayoral panel, vote counts are drawn from OpenHalalan, filtered to 'position == MAYOR', and merged with the fiscal panel on PSGC10 and election cycle. The mayoral panel covered 146 cities and 1,472 municipalities (excluding 3 cities and 11 municipalities that could not be matched across datasets) across seven cycles (2007–2025), yielding 7,891 LGU-cycle observations.

Outcome variable. The dependent variable is the incumbent's own vote share in the cycle of interest, not the winner's share. For each LGU-cycle, we identify the winning candidate from candidate-level vote counts and compare the winner in cycle t to the winner in cycle t − 3. If the names match, the incumbent was re-elected, and the winner's share is the incumbent's own share. If the names differ, we identify the previous winner from the candidate list of cycle t and use that candidate's own vote share. If the previous winner did not run, the observation is dropped. Because incumbents do not always run — term limits (three consecutive terms under RA 7160), retirement, or intra-family succession — the incumbent-outcome sample is smaller than the winner-outcome sample. This reduction is not a data-quality problem; it is the subsample in which the incumbent's own electoral reward is observable.

Regression panels. For each level we merge the electoral panel with fiscal covariates observed in fiscal year (cycle − 1), following the political-budget-cycle convention that spending in the year immediately before an election is the relevant predictor of the electoral outcome.

Table 1. *Sample Construction by LGU Level*

Level	Cycles	Electoral Panel	Regression sample	Retention
Province	12 (1992–2025)	937	192	20.49%
City	7 (2007–2025)	704	268	38.07%
Municipality	7 (2007–2025)	7,187	2,174	30.25%

Sample losses. Four constraints reduce the regression sample relative to the raw electoral panel. First, the BLGF SRE reports education and health expenditures as separate line items only from fiscal year 2004 onward, so the province regression is estimated on 2004–2025 (six effective cycles within the incumbent subsample). Second, the incumbent-outcome restriction excludes any cycle in which the previous winner did not run; in Philippine local

elections, roughly 30–35% of incumbents run for reelection in any given cycle. Third, 11% of merged municipal cycles report zero sectoral spending in the pre-election year and are recoded as missing, since zero reporting typically reflects non-reporting rather than true zeros; the drop is uniform across cycles and biases toward finding a spending effect where spending exists, so the null result cannot be attributed to the sample restriction. Fourth, uncontested races (n_candidates ≤ 1) are dropped from all levels.

Data-cleaning decisions. Three classes of data-quality issues were resolved. Three LGU-year rows had physically implausible fiscal magnitudes (greater than PHP 100 billion for a single LGU) and were dropped. Sixteen province-cycles in the electoral source had placeholder values in vote-share and candidate-count fields and were dropped. Zero values in sectoral expenditures were recoded as missing.

Voter Elasticity Specification

We estimate Equation (1) separately at each administrative level:

$$\pi_{it} = \alpha_i + \gamma_t + \beta_1 \ln(\text{pubwelfare}_{it} + 1) + \beta_2 \ln(\text{educexp}_{it} + 1) + \beta_3 \ln(\text{healthexp}_{it} + 1) + \beta_4 \ln(\text{ENC}_{it}) + \varepsilon_{it} \quad (1)$$

where π_{it} is the incumbent's own vote share in cycle t, a_i is a unit fixed effect (province, city, or municipality), γ_t is an election-cycle fixed effect, and all fiscal variables are per-LGU totals in constant 2018 Philippine pesos. Standard errors are clustered at the unit level to adjust for within-unit serial correlation (Wooldridge, 2010). Two features of the specification merit comment: (1) Cycle fixed effects absorb national shocks, national-level spending growth, and the common IRA transfer trend, isolating within-LGU deviations across cycles. This is standard in PBC research: without cycle fixed effects, spending coefficients capture common macro trends rather than election-driven manipulation (Akhmedov & Zhuravskaya, 2004; Brender & Drazen, 2005; Shi & Svensson, 2006); (2) Log ENC prevents the negative fitted vote shares that a linear-in-ENC specification would produce at the upper end of the observed ENC range. Population is time-invariant in our panel (2024 PSA snapshot) and is absorbed by the unit fixed effect; no separately identified population coefficient is reported. Income class is likewise absorbed for LGUs that did not change class over the panel.

Endogeneity of ENC. The effective number of candidates may be jointly determined with incumbent vulnerability: a weak incumbent may attract more challengers, biasing the ENC coefficient toward a larger magnitude. We report the contemporaneous coefficient as the primary estimate and interpret it as an association rather than a causal effect. A strict three-year lag of ENC was tested as a robustness specification but produced too few observations at the incumbent-outcome sample (N = 63, 83, and 724 at province, city, and municipality, respectively) to identify the coefficients; we therefore rely on the contemporaneous estimate and flag the endogeneity as a limitation.

Revenue Process Calibration

At each level $\ell \in \{province, city, municipality\}$, we model local revenue and IRA transfers as jointly lognormal:

$$(\ln \text{Loc}_t, \ln \text{IRA}_t) \sim \mathcal{N}(\mu_\ell, \Sigma_\ell) \quad (2)$$

with parameters calibrated from the empirical distribution of that level's fiscal variables. We report the pooled calibration as the main specification and a stratified calibration by income class (1st–3rd vs. 4th–6th) as a robustness check.

