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Control Over Resources and Women's Political Empowerment: Evidence from Brazil

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Abstract

Brazil's Bolsa Família Programme, a conditional cash transfer paid exclusively to women, raised the female candidacy rate in municipal elections by 2.46 percent relative to the pre-reform mean while producing no detectable effect on the female electoral success rate. The estimates exploit a 2009 reform that generated plausibly exogenous variation in programme expansion across municipalities. Coarsened Exact Matching is combined with a two-way fixed effects difference-in-differences design across more than 5,000 Brazilian municipalities and three election years. The evidence indicates that targeted financial resources lower barriers to political entry for women without shifting the structural determinants of electoral success.

Keywords: *Political Economy, Development Economics, Economics of Gender, Cash Transfers*

1. Introduction

Women remain politically underrepresented across the world, especially in developing economies, and the causes of this underrepresentation are contested (Casas-Arce and Saiz, 2015; Hessami and da Fonseca, 2020). It is argued that economic constraints are a binding barrier to women's empowerment in general (Duflo, 2012); specifically, poverty limits the time, resources, and networks available to women who might otherwise participate in politics, so that improving women's economic position should expand their political representation. This claim has far-reaching policy implications: if correct, programmes already operating at scale to reduce poverty should simultaneously advance women's political empowerment, and dedicated political interventions would not be required.

This paper tests that proposition by examining whether an exogenous increase in financial resources directed at women can causally affect their political empowerment. Political empowerment is proxied at the local level through two outcomes in municipal elections: the female candidacy rate and the female electoral success rate. Brazil's conditional cash transfer programme (CCT), PBF (Programa Bolsa Família), paid exclusively to women, provides the exogenous variation this design requires.

Gender equality is a normative end in itself, but it is also economically consequential: gender inequality undermines economic development (Klasen and Lamanna, 2009; Hsieh et al., 2019; Cavalcanti and Tavares, 2016). More importantly, women's political under-representation is found to reduce both aggregate output and allocative efficiency (Besley et al., 2017; Chattopadhyay and Duflo, 2004). From a policy standpoint, CCTs, including PBF, have become central policies across the developing world, already reaching hundreds of millions of households, with payments often directed exclusively to women. Therefore, if such programmes shift the composition of political participants even as a side effect, this carries implications for how policymakers evaluate their returns beyond their direct objectives.

To identify a causal effect, the analysis exploits a 2009 reform of PBF that mechanically revised municipal beneficiary quotas using a new statistical methodology developed by the World Bank. As Gerard, Naritomi and Silva (2025) document, the reform generated sharp cross-municipal variation in programme expansion driven by a top-down technical decision that municipalities could neither anticipate nor influence. Because PBF payments are made exclusively to women, this expansion directly increased the financial resources available to female household members. The resulting plausibly exogenous variation is used to define a binary treatment indicator based on whether a municipality experienced above- or below-median expansion following the reform. The identification strategy combines Propensity Score Matching with a two-way fixed-effects Difference-in-Differences framework, applied to a panel of over 5,000 Brazilian municipalities across three municipal election years: 2004, 2008, and 2012.

The results point to a clear asymmetry. PBF expansion significantly increased the female candidacy rate by 2.46 percent relative to the pre-reform mean, while producing no detectable effect on the female electoral success rate. This pattern suggests that targeted financial resources lower the barriers to political entry for women but cannot overcome the structural and institutional disadvantages that determine electoral outcomes.

This paper contributes to the literature on women's political empowerment by providing quasi-experimental evidence on how household-level economic interventions affect women's political participation. Duflo (2012) identifies political representation as the dimension most resistant to improvements in women's economic position; the findings here are consistent with that resistance for electoral success but indicate that economic resources can shift candidacy. The paper also extends the literature on the political economy of cash transfers, which has focused primarily on voting behaviour and incumbent support, to candidacy and electoral outcomes.

2. Literature Review

This paper draws on three strands of literatures: (1) how economic resources targeted at women, especially CCTs, can yield benefits extending to their broader empowerment; (2) how CCTs shape political behaviour more generally; and (3) how women's presence in office strengthens their political power. Each strand informs a distinct aspect of the research question, and the fact that these literatures have rarely been brought together motivates this study.

2.1. Women-Targeted Economic Resources and Women's Empowerment

The first strand of the literature examines the gendered impact of CCTs. A defining feature of several major CCT programmes, including Brazil's Bolsa Família, is that payments are targeted at women. This design rests on the premise that the identity of the income recipient affects how resources are allocated within households. Evidence from a range of settings supports this premise. For example, women-targeted conditional cash transfers are found to reduce domestic violence in Ecuador (Hidrobo and Fernald, 2013), shift household expenditure toward more nutritious food in North Macedonia (Armand et al., 2020), improve children's health outcomes in Mexico (Gertler, 2004), and, most relevant to this study, increase women's decision-making power and attitudinal autonomy in Mexico (Attanasio and Lechene, 2002; Urbina, 2020). These findings demonstrate that CCTs have consequences beyond the transfer itself: they shift bargaining power within the household across multiple dimensions, with gains concentrated among women. Yet whether such shifts extend beyond the household and into the political sphere has received little attention.

The premise that economic resources directed at women can strengthen their position is supported beyond the CCT literature. For example, Duflo (2003) shows that pension income received by grandmothers improved granddaughters' nutritional outcomes; Qian (2008) also finds that exogenous increases in female income raised girls' survival rates and educational attainment. Both findings reinforce the broader point that economic resources directed at women produce consequences that extend well beyond income itself. However, these studies document improvements in outcomes for women's dependants rather than changes in women's own position. The CCT literature goes further as they have been shown to increase women's decision-making power and autonomy within the household. This makes CCTs a particularly promising setting in which to ask whether the empowerment women gain within the household can extend into the political sphere.

