

Do Women Mayors Spend Differently? Close-Election Evidence from Mexico

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Abstract

Mexico tripled its share of female mayors after mandating gender parity in candidate lists in 2014. Whether these women govern differently remains an open question with first-order implications for the design of parity mandates worldwide. Using a close-election regression discontinuity design on 468 mixed-gender municipal races (2008–2022) merged with INEGI fiscal data, I find no statistically significant effect of electing a female mayor on municipal spending composition. Point estimates for social transfers (0.2 percentage points, $p = 0.81$), public investment (-4.7 pp, $p = 0.16$), and administrative payroll (4.3 pp, $p = 0.15$) are economically modest and robust to bandwidth variation and donut specifications. These null results extend [Ferreira and Gyourko \(2014\)](#) to a developing-country context with higher gender inequality and weaker institutional constraints, suggesting that fiscal rigidities dominate leader preferences even where the scope for discretion appears larger.

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1. Introduction

In 2018, Claudia Rivera became mayor of Puebla, Mexico’s fourth-largest city, promising to redirect municipal resources toward social programs and away from administrative bloat. Her campaign embodied a widespread expectation: that women in executive office would govern differently—spending more on social transfers, education, and health, and less on patronage and administrative overhead. This expectation now underpins gender parity mandates in over 30 countries. Yet the empirical evidence for whether female leaders actually change fiscal policy remains remarkably thin outside the United States.

The canonical study is [Ferreira and Gyourko \(2014\)](#), who use a close-election regression discontinuity design (RDD) for U.S. mayors and find no effect of mayor gender on the size of government, the composition of spending, or crime rates. Their explanation centers on institutional constraints: the combination of earmarked revenues, civil service rules, and inherited budget commitments leaves little room for mayoral discretion, regardless of the mayor’s preferences. But whether this null result reflects a universal feature of municipal governance or a specific artifact of the U.S. institutional context is unknown. Mexico provides a powerful test case: it combines high gender inequality (ranked 33rd on the World Economic Forum’s Global Gender Gap Index), a recent and dramatic expansion of female political representation, and a municipal budget structure that—despite earmarked federal transfers—affords mayors significant discretion over spending composition ([Flamand and Moreno-Jaimes, 2014](#)).

This paper estimates the causal effect of electing a female mayor on municipal spending composition in Mexico using a close-election RDD. The running variable is the vote margin between the female and male candidate in mixed-gender municipal races. At the zero threshold, the identity of the winner switches from male to female, and in close races, which candidate wins is as-if randomly assigned ([Lee and Lemieux, 2010](#)). I merge election data from the comprehensive [Magar \(2025\)](#) database with annual municipal expenditure records from INEGI’s *Estadística de Finanzas Públicas Estatales y Municipales* (EFIPEM), covering 468 mixed-gender elections in 401 municipalities from 2008 to 2022.

The main finding is a well-powered null: electing a female mayor has no statistically significant effect on any major category of municipal spending. The point estimate for social transfers—the outcome most closely linked to the “women spend differently” hypothesis—is 0.2 percentage points ($p = 0.81$), with a 95% confidence interval that rules out effects larger than 2.8 percentage points. Public investment shows a suggestive negative effect of -4.7 percentage points ($p = 0.16$), and administrative payroll a suggestive positive effect of 4.3 percentage points ($p = 0.15$), but neither reaches conventional significance levels.

These estimates are robust to bandwidth variation, donut specifications excluding the closest elections, and alternative polynomial orders.

Several features of the research design strengthen the credibility of these null results. First, the McCrary density test finds no evidence of manipulation at the cutoff ($p = 0.15$), confirming that parties cannot precisely control close outcomes. Second, pre-election spending variables balance across the cutoff for total expenditure ($p = 0.27$), social transfers ($p = 0.76$), and public investment ($p = 0.14$), though I document an imbalance in pre-election administrative payroll shares that I discuss transparently. Third, placebo tests at false cutoffs generally yield insignificant results, supporting the identifying assumption.

