Who Benefits from Benefits?
Municipal Consequences of Federal Cash Transfers

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Abstract

This paper estimates whether municipal incumbents extract electoral returns from implementing a federally designed cash transfer program. Brazil's *Bolsa Família* sets eligibility rules, benefit values, and beneficiary quotas at the federal level but delegates family registration to municipal welfare centers (CRAS). Exploiting a 2009 federal reform that exogenously reallocated authorized beneficiary quotas across municipalities, we instrument realized program expansion with the quota shock and estimate its effect on incumbent vote share in the 2012 municipal elections. A one-standard-deviation increase in instrumented beneficiary expansion raises the incumbent's vote share by 1.09 percentage points – equivalent to roughly one third of the typical inter-election decline in incumbent support, and large enough to determine the outcome in close races. Three complementary pieces of evidence identify the mechanism. The first-stage relationship operates almost entirely through municipalities with a CRAS already in place before the reform: federal authorization translates into realized coverage and electoral returns only where inherited state capacity can operationalize the expansion. Among CRAS municipalities, those whose welfare apparatus was already performing active outreach before the reform display a substantially stronger first stage, indicating that what mattered was not administrative presence but operational engagement with the community. The same outreach indicator provides suggestive evidence of inference dilution: municipalities with pre-reform active outreach earned a lower return per standard deviation of beneficiary expansion, consistent with voters expecting effective delivery from already-engaged administrations and updating less upon observing it. Together, the results show that even under highly centralized program architecture, residual administrative discretion over registration generates meaningful electoral returns for local incumbents – and that the size of those returns depends on whether local welfare infrastructure was already substantively engaged with the community it served.

Keywords: cash transfers; electoral returns; state capacity; *Bolsa Família*.
JEL Codes: D72, H53, H75, I38, O17.

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1 Introduction

Governments increasingly rely on cash transfer programs to alleviate poverty while limiting local political discretion. By standardizing eligibility rules, fixing benefit values by federal decree, and processing payments through centralized banking systems, these programs are designed to reduce the scope for patronage and discretionary allocation (Fiszbein and Schady, 2009). Yet access to them often depends on local bureaucracies controlled by elected officials. Can local politicians still extract electoral returns from programs designed precisely to prevent such political capture?

We study this question in the context of Brazil's *Bolsa Família*, one of the largest conditional cash transfer programs in the world.[1] Although eligibility rules, benefit levels, and payments are determined centrally by the federal government, municipalities remain responsible for identifying and registering eligible households through local welfare centers, the CRAS (*Centro de Referência de Assistência Social*). This institutional arrangement creates a narrow but important margin of local discretion: while mayors cannot alter transfer amounts, redirect funds, or withhold payments, they control the administrative pipeline through which households gain access to the program.

This margin matters because access to the program was not automatic. Municipal governments were responsible for locating potentially eligible families, conducting household interviews, collecting documentation, and transmitting registration data to the federal system.[2] Municipal authorities also controlled the local registration apparatus, which gave them influence over the pace, intensity, and reach of enrollment efforts.[3] In practice, two municipalities facing similar levels of underlying need could generate very different realized coverage depending on how actively the local administration pursued registration.

Our empirical strategy exploits a 2009 reform that revised the formula used to compute municipal Bolsa Família quotas. Throughout this period, each municipality faced

[1] By June 2006, Bolsa Família covered 11.1 million families – approximately 46 million people, or about 25% of the Brazilian population. By June 2015, the program supported 13.8 million families, with an average monthly benefit of R$167.15 and a budget equivalent to 0.5% of GDP.

[2] Hellmann (2015) describes enrollment as a municipal responsibility carried out in three stages: identification and localization of eligible families, household interviews, and digital entry of the information into the federal system. The same source reports that, as of September 2014, 77% of registrations were made without a home visit.

[3] Hellmann (2015) notes that municipalities were responsible for preparing the local structure for registration, training personnel, promoting awareness campaigns, and designing *active search* strategies to reach eligible families outside the existing registry. Lindert et al. (2007) report marked heterogeneity across municipalities in registration methods: a 2004 survey of 261 municipalities in the Northeast found that municipalities under Bolsa Família were 69% more likely to adopt geographic targeting mechanisms than under the pre-reform Bolsa Escola.

a federally determined quota – that is, a ceiling on the number of families that could be served under the program.[4] The 2009 reform updated the formula used to calculate those municipal ceilings using census and survey information, generating large and heterogeneous changes in the number of families each municipality was authorized to serve.[5] Crucially, the reform did not enroll a single household automatically. It expanded municipal authorization margins, but whether these additional slots became actual beneficiaries depended on local governments' decisions to identify, locate, and register eligible families.

We combine administrative data on quotas, realized beneficiary counts, and local social-assistance infrastructure with municipal electoral data from the subsequent election.[6] We use the federally induced quota shock as an instrument for realized beneficiary expansion and estimate its effect on incumbent electoral performance. The first stage is strong: municipalities receiving larger quota shocks expanded coverage substantially more than those receiving smaller shocks. This provides direct evidence that local governments actively shaped the conversion of federal authorization into realized program take-up.

Our main finding is that this discretionary expansion generated electoral returns. A one-standard-deviation increase in instrumented beneficiary growth raises the incumbent's vote share by 1.09 percentage points. The effect is robust to alternative definitions of the instrument, alternative outcome measures, and alternative sample restrictions.

We then examine the institutional channels through which local implementation translated into electoral gains. The first-stage relationship is sharply concentrated among municipalities that had a CRAS before the reform: where no welfare center existed, the quota shock generated neither realized coverage growth nor a detectable electoral return. Among municipalities with a CRAS, the relevant margin is not formal presence alone but operational engagement with the community. Municipalities whose welfare apparatus was already performing active outreach (*busca ativa*) before the reform display a substantially stronger first stage, indicating that operationally engaged welfare centers were better equipped to convert federal authorizations into realized

[4] Lindert et al. (2007) explains that Bolsa Família maintained municipal program quotas based on local poverty estimates. Original municipal allocations were constructed by combining the 2001 PNAD with the 2000 Census, generating an initial national target of 11.2 million families when the program was launched in 2003.

[5] Hellmann (2015) notes that Bolsa Família administered municipal quotas using estimates of poor families developed in partnership with IBGE and reports that, in 2009, the federal government refined the measurement of the target population by incorporating methodologies to capture income volatility among vulnerable households. These studies formed a major expansion of coverage, incorporating more than 1.6 million additional families.

[6] The analysis focuses on the 2008 and 2012 municipal elections and covers 2,192 municipalities.

enrollment. The evidence on the electoral return is more nuanced: municipalities with pre-reform active outreach earned a smaller return per standard deviation of beneficiary expansion than those without, consistent with voters expecting effective delivery from already-engaged administrations and updating less upon observing it. This differential is robust without electoral controls but does not survive the most demanding specification, leaving the evidence suggestive rather than conclusive. Taken together, these results show that federal expansion became electorally valuable where local welfare infrastructure existed and where that infrastructure was already substantively engaged with the community it served.

Our paper contributes to several literatures. First, we contribute to the political economy of cash transfer programs. A large body of work shows that incumbents benefit electorally from the expansion of social transfers (Manacorda et al., 2011; De La O, 2013). Much of this literature studies settings in which local discretion is deliberately minimized, relying on centrally determined thresholds, randomized assignment, or other exogenous variation to identify electoral effects. We complement this work by studying a setting in which the program architecture is centralized but the last mile of delivery is not. Our results show that even a narrow administrative margin of discretion can generate measurable political returns. More broadly, our findings suggest that centralized design alone is not sufficient to depoliticize redistribution: even when politicians cannot manipulate benefit rules or payment flows, they may still extract electoral gains by controlling access to the program.

Our paper is especially closely related to Frey (2019), who also studies exogenous variation in *Bolsa Família* coverage but exploits a channel that bypasses local politicians: federal health funding that expands hospital-based registration. He finds that when coverage increases through federal rather than local channels, incumbents receive fewer votes, and their successors are more qualified. Our findings are complementary. We study the opposite margin – one in which coverage expands because local politicians choose to activate the registration apparatus under their control – and find that voters reward precisely that discretionary effort.

Second, we contribute to the literature on political accountability under shared governance. A central question in this literature is whether voters can correctly attribute responsibility when multiple levels of government jointly produce policy outcomes (Manin et al., 1999; Besley, 2006). Ferraz and Finan (2008) show that voters punish corrupt mayors when randomly assigned audits reveal misuse of federal funds, especially where local media facilitate information transmission. Ferraz and Finan (2018) show that audits also reduce corruption through a legal disciplining channel. Our

setting differs because the relevant information concerns positive delivery rather than malfeasance. Voters observe the expansion of a valued program and update their beliefs about the incumbent's competence and effort. The mechanism we document is therefore closer to credit claiming than to sanctioning, but it operates through the same logic: voters use observable policy outcomes to infer politician type. In that sense, our results speak to a broader literature on attribution in multi-level settings by showing that voters can reward local incumbents even when the policy is federally funded and formally centralized, so long as local effort remains salient in implementation.