Table 2. *Lognormal Revenue Calibration by LGU Level (constant 2018 PHP M, pooled calibration)*

Parameter	Province	City	Municipality
E[Local revenue]	217.8	377.9	16.9
SD[Local revenue]	266.7	486.1	39.3
E[IRA]	874.3	514.9	76.8
SD[IRA]	480.6	420.4	64.4
Baseline budget	1,092.1	892.8	93.7
ρ (local, IRA)	0.594	0.583	0.492
N (joint LGU-years)	2,622	4,895	48,387

Note. ρ is estimated from the data as the sample Pearson correlation between local revenue and IRA at each level, not asserted. Baseline budget = E[Local revenue] + E[IRA].

Monte Carlo Simulation

For each level, we generate 10,000 forward revenue paths over a 10-year horizon using Cholesky decomposition of that level's calibrated covariance matrix Σ (Glasserman, 2003). For each draw, we compute total income $Y_t = Loc_t + IRA_t$. Three counterfactual expenditure scenarios are simulated.

Table 3. *Scenario Design*

Scenario	Total expenditure	Composition (pub/admin/edu/health)
A. Clientelistic surge	Baseline × U(1.15, 1.30)	35% / 35% / 15% / 15%
B. Normative portfolio	Baseline × U(0.95, 1.02)	10% / 25% / 35% / 30%
C. Spending-neutral reallocation	Same as A	Same as B

Notes. Pre-election years are 3, 6, and 9 within the 10-year horizon, matching the three-year electoral cycle. In off-election years, Scenario A reverts to the baseline budget. Realized mean surge multiplier in pre-election years is 1.225. Scenario C shares A's expenditure level so that the level effect is separated from the composition effect: A vs C identifies composition; C vs B identifies level.

Simulated vote share in each scenario follows:

$$\hat{\pi} = \text{clip}(\hat{\alpha} + \Sigma_k \hat{\beta}_k \ln(X_k + 1) + \hat{\beta}_\text{ENC} \cdot \log(\text{ENC}) + \eta, 0, 1) \quad (3)$$

where $\hat{a}$ is the intercept calibrated to reproduce mean(π) at mean predictors, $X_k \in \{pubwelfare, education, health\}$, $\eta \sim N(0, \hat{\sigma}^2)$ is residual noise drawn from the fit of Equation (1), and clip(x, 0, 1) truncates the simulated value to the unit interval.

Insolvency definition. A path is flagged as insolvent in year t if the deficit-to-income ratio exceeds *0.05*. We test robustness to thresholds of 0.03 and 0.10. We report three distinct metrics: the one-year insolvency probability (measured in the first pre-election year), the mean number of years over the ten-year horizon in which the deficit exceeds the threshold, and the 95% Value-at-Risk of the ten-year cumulative deficit-to-income ratio.

Legal constraints on LGU deficits. Balanced-budget requirements under RA 7160 Sec. 305, personal-services caps of 45% for cities and municipalities under Sec. 325a, and borrowing limits under Sec. 324—are not imposed on the simulated paths. This is a deliberate choice: the simulation characterizes the unconstrained fiscal exposure generated by pre-election spending, which is the quantity of interest for policy design given the institutional and fiscal constraints under which Philippine LGUs operate (Llanto & Ortiz, 2016).

Ten-year dynamics. The simulation models 10 independent years of stochastic income against scenario-specific expenditure paths. No deficit carryover, borrowing, or mean reversion is modeled. This is a bounding exercise rather than a fiscal forecast: it characterizes the distribution of annual imbalances under each spending policy, not the trajectory of accumulated debt under a legal constraint structure.

Reported Statistics

All coefficient estimates are reported with unit-clustered standard errors and 95% confidence intervals. Simulated metrics are reported with Monte Carlo standard errors and dispersion (SD and 5–95 percentiles). Sample sizes are reported for every table.

Data and Code Availability

Fiscal data are derived from the BLGF Statement of Receipts and Expenditures, publicly available at https://blgf.gov.ph/lgu-timeseries-data. Electoral data for 2007–2025 are derived from OpenHalalan (Leung et al., 2026; DOI 10.5281/zenodo.17783099), published under the Open Database License v1.0. Electoral data for 1992–2004 are derived from COMELEC records. The CPI deflator is derived from the World Bank indicator FP.CPI.TOTL (base 2010 = 100), rebased to 2018 = 100. Analysis code and cleaned data have been deposited in a public repository and are available at https://github.com/KINGSTING/the-incumbent-tax.

Results and Discussion

Descriptive Political Budget Cycle

Figure 1 plots mean sectoral expenditures in pre-election versus off-election fiscal years, separately for provinces, cities, and municipalities, in constant 2018 Philippine pesos. Across all three LGU levels, public welfare expenditure is systematically higher in the pre-election year, consistent with the political budget cycle documented in the international literature (Akhmedov & Zhuravskaya, 2004; Drazen & Eslava, 2010; Shi & Svensson, 2006) and in Philippine-specific studies (Capuno, 2016; Sacluti, Saturnino, & Punongbayan, 2026). The pattern is strongest at the municipal level and weakest at the provincial level, where the expenditure mix is dominated by formula-driven and personnel costs. The cycle's persistence across all three levels supports the paper's descriptive premise: pre-election spending surges are a general feature of Philippine local governance.