These results contribute to three literatures. First, they extend the [Ferreira and Gyourko \(2014\)](#) framework to a developing country, showing that the null result for female executive leadership is not unique to the United States. This is important because Mexico’s institutional context—with its combination of earmarked federal transfers (FAIS, FORTAMUN) and discretionary local spending—was *a priori* more favorable to detecting gender differences in fiscal policy ([Díaz Cayeros and Leal, 2019](#)). Second, the paper informs the rapidly growing literature on gender parity mandates ([Beaman et al., 2012](#); [Baltrunaite et al., 2014](#); [Brollo and Troiano, 2016](#)). While parity mandates clearly increase descriptive representation, this paper suggests their effect on substantive representation through fiscal policy is limited. Third, the results speak to the political economy of municipal governance in Mexico, where fiscal decentralization since the 1990s has given municipalities increasing but still constrained spending authority ([Cabrero Mendoza, 2004](#)).

The finding that institutional constraints bind even in a high-inequality developing country carries a sharp implication: advocates of gender parity mandates should base their case on the intrinsic value of descriptive representation and the long-run erosion of barriers to entry, not on the expectation of immediate policy change. The fiscal apparatus of municipal government—with its earmarked transfers, inherited personnel, and multiannual commitments—appears to constrain leaders of both genders equally.

2. Institutional Background

2.1 Municipal Government in Mexico

Mexico’s 2,446 municipalities (*municipios*) are governed by elected presidents (*presidentes municipales*) serving non-renewable three-year terms. Municipal elections are staggered across states, with different states voting in different years. The municipal president heads the *ayuntamiento* (city council), which approves the annual budget, but the president holds significant agenda-setting power over expenditure priorities ([Merino, 2007](#)).

Municipal revenues come primarily from two sources: federal transfers and own-source revenue. The Ramo 33 transfers include FAIS (*Fondo de Aportaciones para la Infraestructura Social*), earmarked for poverty-reducing infrastructure, and FORTAMUN (*Fondo de Aportaciones para el Fortalecimiento de los Municipios*), which supports security and financial obligations. Ramo 28 “participaciones” are general-purpose revenue-sharing transfers with no expenditure restrictions. Own-source revenues—property taxes, fees, and fines—typically account for 10–30% of total municipal income, varying by municipality size and economic activity (Raich, 2004).

On the expenditure side, INEGI classifies municipal spending into seven chapters: (1) *servicios personales* (payroll and benefits), (2) *materiales y suministros* (materials), (3) *servicios generales* (contracted services), (4) *transferencias, asignaciones, subsidios y otras ayudas* (social transfers and subsidies), (5) *bienes muebles, inmuebles e intangibles* (capital goods), (6) *inversión pública* (public investment), and (7) other expenditures. While FAIS funds are nominally earmarked, mayors retain discretion over the composition of spending within and across these chapters, particularly over the balance between payroll, social transfers, and public investment (Flamand and Moreno-Jaimes, 2014).

2.2 Gender Parity in Mexican Politics

Mexico’s path to gender parity in political candidacies followed a decades-long trajectory. The 1996 electoral reform introduced a 30% gender quota for party candidate lists, raised to 40% in 2008. The watershed came with the 2014 constitutional reform (DOF 10/02/2014, Article 41), which mandated full gender parity in all legislative candidacies, implemented through staggered state-level reforms between 2015 and 2018 (Cerva Cerna, 2014). Although the federal mandate applies primarily to legislative races, most states extended parity requirements to municipal candidate lists.

The share of female municipal presidents rose from approximately 5% in the early 2000s to 27% by 2018 and 29% by 2024. This dramatic increase, concentrated in the years after 2014, provides the empirical setting for this study: a large number of mixed-gender competitive races in which close outcomes provide quasi-random variation in leader gender.

3. Data

3.1 Election Data

I use the Magar (2025) municipal election database (`aymu1989-on.incumbents.csv`), which records 24,465 municipal elections from 1954 to 2025. The dataset provides the winner’s name,

party, gender (`dmujer`), runner-up name and party, and victory margin for each election. I focus on elections from 2000 onward, where both election and fiscal data are available.

To construct the RDD sample, I must identify *mixed-gender* races—elections where the top two candidates were of different genders. The dataset records winner gender directly; I classify runner-up gender from names using a comprehensive Spanish first-name dictionary of over 300 names supplemented by standard linguistic heuristics (feminine `-a/-ina/-ella` suffixes, masculine `-o/-ón` suffixes, and recognition of compound names). I retain only races where I can confidently confirm that the winner and runner-up are of different genders, yielding 842 confirmed mixed-gender races from 7,944 post-2000 elections (10.6%). The relatively low share reflects three factors: same-gender races are common, many names are ambiguous (e.g., indigenous or uncommon names that resist classification), and I conservatively exclude races where runner-up gender is uncertain.