Third, we contribute to the literature on state capacity and decentralized service delivery. A growing body of work examines how local administrative infrastructure shapes the reach and effectiveness of redistributive programs. Alatas et al. (2012) show that the design of targeting mechanisms – whether community-based or formula-driven – has large consequences for who receives benefits, highlighting the role of local implementation choices even under centralized eligibility rules. Muralidharan et al. (2016) demonstrate that technology-driven reforms, such as biometric smartcards, can reduce leakage and improve delivery by limiting discretion at the last mile. Our findings point in a different direction: in a setting where technology had not yet displaced face-to-face bureaucratic contact, inherited local capacity was not merely a source of inefficiency or leakage but a necessary condition for program expansion to occur at all. The contrast suggests that the political consequences of decentralized delivery depend critically on the stage of administrative development. Where digital infrastructure can substitute for local bureaucracies, centralization may succeed in depoliticizing access; where it cannot, the local state remains the binding constraint – and the political returns to controlling it remain substantial.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting of *Bolsa Família*, the quota system, the 2009 reform, and the data sources. Section 3 presents the empirical strategy, defines the instrument, and discusses the identifying assumptions. Section 4 reports the main results and robustness checks, and develops a partial-equilibrium model that rationalizes the estimated parameters. Section 5 presents evidence on the institutional channels through which local implementation generates electoral returns, and extends the model to show that the net effect of inherited capacity on electoral returns is theoretically ambiguous. Section 6 concludes.

2 Background and Data

This section describes the *Bolsa Família* program, focusing on institutional aspects essential for understanding its local implementation dynamics and political implications.

2.1 Institutional Background of the *Bolsa Família* Program

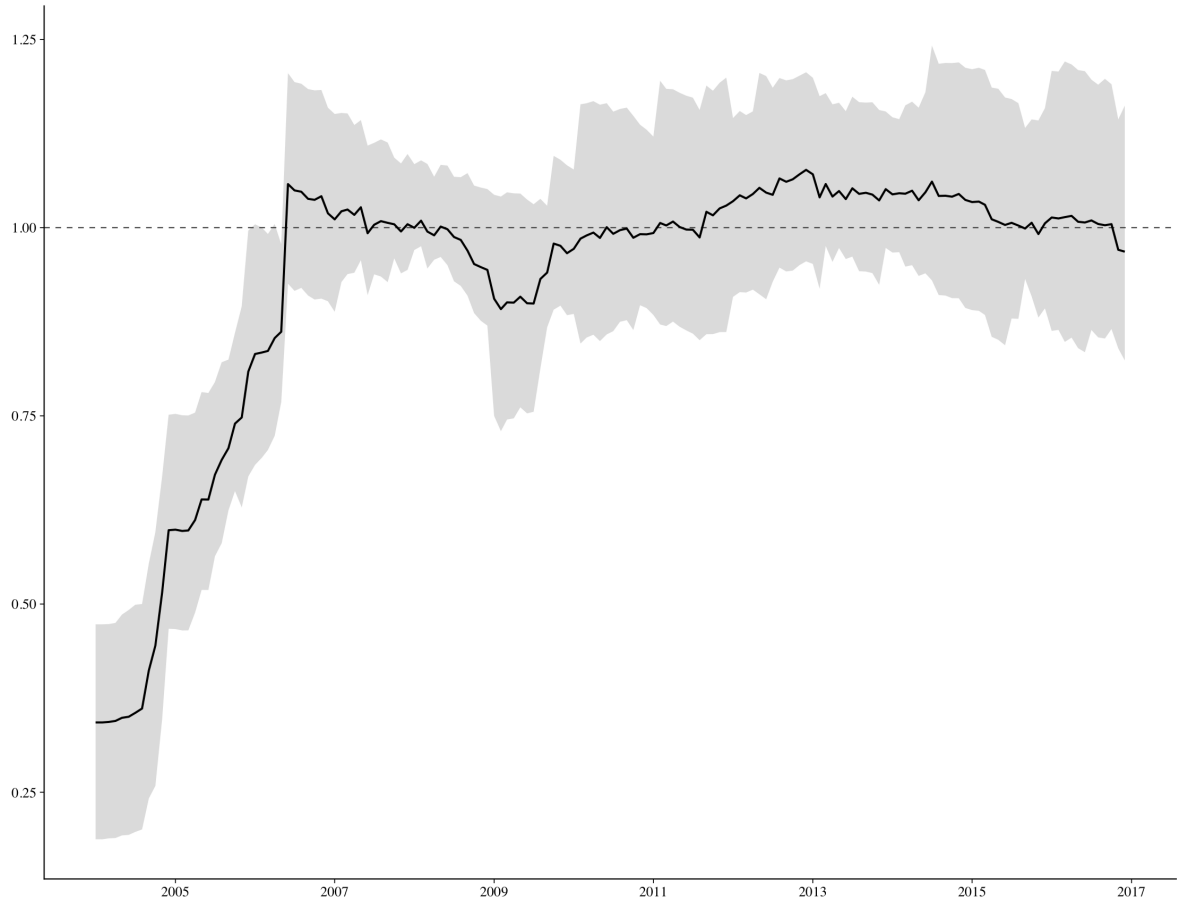
Launched in 2004, *Bolsa Família* consolidated several pre-existing social assistance programs into a single conditional cash transfer system, becoming Brazil's flagship poverty alleviation policy. The program provides monthly transfers to families below two income thresholds. Households with per capita income at or below R$70 per month were classified as extremely poor and receive an unconditional basic benefit of R$68, plus conditional variable payments of R$22 per child under 16 (up to three) and R$33 per adolescent aged 16–17 (up to two), conditional on school attendance and routine health checkups. Families with per capita income between R$70 and R$140 qualify only for the child- and adolescent-linked variable benefits. The maximum monthly transfer per family was R$200 and the average benefit stood at approximately R$95. By September 2009, the program reached approximately 12.4 million families, or roughly a quarter of the Brazilian population.

Central to the program's operations is the *Cadastro Único* (*CadÚnico*), a national registry of low-income households that serves as the gateway to *Bolsa Família* and dozens of other social policies. The registry targets families with per capita income below half the minimum wage (R$255 in 2010), a threshold substantially higher than the program's own eligibility lines, so that the pool of registered families considerably exceeds the set of actual beneficiaries.

Eligibility criteria are nationally uniform, benefit values are fixed by federal decree, and payments are processed and deposited directly into beneficiaries' accounts by the state-owned bank *Caixa Econômica Federal*. Mayors cannot alter transfer amounts, redirect funds, or withhold payments. What municipal governments control is the registration process itself.

Enrollment in the *Cadastro Único* requires a face-to-face interview with a social worker, who fills out a standardized questionnaire covering household composition, housing conditions, assets, employment, and income. In urban areas, families typically present themselves at a *Centro de Referência de Assistência Social* (CRAS) or, for specialized cases, a *Centro de Referência Especializado de Assistência Social* (CREAS) . In rural and remote areas, however, eligible families cannot realistically be expected to travel to

Figure 1: Realized Enrollment as a Share of Authorized Quota

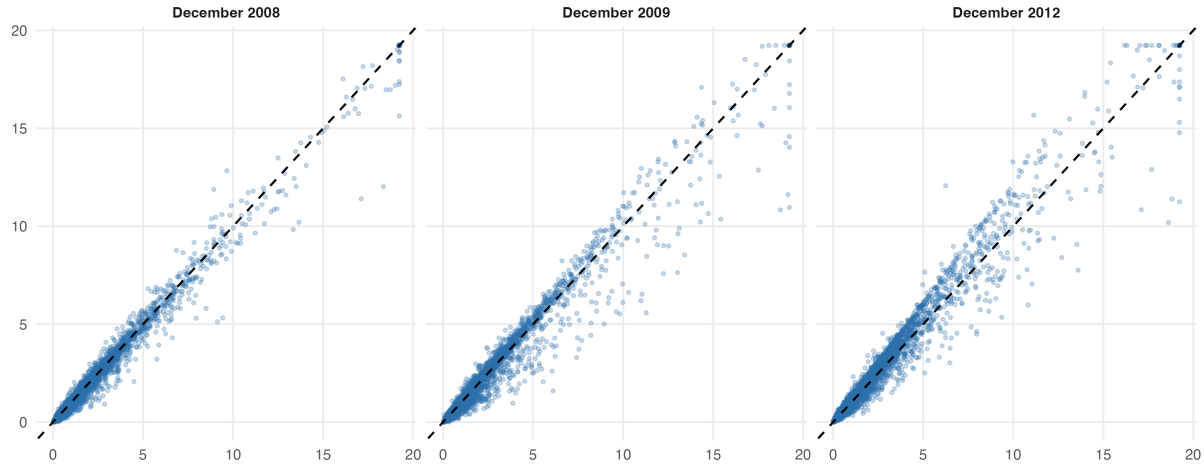


Note: Each point on the line is the cross-municipal mean of the ratio of realized *Bolsa Família* beneficiaries to the authorized quota in a given month. The shaded band spans the 25th to 75th percentile of the cross-municipal distribution. The dashed horizontal line marks full take-up (ratio = 1). Before 2007, municipalities operated well below their authorized ceilings; the sharp convergence toward 1 reflects the rapid administrative build-up of the program in its first years. After 2007, the ratio fluctuates around 1, indicating that authorization constraints became binding for a substantial share of municipalities — the institutional precondition for the 2009 reform to generate identifying variation.

a registration point. There, coverage depends on active outreach: municipal teams must physically locate potential beneficiaries, travel to communities with limited road access, no telephone coverage, and no internet, and conduct household interviews on site. Federal regulations encourage this practice by recommending that interviews take place at the family's home, both to facilitate verification and to reach populations that would otherwise remain invisible to the registry. In 2009, there were approximately 5,800 CRAS units distributed across roughly 4,000 of Brazil's 5,570 municipalities, and all data collection was done on paper.