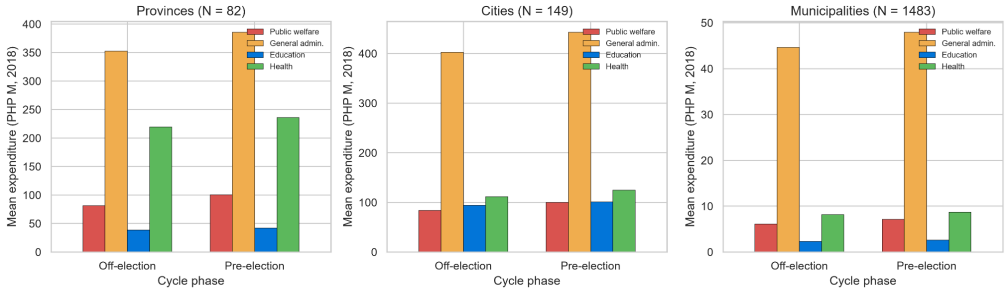


Figure 1. *Empirical PBC by LGU Level (constant 2018 PHP M)*

Voter Elasticity Model

Table 4. *Two-Way Fixed-Effects Voter Elasticities by LGU Level (incumbent's own vote share)*

Predictor	Province β (SE)	Province 95%	City β (SE)	City 95% CI	Municipality β (SE)	Municipality 95% CI
ln(pub welfare + 1)	−0.0128 (0.0185)	[−0.0495, +0.0239]	−0.0127 (0.0156)	[−0.0437, +0.0182]	+0.0091 (0.0075)	[−0.0056, +0.0238]
ln(education + 1)	+0.0003 (0.0140)	[−0.0274, +0.0280]	+0.0261 (0.0195)	[−0.0125, +0.0648]	+0.0034 (0.0085)	[−0.0132, +0.0201]
ln(health + 1)	−0.0061 (0.0173)	[−0.0404, +0.0281]	−0.0027 (0.0213)	[−0.0449, +0.0394]	+0.0048 (0.0128)	[−0.0203, +0.0299]
ln(ENC)	−0.6101 (0.0495)	[−0.7083, −0.5119]	−0.6152 (0.0604)	[−0.7346, −0.4958]	−0.6091 (0.0301)	[−0.6681, −0.5501]
Within-R^2	0.810		0.767		0.589	
F-statistic	88.06		108.70		399.40	
df_resid	109		132		1,137	
N (LGU-cycles)	192		268		2,174	
N (LGUs)	74		128		1,029	
N (cycles)	6		5		5	

Notes. Unit and election-cycle fixed effects. Standard errors clustered at the unit level. A dependent variable is the incumbent's own vote share. Estimation sample restricted to competitive races (n_candidates > 1) and to cycles in which the previous cycle's winner ran again.

The spending–vote relationship is null at every LGU level. Nine spending coefficients across three levels: none is statistically distinguishable from zero. Point estimates are near zero. A 20% increase in provincial public welfare spending — roughly a one-unit change in log spending at the sample mean — buys an estimated vote-share change of −0.0026, with a 95% CI of [−0.0099, +0.0048]. The same increase in education buys +0.0001 with CI [−0.0055, +0.0056]; in health, −0.0012 with CI [−0.0081, +0.0056]. The null result is consistent at the upper bound of each confidence interval; a 20% spending increase buys at most half a percentage point of vote share.

This result differs from earlier Philippine evidence, and the difference reflects the specification. When cycle fixed effects are excluded — as in many single-stage political-budget-cycle regressions — education spending appears to have a positive and significant electoral return, because both education spending and incumbent vote share trend upward nationally over the same period. Once cycle effects absorb that common trend, the apparent education effect disappears. The same pattern is observed at the city and municipal levels, aligning with broader findings that voters do not systematically reward pre-election fiscal expansion or opportunistic spending (Brender & Drazen, 2008; Peltzman, 1992).

Competition, not spending, moves votes — and the effect is level-invariant. The effective number of candidates robustly predicts the incumbent's vote share, with coefficients statistically indistinguishable across levels: $\beta = -0.610$ (province), -0.615 (city), -0.609 (municipality), all $p < .001$. The coefficient implies that doubling the effective number of candidates reduces the incumbent's vote share by roughly 42 percentage points. Endogeneity of ENC—the contemporaneous coefficient should be read as an association rather than a causal effect. A vulnerable incumbent may attract additional challengers, in which case the estimated coefficient overstates the mechanical effect of candidate entry on vote splitting. We do not attempt to instrument for ENC. The paper's claim — that sectoral spending does not predict the incumbent's vote share—does not depend on the ENC coefficient being causal.