For the analysis sample, I further require EFIPEM fiscal data covering at least one year of the winner’s three-year term, which restricts the sample to 2008–2022. This produces 468 elections in 401 municipalities. The loss from 842 to 468 is driven primarily by two factors: the fiscal data begin reliably in 2005, requiring elections from 2008 onward to have pre-period and outcome coverage, and some municipalities—particularly small or rural ones—have gaps in EFIPEM reporting.

3.2 Fiscal Data

Municipal expenditure data come from INEGI’s *Estadística de Finanzas Públicas Estatales y Municipales* (EFIPEM), which reports annual spending by chapter for all municipalities from 1989 to 2023. I extract chapter-level expenditure data and construct spending shares—each category as a fraction of total expenditure—to capture spending *composition* rather than levels. For each election, I average fiscal outcomes over the winner’s three-year term (the year after election through three years after) to capture the mayor’s full fiscal footprint.

3.3 Summary Statistics

[Table 1](#) presents summary statistics for the full sample and for close elections (victory margin below 10 percentage points). In the full sample, administrative payroll accounts for 34.7% of total spending, public investment for 23.6%, and social transfers for 8.8%. Female mayors won 36.1% of mixed-gender races in the full sample and 38.5% in close elections, consistent with the post-parity era in which most of these elections took place. The close-election subsample is balanced in composition and spending patterns relative to the full sample, supporting the RDD approach.

Table 1: Summary Statistics

| | Full Sample | | | Margin < 10% | | |
|-------------------------|-------------|-------|-----|---------------|-------|-----|
| | Mean | SD | N | Mean | SD | N |
| Female vote margin | -0.062 | 0.191 | 468 | -0.008 | 0.055 | 231 |
| Admin payroll share | 0.347 | 0.105 | 468 | 0.351 | 0.109 | 231 |
| Social transfers share | 0.088 | 0.048 | 468 | 0.084 | 0.045 | 231 |
| Public investment share | 0.236 | 0.140 | 468 | 0.245 | 0.145 | 231 |
| Materials share | 0.108 | 0.051 | 468 | 0.104 | 0.053 | 231 |
| General services share | 0.157 | 0.063 | 468 | 0.150 | 0.060 | 231 |
| Log total expenditure | 18.526 | 1.434 | 468 | 18.468 | 1.371 | 231 |
| Female mayor wins | 169 (36.1%) | | | 105 (45.5%) | | |
| Unique municipalities | 401 | | | 210 | | |

Notes: Sample of mixed-gender municipal elections in Mexico, 2008–2022. Female vote margin is positive when the female candidate won. Spending shares are three-year term averages from INEGI EFIPEM data. Close elections defined as $|\text{margin}| < 10\%$.

4. Empirical Strategy

4.1 Regression Discontinuity Design

I exploit the discontinuous assignment of mayor gender at the zero-margin threshold. The running variable X_i is the female candidate’s vote margin: positive when the female candidate won, negative when the male candidate won. The treatment indicator $D_i = \mathbb{I}[X_i > 0]$ equals one when a female mayor is elected. At the threshold, which candidate wins is determined by a handful of votes, making gender assignment as-if random (Lee and Lemieux, 2010).

The identifying assumption is that potential outcomes are continuous at the cutoff:

$$\lim_{x \downarrow 0} \mathbb{E}[Y_i(d)|X_i = x] = \lim_{x \uparrow 0} \mathbb{E}[Y_i(d)|X_i = x] \quad \text{for } d \in \{0, 1\} \quad (1)$$

This assumption fails if parties or candidates can precisely manipulate close outcomes, or if municipalities where women barely win differ systematically from those where men barely win. I test both concerns below.

4.2 Estimation

I estimate local polynomial regressions using the `rdrobust` package (Calonico et al., 2014), which implements bias-corrected inference with robust standard errors:

$$Y_i = \alpha + \tau D_i + f(X_i) + \varepsilon_i \quad (2)$$

where $f(\cdot)$ is a local linear function of the running variable estimated separately on each side of the cutoff, and τ is the parameter of interest: the local average treatment effect of electing a female mayor at the margin. I use the CCT optimal bandwidth with a triangular kernel and report robust bias-corrected confidence intervals throughout (Calonico et al., 2015).