2.2. Cash Transfers and Political Behaviour

The second strand of the literature investigates whether CCTs influence political behaviour, with most findings centred on voting. A consistent pattern emerges: CCTs increase incumbent support. Manacorda, Miguel and Vigorito (2011) find that receipt of Uruguay's PANES programme increased support for the incumbent national government by 11 to 13 percentage points. De La O (2013) shows that early exposure to Mexico's Progresa programme increased both incumbent vote shares and voter turnout in presidential elections. In the Brazilian context, Zucco (2013) finds that PBF increased electoral support for presidential incumbents, though identification relies on observational methods rather than a quasi-experimental design.

More relevant to the purpose of this study, Conover et al. (2020) document gender-differentiated political responses to Colombia's Familias en Acción: female beneficiaries became more likely to register, to vote, and to support incumbents. They attribute these effects in part to recurrent interactions between female recipients and programme administrators, which increase women's exposure to public institutions and raise their awareness of political participation.

These studies provide strong evidence that cash transfers shape political behaviour. Two aspects, however, remain outside their scope and are central to the study of women's political empowerment. First, they measure voting and partisan preferences rather than political candidacy and representation. Whether the mechanisms documented here extend to higher forms of political engagement, such as standing for office, remains untested. Second, they focus on national elections, where individual beneficiaries are a negligible share of the electorate and outcomes are dominated by macro-political and macroeconomic conditions rather than the local dynamics most likely to mediate women's political participation.

2.3. Women's Political Empowerment

A more consequential dimension of women's political empowerment than voting is their entry into political office, since officeholding carries direct policy implications that voting alone does not. Chattopadhyay and Duflo (2004) found that when women hold office, public goods provision shifts measurably toward women's stated preferences, using randomly assigned gender quotas for village council leadership in India. Beaman et al. (2009) followed to examine how such quotas produced lasting consequences, finding that they increased the likelihood of women winning elections more broadly and reduced stereotypical perceptions of women in the public sphere. At a higher level of government, Bhalotra, Clots-Figueras, and Iyer (2018), using a regression discontinuity design on Indian constituency-level data, found that women's electoral success as state legislators leads to an increase in women's candidacy in subsequent election rounds, although this increase is primarily driven by incumbents choosing to recontest.

These findings establish that women's political representation in office is not merely symbolic but substantively improves women's political empowerment in both the short and long run, making candidacy and electoral success an important metric for measuring female political empowerment. More importantly, women serving as decision-makers in office represents a stronger form of political empowerment than women participating solely as voters, at least when considering which dimension offers a more meaningful measure of empowerment.

The studies discussed above, however, examine the consequences of women holding office rather than the pathways through which women attain it. This research therefore focuses on the latter question: specifically, whether economic resources can improve women's chances of winning office, which in turn would strengthen their political empowerment.

2.4. What Is Missing

Despite the breadth of these three strands of literature, no study (to the best of my knowledge) examines whether female-targeted resource increases (from conditional cash transfer programmes) translate into women's political empowerment, measured through candidacy and electoral success. The novelty of this paper therefore lies in combining an exogenous, reform-driven shock to the coverage of Brazil's women-targeted conditional cash transfer program with municipal-level electoral data on women's candidacy and electoral outcomes to identify the causal effect of expanding economic resources targeted at women on their political empowerment.

3. Data

This study assembles a municipality level panel dataset from three primary sources: Bolsa Família programme data, municipal socioeconomic indicators, and electoral records from Brazilian municipal elections. These datasets are merged using IBGE and TSE municipality codes¹, resulting in a panel of over 5,000 municipalities.

3.1. Bolsa Família Program Data

The first dataset is obtained from administrative records published by the Ministry of Social Development (MDS; *Ministério do Desenvolvimento Social e Combate à Fome*). It provides the annual number of beneficiary households for each municipality from 2007 to 2011, from which the change in per capita programme coverage following the 2009 reform is computed to define the treatment variable.

3.2. Municipal Socioeconomic Indicators

The second dataset draws on the Institute of Applied Economic Research (IPEA; Instituto de Pesquisa Econômica Aplicada) and the Brazilian Institute of Geography and Statistics (IBGE; Instituto Brasileiro de Geografia e Estatística). It contains baseline municipal characteristics predetermined with respect to the 2009 reform: GDP per capita, HDI, Gini coefficient, non-white population ratio, and total population. These variables serve as matching covariates for propensity score estimation.

3.3. Electoral Data

The third dataset is obtained from the Superior Electoral Court (TSE; *Tribunal Superior Eleitoral*), Brazil's national electoral authority. It provides candidate level records for the 2004, 2008 and 2012 municipal elections, covering two pre-reform periods and one post-reform period. Two outcome variables are constructed from these records. The first, the female candidacy rate, is the share of candidates in municipality m in election year e who are female, capturing women's entry into the political process. The second, the female electoral success rate, is the proportion of female candidates in municipality m in election year e who were elected, capturing women's ability to convert candidacy into office conditional on entry. The two outcomes are examined in separate regressions, as they proxy distinct dimensions of political empowerment: participation and representation.

4. Methodology

4.1. Identification Strategy

4.1.1. The 2009 Quota Reform

This study's identification strategy is inspired by Gerard, Naritomi and Silva (2025), who exploit a 2009 reform of PBF to generate quasi-experimental variation in programme coverage across municipalities.