Eligibility alone does not guarantee benefit receipt. Access to *Bolsa Família* depends on the availability of municipal quotas: caps on the number of beneficiaries per mu-

Figure 2: Authorized Quota vs. Realized Enrollment

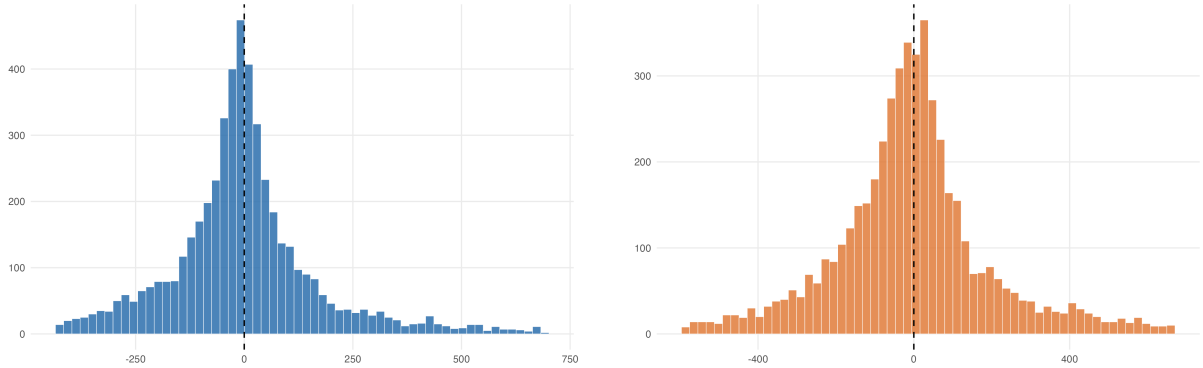


Note. Each dot is a municipality. The horizontal axis shows the federally authorized quota and the vertical axis shows the number of families actually enrolled in *Bolsa Família*, both in thousands. The dashed line marks the 45-degree locus of full take-up (realized = authorized). In December 2008, most municipalities lie close to or slightly below the 45-degree line, indicating that authorizations were approximately binding. In December 2009 – immediately following the federal quota reform – a substantial mass of municipalities shifts to the right of the 45-degree line, reflecting the expansion of authorized quotas that had not yet been translated into realized enrollment. The gap between the 45-degree line and the scatter in 2009 is the slack that our instrument exploits.

nicipality, defined nationally and revised approximately every three years based on poverty estimates derived from census and survey data. These quotas are intended to limit coverage per locality, but in practice, they operate as flexible benchmarks rather than rigid ceilings. Municipalities routinely register somewhat fewer or more families than their formal allocation without facing immediate sanctions. Vacant slots are reallocated through a federal priority index that considers each municipality's ratio of current beneficiaries to its quota and the number of eligible-but-unserved families. Effective coverage therefore depends not only on federal budget constraints and allocation formulas, but also on the administrative initiative of local governments, creating a discretionary margin where political will and organizational capacity determine how many eligible families actually receive benefits.

In 2009, the federal government revised the methodology used to compute these municipal quotas, producing a large and heterogeneous reallocation of authorized beneficiary slots across municipalities. The previous methodology, in place since 2006, distributed slots in proportion to each municipality's estimated number of poor families from the 2000 Census, adjusted for intercensal population growth. The 2009 revision used updated poverty estimates from the 2006 *Pesquisa Nacional por Amostra de Domicílios (PNAD)* household survey, applied new poverty thresholds corresponding to one quarter and one half of the minimum wage, and incorporated a scaling factor

Figure 3: Distribution of Slack Between Authorized Quota and Realized Enrollment



Note. Each panel shows the distribution of slack — defined as the authorized quota minus realized enrollment — across municipalities in December 2009, immediately following the federal reform. The left panel uses the official authorized quota; the right panel uses the counterfactual quota. Positive values indicate that a municipality received an authorization it had not yet translated into realized enrollment. The dashed vertical line marks zero slack. Both distributions are trimmed at the 5th and 95th percentiles to limit the influence of extreme observations. The mass of positive values in both panels confirms that the reform expanded quotas beyond contemporaneous enrollment levels for a substantial share of municipalities.

of 1.18 to account for income volatility among families near the poverty line (Gerard et al., 2025). The resulting reallocation increased the national cap by approximately 17%, adding nearly two million new authorized slots. This federal authorization did not automatically enroll anyone. It raised the ceiling, but whether and how quickly new families entered the program depended entirely on the municipal government's decision to identify, locate, and register them.

2.2 Data Sources

Our empirical analysis combines four municipal-level data sources: administrative records on *Bolsa Família*, electoral returns, census and household-survey data, and administrative records on local social-assistance infrastructure.

We begin with monthly administrative records from the *Ministério do Desenvolvimento Social* on *Bolsa Família* quotas, realized coverage, and total transfers from 2004 to 2016. These data allow us to measure both authorized and realized program expansion and to construct the treatment and instrument variables used in the empirical analysis.

We then use electoral records from the *Tribunal Superior Eleitoral* (*TSE*) covering municipal elections from 2000 to 2016. These data provide candidate-level vote totals, turnout, and detailed candidate characteristics, including education, prior occupation, and party affiliation.

To reconstruct counterfactual municipal quotas under the pre-reform allocation rule, we combine the 2000 Population Census from the *Instituto Brasileiro de Geografia e Estatística* (*IBGE*) with *Pesquisa Nacional por Amostra de Domicílios* (*PNAD*)-based estimates. These sources provide the inputs needed to approximate the quota formula that would have prevailed absent the reform.

Finally, we use administrative records from the *Censo SUAS* covering the universe of CRAS and *CREAS* units in 2008 and 2009. In addition to information on the identity and tenure of directors and staff, these data include measures of local infrastructure and service provision, such as whether units conduct active searches. We use these records to study the institutional mechanisms linking local administrative capacity and control over implementation to electoral outcomes.

Additional institutional details on the program's structure, payment mechanisms, registration process, fraud-prevention efforts, and the political incentives operating at each level of government are provided in Appendix A. Further details on the datasets, sample construction, and variable definitions are provided in Appendix B.

3 Empirical Strategy

Our empirical strategy exploits a 2009 federal reform to estimate the electoral impact of discretionary expansions in *Bolsa Família* coverage. The reform generated plausibly exogenous variation in municipalities' authorized quotas, but the translation of that authorization into actual beneficiary enrollment depended on local implementation decisions. We use this wedge – between what municipalities were allowed to do and what they chose to do – to identify the political returns to program expansion. The analysis implements a differences-in-differences design combined with two-stage least squares, using the federally granted quota expansion as an instrument for realized changes in beneficiary enrollment.

This section proceeds in three parts. First, we describe the research design: the estimating equations, the outcome variable, the estimation sample, and the specification choices. Second, we describe the construction of the instrument and present evidence that the 2009 quota reform provides a credible source of exogenous variation. Third, we assess the main threats to identification.

3.1 Research Design

The design compares changes in incumbent electoral performance between 2008 and 2012 across municipalities facing different intensities of authorized quota expansion. Under the identifying assumption that, absent the quota shock, municipalities with higher and lower authorized expansions would have followed parallel trends in incumbent vote shares, the strategy isolates the causal effect of discretionary program expansion on subsequent electoral outcomes.

The primary challenge in estimating the electoral impact of *Bolsa Família* expansion is the endogeneity of realized enrollment. Because mayors influence how many families are actually registered, coverage growth is endogenous to local political incentives: municipalities where incumbents face stronger electoral pressures may expand coverage more aggressively precisely because the expected political return is higher (Pereira et al., 2020). Estimating the effect of realized beneficiary growth on incumbent vote share directly would therefore conflate the electoral return to expansion with the political motivations that drive it.

To address this concern, we employ an instrumental variable approach, using the exogenous federal authorization as an instrument for realized beneficiary expansion. We estimate the following two-stage least squares specification:

$$\Delta\text{Beneficiaries}_m = \pi_0 + \pi_1 \Delta\text{Quota}_m + X_m'\pi_2 + \eta_m \tag{1}$$

$$\Delta\text{IncumbentVoteShare}_m = \beta_0 + \beta_1 \widehat{\Delta\text{Beneficiaries}}_m + X_m'\beta_2 + \varepsilon_m \tag{2}$$

where $\Delta\text{Beneficiaries}_m$ is the standardized change in the number of beneficiary families between January 2009 and September 2012 in municipality m; ΔQuota_m is the standardized exogenous component of the quota expansion; and $\Delta\text{IncumbentVoteShare}_m$ is the change in incumbent vote share between the 2008 and 2012 elections. Both the treatment and the endogenous regressor are standardized to have mean zero and unit standard deviation, so that first-stage coefficients are interpretable as standard-deviation-to-standard-deviation elasticities and second-stage coefficients as the percentage-point change in vote share per standard deviation of instrumented beneficiary expansion.

The coefficient β_1 captures the local average treatment effect: the electoral gain per standard deviation of beneficiary expansion among municipalities whose coverage was shifted by the federal quota reform. This is the paper's primary parameter of interest.

Outcome. The main outcome is the change in incumbent vote share between the 2008 and 2012 municipal elections:

$$\Delta \text{IncumbentVoteShare}_m = \frac{\text{IncumbentVotes}_m^{2012}}{\text{ValidVotes}_m^{2012}} - \frac{\text{IncumbentVotes}_m^{2008}}{\text{ValidVotes}_m^{2008}}$$

where $\text{ValidVotes}_{m,t}$ denotes the total number of valid votes cast in municipality m in election year t. We focus on vote share rather than a binary reelection indicator because the object of interest is whether discretionary expansion generated *additional* electoral support, not whether it was sufficient to win. Normalizing by valid votes rather than by the eligible electorate ensures that the measure reflects the incumbent's competitive position among voters who actually expressed a preference for a candidate, excluding blank and null ballots. In the Appendix D, we show that results are robust to alternative outcome definitions, including vote share among the eligible electorate, vote share among those who actually voted, absolute vote counts, and votes per 1000 people.

