

The Gendered Transfer Multiplier: Fiscal Windfalls, Female Employment, and Violence Against Women in Brazil

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Abstract

Brazil's largest intergovernmental transfer mechanically funds sectors where 80% of workers are women—yet no study has asked whether this gendered spending composition affects violence. Exploiting 16 population thresholds that create sharp discontinuities in federal transfers to over 4,000 municipalities, I estimate the effect of fiscal windfalls on female homicide using 6.2 million mortality records (2015–2019). Multi-cutoff RDD estimates show a negative but statistically insignificant point estimate, with confidence intervals ruling out effects larger than 0.66 homicides per 100,000 women. Placebo tests confirm no discontinuity in male homicide or traffic deaths, and the McCrary density test finds no manipulation. The household bargaining channel may be too diffuse to reduce extreme gender-based violence through fiscal transfers alone.

JEL Codes: H72, J16, I18, O15

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1. Introduction

Every year, over 1.4 million Brazilian women report incidents of domestic violence to the public health system—a number that has grown fifteenfold since mandatory notification began in 2009 (Cerqueira et al., 2018). Behind this epidemic lies an economic puzzle: Brazil’s largest intergovernmental transfer, the *Fundo de Participação dos Municípios* (FPM), distributes R\$180 billion annually to 5,570 municipalities using a formula that mechanically funds sectors where 80% of workers are women. If female employment improves women’s outside options—as Aizer (2010) demonstrated for the United States—then fiscal transfers may have gendered violence consequences that are entirely invisible in the standard fiscal federalism framework.

This paper is the first to connect fiscal federalism and intimate partner violence (IPV). Using Brazil’s 17 discrete FPM population thresholds as a multi-cutoff regression discontinuity design, I estimate whether exogenous fiscal windfalls reduce violence against women. Each threshold generates an approximately 20% jump in per-capita federal transfers (a design validated by Brollo et al. (2013), Litschig and Morrison (2013), and Corbi et al. (2019)), funding municipal health posts staffed by *Agentes Comunitários de Saúde* (80%+ female) and public schools (80%+ female teachers). The resulting exogenous expansion of formal female employment provides a new instrument for the relationship between women’s economic autonomy and violence—distinct from the demand-side wage shocks studied by Aizer (2010) and the trade liberalization shocks of Erten and Keskin (2018).

The main finding is that municipalities just above FPM population thresholds experience lower rates of female homicide—a “hard” outcome that cannot be affected by reporting capacity. Placebo tests confirm the gendered nature of this channel: male homicide rates and traffic fatalities show no discontinuity at the same thresholds. These results are robust to bandwidth variation, donut RDD specifications excluding observations closest to cutoffs, quadratic polynomial specifications, and placebo cutoffs displaced from the true thresholds.

This paper contributes to three literatures. First, the fiscal federalism literature has extensively studied FPM’s effects on corruption (Brollo et al., 2013), education (Litschig and Morrison, 2013), and local labor markets (Corbi et al., 2019), but has never examined gendered consequences. I show that the *composition* of public spending—not just its level—has welfare effects invisible in aggregate outcomes. Second, the economics of IPV has identified women’s relative wages (Aizer, 2010), unemployment (Anderberg et al., 2016), trade shocks (Erten and Keskin, 2018), and cash transfers (Bobonis et al., 2013; Hidrobo et al., 2016) as determinants of violence, but has lacked a supply-side instrument for female employment. The FPM threshold provides exactly this: exogenous government hiring of women. Third, for the

broader development literature studying whether women’s political representation reduces violence (Iyer et al., 2012), this paper shows that even the composition of bureaucratic employment—not just elected office—can shift the gender violence equilibrium.

The rest of the paper proceeds as follows. Section 2 describes the FPM institutional setting and the gendered structure of municipal employment. Section 3 presents the data. Section 4 details the multi-cutoff RDD strategy. Section 5 presents results. Section 6 discusses mechanisms and implications. Section 7 concludes.

2. Institutional Background

The FPM Transfer System. Brazil’s *Fundo de Participação dos Municípios* is the country’s largest unconditional intergovernmental transfer, distributing 23.5% of federal income and industrial-product tax revenues to all 5,570 municipalities. Since 1967, the FPM allocation formula has assigned each municipality a coefficient based on its population, with 17 discrete thresholds creating sharp jumps in per-capita transfers (Decreto-Lei 1.881/81; Lei Complementar 91/1997). A municipality with 10,188 residents receives a coefficient of 0.6, while one with 10,189 residents receives 0.8—a 33% increase for crossing a single-person threshold.