In PBF, each municipality is allocated a quota, defined as the maximum number of beneficiary slots, based on poverty measures computed by the Brazilian Census Bureau. These quotas are not strict limits, but in practice higher quotas induced greater PBF expansion; that is, municipalities with larger quotas received more new beneficiary slots. Quotas are recalculated every three years using a formula applied to demographic characteristics from earlier census rounds.

The 2009 revision introduced, for the first and only time, a new statistical methodology developed by the World Bank to compute municipal poverty measures. The cross-municipal variation in quota changes arising from this reform is plausibly exogenous because it was driven by a one-off, top-down adoption of a new statistical methodology — a technical decision made at the national level that individual municipalities could neither anticipate nor influence. Moreover, since the new method's inputs were drawn entirely from predetermined data sources, the variation in how quotas shifted across municipalities reflects pre-existing demographic characteristics rather than any contemporaneous change in local economic or political conditions.²

Using realised expansion directly may introduce endogeneity concerns, since realised coverage could partly reflect municipality-level characteristics correlated with female political empowerment. However, if the 2009 reform was the dominant driver of PBF expansion in this period, then cross-municipal variation in realised coverage closely tracks variation in quota changes, and so inherits much of its exogeneity. Gerard, Naritomi and Silva (2025) provide evidence that this condition holds: they estimate the reform raised the number of beneficiaries by 17%, nearly two million families, during a period of steady economic growth, and find it induced the sharpest cross-municipal expansion in PBF coverage on record, confirming that municipalities broadly acted on their revised quotas. The endogeneity concern is therefore substantially mitigated.

Nonetheless, endogeneity likely persists, albeit attenuated by the 2009 reform. This research therefore takes further steps to address this concern, as discussed later in this section.

4.2. Baseline Differences-in-Differences Specification

Following Gerard, Naritomi and Silva (2025), the primary empirical specification is a two-way fixed effects (TWFE) Difference-in-Differences (DiD) model.

4.2.1. Construction of the Treatment and Control Groups

The treatment variable is constructed as the change in per capita Bolsa Família coverage in each municipality following the 2009 reform. Specifically, for municipality m , the expansion measure is defined as:

$$Expansion_m = \frac{\frac{1}{2}(B_{m,2010} + B_{m,2011})}{Pop_{m,2010}} - \frac{\frac{1}{2}(B_{m,2007} + B_{m,2008})}{Pop_{m,2007}}$$

where $B_{m,t}$ denotes the number of PBF beneficiary households in municipality m in year t , and $Pop_{m,2007}$ and $Pop_{m,2010}$ are municipal population from the 2007 and 2010 census counts respectively. The post-reform average covers 2010 and 2011, and the pre-reform average covers 2007 and 2008. The treatment year, 2009, is excluded. Each averaging window is divided by the population from the census year nearest to it. Two-year averages on each side of the reform, rather than a single year, are

¹ TSE and IBGE municipality codes are matched using the crosswalk compiled by Johannessen (2019).

² Specifically, the 2009 quota was calculated as

$$Quota_{m,2009} = Poor_{s,2006} \times \frac{Poor_{m,s,2006}}{\sum_{m \in s} Poor_{m,s,2006}}$$

Where $Poor_{m,s,2006}$ is a municipal-level estimate of the number of poor families in 2006, constructed by training a per capita income prediction model on the 2006 PNAD survey and applying it to 2000 census microdata. The state-level poverty count $Poor_{s,2006}$ is drawn directly from the 2006 PNAD. All inputs to this formula therefore rely exclusively on data from 2006 or earlier, meaning the quota variation they generate is predetermined relative to any outcomes observed after the reform's implementation in 2009.

used to smooth out year-to-year noise in administrative enrolment data, producing a more stable estimate of the underlying shift in coverage.

Municipalities are then classified into a binary treatment indicator: those with above-median expansion are assigned to the treatment group and those with below-median expansion to the control group. The median is a natural threshold, as it balances the treated and control groups by construction and maximises statistical power. To assess whether the choice of cutoff drives the results, Appendix A reports the full DiD estimation across four alternative threshold definitions. The candidacy coefficient is positive across all specifications and rises monotonically with contrast sharpness, while the electoral success coefficient is indistinguishable from zero throughout. A continuous treatment specification is also reported in the appendix. It is worth noting that this treatment variable represents a simplified version of what Gerard, Naritomi and Silva (2025) construct. Their measure isolates the exogenous component of expansion by computing the difference between actual 2009 quota and counterfactual 2009 quota constructed using the old formula, which more cleanly separates the policy-driven variation from any endogenous uptake. Since constructing such a counterfactual requires the confidential quota data, this study relies on realised coverage changes directly. The absence of quota data therefore worsens the endogeneity concern: it precludes not only the construction of an IV instrument, but also the identification of a clean counterfactual group. This further motivates the pre-trend analysis and matching presented later.

4.2.2. The TWFE DiD Specification

$$Y_{m,e} = \alpha_m + \theta_e + \beta(Treat_m \times Post_e) + \varepsilon_{m,e}$$

where $Y_{m,e}$ is an outcome variable measuring women's political empowerment in municipality m during election year e . Two outcomes are considered in separate regressions: the share of female candidates and the electoral success rate of female candidates, to proxy female political power. The municipality fixed effects α_m absorb all time-invariant differences across municipalities, and the election-year fixed effects θ_e control for nation-wide shocks common to all municipalities in a given election year. The treatment group indicator $Treat_m$ equals one for above-median expansion municipalities, and $Post_e$ equals one for the post-reform election year (2012). The parameter of interest, β , captures the differential change in the outcome between high- and low-expansion municipalities after the reform. The two DiD regressions give the following results:

Table 1: Difference-in-Difference Estimates

	(1) Female Candidacy	(2) Female Success
$Treat_m \times Post_e$	0.00422*** (0.00162)	-0.00162 (0.00358)
<i>Municipality FE</i>	Yes	Yes
<i>Year FE</i>	Yes	Yes
<i>Observations</i>	16,646	16,593
<i>Within R²</i>	6.51×10^{-4}	1.51×10^{-5}
<i>Adjusted R²</i>	0.614	0.371

Notes: Standard errors clustered at the municipality level in parentheses. The success-rate sample is smaller because the outcome is undefined for municipality-years with zero female candidates.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For the female candidacy outcome, the DiD estimate is positive ($\beta = 0.00422$) and statistically significant at the 1% level ($p = 0.0093$), suggesting that municipalities with above-median Bolsa Familia expansion experienced a relative increase in the share of female candidates following the 2009 reform. For the electoral success rate, the DiD estimate is negative and statistically insignificant ($\beta = -0.00162$, $p = 0.652$), providing no evidence that programme expansion affected female candidates' electoral success. The within R^2 is small for both outcomes, as is typical in TWFE specifications where municipality and year fixed effects absorb a large share of outcome variation; it therefore offers limited information and is not a substantive concern here. The contrast between the two outcomes motivates treating them separately throughout the analysis.

4.2.3. Diagnostic Checks

A critical identifying assumption of the DiD framework is the parallel trends assumption: in the absence of the 2009 reform, high- and low-expansion municipalities would have followed comparable trajectories in female political empowerment. This assumption is not directly testable, since the counterfactual outcome for treated municipalities is never observed. However, its plausibility can be assessed indirectly through a series of diagnostic checks, each of which is informative but not definitive.

(i) Event Study. A standard event-study specification is estimated for both outcomes, using 2004 as the omitted base year and 2008 as the single pre-period coefficient. Under the null of parallel pre-trends, the 2008 coefficient should be statistically indistinguishable from zero; the estimates for both the female candidacy rate and the success rate are indeed insignificant. The 2012 coefficient corresponds to the post-reform treatment effect on the unmatched panel, and is consistent in sign and significance with the matched estimate reported in Table 6. These provide some support for the validity of the DiD design.

$$Y_{m,e} = \alpha_m + \theta_e + \sum_{e \neq 2004} \beta_e (Treat_m \times \mathbf{1}\{e\}) + \varepsilon_{m,e}$$

with β_{2008} as the pre-period placebo coefficient and β_{2012} as the treatment effect. This test should nonetheless be interpreted cautiously, since a one-period single insignificant coefficient provides weak evidence against pre-existing differential trends. As a result, the event study is a necessary but not sufficient diagnostic.

(ii) Imbalance in Baseline Characteristics. Passing pre-trend tests establishes that treatment and control municipalities moved in parallel prior to the reform, but it does not ensure that the two groups are otherwise comparable. A more substantive concern arises from the composition of the treatment and control groups; if municipalities with above-median expansion systematically differ from those with below-median expansion in characteristics that independently affect female political empowerment, the post-reform divergence in outcomes cannot be attributed to the programme alone, as it is confounded by other variables. Gerard, Naritomi and Silva (2025) show that defining treatment on the basis of realised coverage in 2008 produces systematically imbalanced groups, with treated and control municipalities differing significantly across several baseline economic characteristics. Although their argument draws on 2008 realised expansion specifically, it applies equally to this study's treatment and control groups, which are similarly constructed on the basis of realised expansion averaged across years.

Table 2: Event Study Estimates

	(1) Female Candidacy	(2) Female Success
$Treat_m \times \mathbf{1}\{e = 2008\}$	0.00128 (0.00175)	0.00110 (0.00529)
$Treat_m \times \mathbf{1}\{e = 2012\}$	0.00486*** (0.00183)	-0.00107 (0.00451)
<i>Municipality FE</i>	Yes	Yes
<i>Year FE</i>	Yes	Yes
<i>Observations</i>	16,646	16,593
<i>Within R²</i>	6.96×10^{-4}	1.89×10^{-5}
<i>Adjusted R²</i>	0.614	0.370

Notes: 2004 is the omitted base year. Standard errors clustered at the municipality level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This concern is confirmed empirically: regressing the treatment indicator on baseline log GDP per capita yields a strongly negative and statistically significant coefficient ($\beta = -0.25$, $p \approx 0$), as reported in Table 3, indicating that municipalities with higher pre-reform GDP per capita are systematically less likely to be assigned to the treatment group. This is intuitive: since Bolsa Família is designed to reach poorer households, municipalities that expanded more following the reform tend to be those that were already poorer before it. GDP per capita is itself likely correlated with female political empowerment, since wealthier municipalities tend to have more educated populations and stronger civic infrastructure. Combined with its correlation with PBF expansion, GDP would confound the treatment-outcome relationship.

Moreover, since GDP per capita is correlated with the other socioeconomic indicators including HDI, the Gini coefficient, racial composition, and population, this finding raises the concern that those variables would exhibit comparable or even

greater imbalance across the treated and control groups. Correcting for a single covariate is therefore unlikely to be sufficient, motivating the multi-covariate matching approach adopted in the next section.

This imbalance is confined to economic and demographic characteristics, however. A related concern is that realised expansion may correlate with local gender norms, administrative capacity, or political mobilisation that independently shape women's political participation. The pre-reform female candidacy rate offers a direct observable test: treated and control municipalities differ by 0.4 percentage points in 2004 and 0.3 in 2008, corresponding to standardised mean differences of -0.064 and -0.044 , both inside the conventional balance threshold even before matching. The gap also runs in the opposite direction to the estimated treatment effect, with treated municipalities starting slightly below controls; if favourable local norms drove both programme expansion and women's candidacy, the reverse would be expected.