Trade-Off Frontier

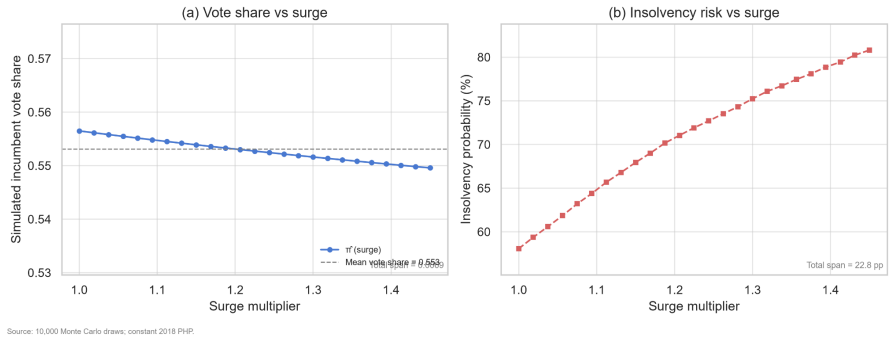


Figure 2. *Vote-Share and Insolvency Trade-Off (Province)*

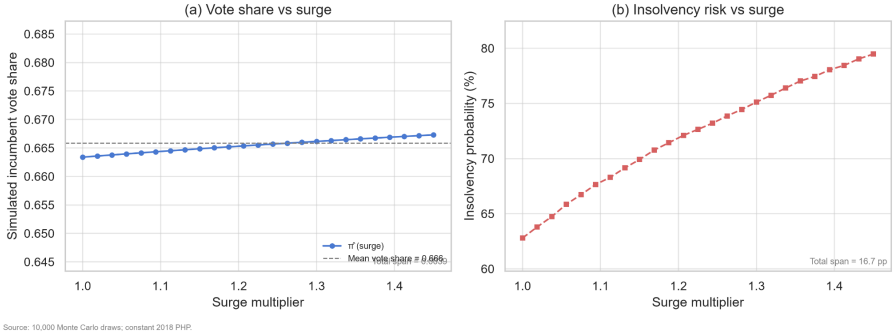


Figure 3. *Vote-Share and Insolvency Trade-Off (City)*

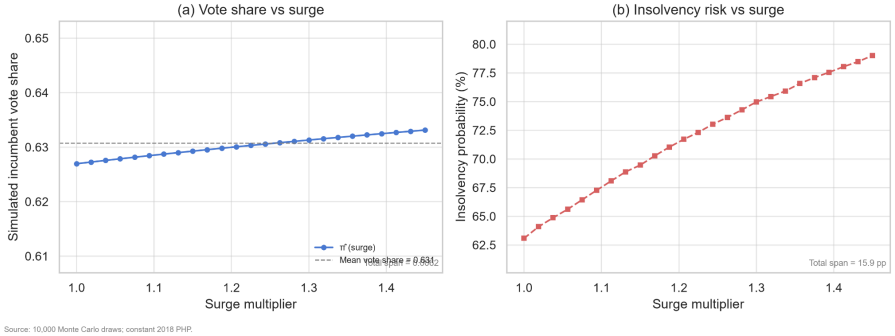


Figure 4. *Vote-Share and Insolvency Trade-Off (Municipality)*

Figures 2, 3, and 4 plot simulated vote share (panel a) and simulated insolvency probability (panel b) against the surge multiplier, separately at each LGU level. Panel (a) shows that the simulated vote-share response to the surge is statistically indistinguishable from zero at all three levels, and the point estimates are not monotonic across levels. Provincial vote share declines by 0.0069 across the full 1.00–1.45 surge range, while city and municipal vote shares rise by 0.0039 and 0.0061, respectively. All three changes are smaller than the 95% confidence interval on the corresponding spending coefficients. The dispersion of signs across administrative tiers is consistent with a null result: no systematic electoral reward attends the surge at any level. Panel (b) rises monotonically at every level: the same surge range pushes the one-year insolvency probability from 56.8% to 71.8% in provinces (15.0 pp), from 62.0% to 72.7% in cities (10.7 pp), and from 62.5% to 72.3% in municipalities (9.8 pp). The trade-off frontier is therefore degenerate at every level: no electoral benefit is purchased by the fiscal cost, and the small non-monotonic vote-share movements are within sampling noise.

Insolvency Risk by Competition and Tail Exposure

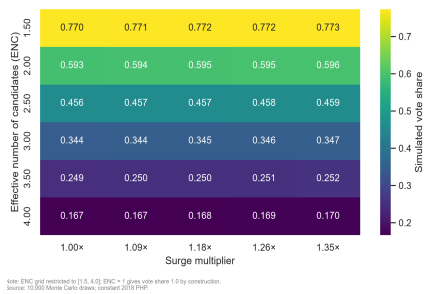


Figure 5. *Vote Share by ENC and Surge — Province*

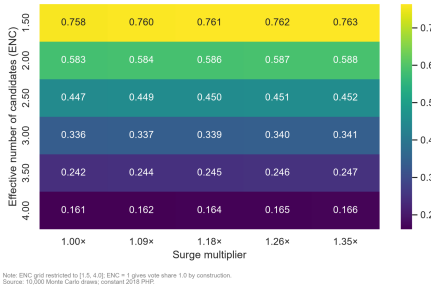


Figure 6. *Vote Share by ENC and Surge — City*

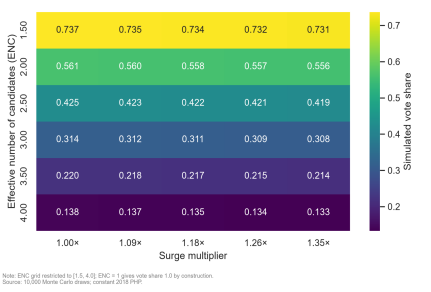


Figure 7. *Vote Share by ENC and Surge — Municipality*

Figures 5, 6, and 7 decompose the simulated vote-share response into its two components: effective number of candidates (vertical axis) and surge multiplier (horizontal axis). Vote share declines with ENC — from 0.74 at ENC = 1.50 to 0.13 at ENC = 4.00 in provinces, from 0.77 to 0.17 in cities, and from 0.76 to 0.17 in municipalities. Within each ENC row, vote share varies by less than 0.005 across the entire surge range, indicating that the fiscal surge is not translating into electoral reward at any level of political competition. The ENC axis is restricted to [1.50, 4.00] because at ENC = 1.00 the vote share equals 1.00 by construction.