Sample. The outcome is defined only for incumbents who were elected in 2008 and sought re-election in 2012. Of Brazil's 5,568 municipalities, 2,396 satisfy this condition. We further exclude municipalities with missing information in the construction of the treatment variable or the outcome, as well as those that did not have contested elections in both periods. The final estimation sample retains 2,192 municipalities, roughly 39 percent of all Brazilian municipalities. If the selection criteria were correlated with the instrument, the estimation sample would be non-random, potentially biasing our estimates. In Section: Threats to Identification, we show that the instrument does not predict the incumbent's decision to run again, and that the estimation sample remains representative of the full population of Brazilian municipalities.

Specification. All specifications include municipality and year fixed effects. In some specifications, we additionally control for the number of candidates in the relevant election and include incumbent party fixed effects. Because municipality fixed effects are present throughout, time-invariant baseline characteristics are absorbed and do not enter separately. All regressions are weighted by the 2000 municipal population. Standard errors are clustered at the state level, following Abadie et al. (2023): because the quota allocation formula operates within states, the instrument is mechanically correlated within states, and clustering at the level of treatment assignment is the appropriate adjustment[7].

[7]With 27 states, cluster-robust inference may suffer from finite-sample bias. We verify that all main conclusions are robust to wild cluster bootstrap inference using the Webb (2022) six-point distribution.

3.2 The 2009 Quota Reform as Source of Identification

An important concern for any study linking program coverage to electoral outcomes is that observed variation in coverage may be driven by political manipulation. In the case of the 2009 expansion, this concern is mitigated by the institutional framework governing quota allocation. The methodology for computing municipal poverty indicators and distributing quotas was implemented by the *Ministério do Desenvolvimento Social (MDS)* , based on census and survey data, following a standardized algorithm with no discretion at either the local or federal executive level. Brollo et al. (2020) provide direct evidence supporting this claim: while they find evidence of manipulation in the enforcement of program conditionalities, they find no indication of political influence over the allocation of quotas themselves.

As described in Section: Background and Data, the 2009 reform revised the methodology used to compute municipal quotas, generating heterogeneous changes in authorized beneficiaries across municipalities. To isolate the variation attributable solely to the change in allocation rules, we construct counterfactual 2009 quotas by applying the pre-reform methodology to the same underlying data:

$$\text{CountQuota}_m^{2009} = \frac{\text{Poor}_m^{2000} \cdot n_m^{[2000,2006]}}{\sum_{k \in s} \text{Poor}_k^{2000} \cdot n_k^{[2000,2006]}} \cdot 1.18 \cdot \text{Poor}_s^{2006} \tag{3}$$

where Poor_m^{2000} denotes the number of poor families in municipality m of state s in the 2000 Census, and $n_m^{[2000,2006]}$ is an estimate of municipal population growth between 2000 and 2006. The preferred instrument is the difference between the actual 2009 quota and this counterfactual:

$$\Delta\text{Quota}_m^{2009} = \text{Quota}_m^{2009} - \text{CountQuota}_m^{2009} \tag{4}$$

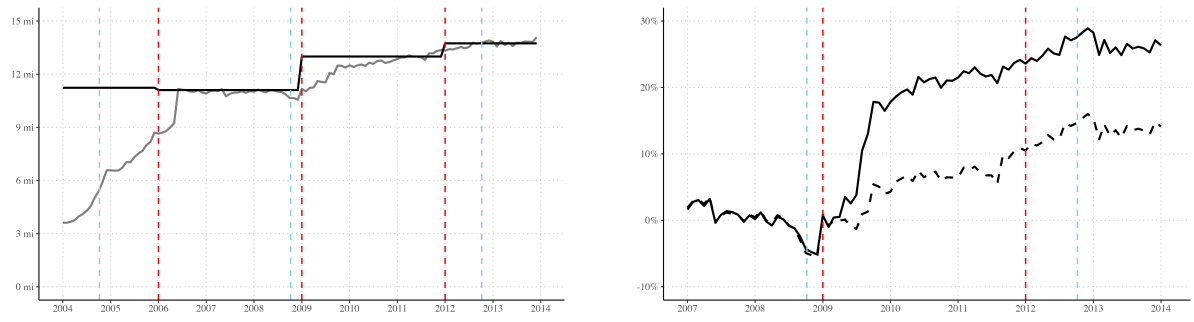
This measure captures the municipality-level variation in authorized expansion that is attributable to the change in federal allocation rules, rather than to pre-existing differences in local poverty or political conditions. We report results under two definitions of the quota shock. The preferred measure, reported in columns 1 and 2 of each table, uses the counterfactual construction described above. The alternative measure, reported in columns 3 and 4, is simply the difference between the 2009 and 2008 quotas. This captures the raw increase in authorization but does not purge variation arising from changes in underlying municipal characteristics between the two years. The

Bootstrap p-values are reported in Appendix D.

counterfactual-based measure is the sharper instrument because it holds the allocation methodology constant and varies only the rule; the 2009-versus-2008 measure serves as a robustness check. As we show in the identification exercises below, the two instruments differ in their vulnerability to pre-existing political conditions.

Figure 4 illustrates the first-stage variation. Panel (a) plots the national time series of authorized quotas and realized beneficiaries, showing the sharp increase in both following the 2009 reform. Panel (b) separates municipalities by their position in the distribution of $\Delta\text{Quota}_m^{2009}$: those above the state median experienced substantial increases in realized coverage, while those below the median saw little to no change, despite similar pre-trends. This pattern of parallel trajectories before 2009 followed by sharp divergence provides visual support for the identifying assumption. For further technical details on the construction of counterfactual quotas, see Gerard et al. (2025).[8]

Figure 4: Authorized Quotas, Realized Coverage, and the 2009 Expansion



(a) authorized and realized beneficiaries

(b) change in coverage by treatment intensity

Notes: Panel (a) displays the national time series of authorized *Bolsa Família* quotas (black line) and realized beneficiaries (gray line). Dashed vertical lines indicate the timing of the 2006, 2009, and 2012 quota revisions. Panel (b) plots the average change in realized beneficiaries separately for municipalities above (dashed) and below (solid) the state median of the instrument $\Delta\text{Quota}_m^{2009}$.

3.3 Threats to Identification

Our identification strategy relies on the assumption that federally induced quota expansions affect incumbent electoral outcomes only through realized changes in beneficiary enrollment. We assess the credibility of this assumption through some exercises. First, we compare baseline characteristics of the estimation sample to the full population of municipalities. Second, we test whether the instrument predicts the incumbent's decision to seek re-election. Third, we conduct placebo regressions using 2008 electoral

[8]The working paper version with full technical appendix is available at Gerard et al. (2025).

outcomes as dependent variables. Fourth, we examine whether pre-reform incumbent electoral strength predicts subsequent realized expansion.

3.3.1 Balance

Table 1 compares baseline characteristics of the 2,192 municipalities in the estimation sample with those of the full population of municipalities. The two groups are broadly similar across observable dimensions. None of the differences are economically meaningful, suggesting that municipalities in which incumbents chose to run for reelection are not systematically different from the population of Brazilian municipalities.[9]

Table 1: Baseline Characteristics: Estimation Sample vs. Population

	Sample	Population
Population	26,400.793	29,808.787
	(136,580.319)	(181,046.978)
Urban Population	21,171.826	24,445.520
	(138,032.465)	(177,105.334)
Human Development Index	0.522	0.523
	(0.105)	(0.104)
Gini Coefficient	0.548	0.547
	(0.069)	(0.069)
Poverty Rate	41.243	41.069
	(22.834)	(22.771)

Notes: This table reports means and standard deviations for municipal characteristics measured in 2000. The "Sample" column includes the 2,192 municipalities in the estimation sample; the "Population" column includes all municipalities with available baseline data.

3.3.2 Selection into Re-Running

Because the estimation sample is restricted to incumbents who sought re-election, a natural concern is that the instrument may affect the decision to run. If municipalities receiving larger quota shocks are systematically more or less likely to see their incumbents on the ballot, the estimation sample is non-random with respect to the instrument.

[9] Additional sample comparison tables are reported in Appendix C.

Table 2: Instrument and the Incumbent's Decision to Run Again in 2012

	$\text{Quota}_m^{2009} - \text{CountQuota}_m^{2009}$		$\text{Quota}_m^{2009} - \text{Quota}_m^{2008}$	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3
ΔQuota_m : $\hat{\gamma}_1$	−0.0051	−0.0043	−0.0111	−0.0113*
	(0.0044)	(0.0039)	(0.0055)	(0.0046)
Controls: N. of Candidates	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes
Observations	5,491	5,490	5,563	5,562

Notes: The dependent variable is an indicator equal to one if the 2008 incumbent appears on the ballot in the 2012 municipal election. The regression is estimated on the full universe of municipalities. Even-numbered columns include the number of candidates in the election and party fixed effects based on the 2008 winner. Robust standard errors clustered at the state level are reported in parentheses. All regressions are weighted by population in 2000. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

Table 2 examines whether the quota shocks predict the decision of the 2008 incumbent to run again in 2012. This is a first-order selection concern, since the main estimation sample is restricted to municipalities in which the incumbent re-enters the race. Under the preferred counterfactual-based 2009 instrument, the estimated coefficients are small in magnitude and statistically indistinguishable from zero in both specifications. A one-standard-deviation increase in the preferred shock is associated with only a 0.5 to 0.4 percentage point reduction in the probability that the incumbent runs again.