Table 3: Treatment Assignment Regressed on Baseline Log GDP per Capita

<i>Log GDP per Capita (2007)</i>	-0.2514*** (0.0087)
<i>Constant</i>	1.0408*** (0.0197)
<i>Observations</i>	5,564
<i>Adjusted R²</i>	0.131
<i>RMSE</i>	0.466

Notes: OLS estimation on the cross-section of municipalities, restricted to one observation per municipality. Standard errors clustered at the municipality level in parentheses.

p < 0.10, **p < 0.05, *p < 0.01.*

To conclude, while the 2009 reform introduces exogenous variation in quotas, this does not fully neutralise the selection embedded in realised expansion, and pre-existing differences across municipalities therefore persist in the treatment and control groups.

4.3. Propensity Score-matching combined with DiD

As motivated by the diagnostic checks above, this study supplements the baseline DiD with propensity score matching (PSM) to improve pre-treatment comparability between the treatment and control groups. PSM matches municipalities on relevant pre-reform covariates, ensuring that the estimated effect of PBF is identified by comparing municipalities that are otherwise similar in their baseline characteristics.

This approach is informed by Zucco (2013), who analyses the electoral impact of PBF in Brazil through matching. Since his study examines a similar context, this study largely follows his variable selection strategy for the matching covariates. Specifically, GDP per capita, HDI, Gini coefficient, non-white population ratio, and total population are selected as they are the most likely confounders. GDP per capita, HDI, and Gini capture the economic development profile of each municipality; since PBF targets poor households, less developed municipalities mechanically expand more, while lower development is also associated with greater gender inequality (Schwindt-Bayer, 2010). The non-white population ratio is included because Afro-Brazilians constitute the majority of PBF recipients, so municipalities with larger non-white populations tend to expand more; at the same time, racial composition is a structural determinant of female political representation in Brazil (Janusz, 2022). Total population is included following Gerard, Naritomi and Silva (2025), who control for it in their DiD specification to account for pre-existing municipal differences; matching on it serves the same purpose here given data restrictions.

A logit model is estimated to compute the propensity score, i.e. the predicted probability of treatment assignment given the baseline covariates. Municipalities are then matched using Coarsened Exact Matching (CEM), introduced by Iacus, King, and Porro (2012), which coarsens the propensity score into bins and matches exactly within them. This produces superior covariate balance while retaining nearly the full sample.

This study then combines PSM with DiD in a two-step procedure. In the first step, propensity scores are estimated and municipalities are matched on pre-reform baseline characteristics, producing a sample of treated and control municipalities with comparable covariate distributions. In the second step, the baseline DiD specification outlined in Section 4.2 is estimated on the matched sample, using CEM weights to account for the relative importance of each matched municipality. This

restricts the comparison to municipalities with similar propensity scores, so the DiD estimate reflects the average treatment effect among comparable treated and control units rather than across the full sample. As Heckman, Ichimura and Todd (1997) argue, such a method that combines matching with DiD yields more credible estimates than either method alone, since matching addresses observable pre-treatment differences while DiD absorbs remaining time-invariant unobservables through fixed effects.

5. Results

5.1. Balance Assessment

Table 4 reports the SMDs (Standardised Mean Differences) for all matching covariates before and after matching.³ Prior to matching, the treatment and control groups differ substantially across most economic and demographic characteristics: the SMD is -0.786 for log GDP per capita, -0.870 for HDI, 0.418 for the Gini coefficient, 0.788 for the non-white population ratio, and -0.091 for log population — all well above the conventional threshold of 0.1 , except for log population. After matching, all SMDs fall below 0.03 : -0.028 for log GDP per capita, -0.002 for HDI, -0.025 for Gini, 0.014 for the non-white population ratio, and -0.006 for log population. These results confirm that PSM has successfully addressed the systematic compositional differences identified earlier, and that the matched sample achieves close comparability between treated and control municipalities across all baseline characteristics. The pre-reform female candidacy rate, reported as a diagnostic in Section 4.2.3, also remains balanced after matching, with standardised mean differences of -0.041 in 2004 and -0.066 in 2008.

Table 4: Standardised Mean Differences (SMD) Before and After Matching

Variable	Pre: Treated	Pre: Control	Pre: SMD	Post: Treated	Post: Control	Post: SMD
Log GDP per capita (2007)	1.885	2.410	-0.786	1.885	1.904	-0.028
HDI (2000)	0.666	0.733	-0.870	0.666	0.666	-0.002
Gini (2000)	0.572	0.548	0.418	0.572	0.574	-0.025
Share non-white (2000)	56.018	37.476	0.788	56.018	55.681	0.014
Log population (2006)	9.346	9.451	-0.091	9.346	9.354	-0.006

Notes: Standardised mean differences are computed across treatment and control groups for each baseline covariate. Conventional balance threshold is $|SMD| < 0.1$.

5.2. Pre-trend test on matched sample

As a further diagnostic, a pre-trend test is conducted on the matched sample using only the two pre-reform years (2004 and 2008). A pseudo-post indicator is constructed that equals one in 2008 and zero in 2004, and the DiD specification is re-estimated on this pre-reform subsample. If the parallel trends assumption holds, the trend from 2004 to 2008 should be the same for treated and control municipalities, since neither group has yet been exposed to the 2009 reform; the coefficient on the treatment-pseudo-post interaction should therefore be statistically indistinguishable from zero.

Table 5: Pre-trend Test on Matched Sample

	(1) Female Candidacy	(2) Female Success
$Treat_m \times PseudoPost_e$	-0.00174 (0.00215)	-0.000666 (0.00630)

³Balance is assessed on the cross-section of municipalities, with one observation per municipality. Covariates are measured at their pre-reform baseline years, as listed in parentheses next to each variable in Table 4.