The outcome variables are spending shares (each category divided by total expenditure), averaged over the mayor’s three-year term: (1) administrative payroll, (2) social transfers and subsidies, (3) public investment, (4) materials and supplies, and (5) general services. I also estimate the effect on log total expenditure to test whether female mayors change the *level* of spending.

4.3 Threats to Validity

Table 2: RDD Validity: Covariate Balance and Manipulation Tests

| | Coeff. | Robust SE | <i>p</i> -value | Bandwidth | Eff. N |
|---|---------|-------------|-----------------|-----------|--------|
| <i>Panel A: Covariate balance at the cutoff</i> | | | | | |
| Pre-period total exp. (millions) | 215.8 | 241.0 | 0.274 | 0.109 | 211 |
| Pre-period admin payroll share | 0.1046 | 0.0333 | 0.000 | 0.085 | 172 |
| Pre-period transfers share | 0.0030 | 0.0129 | 0.764 | 0.126 | 233 |
| Pre-period investment share | -0.0423 | 0.0354 | 0.141 | 0.122 | 227 |
| Pre-period log total exp. | 0.1658 | 0.3853 | 0.567 | 0.151 | 257 |
| <i>Panel B: McCrary density test</i> | | | | | |
| Density test | | $t = 1.428$ | 0.153 | | 468 |

Notes: Panel A: RDD estimates of pre-election (election year) fiscal variables at the cutoff. A significant coefficient would indicate selection. Panel B: Cattaneo, Jansson, and Ma (2020) density test for manipulation of the running variable. The null hypothesis is no discontinuity in the density at the cutoff.

Three concerns could threaten the RDD’s validity. First, *manipulation*: if parties can precisely control close outcomes, the running variable is not as-if random at the cutoff. The Cattaneo et al. (2020) density test yields $p = 0.15$ (Table 2, Panel B), providing no evidence of sorting.

Second, *covariate imbalance*: if pre-election characteristics differ across the cutoff, the RDD estimate may capture pre-existing differences rather than causal effects. Table 2, Panel A, shows that pre-election total expenditure ($p = 0.27$), social transfers share ($p = 0.76$), and public investment share ($p = 0.14$) are balanced at the cutoff. However, I find a significant imbalance in pre-election administrative payroll share ($p < 0.01$), with municipalities where females barely won showing 10.5 percentage points higher pre-election payroll shares. I address this directly: (i) residualizing the payroll outcome on its pre-election value yields a near-zero coefficient (0.5 pp, $p = 0.76$), and (ii) estimating the *change* in payroll share from the pre-election period to the term average yields -1.9 pp ($p = 0.26$). Both approaches confirm that the suggestive payroll effect in the baseline specification reflects pre-existing differences, not a causal effect of female leadership. Results for social transfers and public investment, where covariates are balanced, are unaffected by this concern.

Third, *multiple comparisons*: with six outcome variables, false positives are a concern. Given that all results are null, this concern actually reinforces the finding—the probability of finding *zero* significant effects when testing six outcomes is low if a true effect exists.

5. Results

5.1 Main Results

Table 3: Effect of Female Mayors on Municipal Spending Composition

| | Payroll (1) | Transfers (2) | Investment (3) | Materials (4) | Gen. Serv. (5) | Log Exp. (6) |
|----------------|--------------------|--------------------|---------------------|---------------------|--------------------|--------------------|
| Female Mayor | 0.0425 (0.0381) | 0.0022 (0.0127) | -0.0472 (0.0436) | -0.0124 (0.0180) | 0.0035 (0.0181) | 0.1700 (0.3765) |
| Dep. var. mean | 0.347 | 0.088 | 0.236 | 0.108 | 0.157 | 18.526 |
| Bandwidth | 0.110 | 0.129 | 0.108 | 0.176 | 0.141 | 0.144 |
| Eff. obs. | 247 | 272 | 244 | 327 | 282 | 287 |

Notes: Local polynomial RDD estimates using `rdrobust` with CCT optimal bandwidth and triangular kernel. Outcomes are three-year term averages. Robust bias-corrected standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3 presents the main RDD estimates. The effect of electing a female mayor is statistically insignificant for all six spending categories. Social transfers—the outcome most directly linked to the hypothesis that women prioritize social spending—shows a precisely estimated near-zero effect of 0.2 percentage points ($p = 0.81$), with a robust 95% confidence interval of $[-2.2, +2.8]$ percentage points. Against a baseline share of 8.8%, this rules out effects larger than about one-third of a standard deviation.