The alternative 2009-versus-2008 measure performs less well. Its estimated coefficients are roughly twice as large in absolute value, around 1.1 percentage points, and the specification with controls and party fixed effects reaches conventional levels of statistical significance. This pattern suggests that the simpler 2009-versus-2008 gap is more closely related to pre-existing political conditions affecting incumbent re-entry. By contrast, the counterfactual-based measure does not appear to shift the composition of the estimation sample through the re-running margin.

3.3.3 Pre-Reform Placebo

A more direct threat to the exclusion restriction would arise if the quota shock were correlated with political conditions that already favored incumbents before the reform. If so, the instrument would predict electoral outcomes even in 2008, before any federally induced expansion could have been translated into realized enrollment. To assess this

possibility, we estimate placebo regressions of pre-reform incumbent vote shares on the post-reform quota shocks:

$$\text{IncumbentVoteShare}_m^{2008} = \delta_0 + \delta_1 \Delta\text{Quota}_m + X_m' \delta_2 + \omega_m \tag{5}$$

Table 3 reports the results using the incumbent's 2008 vote share relative to valid votes as the dependent variable.

Table 3: Placebo: 2008 Incumbent Vote Share on Post-Reform Quota Shocks

	$\text{Quota}_m^{2009} - \text{CountQuota}_m^{2009}$		$\text{Quota}_m^{2009} - \text{Quota}_m^{2008}$	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3
ΔQuota_m : $\hat{\delta}_1$	0.1008^{**}	0.0840	0.1734^{*}	0.2081
	(0.0360)	(0.0719)	(0.0756)	(0.1050)
Controls: N. of Candidates	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes
Observations	5,490	5,490	5,562	5,562

Notes: The dependent variable is the incumbent's vote share in the 2008 election, defined as incumbent votes divided by valid votes. Even-numbered columns include the number of candidates in the election and party fixed effects based on the 2008 winner. A valid instrument should not predict pre-reform outcomes. Robust standard errors clustered at the state level are reported in parentheses. All regressions are weighted by population in 2000. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$.

Under the preferred counterfactual-based 2009 instrument, the placebo coefficient is positive and statistically significant in the raw specification (column 1), but falls by roughly 17% and loses statistical significance once the control vector and party fixed effects are included (column 2). Two observations discipline the interpretation of this pattern. First, the relevant comparison is between the placebo coefficient and the main effect under the same specification. The main second-stage estimate conditions on the same controls and party fixed effects that render the placebo statistically fragile. Under this common specification, the placebo point estimate of 0.084 represents less than 8% of the second-stage effect of 1.09 percentage points – a magnitude that is difficult to reconcile with a first-order violation of the exclusion restriction. Second, the loss of significance between columns 1 and 2 is not merely a matter of inflated standard errors absorbing a stable coefficient: the point estimate itself falls from 0.101 to 0.084, indicating that part of the unconditional association reflects cross-sectional differences in political structure rather than a direct link between the quota shock and pre-existing incumbent strength. The alternative 2009-versus-2008 measure performs less well: the placebo coefficient remains large and, under some definitions, retains marginal

significance even with controls, reinforcing the choice of the counterfactual-based shock as the preferred instrument.

3.3.4 Pre-Reform Electoral Strength and Subsequent Expansion

A final concern is that municipalities in which incumbents were electorally stronger before the reform may have subsequently generated larger realized expansions in program coverage. In that case, the second-stage estimates could partly capture selective implementation by politically stronger incumbents rather than the causal effect of the federally induced expansion itself. To assess this possibility, we regress realized beneficiary expansion in 2012 on the incumbent's vote share in 2008:

$$\Delta\text{Beneficiaries}_m^{2012} = \kappa_0 + \kappa_1 \,\text{IncumbentVoteShare}_m^{2008} + X_m'\kappa_2 + \xi_m \tag{6}$$

Table 4 reports the results using three alternative definitions of pre-reform incumbent vote share: votes divided by eligible voters, turnout, and valid votes. Across all specifications, the estimated coefficients are small and statistically indistinguishable from zero. Municipalities where incumbents were electorally stronger in 2008 did not subsequently experience systematically larger realized expansions in *Bolsa Família* coverage. This weakens the interpretation that the second-stage results are driven by selective take-up in politically stronger municipalities.

Table 4: Pre-Reform Incumbent Strength and Subsequent Realized Expansion

	Eligible Voters		Valid Votes		Valid Votes	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3	Alt. 4	Alt. 5
$\hat{\kappa}_1$: $\text{IncumbentVoteShare}_m^{2008}$	0.0149	−0.0011	0.1033	0.0783	0.0332	0.0227
	(0.0953)	(0.0817)	(0.0802)	(0.0572)	(0.0529)	(0.0429)
Controls: N. of Candidates	No	Yes	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes	No	Yes
Observations	5,562	5,562	5,562	5,562	5,562	5,562

Notes: The dependent variable is realized beneficiary expansion in 2012, standardized as in the main analysis. The regressor is the incumbent's vote share in the 2008 election, alternatively defined as incumbent votes divided by eligible voters, turnout, or valid votes. Even-numbered columns include the number of candidates in the 2008 election and party fixed effects based on the 2008 winner. Robust standard errors clustered at the state level are reported in parentheses. All regressions are weighted by population in 2000. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, . $p < 0.1$.

3.3.5 The 2012 Quota Revision

Our baseline treatment definitions focus on the 2009 quota revision. This is the first reform-induced change in municipal authorization between the 2008 and 2012 elections and, therefore, the clearest source of between-election variation in potential program expansion.

A related concern is that municipal authorization margins were revised again in 2012, shortly before the municipal election.[10] This later revision was considerably smaller than the 2009 expansion in municipal authorization, but it may still have affected electoral outcomes in 2012. For that reason, the 2012 election may reflect not only the implementation consequences of the 2009 reform but also the effects of a smaller update in federal quota targets carried out in the election year itself.

We therefore treat the 2009 revision as the main source of identifying variation and use 2012-based measures only as supplementary robustness checks. Throughout the Appendix, we report two additional specifications that mirror the main 2009 treatment definitions. The first replaces the baseline counterfactual-based 2009 shock with the difference between the 2012 estimated quota and the counterfactual quota. The second replaces the baseline 2009-versus-2008 measure with the simple difference between the 2012 and 2008 quotas. These alternative specifications are not our preferred treatment definitions, but they allow us to assess whether the main results are sensitive to the presence of the 2012 revision.

This choice is substantively appealing. The 2009 reform is the first federal change in municipal authorization after the 2008 election and thus has the full inter-election period to affect local registration efforts, realize program expansion, and voters' perceptions of incumbent performance. By contrast, the 2012 update is both smaller in magnitude and much closer to the election. Our baseline results therefore emphasize the 2009 authorization shock, while the Appendix C shows that the main conclusions are not driven by ignoring the later 2012 revision.

3.3.6 Summary

Taken together, the exercises above do not eliminate every possible identification concern, but they substantially narrow the set of plausible threats and establish a clear ranking among the candidate instruments. The preferred counterfactual-based 2009 shock does not predict the incumbent's decision to run again in 2012 (Table 2), and its

[10] Official *MDS* documents report that, following the release of the 2010 Census sample, the federal government updated municipal estimates used to guide program expansion in 2012.

association with pre-reform electoral outcomes becomes statistically fragile once the specification mirrors the main control set (Table 3). In addition, municipalities where incumbents were electorally stronger before the reform do not display systematically larger subsequent realized expansions in program coverage (Table 4).

The alternative 2009-versus-2008 measure performs less well across these validation exercises. It is more strongly associated with incumbent re-entry decisions and retains a more persistent correlation with pre-reform electoral outcomes, even after controls and party fixed effects are introduced. This consistent pattern suggests that the simpler quota-gap measure is more exposed to pre-existing municipal political conditions.

We therefore use the counterfactual-based 2009 shock as our preferred instrument in the main specifications and treat the 2009-versus-2008 measure primarily as a supplementary robustness check. Additional validation results, including alternative placebo outcomes, pre-reform placebo exercises for 2004, and supplementary specifications using 2012-based quota revisions, are reported in Appendix C.

A natural concern is that municipalities aligned with the federal governing party may have expanded the program more aggressively or received differential quota allocations. In Appendix D, we examine heterogeneity by federal-municipal partisan alignment and find no evidence that the electoral returns to program expansion differ between municipalities whose mayor was aligned with the president and those whose mayor was not, consistent with the quota allocation mechanism operating through a standardized formula rather than through partisan channels.

4 Main Results

We study whether discretionary local expansion of *Bolsa Família* generated electoral returns for incumbent mayors. Our empirical strategy exploits cross-municipality variation in the expansion of federally authorized program slots induced by the 2009 revision of the quota allocation methodology. We use this reform-induced variation in municipal authorization to instrument realized beneficiary expansion and estimate its effect on incumbent vote share.

The analysis focuses on three objects. First, we estimate the first stage relating the reform-induced quota shock to realized beneficiary growth, asking whether municipalities that were authorized to expand more in 2009 in fact expanded coverage more. Second, we estimate the reduced-form relationship between the quota shock and incumbent electoral performance. Third, we combine these two moments to recover

the effect of instrumented beneficiary expansion on incumbent vote share.

Throughout, both the treatment measure and the endogenous regressor are standardized to have mean zero and unit standard deviation. First-stage coefficients are therefore interpreted as standard-deviation changes in realized beneficiary expansion associated with a one-standard-deviation increase in the quota shock, while second-stage coefficients capture the percentage-point change in incumbent vote share associated with a one-standard-deviation increase in instrumented beneficiary expansion.

The outcome is the incumbent's vote share among valid voters. In our richer specifications, we include the number of candidates and incumbent-party fixed effects in addition to year and municipality fixed effects. All regressions are weighted by population in 2000, and standard errors are clustered at the state level.