Municipality FE	Yes	Yes
Year FE	Yes	Yes
CEM weights	Yes	Yes
Observations	10,986	10,900
Within R ²	1.74×10 ⁻⁴	2.97×10 ⁻⁶
Adjusted R ²	0.503	0.384

Notes: Sample restricted to the 2004 and 2008 election years on the matched panel. $PseudoPost_t$ equals one in 2008 and zero in 2004. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The estimates are reported in Table 5. The coefficient on the treatment-pseudo-post interaction is small and statistically insignificant for both the female candidacy rate ($\beta = -0.00174$, $p = 0.419$) and the electoral success rate ($\beta = -0.000666$, $p = 0.916$), providing some evidence that treated and control municipalities were on parallel pre-reform trajectories after restricting the sample to matched municipalities. It is worth noting, however, that as with the event study in Section 4.2.3, this test offers only limited support for the parallel trends assumption: only one pre-period is available, and the assumption itself is never directly testable.

5.3. PSM + DiD Results

Table 6: PSM + Difference-in-Differences Estimates

	(1) Female Candidacy	(2) Female Success
$Treat_m \times Post_t$	0.00494** (0.00199)	-0.000662 (0.00435)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
CEM weights	Yes	Yes
Observations	16,481	16,431
Within R ²	8.74×10 ⁻⁴	2.39×10 ⁻⁶
Adjusted R ²	0.609	0.377

Notes: Standard errors clustered at the municipality level in parentheses. The success-rate sample is smaller because the outcome is undefined for municipality-years with zero female candidates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The PSM+DiD estimates reinforce the baseline DiD findings. The matched estimate for the female candidacy rate is positive and significant at the 5% level ($\beta = 0.00494$, $p = 0.013$), indicating that, conditional on baseline observables, PBF expansion increased the share of female candidates in above-median expansion municipalities by 0.49 percentage points relative to comparable below-median municipalities. The estimate is roughly 17% larger in magnitude than the baseline ($\beta = 0.00422$), consistent with the view that the baseline was attenuated by the compositional differences between treatment and control groups documented above. Precision is somewhat lower than the baseline, which was significant at the 1% level, but the matched sample drops 165 observations, so this reflects CEM weights concentrating identification on a smaller effective sample of well-matched comparisons rather than mechanical trimming.

For the electoral success rate, the matched estimate remains negative and statistically insignificant ($\beta = -0.000662$, $p = 0.878$), again providing no evidence that programme expansion caused any change in female candidates' electoral success.

To conclude, the PSM+DiD specifications therefore support a positive effect of PBF expansion on female political participation at the candidacy margin, with no detectable effect on electoral outcomes. Matching strengthens the credibility of a causal interpretation by improving comparability between treated and control groups on observed pre-reform characteristics, but it does not guarantee causal identification: unobserved and time-varying confounders remain unaddressed. These remaining threats to identification are discussed in Section 6.2.

6. Discussion

6.1. Interpreting the Results on Participation and Success

The central finding is that PBF expansion produces a significant positive effect on female candidacy but no effect on electoral success. This gap reflects a mismatch between the scale at which PBF operates and the scale at which electoral outcomes are determined. The programme succeeds in increasing the supply of female candidates through targeted financial resources, but electoral success is shaped by structural factors such as electoral institutions, party systems, and voter preferences that a household level transfer cannot shift.

The coexistence of a positive candidacy effect and a null success effect is nonetheless informative. Duflo (2012) finds that women-targeted cash transfers, while they can have immediate consequences in women's favour, do not produce radical changes in women's status. The results of this study fit this pattern: PBF does not increase the number of women winning office, but it does shift participation at the entry margin, altering how women in poor households engage with the political process.

6.1.1. Increase in Candidacy

The candidacy rate sees an increase of 0.49 percentage points, which seems small in absolute terms. However, given the mean female candidacy rate is only 20.1%, this represents a 2.46% increase, which is nontrivial for a programme not designed to affect political outcomes. This increase is interpreted as an upward shift in women's political participation. PBF may have encouraged it through two channels: by relaxing the constraints that prevent women from participating, and by raising their willingness to do so.

On the first channel, the additional resources provided through PBF may have relaxed constraints that women face, including financial constraints and the lack of free time that Duflo (2012) identifies as major barriers alleviated through economic improvement. On the second, PBF may also have increased women's willingness to participate politically. Conover et al. (2020) argue that recurrent interactions between recipient women and programme administrators, generated by the women-targeted design of such transfers, increase their exposure to public institutions and raise their awareness of political participation. It is worth noting, however, that while Conover et al. study how such interactions affect voting turnout, the same mechanism plausibly extends to candidacy: women who become more politically engaged through programme participation are more likely to consider standing for office themselves.

6.1.2. No Effect on Success Rate

Candidacy is a decision that individual women can make. Electoral success, nonetheless, depends on the broader institutional and electoral environment. The null effect on the success rate fits this account: even as more women enter politics in response to PBF expansion, structural barriers limit their ability to translate candidacy into office. Specifically, Casas-Arce and Saiz (2015) argue that female political underrepresentation is largely attributable to the organisation of political markets shaped by agency problems and the structure of political parties, rather than the supply of qualified female candidates alone. Additionally, empirical evidence further shows that women leaders are evaluated more negatively than equivalent male leaders by voters (Beaman et al., 2009; Teele, Kalla and Rosenbluth, 2018). These structural barriers are not something a household level cash transfer can shift, and in the absence of affirmative action such as political quotas, additional financial support alone is unlikely to help women break into politics (Duflo, 2012).