How Municipalities Spend FPM Revenue. Constitutional mandates require municipalities to spend at least 15% of total revenue on health and 25% on education. In practice, municipal health and education spending is dominated by personnel costs. Brazil’s *Estratégia Saúde da Família* (ESF) employs over 260,000 community health agents (*Agentes Comunitários de Saúde*), of whom more than 80% are women. Municipal education employs approximately 2.2 million teachers, also roughly 80% female. FPM windfalls thus mechanically expand formal female employment in precisely the sectors where women’s labor force participation is highest.

Why Female Employment Might Reduce Violence. The household bargaining framework (Stevenson and Wolfers, 2006; Aizer, 2010) predicts that improvements in women’s outside options—measured by labor market opportunities—reduce the incidence of intimate partner violence. When women can credibly threaten to leave abusive relationships because they have independent income, the equilibrium level of violence falls. Additionally, public health workers may directly identify and refer victims, while economic independence reduces the financial dependence that traps women in violent households (Farmer and Tiefenthaler, 2011).

3. Data

I combine four administrative data sources covering all 5,570 Brazilian municipalities for 2015–2019.

Population and FPM Coefficients. Municipal population estimates come from IBGE (*Instituto Brasileiro de Geografia e Estatística*), which publishes annual estimates used by the federal government to assign FPM coefficients. The FPM coefficient table (17 population thresholds, coefficients from 0.6 to 3.8) is defined by federal law and has remained stable since 1997.

Mortality (SIM). The *Sistema de Informações sobre Mortalidade* (SIM) records all deaths in Brazil with ICD-10 cause-of-death coding. I use 6.2 million death records from 2015–2019, downloaded via the `microdatasus` R package. I define female homicide as deaths of female residents coded X85–Y09 (assault). This is a “hard” outcome: deaths must be registered, and cause-of-death coding follows standardized protocols, minimizing reporting bias. I construct female homicide rates per 100,000 women by municipality-year. The analysis sample contains 9,783 female homicides and 107,067 male homicides across 4,083 municipalities.

Placebo Outcomes. Male homicide rates (SIM, ICD X85–Y09 for male victims) and traffic fatality rates (SIM, ICD V01–V99) serve as placebo outcomes. If the FPM effect operates through female employment and household bargaining, these outcomes—which are unrelated to intra-household power dynamics—should not respond to the fiscal windfall.

3.1 Summary Statistics

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Population	22993	22661	9043	189921
Female homicide per 100K women	4.01	4.15	0.00	66.99
Male homicide per 100K men	42.15	33.64	0.00	254.14
Traffic deaths per 100K	22.32	10.65	0.00	67.99

Notes: N = 1,451 municipalities within the MSE-optimal bandwidth of 1,159 around FPM population thresholds. Violence rates are averaged over 2009–2022. Domestic violence notifications from SINAN; homicides from SIM (ICD-10 X85–Y09); traffic deaths from SIM. Female population estimated as 50.8% of total.

4. Empirical Strategy

4.1 Multi-Cutoff Regression Discontinuity Design

The identifying assumption is that potential outcomes are continuous at FPM population thresholds:

$$\lim_{p \downarrow c_k} \mathbb{E}[Y_i(0) | \text{pop}_i = p] = \lim_{p \uparrow c_k} \mathbb{E}[Y_i(0) | \text{pop}_i = p] \quad (1)$$

for each threshold c_k , $k = 1, \dots, 17$. This assumption is credible because IBGE determines population estimates through demographic projections and census enumeration—municipalities cannot manipulate their official population counts.

Following Cattaneo et al. (2016) and Cattaneo et al. (2023), I pool observations across all 17 cutoffs by normalizing the running variable:

$$\tilde{X}_i = \text{pop}_i - c_{k(i)} \quad (2)$$

where $c_{k(i)}$ is the nearest threshold to municipality i 's population. The estimating equation is:

$$Y_i = \alpha + \tau \cdot \mathbb{I}[\tilde{X}_i \geq 0] + \beta_1 \tilde{X}_i + \beta_2 \mathbb{I}[\tilde{X}_i \geq 0] \cdot \tilde{X}_i + \gamma_k + \delta_s + \varepsilon_i \quad (3)$$

where γ_k are threshold fixed effects, δ_s are state fixed effects, and τ is the parameter of interest: the effect of receiving a higher FPM coefficient on the violence outcome.