The empirical design identifies whether federally induced expansion translated into electoral gains, but it does not by itself specify through which local margins this transmission occurs. To discipline that interpretation, we introduce a simple model of local implementation in which inherited administrative capacity and mayoral effort jointly determine whether authorized quota space becomes realized program expansion and, ultimately, incumbent votes.

4.1 A Model of Local Implementation

This subsection develops a partial-equilibrium model that organizes the empirical findings. The model is intentionally stylized: its purpose is not to support structural estimation, but to clarify how an exogenous federal quota shock translates into realized program expansion and electoral returns. A complete derivation with all proofs is provided in Appendix E.

4.1.1 Environment

Consider a municipality m with three types of agents. The **federal government** is not a strategic player: it sets the transfer value by decree and assigns a beneficiary quota $Q_m \geq 0$ through a standardized national formula based on census and survey data, with no input from municipal officials. The quota is exogenous from the perspective of local politics.

The **incumbent mayor** is the strategic agent on the supply side. He observes the quota Q_m, inherits a stock of local administrative capacity $\psi_m \equiv \psi(K_m^{2008})$ and chooses discretionary implementation effort $a_m \geq 0$. This effort captures active search

for eligible families, mobilization of social assistance staff, organization of outreach campaigns, and resolution of administrative bottlenecks. The key distinction is that ψ_m is *predetermined* whereas a_m is *chosen* by the current incumbent.

The mayor has an unobserved type $q_m \in \{0, 1\}$, drawn by nature with prior $p_0 = \Pr(q_m = 1)$, where $q_m = 1$ denotes a competent mayor. Competence lowers the cost of effort: the type-dependent cost parameter is $c(q_m) = c_0 - c_1 q_m$ with $c_0 > c_1 > 0$, so that competent mayors face strictly lower marginal costs of discretionary implementation.

The **voters** value the transfer if they receive it, care about whether the incumbent is competent, and use observable implementation outcomes to update beliefs about the mayor's type. Voter i's latent utility difference from supporting the incumbent is

$$\Delta U_{im} = \alpha\, d_{im} + \beta\, \Pr(q_m = 1 \mid s_{im}) + \eta_{im} \tag{7}$$

where $d_{im} \in \{0, 1\}$ indicates beneficiary status, $\alpha > 0$ is the material value of the transfer, $\beta > 0$ captures the value of competence, s_{im} is a noisy signal of implementation quality, and η_{im} is idiosyncratic ideology. The signal $s_{im} = \mu_0 + \mu_1 d_{im} + \mu_2 e_m + \varepsilon_{im}$ combines direct program experience and aggregate implementation visibility.

4.1.2 Implementation and the Mayor's Problem

The municipality's quota utilization rate is $e_m = \psi_m + a_m$, and realized beneficiaries are $B_m = e_m\, Q_m$. Utilization is not bounded above by one: as documented in Section Background and Data, many municipalities operate above their authorized quota.

Aggregating voter behavior and linearizing around an interior support level yields a municipal vote share of the form

$$V_m = V_0 + \lambda_1 B_m + \lambda_2 e_m \tag{8}$$

where $\lambda_1 > 0$ is the electoral return to expanding realized beneficiaries (*material channel*) and $\lambda_2 \geq 0$ is the additional return to implementation visibility (*inference channel*).

The mayor chooses effort to maximize electoral support net of two costs: a type-dependent quadratic effort cost $\frac{1}{2} c(q_m) a_m^2$, reflecting increasing marginal difficulty of reaching additional families; and an enforcement cost $\frac{1}{2}\kappa(e_m - 1)^2$ with $\kappa > 0$, reflecting institutional pressure to remain close to full quota utilization. The objective is

$$\max_{a_m \geq 0}\; V_0 + (\psi_m + a_m)(\lambda_1 Q_m + \lambda_2) - \tfrac{1}{2} c(q_m) a_m^2 - \tfrac{1}{2}\kappa(\psi_m + a_m - 1)^2 \tag{9}$$

4.1.3 Equilibrium Effort

The first-order condition balances the marginal electoral benefit of effort against the marginal cost of effort and the marginal enforcement cost. Setting the derivative equal to zero and solving yields:

Proposition 1 (Optimal Discretionary Effort)**.** *The unique interior optimum is*

$$a_m^*(q_m) = \frac{(\lambda_1 Q_m + \lambda_2) + \kappa(1 - \psi_m)}{c(q_m) + \kappa} \tag{10}$$

with equilibrium utilization $e_m^* = \psi_m + a_m^*$ and realized beneficiaries $B_m^* = e_m^* Q_m$. Competent mayors exert strictly more effort, achieve higher utilization, and produce more beneficiaries.

The expression has a transparent interpretation. The numerator contains two forces pulling the mayor toward higher effort: the marginal electoral benefit of implementation, $\lambda_1 Q_m + \lambda_2$, and the enforcement pull toward full utilization, $\kappa(1 - \psi_m)$, which is stronger when the municipality starts further below $e_m = 1$. The denominator reflects the combined resistance from type-dependent effort costs and enforcement pressure.

Because competent mayors choose higher effort in equilibrium, higher signals are rationally more indicative of competence – the monotone likelihood ratio property holds as an equilibrium consequence, not as an assumption.[11]

4.2 First Stage: Authorization and Coverage

We begin by establishing that the 2009 quota reform generated substantial variation in realized program expansion. Table 5 reports the first-stage relationship between the authorized quota shock and the realized increase in *Bolsa Família* coverage.

[11] See Appendix D for details.

Table 5: First-stage Estimates

	$\text{Quota}^m_{2009} - \text{CountQuota}^m_{2009}$		$\text{Quota}^m_{2009} - \text{Quota}^m_{2008}$	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3
ΔQuota_m : π_1	1.2436^{***}	1.1220^{***}	1.5769^{***}	1.1678^{***}
	(0.0391)	(0.0495)	(0.3604)	(0.1896)
Controls: N. of Candidates	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes
Observations	4,384	4,384	4,384	4,384

Notes: Each coefficient reports the first-stage relationship between the standardized quota shock and standardized realized beneficiary expansion. Robust standard errors clustered at the state level are reported in parentheses. All regressions include year and municipality fixed effects and are weighted by population in 2000. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The first-stage estimates show that the 2009 authorization shock translated into substantial realized expansion in *Bolsa Família* coverage. In the preferred specification (column 2), a one-standard-deviation increase in the counterfactual-based 2009 quota shock raises standardized beneficiary expansion by 1.12 standard deviations. The magnitude is economically large and precisely estimated, indicating that municipalities receiving larger federally authorized expansions did, in fact, expand effective program coverage more strongly in the period leading to the 2012 election.

The first stage is also robust to alternative ways of measuring the authorization shock. Under the preferred counterfactual-based definition, the coefficient is 1.24 without additional electoral controls and 1.12 once controls and party fixed effects are included. Using instead the simpler 2009-versus-2008 quota gap, the corresponding estimates are 1.58 and 1.17. Although the raw 2009-versus-2008 measure yields somewhat larger coefficients in the more parsimonious specification, both treatment definitions imply a strong and precisely estimated relationship between authorized quota expansion and realized program take-up.

This pattern matters for the interpretation of the IV design. The federal revision in municipal quotas was not merely nominal: it altered the effective scope for local implementation in ways that generated substantial differences in realized coverage across municipalities. At the same time, the preferred counterfactual-based measure delivers a first stage that remains large, stable, and tightly estimated once the specification mirrors the baseline controls. Combined with the validation exercises reported above, this is why we treat the counterfactual-based 2009 shock as the preferred source

of identifying variation in the main analysis.

4.3 Reduced Form: Authorization and Electoral Returns

As a direct check on the main result, we report the reduced-form relationship between the quota shock and incumbent vote share, bypassing the second-stage instrumentation entirely. Under exact identification, the reduced-form coefficient equals the product of the first-stage and second-stage coefficients ($\hat{\rho}_1 = \hat{\pi}_1 \times \hat{\beta}_1$). Examining it directly therefore provides a transparent test of whether the exogenous component of quota variation is itself associated with electoral gains, without relying on the second-stage scaling from realized expansion to votes.

$$\Delta\text{IncumbentVoteShare}_m = \rho_0 + \rho_1 \Delta\text{Quota}_m + X_m'\rho_2 + u_m \tag{11}$$

Table 6 reports these estimates. The reduced-form estimates confirm that the exogenous component of quota variation is directly associated with better electoral performance for incumbents. In the preferred specification, a one-standard-deviation increase in the counterfactual-based 2009 shock raises incumbent vote share by 1.22 percentage points.

Table 6: Reduced-form Estimates

	$\text{Quota}^m_{2009} - \text{CountQuota}^m_{2009}$		$\text{Quota}^m_{2009} - \text{Quota}^m_{2008}$	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3
$\Delta\text{Quota}_m : \rho_1$	0.6015^{***}	1.2182^{**}	0.7286^{**}	0.8696
	(0.0678)	(0.3294)	(0.2229)	(0.5205)
Controls: N. of Candidates	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes
Observations	4,384	4,384	4,384	4,384

Notes: Each coefficient reports the reduced-form effect of a one-standard-deviation increase in the quota shock on incumbent vote share, measured in percentage points of valid votes. Robust standard errors clustered at the state level are reported in parentheses. All regressions include year and municipality fixed effects and are weighted by population in 2000. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The reduced form is positive in all four specifications. Under the preferred measure, the estimates are economically meaningful and statistically precise. The coefficient is 0.60 percentage points without additional electoral controls and 1.22 percentage points in the main specification. Under the simpler 2009-versus-2008 measure, the

point estimates are of similar order of magnitude, but the specification with controls is substantially less precise and is no longer statistically distinguishable from zero at conventional levels.