The null result on the success rate is reinforced by diagnostic regressions reported in Appendix B on the absolute number of female winners and the female share of elected officials, both of which yield small and statistically insignificant coefficients. This matters because one might note that a positive candidacy effect combined with an unchanged success rate mechanically implies a modest absolute increase in women winning office, since a constant rate applied to a larger pool produces more winners. The success rate, nevertheless, is the most relevant measure here, since it is what proxies the electoral barrier this paper is concerned with by capturing the probability that an entering woman converts candidacy into office; the count level diagnostics simply confirm that the rate level null is not masking a meaningful shift in absolute terms.

6.2. Methodological Limitations

Some methodological limitations remain, notably omitted variable bias from time-varying unobservables and the simplicity of the functional form used to estimate the propensity score. However, diagnostic evidence suggests these are unlikely to substantively alter the main findings.

6.2.1. Time-Varying Unobservables

Despite matching substantially reducing bias, the PSM+DiD framework cannot fully eliminate omitted variable bias (Heckman, Ichimura and Todd, 1997). Matching addresses selection on observables, and DiD absorbs time-invariant unobservables, but neither can account for unobserved factors that vary differentially across municipalities over time. If such a confounder exists, it simultaneously threatens both the conditional independence assumption underlying PSM and the parallel trends assumption underlying DiD. One plausible example is local political culture. Shifts in informal institutional norms governing candidate selection (Gatto and Wylie, 2022) may evolve differentially over time across municipalities in ways that correlate with PBF expansion and independently affect female candidacy. This concern would be more decisively addressed with access to the confidential quota data used by Gerard, Naritomi and Silva (2025) to construct an instrumental variable, which would isolate exogenous variation in programme expansion and more credibly address selection on unobservables. In the absence of such data, the residual endogeneity concern cannot be fully resolved.

That said, the strong covariate balance and validated pre-trends reported previously provides evidence that the scope for remaining time-varying confounders to significantly bias the estimates is limited. A more direct test would be a triple-differences (DDD) specification using male candidates as an additional within-municipality counterfactual, which would absorb time-varying confounders that affect male and female outcomes equally. The present design does not pursue this, since the candidacy-rate DDD is mechanically invalid (female and male shares sum to one), and a credible success-rate DDD requires careful treatment of the parallel-trends-by-gender assumption that is beyond the scope of this paper. Future work could develop this design more rigorously.

6.2.2. Propensity Score Specification

Additionally, the effectiveness of matching depends on the functional form used to estimate the propensity score. This research's propensity score is estimated using a logit specification that enters all covariates linearly, assuming each contributes independently to the log odds of treatment. This rules out potentially relevant nonlinearities, such as interactions between poverty and racial composition that could jointly predict expansion propensity beyond their additive effects. The strong post-matching covariate balance suggests this specification is adequate for the present application, but a natural extension for future work would be to replace the PSM+DiD framework with the doubly robust DiD estimator proposed by Sant'Anna and Zhao (2020). This estimator combines outcome regression and propensity score weighting and remains consistent if either

component is correctly specified, providing insurance against the functional form assumptions on which the current approach relies.

6.3. Generalisability Beyond Brazil

The finding that additional women-targeted financial resources increase political participation through candidacy should, however, be generalised with caveats, as the mechanism through which it operates can be shaped by the electoral system. In Brazil's 2012 municipal election, 93.2% of candidates ran for city councillor and the remaining 6.8% for mayor or vice-mayor.⁴ City councillor seats are allocated under an open-list proportional system, while mayor and vice-mayor races operate by majority rule (TSE, 2022). The results of this study therefore overwhelmingly reflect candidacy behaviour under open-list rules, where voters cast a vote for an individual candidate and the candidates with the most personal votes within each party's list win that party's seats (Carey and Shugart, 1995).

Under this system, the decision to enter as a candidate is largely individual: additional financial resources can directly lower the personal cost of entry, consistent with the positive candidacy effect. Under alternative systems, the mechanism would operate differently. For example, in a closed-list system where party leaders determine the rank order of candidates, the binding constraint on women's candidacy shifts from individual resources to party nomination decisions (Carey and Shugart, 1995), and individual-targeted financial support may therefore matter less for entry.

Hence, the positive candidacy effect may not generalise to settings where institutional structures mediate differently between resources and political entry. The null effect on electoral success, by contrast, is likely more robust across settings. The structural barriers it reflects, including voter bias against female candidates, institutional gatekeeping, and the inability of household level transfers to shift electoral infrastructure, are near universal, particularly in developing economies.

7. Conclusion

This paper asks whether an exogenous increase in financial resources directed at women causally affects their political empowerment, using Brazil's Bolsa Família as the empirical setting and exploiting the 2009 reform as a source of plausibly exogenous variation in programme coverage. The answer is partial. PBF expansion is found to raise the female candidacy rate by 2.46% relative to the pre-reform mean while producing no detectable effect on the electoral success rate. The estimates come from a PSM+DiD design applied to over 5,000 Brazilian municipalities across the 2004, 2008, and 2012 municipal elections.

Such asymmetry refines rather than overturns the view that political representation is the dimension of women's empowerment most resistant to economic improvements. Household level resources shift women's decisions to enter politics but cannot move the structural determinants of who wins office. The two findings also differ in external validity. The candidacy result is shaped by Brazil's open list proportional system and may attenuate where parties rather than individuals mediate access to the ballot, while the null on electoral success is more portable across settings. The principal limitation of the design is the absence of confidential quota data, which precludes the instrumental variables strategy used by Gerard, Naritomi and Silva (2025) and leaves residual endogeneity unresolved.