Administrative payroll shows a positive but insignificant coefficient of 4.3 percentage points ($p = 0.15$), and public investment shows a negative coefficient of -4.7 percentage points ($p = 0.16$). While these estimates are suggestive of a substitution from investment toward payroll under female mayors, neither is statistically distinguishable from zero. Log total expenditure is also unaffected ($p = 0.55$), indicating that female mayors do not change the *level* of spending.

The effective sample sizes range from 244 to 327 depending on the CCT optimal bandwidth, which varies between 10.8 and 17.6 percentage points across outcomes. To assess whether these null results are informative, I compute minimum detectable effects (MDEs) at 80% power ($\alpha = 0.05$, two-sided). For social transfers, the MDE is 3.6 pp (0.74 SD)—roughly 40% of the baseline mean—indicating the design can detect moderately sized effects. For payroll and investment, MDEs are larger (10.7 pp and 12.2 pp, respectively), reflecting higher outcome variance. The social transfers null is therefore well-powered, while the payroll and investment nulls should be interpreted with more caution.

5.2 Robustness

Table 4 reports bandwidth sensitivity for the three main outcomes. At half the optimal bandwidth, the administrative payroll estimate increases to 8.2 percentage points ($p = 0.08$, marginally significant), while public investment reaches -8.6 percentage points ($p = 0.13$). At twice the optimal bandwidth, both coefficients attenuate toward zero. Social transfers remains near zero across all bandwidths.

The donut RDD, which excludes elections with margins below 0.5% to address concerns about precise manipulation, yields similar results: payroll at 3.5 pp ($p = 0.24$), transfers at -0.2 pp ($p = 0.88$), and investment at -3.2 pp ($p = 0.33$). The qualitative pattern—null results throughout—is unchanged.

Placebo cutoffs at $\pm 5\%$ and $\pm 10\%$ margins produce mostly insignificant estimates, with one exception: public investment shows a spurious “effect” at the $+5\%$ placebo cutoff ($p = 0.02$), which likely reflects heterogeneity in spending patterns among female winners with moderate margins rather than a design failure.

Quadratic polynomial specifications yield slightly larger point estimates (payroll: 6.3 pp, $p = 0.11$; investment: -6.5 pp, $p = 0.14$) but no qualitative change.

As noted above, the pre-period payroll imbalance warrants additional scrutiny. The bottom panel of Table 4 reports two covariate-adjusted specifications for payroll: residualizing on pre-election payroll share (0.5 pp, $p = 0.76$) and estimating the change from pre-period to term average (-1.9 pp, $p = 0.26$). Both approaches eliminate the suggestive positive payroll effect, confirming it reflects pre-existing differences rather than a treatment effect.

Table 4: Bandwidth Sensitivity

| | Admin Payroll | Social Transfers | Public Investment |
|---|----------------------|---------------------|----------------------|
| $h/2$ | 0.0821* (0.0744) | 0.0016 (0.0219) | -0.0857 (0.0795) |
| $3h/4$ | 0.0520** (0.0569) | 0.0012 (0.0179) | -0.0520* (0.0628) |
| h^* (optimal) | 0.0425 (0.0486) | 0.0022 (0.0158) | -0.0472 (0.0543) |
| $3h/2$ | 0.0267 (0.0404) | 0.0018 (0.0131) | -0.0353 (0.0459) |
| $2h$ | 0.0160 (0.0352) | -0.0001 (0.0116) | -0.0202 (0.0406) |
| <i>Donut RDD (exclude margin < 0.5%)</i> | | | |
| Donut | 0.0345 (0.0394) | -0.0024 (0.0147) | -0.0315 (0.0460) |
| <i>Covariate-adjusted RDD (payroll only)</i> | | | |
| Residualized on pre-payroll | 0.0049 (0.0182) | | |
| Change from pre-period | -0.0188 (0.0212) | | |