These estimates serve two purposes. First, they show that the main finding is not an artifact of second-stage scaling: the quota shock itself is positively associated with incumbent electoral gains. Second, they provide a transparent benchmark for the IV estimates that follow. The preferred counterfactual-based design yields a reduced form that is positive, economically relevant, and internally consistent with both the first stage and the second stage, reinforcing the interpretation that federally induced quota expansion translated into electoral returns for local incumbents.

4.4 Second Stage: Coverage and Electoral Returns

Having established that the quota shock generated strong and robust variation in realized coverage, we next ask whether this induced expansion translated into electoral gains for incumbent mayors. Table 7 reports the two-stage least squares estimates, where standardized realized beneficiary expansion is instrumented by the standardized quota shock.

Table 7: Second-stage Estimates

	$\text{Quota}^{m}_{2009} - \text{CountQuota}^{m}_{2009}$		$\text{Quota}^{m}_{2009} - \text{Quota}^{m}_{2008}$	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3
$\Delta\text{Beneficiaries}_m$: β_1	0.4837^{***}	1.0858^{***}	0.4620^{***}	0.7447^{*}
	(0.0491)	(0.2354)	(0.0878)	(0.3194)
Controls: N. of Candidates	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes
Observations	4,384	4,384	4,384	4,384

Notes: Each coefficient reports the 2SLS estimate of the effect of a one-standard-deviation increase in instrumented beneficiary expansion on incumbent vote share, measured in percentage points of valid votes. All regressions include year and municipality fixed effects and are weighted by population in 2000. Robust standard errors clustered at the state level are reported in parentheses. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

The preferred estimate, reported in column 2, implies that a one-standard-deviation increase in instrumented beneficiary expansion raises the incumbent's vote share by 1.09 percentage points. This is the paper's main result. In municipalities with relatively close races, an effect of this size can meaningfully shift the incumbent's electoral position.

The estimated electoral return is positive in all four specifications. Under the preferred counterfactual-based instrument, the coefficient is 0.48 without additional electoral controls and 1.09 in the main specification. Under the simpler 2009-versus-2008 instrument, the corresponding estimates are 0.46 and 0.74. The sign is stable throughout, but the counterfactual-based design yields the largest and most precisely estimated effect once the specification mirrors the baseline controls.

Taken together, the second-stage estimates indicate that the political return to *Bolsa Família* operated through realized local expansion rather than through federal authorization alone. The federal government created scope for expansion by revising municipal quotas, but municipalities differed in how much of that new margin they translated into actual coverage. The LATE estimates imply that where this induced expansion was larger, incumbents performed better at the polls. This pattern is consistent with the interpretation that voters responded to the local implementation of program growth, not merely to the existence of the federal reform in the background.

To assess the economic relevance of this estimate, consider the distribution of vote-share changes in the estimation sample. Between the 2008 and 2012 elections, the median incumbent lost approximately 3 percentage points of vote share, and 60 percent of incumbents in the sample experienced some decline. Against this backdrop, a gain of 1.09 percentage points represents roughly one third of the typical inter-election decline – a meaningful attenuation of the secular erosion most incumbents face. For the 55 incumbents who won re-election in 2012 by a margin of less than 1.09 percentage points, an effect of this magnitude would have been sufficient to determine the outcome. The estimated return to program expansion is therefore not merely statistically significant: it is large enough to matter for re-election prospects in a non-trivial share of competitive races.

4.5 Model and the Main Results

The empirical results are consistent with the framework introduced at the beginning of this section. In the model, a positive quota shock ($\Delta Q_m > 0$) expands the scope for local implementation, raises equilibrium effort, and increases realized beneficiary growth whenever municipalities operate away from the full-compliance benchmark. This is exactly the first-stage pattern we document in the data: municipalities that received larger federally authorized quota increases subsequently expanded effective coverage more strongly.

The model also provides a natural interpretation for the second stage. Additional

beneficiaries raise incumbent electoral performance through the combined material and inference channels summarized in the voter payoff function. Empirically, the IV estimates indicate that larger realized expansions translated into higher incumbent vote shares, which is the main prediction of the model on the electoral margin. In that sense, the data support the central mechanism emphasized by the framework: federal authorization affects electoral outcomes only insofar as it is converted into visible local delivery.

The framework is also useful for interpreting why the first stage is not uniform across municipalities. In the model, the quota shock generates no discretionary margin when implementation is already mechanically pinned down, and it may generate little or no realized expansion when inherited capacity is too weak for local effort to translate quota space into effective enrollment. These limiting cases map naturally into the heterogeneity patterns studied below, where the strength of the first stage depends sharply on pre-existing administrative capacity.

It is worth noting what patterns in the data would be difficult to reconcile with the framework. First, if the quota shock generated electoral returns without a corresponding first-stage effect on realized coverage, the model's central mechanism – that federal authorization matters only insofar as it is converted into local delivery – would be rejected. The data are consistent with this prediction: the reduced-form effect operates entirely through municipalities where the first stage is strong. Second, a negative first-stage coefficient would imply that municipalities responded to larger authorization by reducing enrollment, which the model rules out under standard parameter restrictions. No specification produces such a pattern. Third, if municipalities without any welfare infrastructure displayed a stronger first stage than those with a CRAS, the role of inherited capacity ψ_m as a facilitator of implementation would be contradicted. As shown in Section Possible Mechanisms, the opposite is true: the first stage is concentrated entirely in CRAS municipalities. The model is therefore not merely consistent with the data – it survives the natural tests that would reject it.

Finally, the model clarifies the interpretation of the IV coefficient. Since vote share can be written as $V_m = V_0 + [\lambda_1 + \lambda_2/Q_m]B_m$, the estimated IV effect should be read as the total electoral return to an additional beneficiary among municipalities whose realized coverage was shifted by the federal quota reform. The empirical design does not separately identify the material (λ_1) and the inference (λ_2/Q_m) components of this return. What it identifies is their combined effect on incumbent electoral performance.

5 Possible Mechanisms

We present complementary evidence on the mechanisms through which local administrative capacity mediates the electoral returns to federal program expansion. The main result establishes that a one-standard-deviation increase in instrumented beneficiary expansion raises the incumbent's vote share by 1.09 percentage points. This section asks *through which administrative channels* that return materializes, and whether it is concentrated among municipalities with particular institutional characteristics.

Two questions are conceptually distinct. The first is whether capacity shapes the *quantity* of expansion a municipality achieves given a common quota shock – the implementation margin. The second is whether capacity shapes the *electoral return per standard deviation of beneficiary expansion* conditional on expansion having occurred – the inference margin. The framework introduced in Section Main Results implies that these two margins need not move together, and the empirical evidence confirms this distinction. We begin by examining whether baseline CRAS presence constitutes a necessary condition for the electoral return to materialize, then move to more granular operational measures of frontline activity.

5.1 State Capacity and the Margin of Electoral Returns

The mechanism results can be organized through a simple extension of the framework introduced in Section Main Results. Realized beneficiaries are given by

$$B_m^* = (\psi_m + a_m^*)Q_m, \tag{12}$$

where ψ_m is inherited administrative capacity and a_m^* is equilibrium discretionary effort. Both objects enter the utilization rate $e_m^* = \psi_m + a_m^*$, so the model does not impose a priori whether responsiveness to the quota shock is driven primarily by inherited infrastructure or by effort. Differentiating with respect to Q_m yields

$$\frac{\partial B_m^*}{\partial Q_m} = \psi_m + a_m^* + Q_m \frac{\partial a_m^*}{\partial Q_m}. \tag{13}$$

This expression highlights three determinants of first-stage responsiveness: baseline inherited capacity, equilibrium effort, and the endogenous response of effort to the quota margin. Municipalities with higher ψ_m or higher a_m^* will tend to display a stronger first stage, all else equal, and this effect is amplified when effort itself rises with the quota shock.

The same framework also clarifies how inherited capacity shapes the electoral interpretation of realized expansion. In the extended model, the electoral return to an additional beneficiary can be written as

$$\hat{\beta} = \lambda_1 + \frac{\lambda_2(\psi_m)}{Q_m}, \tag{14}$$

where λ_1 captures the material return and $\lambda_2(\psi_m)/Q_m$ captures the inference component, which may itself depend on inherited capacity. The model therefore implies that state capacity can strengthen the first stage while having an ambiguous net effect on the total reduced-form electoral return.

Proposition 2 (State Capacity, Implementation, and Electoral Returns)**.** *Under the extended model with $\lambda_2'(\psi_m) < 0$, and maintaining the regularity condition $|\lambda_2'(\psi_m)| < c(q_m)$:*

(i) ***First stage.*** *The responsiveness of realized beneficiaries to the quota shock is increasing in inherited capacity: higher ψ_m raises $\partial B_m^*/\partial Q_m$.*

(ii) ***Second stage.*** *The electoral return per additional beneficiary is decreasing in inherited capacity: higher ψ_m lowers $\lambda_1 + \lambda_2(\psi_m)/Q_m$.*

(iii) ***Reduced form.*** *The net effect of inherited capacity on the total electoral return to a quota shock is theoretically ambiguous.*

5.2 State Capacity as a Necessary Condition for Coverage

We investigate whether the first-stage and electoral-return relationships differ by pre-existing local capacity by estimating

$$\begin{aligned}\Delta\text{Beneficiaries}_m = {} & \rho_0 + \rho_1 \Delta\text{Quota}_m \\ & + \rho_2 \left(\Delta\text{Quota}_m \cdot \mathbb{1}(\text{CRAS}_m^{2008})\right) + X_m'\rho_3 + \eta_m \end{aligned} \tag{15}$$

$$\begin{aligned}\Delta\text{IncumbentVoteShare}_m = {} & \delta_0 + \delta_1 \Delta\widehat{\text{Beneficiaries}}_m \\ & + \delta_2 \left(\Delta\widehat{\text{Beneficiaries}}_m \cdot \mathbb{1}(\text{CRAS}_m^{2008})\right) + X_m'\delta_3 + \varepsilon_m \end{aligned} \tag{16}$$

where $\mathbb{1}(\text{CRAS}_m^{2008})$ indicates whether municipality m had at least one CRAS in 2008. The omitted group consists of municipalities without a CRAS, so ρ_1 and δ_1 capture effects in the no-CRAS subgroup, while ρ_2 and δ_2 capture the differential for municipalities with pre-existing welfare infrastructure.