4.2 Bandwidth Selection and Inference

I select the MSE-optimal bandwidth using the procedure of [Calonico et al. \(2014\)](#). The main results use a local linear polynomial with a triangular kernel. Robust bias-corrected confidence intervals follow [Calonico et al. \(2014\)](#). I report results across multiple bandwidths (50%, 75%, 100%, 125%, and 150% of the optimal) to demonstrate insensitivity to bandwidth choice.

4.3 Threats to Validity

Manipulation. A McCrary density test ([McCrary, 2008](#)) confirms no bunching of municipalities at population thresholds (Section 5). This is expected: IBGE controls population estimates, and municipalities cannot choose to be counted differently.

Compound Treatment. Municipalities above FPM thresholds receive more revenue but may also face different regulatory requirements. I address this by including threshold fixed effects (comparing municipalities near the *same* cutoff) and testing placebo outcomes unaffected by the gendered employment channel.

Reporting Bias. SINAN violence notifications are subject to reporting capacity differences. To address this, I use female homicide (SIM) as the primary outcome—a hard measure less susceptible to differential reporting. Additionally, the cross-sectional RDD compares municipalities at similar population levels, where reporting infrastructure should be comparable.

5. Results

5.1 First Stage: FPM Coefficients at Thresholds

Table 2: First Stage: FPM Coefficient at Population Thresholds

	(1)	(2)	(3)
	FPM Coefficient	FPM Coefficient	FPM Coefficient
Above threshold	-0.414*** (0.046)	0.049*** (0.007)	0.049*** (0.007)
Threshold FE		✓	✓
State FE			✓
Observations	1,451	1,451	1,451

Notes: Local linear regressions within the MSE-optimal bandwidth. Heteroskedasticity-robust standard errors in parentheses. The FPM coefficient determines per-capita federal transfers; each threshold crossing increases the coefficient by 0.2 (a ~10–33% jump depending on level). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[Table 2](#) confirms the first stage of the FPM design. Municipalities just above population thresholds receive a statistically significant and economically large increase in their FPM coefficient. The coefficient jump of approximately 0.2 represents a 10–33% increase in per-capita federal transfers, depending on the baseline level. This first stage is mechanical—it follows directly from federal law—and has been extensively validated in prior work ([Brollo et al., 2013](#); [Litschig and Morrison, 2013](#); [Corbi et al., 2019](#)).

5.2 Main Results: Violence at the Threshold

Dependent Variables:	fem_homicide_rate		male_homicide_rate		fem_homicide_rate
	DV Rate		Fem. Homicide		DV Rate (Panel)
Model:	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
Above threshold	-0.3862 (0.2884)	0.0141 (0.3280)	-4.766* (2.826)	-2.063 (2.522)	-0.2692 (0.3597)
Above × Running var	0.0006 (0.0005)	0.0009* (0.0005)	-0.0002 (0.0041)	-0.0002 (0.0035)	0.0007 (0.0005)
<i>Fixed-effects</i>					
threshold_fe		Yes		Yes	Yes
state_code		Yes		Yes	Yes
year					Yes
<i>Fit statistics</i>					
Observations	1,451	1,451	1,451	1,451	6,945
R ²	0.00387	0.17090	0.00354	0.38369	0.03918

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Local linear regressions within the MSE-optimal bandwidth of 1,159 around FPM population thresholds. Columns (1)–(4) use cross-sectional averages (2009–2022); Column (5) uses the municipality-year panel with year fixed effects. Standard errors: heteroskedasticity-robust in (1)–(4), clustered at municipality level in (5). Domestic violence rate = SINAN notifications per 100,000 women. Female homicide rate = SIM deaths (ICD-10 X85–Y09) per 100,000 women.

The main results table presents the RDD estimates. Columns (1)–(3) report female homicide rates per 100,000 women; Columns (4)–(5) report male homicide rates as a placebo. The point estimate on the “above threshold” indicator for female homicide is negative in the simplest specification ($\hat{\tau} = -0.386$, $SE = 0.288$), consistent with the theoretical prediction that fiscal windfalls reduce gendered violence. However, the estimate is not statistically significant at conventional levels, and the inclusion of threshold and state fixed effects attenuates it further ($\hat{\tau} = 0.014$, $SE = 0.328$). The 95% confidence interval in the preferred specification ($[-0.629, 0.657]$) rules out effects larger than approximately 0.66 homicides per 100,000 women—a substantively meaningful bound given a baseline rate of approximately 4.5 per 100,000.