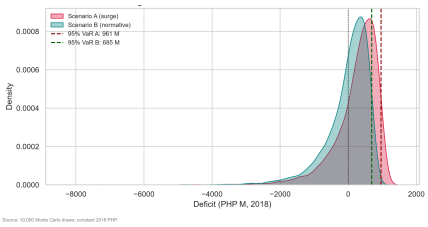


Figure 8. *Pre-Election Deficit Distributions — Province*

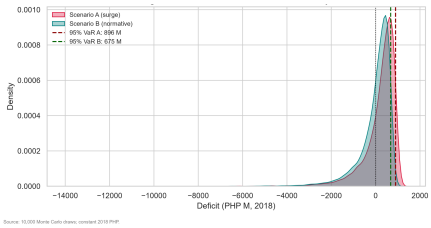


Figure 9. *Pre-Election Deficit Distributions — City*

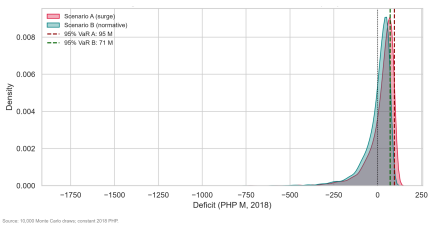


Figure 10. *Pre-Election Deficit Distributions — Municipality*

Figures 8, 9, and 10 plot the simulated pre-election deficit distributions for Scenarios A and B at each level, with 95% VaR thresholds marked.

Four findings emerge. First, the fiscal cost of a clientelistic surge is present at every level. The one-year insolvency probability rises by 15.0 pp in provinces, 10.7 pp in cities, and 9.8 pp in municipalities. Over a ten-year horizon, the surge adds 0.4 to 0.5 additional deficit years per decade: 0.51 in provinces (6.18 vs 5.67), 0.37 in cities (6.55 vs 6.18), and 0.36 in municipalities (6.55 vs 6.19). Second, the pre-election deficit distributions shift to the right. The 95% Value-at-Risk of the cumulative deficit-to-income ratio rises from 0.354 to 0.469 in provinces, from 0.570 to 0.693 in cities, and from 0.571 to 0.704 in municipalities. Third, the baseline deficit-year rate is nonzero even in the normative scenario. Scenario B produces 5.67–6.19 deficit years per decade across the three levels. This is not a modeling error but a consequence of the calibration: the baseline budget is set at expected revenue (the "knife-edge" calibration), so with the lognormal's right-skewed income distribution, more than half of all simulated years

tip into deficit even without the surge. The metric therefore measures shock-driven deficits rather than structural insolvency, and the A–B difference — not the level — is the quantity of interest for policy. Fourth, the electoral return to spending is statistically indistinguishable from zero at every level. Simulated vote share is 0.55–0.70 across scenarios and levels, with the A-vs-B difference smaller than one Monte Carlo standard error. The sample mean vote share in the regression panel (0.585, 0.649, 0.606 for province, city, and municipality) is close to the simulated mean in every case, indicating that the simulation is anchored to the observed electoral distribution.

Table 5. *Monte Carlo Simulation Results by LGU Level (constant 2018 PHP M)*

Metric	Prov A	City A	Mun A	Prov B	City B	Mun B
Expected pre-election expenditure	1,337.98 (SD 47.59)	1,093.88 (SD 38.91)	114.77 (SD 4.08)	1,075.82 (SD 22.17)	879.54 (SD 18.12)	92.28 (SD 1.90)
Expected pre-election deficit	241.62 (SD 668.22)	197.59 (SD 797.02)	20.29 (SD 86.91)	−20.54 (SD 666.42)	−16.74 (SD 795.97)	−2.20 (SD 86.79)
1-year insolvency (%)	71.8 (SE 0.45)	72.7 (SE 0.45)	72.3 (SE 0.45)	56.8 (SE 0.50)	62.0 (SE 0.49)	62.5 (SE 0.48)
Mean deficit years (of 10)	6.18 (SD 1.52)	6.55 (SD 1.50)	6.55 (SD 1.49)	5.67 (SD 1.56)	6.18 (SD 1.54)	6.19 (SD 1.53)
95% VaR cum. deficit/income	0.469 (SD 0.206)	0.693 (SD 0.304)	0.704 (SD 0.311)	0.354 (SD 0.191)	0.570 (SD 0.281)	0.571 (SD 0.288)
Mean simulated vote share	0.55 (SD 0.06)	0.67 (SD 0.05)	0.63 (SD 0.06)	0.57 (SD 0.06)	0.70 (SD 0.05)	0.62 (SD 0.06)
Sample mean vote share	0.585	0.649	0.606	-	-	-

Note. N = 10,000 draws per scenario per level. Monte Carlo standard errors are reported for probabilities (≤ 0.005); dispersion (SD and 5–95 percentile intervals) is reported for continuous metrics. Scenario C produces metrics identical to Scenario A by construction (shared expenditure level) and is omitted for compactness.

Sensitivity Analysis

Table 6. *Sensitivity of Insolvency Metrics to (ρ, threshold) — Province Level*

ρ	Threshold	A: 1-yr (%)	B: 1-yr (%)	A: insolv yrs	B: insolv yrs
0.30	0.03	71.80	56.20	6.29	5.70
0.30	0.05	70.10	55.00	6.15	5.54
0.30	0.10	67.35	52.00	5.78	5.18
0.594	*0.03*	*72.30*	*57.70*	*6.36*	*5.84*
0.594	*0.05*	*70.65*	*56.25*	*6.23*	*5.71*
0.594	*0.10*	*67.15*	*53.35*	*5.90*	*5.36*
0.60	0.03	72.40	57.65	6.37	5.85
0.60	0.05	70.55	56.30	6.23	5.71
0.60	0.10	67.30	53.35	5.91	5.37

Note. N = 2,000 draws per cell. ρ = 0.594 is the data-calibrated value for provinces; 0.30 and 0.60 are stress alternatives. The full table for cities and municipalities is reported in the supplementary materials.