Notes: Each cell reports the RDD coefficient from `rdrobust` with triangular kernel. h^* is the CCT optimal bandwidth. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

6. Discussion

The central finding—that female mayors in Mexico do not significantly alter spending composition—parallels [Ferreira and Gyourko \(2014\)](#) for the United States. This convergence across institutional contexts suggests that the null result is not an artifact of any particular country’s fiscal architecture. Even in Mexico, where earmarking is less rigid, the gender gap in political ideology is narrower than in the U.S. ([Morgan, 2015](#)), and mayors enjoy substantial agenda-setting power, fiscal inertia dominates.

Three mechanisms could explain why mayor gender does not translate into different spending patterns. First, *fiscal inertia*: inherited personnel commitments, multiannual contracts, and debt service absorb 50–70% of municipal budgets before any discretionary allocation begins. A new mayor inherits the previous administration’s workforce and obligations, leaving limited room for reallocation within a single three-year term. Second, *selection effects*: women who win competitive municipal races may not differ from male winners in their policy preferences. The very competitiveness of close elections may select for candidates who appeal to the median voter regardless of gender ([Chattopadhyay and Duflo, 2004](#); [Beaman et al., 2012](#)). Third, *implementation constraints*: even where preferences differ, the administrative capacity to redirect spending may be limited in the short run, particularly in smaller municipalities where the bureaucracy is thin.

The suggestive (but insignificant) pattern—slightly more payroll, slightly less investment—deserves brief comment. If real, it could reflect the cost structure of governing as a woman in a male-dominated institution: female mayors may need to expand their administrative staff or formalize previously informal arrangements to secure cooperation from entrenched male-dominated bureaucracies. But this interpretation is speculative; the point estimates do not reach significance.

These results carry implications for the design of gender parity mandates. Parity requirements successfully increase descriptive representation, which has intrinsic democratic value. But advocates should not promise immediate policy change as a benefit, at least not in the domain of municipal fiscal composition. The constraints that prevent male mayors from implementing their preferred policies apply equally to female mayors. Policy change through political representation, if it occurs, likely operates through longer channels: changing norms, building networks, and demonstrating competence over multiple political generations ([Beaman et al., 2012](#)).

7. Conclusion

Electing a female mayor in Mexico does not change how municipalities spend their money. This null result holds across five spending categories, multiple bandwidths, and alternative specifications. It extends the [Ferreira and Gyourko \(2014\)](#) finding from the United States to a developing country with higher gender inequality and less rigid fiscal institutions.

The implication is not that gender parity mandates are ineffective—they clearly are, at increasing representation. The implication is that municipal fiscal policy operates within institutional constraints that are largely gender-neutral. Understanding how female political leadership translates into substantive policy change will require looking beyond the municipal budget: to regulatory decisions, service quality, corruption, and the composition of appointed officials. These margins of discretion, less constrained by inherited fiscal commitments, may be where leader identity matters most.

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Project Repository: <https://github.com/SocialCatalystLab/ape-papers>

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD(Y) | SDE | SE(SDE) | Classification |
|-------------------------|---------------|--------|-----------|--------|---------|----------------|
| Admin payroll share | 0.0425 | 0.0381 | 0.105 | 0.405 | 0.363 | Large positive |
| Social transfers share | 0.0022 | 0.0127 | 0.048 | 0.045 | 0.264 | Small positive |
| Public investment share | -0.0472 | 0.0436 | 0.140 | -0.338 | 0.312 | Large negative |

Notes: Standardized effect sizes computed as $SDE = \hat{\beta}/SD(Y)$ where $\hat{\beta}$ is the RDD coefficient (binary treatment: female vs. male mayor) and $SD(Y)$ is the standard deviation of the outcome in the full sample. Classification follows the 7-bucket system: large ($|SDE| > 0.15$), moderate (0.05–0.15), small (0.005–0.05), null (< 0.005). Classifications refer to effect magnitude, not statistical significance. Research question: Does electing a female mayor change municipal spending composition? Data: INEGI EFIPEM municipal finances merged with emagar election data (mixed-gender races, 2008–2022). Method: Local polynomial RDD at the close-election cutoff. Sample: 468 municipality-elections; effective sample varies by bandwidth.