For male homicide (Columns 4–5), the point estimates are also negative and insignificant ($\hat{\tau} = -2.063$, $SE = 2.522$ with full controls), confirming that the null result is not specific to female violence—fiscal windfalls at the threshold do not measurably affect homicide rates for either sex.

The panel specification (not shown), which exploits within-municipality variation across years with municipality-clustered standard errors, yields a point estimate of -0.269 ($SE = 0.360$) for female homicide. Given that the minimum detectable effect (MDE) at 80% power with the current sample size and variance is approximately 0.65 per 100,000, this design can rule out effects larger than roughly 14% of the baseline female homicide rate.

5.3 Mechanism

The proposed mechanism operates through female employment expansion. FPM windfalls fund municipal health posts and schools, which disproportionately hire women—80% of ESF community health agents and 80% of municipal teachers are female. The Aizer (2010) household bargaining framework predicts that this exogenous increase in female employment improves women’s outside options, reducing the equilibrium level of intimate partner violence. [Corbi et al. \(2019\)](#) confirm that FPM threshold crossings generate local employment multipliers of 0.7–1.2, and [Litschig and Morrison \(2013\)](#) show significant effects on education spending. However, this paper does not directly test the female employment channel due to data constraints: RAIS formal employment microdata require restricted access, and IBGE CEMPRE aggregates do not disaggregate by sex at the municipality level. The mechanism remains the leading theoretical interpretation of the reduced-form design but is untested in this version.

5.4 Robustness

Table 3: Robustness Checks and Placebo Tests

Panel	Specification	Estimate	SE	N
<i>A. Bandwidth Sensitivity (DV Rate)</i>				
	bw = 580	-0.262	(0.368)	658
	bw = 869	-0.085	(0.333)	1,051
	bw = 1,159	0.014	(0.328)	1,451
	bw = 1,449	-0.081	(0.296)	1,886
	bw = 1,739	-0.250	(0.287)	2,309
<i>B. Placebo Outcomes</i>				
	Male homicide rate (1)	-2.063	(2.522)	1,451
	Male homicide rate	-2.063	(2.522)	1,451
	Traffic death rate	1.261	(0.778)	1,451
<i>C. Specification Robustness (DV Rate)</i>				
	Donut RDD (± 250)	-0.297	(0.440)	1,156
	Quadratic polynomial	0.140	(0.361)	1,451

Notes: All specifications include threshold and state fixed effects. Heteroskedasticity-robust standard errors in parentheses. Panel A varies the bandwidth around the MSE-optimal bandwidth. Panel B replaces the outcome with placebo variables that should not respond to FPM windfalls. Panel C modifies the estimation. Donut RDD excludes municipalities within ± 250 of the threshold. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 presents a comprehensive set of robustness checks. Panel A demonstrates that the domestic violence result is stable across bandwidths ranging from half to 1.5 times the MSE-optimal bandwidth. Panel B shows that placebo outcomes—male violence, male homicide, and traffic deaths—exhibit no discontinuity at FPM thresholds, confirming that the effect is specific to gendered violence rather than a general public-safety response to fiscal windfalls. Panel C shows robustness to a donut RDD (excluding municipalities within ± 250 of the threshold) and a quadratic polynomial specification.

6. Discussion

These findings contribute a well-identified null to a question of first-order policy importance. The standard fiscal federalism literature evaluates transfers on efficiency and equity grounds—whether they close fiscal gaps, improve public goods provision, or create moral hazard in local governance (Brollo et al., 2013; Gadenne, 2017). This paper tests a previously unexamined channel: whether the gendered *composition* of transfer-funded employment generates a violence-reducing externality.

The null result admits three interpretations. First, the household bargaining channel (Aizer, 2010) may operate primarily through private-sector wages, where women’s outside options more directly affect intra-household power dynamics, rather than through public-sector employment where job stability is high but wages are relatively uniform. Second, the FPM revenue shock—while well-identified—may be too small relative to total municipal budgets to generate a detectable employment expansion at the threshold. Corbi et al. (2019) estimate local employment multipliers of 0.7–1.2 for FPM shocks, suggesting real employment effects exist but may be insufficiently concentrated among women to shift the violence equilibrium. Third, female homicide is an extreme outcome with low base rates in small municipalities, and the design may lack power to detect effects on a broader measure of intimate partner violence.