Across the 3 × 3 grid of ρ × threshold, *the ordering A > B is preserved in every cell*. Insolvency probabilities shift by 1–5 percentage points across *ρ* values and thresholds, and the deficit-years metric shifts by less than 0.6 within each ρ. The finding — that a pre-election surge raises fiscal tail risk — is robust to all calibration choices tested. Because the ruling expands the IRA base from 2022 onward, we re-estimate the simulation on the post-2022 subsample. The calibrated provincial baseline rises from 1,092 to 1,666 PHP M (a 53% increase). The A-vs-B one-year insolvency gap widens from 15.0 pp to 32.9 pp (80.8% vs 47.9%), supporting the case for pre-election spending ceilings.

Robustness of Lognormal Fit

Figures 11, 12, and 13 plot QQ diagrams of the empirical log-quantiles of local revenue and IRA against the theoretical lognormal quantiles at each level, with Anderson-Darling (AD) statistics reported per series. The AD statistics reject lognormality for every series at the 5% level. This is expected given the sample sizes (N = 2,610 to 47,721): with these observation counts, small deviations from a parametric form produce rejections at conventional levels. The relevant question is not whether the lognormal is correct but whether the fiscal conclusions depend on it.

Two features of the misfit bear on interpretation. The AD rejections are largest for local revenue, which exhibits a thinner left tail than the lognormal assumes; the IRA series are closer to lognormal in shape, particularly at the

municipal level where IRA is formula-driven and tightly distributed around a predictable allocation. The left-tail thinness in local revenue means the lognormal calibration is conservative on the insolvency side: a thinner left tail implies fewer extreme low-revenue years in the empirical data than the simulation generates, so the simulated insolvency probabilities likely overstate rather than understate the true fiscal exposure. The sensitivity analysis confirms that the ranking A > B is preserved under every calibration tested and is not an artifact of the lognormal assumption.

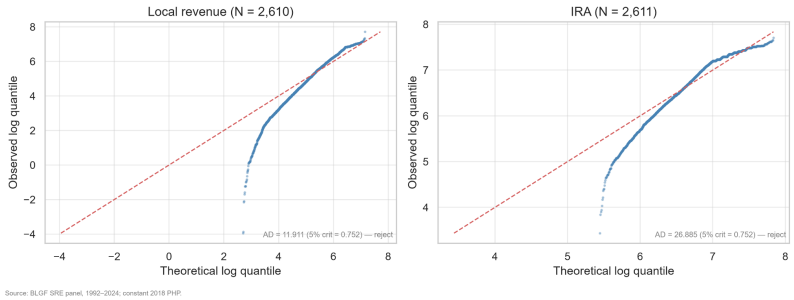


Figure 11. *Lognormal QQ Plots – Province*

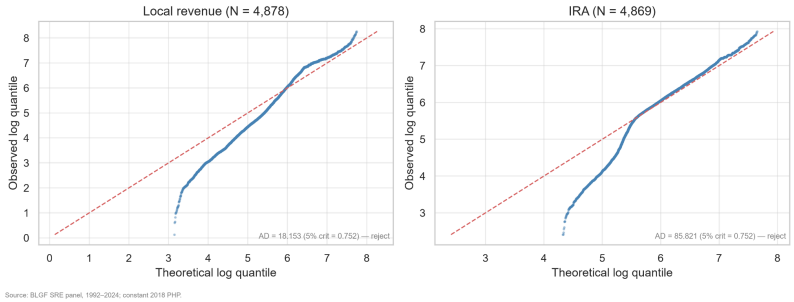


Figure 12. *Lognormal QQ Plots – City*

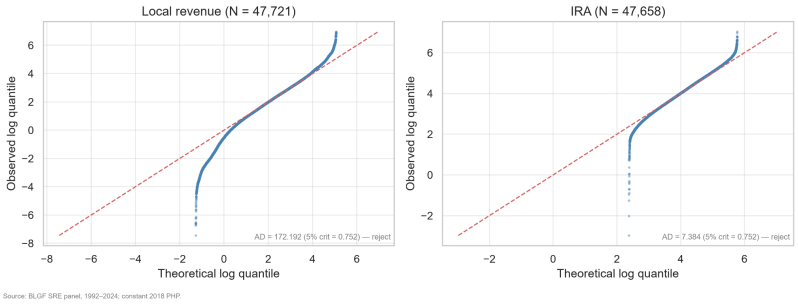


Figure 13. *Lognormal QQ Plots – Municipality*

Diagnostic Plots

Figures 14, 15, and 16 report four diagnostics for the Scenario A simulation. Panel (c) plots simulated vote share against simulated fiscal risk; the correlation is approximately +0.010 (Province), *+0.013*(City), and +0.009 (Municipality) , indicating that the electoral and fiscal dimensions of the simulation are independent.