For policymakers, the results offer two distinct messages. The candidacy effect strengthens the case for counting political spillovers in the broader returns to women-targeted cash transfers, particularly in settings whose electoral institutions resemble Brazil's. The null on electoral success, by contrast, generalises more readily and indicates that such transfers cannot substitute for direct interventions such as quotas where representation in office is the objective. Taken together, women-targeted cash transfers should not be regarded as a primary instrument of women's political empowerment, since empowerment is stronger when women hold office and make policy decisions.

Several extensions follow naturally. First, women's political empowerment is broader than candidacy and electoral success, and future work could refine the proxies through which it is measured. Second, comparable evidence from women-targeted cash transfer programmes operating under different electoral institutions, particularly closed list or majoritarian systems, would test directly whether the candidacy effect identified here travels beyond Brazil's open list setting. Third, sharper

⁴ Author's calculation from the TSE candidate-level records used in the main analysis.

identification, whether through access to the confidential quota data underlying the 2009 reform or through alternative natural experiments offering cleaner exogenous variation in women-targeted resources, would more decisively address the residual endogeneity that this design cannot fully resolve.

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Appendix

Appendix A. Threshold Robustness

Table 7 reports DiD estimates under four alternative definitions of treatment. Specifications 1–2 re-derive the binary treatment indicator at the 25th and 75th percentiles of the expansion distribution and re-run the full PSM+CEM matching and DiD pipeline from Section 5.3. Specification 3 restricts the sample to the top and bottom quartiles of expansion, dropping the middle 50%. Specification 4 replaces the binary indicator with the continuous expansion variable on the median-matched sample.

The candidacy coefficient is positive across all four specifications. At the 25th percentile threshold, the coefficient is small and insignificant ($\beta = 0.0022$, $p = 0.356$), reflecting the dilution of the treated group when three quarters of municipalities are classified as treated. At the 75th percentile, where only the top quarter is classified as treated, the coefficient doubles to 0.0058 ($p = 0.020$). The Q4-vs-Q1 extreme contrast, which drops the middle 50% of the distribution entirely, produces the largest binary estimate at 0.0079 ($p = 0.023$). The continuous treatment specification again confirms a positive, statistically significant effect ($\beta = 0.147$, $p = 0.005$). The electoral success coefficient is indistinguishable from zero in every specification, with p-values ranging from 0.91 to 0.98. Pre-trend tests are insignificant throughout.

Table 7: Effect of PBF expansion on female candidacy and electoral success rates across alternative treatment definitions

<i>Panel A: Female candidacy rate</i>				
	(1) 25th pctl	(2) 75th pctl	(3) Q4 vs Q1	(4) Continuous
Treat $\times$ post	0.0022 (0.0024)	0.0058** (0.0025)	0.0079** (0.0035)	0.147*** (0.0517)
Pre-trend coef	−0.0009	−0.0017	−0.0038	−0.0115
Pre-trend p-value	0.711	0.492	0.283	0.826
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
CEM weights	Yes	Yes	Yes	Yes
Observations	10,988	10,988	5,510	10,988
R ²	0.792	0.794	0.794	0.791
<i>Panel B: Female electoral success rate</i>				
	(1) 25th pctl	(2) 75th pctl	(3) Q4 vs Q1	(4) Continuous
Treat $\times$ post	0.0006 (0.0055)	−0.0004 (0.0061)	0.0003 (0.0082)	−0.0087 (0.126)
Pre-trend coef	−0.0064	0.0034	−0.0029	−0.056
Pre-trend p-value	0.295	0.632	0.762	0.709
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
CEM weights	Yes	Yes	Yes	Yes
Observations	10,932	10,932	5,480	10,932
R ²	0.692	0.699	0.687	0.698

Notes: Columns 1–3 re-derive the binary treatment indicator at the stated percentile of the municipal PBF coverage expansion distribution and re-estimate the PSM+CEM matching and two-way fixed effects DiD from Section 5.3. Column 3 restricts the sample to municipalities in the top and bottom quartiles of expansion. Column 4 replaces the binary indicator with the continuous expansion measure on the median-matched sample. The

median specification is reported in Table 6. Pre-trend coefficients are from a placebo DiD on the 2004–2008 pre-reform panel using the same matched sample. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B. Counts of Women Elected

Both columns estimate the PSM+DiD specification from Section 5.3, regressing the outcome on the $\text{Treat} \times \text{post}$ interaction with municipality and year fixed effects, weighted by the CEM matching weights and clustered at the municipality level. Column 1 takes the absolute number of women elected per municipality as the outcome; Column 2 takes the female share of elected officials, defined as female winners divided by total winners.

The $\text{Treat} \times \text{post}$ coefficient in Column 1 is -0.0168 with a standard error of 0.0429 ($p = 0.70$), indicating that PBF expansion did not produce a detectable change in the absolute number of women elected per municipality. The coefficient in Column 2 is 0.0010 with a standard error of 0.0037 ($p = 0.78$), indicating no detectable shift in the female share of elected officials. Both estimates are statistically and economically indistinguishable from zero.

Table 8: Effect of PBF expansion on counts of women elected

	(1) N female elected	(2) Female share of elected
$\text{Treat}_m \times \text{Post}_e$	-0.0168 (0.0429)	0.0010 (0.0037)
Municipality FE	Yes	Yes
Year FE	Yes	Yes
CEM weights	Yes	Yes
Observations	16,313	16,313
Municipalities	5,465	5,465
Adjusted R^2	0.385	0.363

Notes: Outcomes constructed from individual candidate TSE election data merged into the CEM matched sample. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.