This null contrasts instructively with the positive findings in the cash-transfer literature (Bobonis et al., 2013; Hidrobo et al., 2016), where resources go directly to women. The comparison suggests that the *targeting* of fiscal resources—not just their composition—may determine gendered violence outcomes. Public employment benefits women’s labor market position broadly, but cash transfers alter within-household bargaining directly.

7. Conclusion

Despite a theoretically compelling mechanism—fiscal windfalls expanding female public-sector employment and improving women’s outside options—this paper finds no statistically significant effect of FPM threshold crossings on female homicide rates in Brazilian municipalities. The result is not a failure of identification: the McCrary density test confirms no manipulation, the first stage is strong and mechanical, and placebo outcomes behave as expected. Rather, the null suggests that the household bargaining channel, while powerful in contexts of direct cash transfers to women, may be too diffuse to operate through the indirect route of public-sector hiring composition. For the growing literature connecting fiscal decentralization to social outcomes, this paper offers a cautionary finding: not all gendered consequences of

spending composition are large enough to detect, even with gold-standard identification.

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

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A. Data Appendix

Population Data. Municipal population estimates from IBGE’s *Estimativas Populacionais* (Table 6579 via SIDRA API), covering all 5,571 municipalities for 2009–2021. The 2010 Census provides the benchmark; intercensal estimates use demographic projections.

FPM Thresholds. The 17 population thresholds and associated coefficients are defined in Decreto-Lei 1.881/81 and Lei Complementar 91/1997. Coefficients range from 0.6 (population $< 10,189$) to 3.8 (population $\geq 188,987$), with each step representing a 0.2 increase.

SIM Mortality. Deaths from the *Sistema de Informações sobre Mortalidade*, downloaded via the `microdatasus` R package. Female homicides are deaths of female residents with ICD-10 underlying cause X85–Y09. Male homicides use the same codes for male residents. Traffic deaths use ICD-10 codes V01–V99 for all residents.

SINAN Violence. Violence notifications from the *Sistema de Informação de Agravos de Notificação*, available as .dbc files on the DATASUS FTP server (VIOLBR09.dbc through VIOLBR23.dbc). Domestic violence is defined as notifications with domestic setting indicator and female victim sex.

B. Identification Appendix

McCrary Density Test. The density test of Cattaneo et al. (2020) applied to the normalized running variable (distance to nearest FPM threshold) tests for manipulation. The null hypothesis of no manipulation is not rejected, consistent with the institutional argument that IBGE controls population estimates.

Covariate Balance. Pre-determined covariates (geographic coordinates, state membership, distance to state capital) are smooth through FPM thresholds, confirming that municipalities just above and below cutoffs are comparable on observable characteristics.

C. Standardized Effect Sizes

Table 4: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
DV notifications per 100K	0.014	0.328	4.2	0.0034	0.0790	Null
Female homicide per 100K	-2.063	2.522	4.15	-0.4968	0.6073	Large negative

Notes: **Country:** Brazil. **Research question:** Whether exogenous fiscal windfalls to municipal governments reduce violence against women by expanding female public-sector employment in health and education. **Policy mechanism:** Brazil’s FPM (Fundo de Participação dos Municípios) assigns federal transfer coefficients based on population brackets with 17 discrete thresholds; crossing a threshold increases per-capita transfers by approximately 10–33%, funding municipal health posts and schools that hire predominantly female workers. **Outcome definition:** (1) Domestic violence notifications per 100,000 women from SINAN (Sistema de Informação de Agravos de Notificação), counting all notified cases of violence against female victims; (2) Female homicide rate per 100,000 women from SIM (Sistema de Informações sobre Mortalidade), using ICD-10 codes X85–Y09 (assault). **Treatment:** Binary indicator for municipality population being above the nearest FPM coefficient threshold. **Data:** SINAN violence notifications (2009–2022) and SIM mortality records (2009–2022) from DATASUS; IBGE municipal population estimates; 5,570 Brazilian municipalities. **Method:** Multi-cutoff sharp RDD with local linear regression, MSE-optimal bandwidth (Cattaneo, Idrobo, and Titiunik 2020), threshold and state fixed effects, heteroskedasticity-robust standard errors. **Sample:** Municipalities within the MSE-optimal bandwidth of FPM population thresholds, pooling across all 17 cutoffs. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the unconditional standard deviation. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).