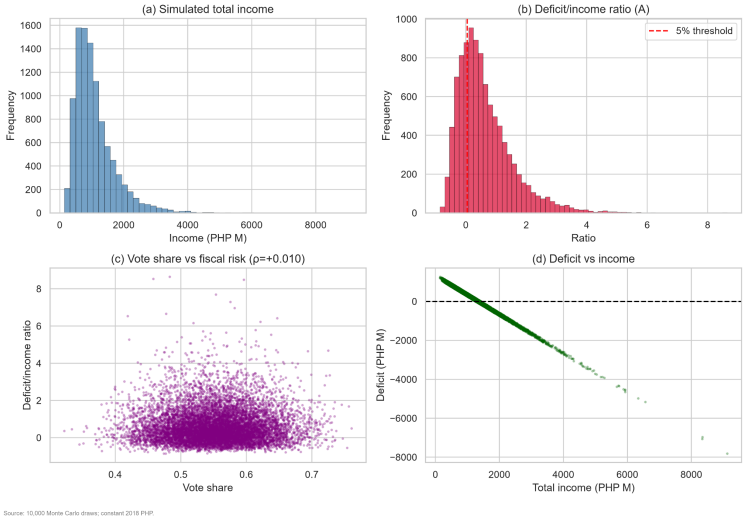


Figure 14. *Diagnostics — Province*

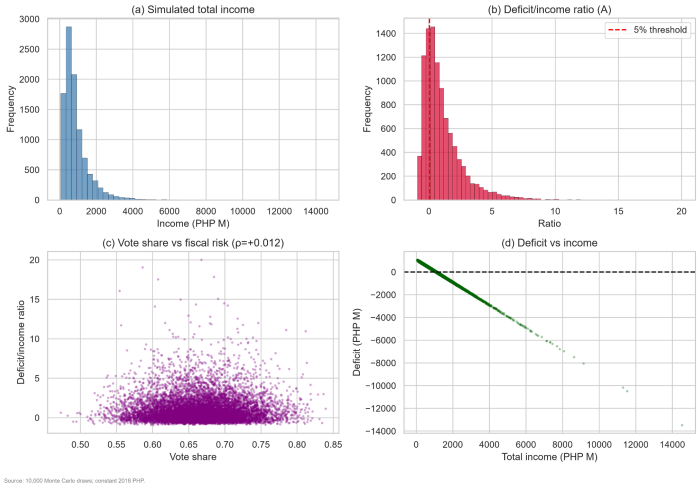


Figure 15. *Diagnostics — City*

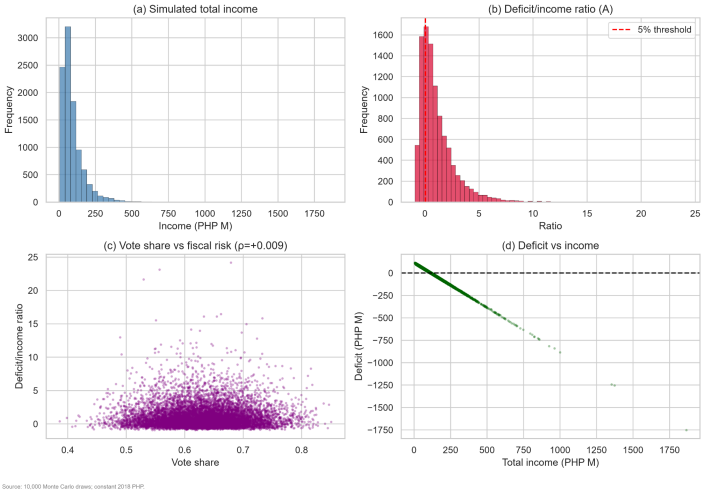


Figure 16. *Diagnostics — Municipality*

Ten-Year Cumulative Trajectories

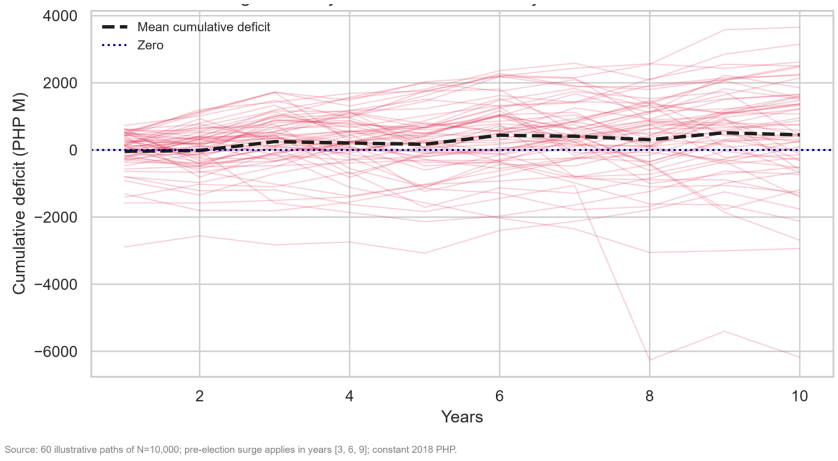


Figure 17. *10-Year Trajectories — Province*

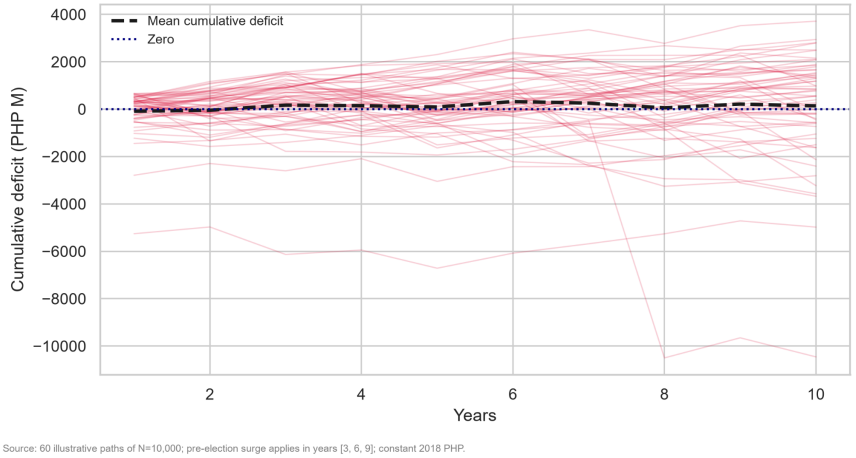


Figure 18. *10-Year Trajectories — City*

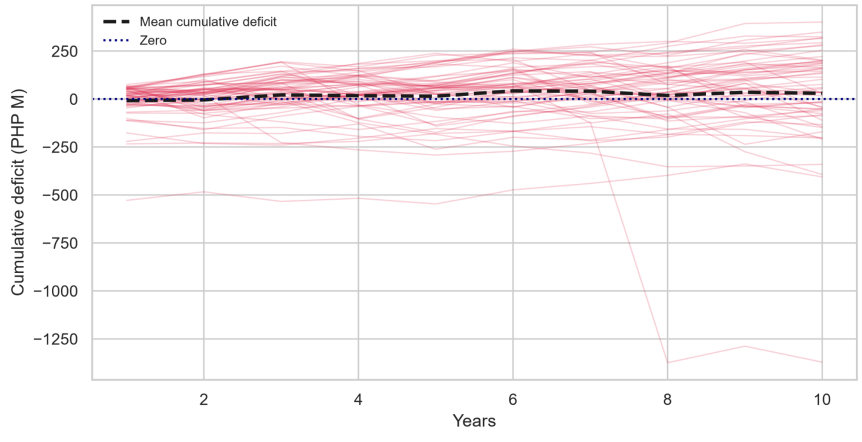


Figure 19. *10-Year Trajectories — Municipality*

Figures 17, 18, and 19 plot 60 illustrative ten-year cumulative deficit trajectories under Scenario A at each LGU level, with the mean path overlaid. The trajectories fan out over the horizon; the mean cumulative deficit at year 10 remains modest, reaching approximately PHP 200 M in provinces and under PHP 50 M in cities and municipalities. However, tail risk is substantial, with the worst-drawn paths reaching cumulative deficits of PHP 10,000 M or more at the provincial level.

Policy Implications

The three-level finding that spending has no measurable electoral return has three implications for Philippine fiscal policy. Pre-election discretionary spending ceilings should be uniform across LGU levels, adhering to international principles for local government financial management (Council of Europe, 2022). Because the incumbent tax applies to provinces, cities, and municipalities alike, a targeted ceiling at any single level would leave the mechanism intact elsewhere. The DBM's existing Local Budget Circular authority and its annual review of LGU budget submissions provide the administrative mechanism. A ceiling on discretionary expenditure growth in the fiscal year immediately preceding national and local elections, issued as a DBM circular applicable to all LGU levels, would not require new legislation. The ceiling would be enforced through the existing budget review process, with the BLGF monitoring implementation and the DILG providing technical assistance to LGUs that exceed the ceiling.

Fiscal stress tests should be calibrated separately at each level. Given existing subnational fiscal and institutional constraints (Llanto & Ortiz, 2016; Punongbayan, 2025), the BLGF's existing LGU Fiscal Sustainability Scorecard could incorporate level-specific stochastic stress tests based on the framework developed here. The magnitude of the surge's fiscal cost differs by level — municipalities and cities start from higher baseline insolvency risk — so a single national threshold would not identify the LGUs most at risk. The case for intervention does not depend on spending composition. Because composition does not affect electoral or fiscal outcomes in our simulations, the policy case for ceilings rests on spending levels. These recommendations should be read in light of the Mandanas-Garcia ruling, which expands the IRA base from 2022 onward. The ruling supports the case for pre-election spending ceilings across all LGU levels.

Conclusion

This paper develops a stochastic simulation framework to quantify the electoral and fiscal consequences of pre-election spending at all three Philippine LGU levels. Using a PSGC-keyed panel of 1,714 LGUs (fiscal 1992–2024) merged with gubernatorial and mayoral election results, we estimate voter elasticities with two-way fixed-effects models using the incumbent's own vote share as the outcome, and propagate them through 10,000 Monte Carlo revenue paths. Findings have implications for fiscal policy. To address structural fiscal constraints and mitigate election-driven instability (Llanto & Ortiz, 2016; Punongbayan, 2025), the Department of Budget and Management (DBM), the Bureau of Local Government Finance (BLGF), and the Department of the Interior and Local Government (DILG) could implement pre-election discretionary spending ceilings enforced through DBM Local Budget Circular authority and mandatory fiscal stress tests integrated into the BLGF's existing LGU Fiscal Sustainability Scorecard. An extension of this work is to apply the framework to the Mandanas-Garcia ruling's post-2022 transfer expansion, where larger IRA allocations may amplify the fiscal consequences documented here. Future research could trace the ruling's long-term impact on LGU fiscal independence and refine these dynamic guardrails accordingly.

Six limitations apply to interpreting these results. Public welfare spending is an imperfect proxy for clientelistic expenditure. The results should be read as applying to discretionary sectoral spending rather than to clientelism in the strict sense. The province regression is estimated in 2004–2025 because the BLGF SRE did not report education and health expenditures as separate line items before 2004. The municipal regression drops 11% of merged observations because some municipalities report zero sectoral spending. The incumbent-outcome restriction reduces the sample to cycles in which the previous winner ran again. The Monte Carlo framework depends on three calibrated inputs: the lognormal revenue distribution, the deficit-to-income threshold, and the assumption of constant elasticities across each level's sample. Anderson-Darling tests reject lognormality for every revenue series, though the direction of the misfit (a thinner empirical left tail) makes the calibration conservative on the insolvency side. The threshold and elasticity constancy are tested for sensitivity. The baseline knife-edge calibration (expected expenditure = expected income) means that the deficit-years metric measures shock-driven, not structural, deficits. The three-level analysis relies on a name-to-PSGC crosswalk between OpenHalalan LGU

names and panel LGU names. Three cities and eleven municipalities could not be matched and are excluded; the excluded units account for under 1% of mayoral races.

Contributions of Authors

The author conceived the study, designed the simulation framework, wrote the analysis code, and prepared the manuscript.

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Conflict of Interests

The author declares no conflict of interest.

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