Table 8 reports the estimates. The first stage is stark. Under the preferred treatment, the quota shock has no detectable effect on realized coverage in municipalities without a CRAS in 2008: the coefficient ρ_1 is small and statistically indistinguishable from zero across both specifications. The interaction ρ_2 is positive, large, and precisely estimated, so that the implied total effect for municipalities with a CRAS – $\rho_1 + \rho_2 = 0.375$ in the main specification. The same pattern holds under the alternative 2009-versus-2008 treatment. The quota reform generated realized coverage growth only where local welfare infrastructure was already in place to act on the federal authorization.

The reduced-form estimates are consistent with this reading. The baseline coefficient ϕ_1 is large and negative but imprecisely estimated, reflecting the absence of a first-stage relationship in the no-CRAS subgroup. The interaction ϕ_2 offsets this, and the implied total effect for municipalities with a CRAS – $\phi_1 + \phi_2 = 1.215$ percentage points in the main specification – closely mirrors the aggregate reduced form reported in Table 6. Municipalities without a CRAS generated no electoral return to the quota shock; municipalities with a CRAS generated a return indistinguishable from the full-sample estimate.

Panel C reports the IV decomposition. Under the preferred treatment, δ_1 explodes to implausible magnitudes with enormous standard errors, and δ_2 offsets it almost exactly, leaving the joint effect $\delta_1 + \delta_2$ imprecisely estimated. This pattern is a mechanical consequence of the near-zero first stage in the no-CRAS subgroup: dividing the reduced form by a first stage close to zero produces an unstable ratio, not evidence of a large negative effect. The explosion of subgroup IV is therefore not a problem to be explained away – it is the IV representation of the same fact already established in Panel A. Where the instrument does not move the treatment, it cannot identify the return, and the IV ratio is undefined. The instability of δ_1 confirms that the identifying variation in the main results is concentrated among municipalities with pre-existing CRAS infrastructure, and that the 1.09 percentage-point estimate from Table 7 should be read as applying to that margin.

5.3 Substantive Engagement and the Margin of Electoral Returns

The results on operational engagement refine the interpretation of inherited capacity in the model. In the baseline formulation, (ψ_m) is a scalar index of local administrative capacity. The evidence discussed below suggests that this object should not be interpreted merely as the organizational presence of a welfare center. What matters empirically is not only whether a municipality had a CRAS in place, but also how actively that CRAS

Table 8: Heterogeneity by Baseline CRAS Presence

	$\text{Quota}^m_{2009} - \text{CountQuota}^m_{2009}$		$\text{Quota}^m_{2009} - \text{Quota}^m_{2008}$	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3
Panel A. First Stage				
$\rho_1 : \Delta\text{Quota}_m$	-0.0642	-0.1750	0.1191^{*}	-0.0293
	(0.1123)	(0.0870)	(0.0511)	(0.1647)
$\rho_2 : \Delta\text{Quota}_m \times \mathbb{1}(\text{CRAS}^{2008}_m)$	0.5007^{***}	0.5499^{***}	0.3968^{*}	0.4532^{*}
	(0.1187)	(0.0897)	(0.1706)	(0.1943)
$\rho_1 + \rho_2$	0.4364^{***}	0.3750^{***}	0.5159^{***}	0.4239^{***}
	(0.0613)	(0.0504)	(0.1445)	(0.1253)
Panel B. Reduced Form				
$\phi_1 : \Delta\text{Quota}_m$	-4.6080	-4.4792	-4.9606^{*}	-4.8650^{*}
	(3.0966)	(2.7055)	(2.3856)	(2.3591)
$\phi_2 : \Delta\text{Quota}_m \times \mathbb{1}(\text{CRAS}^{2008}_m)$	5.4185	5.6937^{*}	5.6838^{*}	5.9313^{*}
	(3.0640)	(2.6440)	(2.3795)	(2.3005)
$\phi_1 + \phi_2$	0.8105^{***}	1.2146^{***}	0.7233	1.0663
	(0.1132)	(0.2788)	(0.3739)	(0.6048)
Panel C. Instrumental Variable				
$\delta_1 : \Delta\widehat{\text{Beneficiaries}}_m$	-457.9477	-330.5999	$-13.0962^{\cdot}$	-11.5788
	(3244.5911)	(2348.1171)	(7.5104)	(7.0308)
$\delta_2 : \Delta\widehat{\text{Beneficiaries}}_m \times \mathbb{1}(\text{CRAS}^{2008}_m)$	458.8026	331.9399	$13.5624^{\cdot}$	$12.4539^{\cdot}$
	(3246.1534)	(2348.9716)	(7.5175)	(6.9586)
$\delta_1 + \delta_2$	0.8550	1.3400	0.4662^{***}	0.8751^{**}
	(1.5704)	(0.8803)	(0.1364)	(0.3216)
Controls: N. of Candidates	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes
Observations	4,384	4,384	4,384	4,384

Notes: This table reports heterogeneous first-stage, reduced-form, and IV estimates by baseline CRAS presence. The omitted group consists of municipalities without a CRAS in 2008. In each panel, the first row reports the base effect for municipalities without a CRAS, the second row reports the differential effect for municipalities with a CRAS, and the third row reports the total effect for municipalities with a CRAS, computed as the sum of the first two coefficients. The standard errors reported in the third row are obtained from the estimated variance-covariance matrix of the corresponding linear combination. All regressions include year and municipality fixed effects, standard errors clustered at the state level, and weights by population in 2000. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\cdot}$ $p < 0.1$.

was already engaging with the community before the reform. A unit that conducts active outreach, maintains systematic contact with beneficiary families, and searches for eligible households beyond its physical premises embodies a qualitatively different level of inherited capacity than one that exists formally but remains operationally thin.

To capture this distinction, let

$$\psi_m = \psi(\kappa_m),$$

where $\kappa_m \in \mathbb{R}^K$ is a vector of pre-reform operational characteristics of the local CRAS. The relevant assumption is that $\psi(\cdot)$ is increasing in the components of κ_m that proxy substantive frontline engagement. The comparative static

$$\frac{\partial B_m^*}{\partial Q_m} = \psi_m + a_m^* + Q_m \frac{\partial a_m^*}{\partial Q_m} \tag{17}$$

then implies that municipalities with more operationally active CRAS units should display a stronger first-stage response to a common quota shock, holding fixed the rest of the environment. In this extended reading of the model, heterogeneity in pre-reform operational engagement provides empirical variation in the inherited-capacity margin ψ_m.

The same logic extends to the reduced form. Since vote share satisfies

$$V_m = V_0 + \left[\lambda_1 + \frac{\lambda_2(\psi_m)}{Q_m}\right] B_m^*,$$

the effect of a quota shock on electoral outcomes depends both on how inherited capacity shapes implementation and on how it affects the inference component of electoral returns. Differentiating with respect to an operational characteristic in κ_m yields

$$\frac{\partial}{\partial \kappa_m}\left[\frac{\partial V_m}{\partial Q_m}\right] = \underbrace{\left[\lambda_1 + \frac{\lambda_2(\psi_m)}{Q_m}\right] \frac{\partial^2 B_m^*}{\partial Q_m \, \partial \psi_m} \frac{\partial \psi_m}{\partial \kappa_m}}_{\text{facilitation}} + \underbrace{\frac{\lambda_2'(\psi_m)}{Q_m} \frac{\partial B_m^*}{\partial Q_m} \frac{\partial \psi_m}{\partial \kappa_m}}_{\text{inference dilution}}. \tag{18}$$

The first term is positive: stronger inherited capacity makes it easier to convert quota authorization into realized expansion. The second term is negative under $\lambda_2'(\psi_m) < 0$: when implementation is already more institutionalized, the informational content of an additional beneficiary may be weaker. The sign of the reduced-form interaction is therefore theoretically ambiguous, but positive interactions arise whenever the implementation effect dominates the inference-dilution effect over the range of ψ_m.

The empirical results below are therefore best interpreted as tests of whether pre-reform operational engagement proxies inherited state capacity in the sense of the model. If so, municipalities with more active CRAS operations before the reform should display a stronger first stage and, potentially, a stronger reduced-form electoral response to a common quota shock. Whether this also translates into a larger IV effect per additional beneficiary is a more demanding question, since the model allows inherited capacity to strengthen implementation while attenuating the informational component of electoral returns.

5.4 Substantive Engagement as an Amplifier for Coverage

Having established that baseline CRAS presence is a necessary condition for the electoral returns to materialise, we now ask whether the *intensity* of frontline engagement prior to the reform further amplified those returns. We focus on a pre-reform operational measure drawn from the 2008 *Censo SUAS*: whether the municipality reported performing active outreach (*busca ativa*). This is a natural indicator of substantive engagement with the community: municipalities whose welfare apparatus was already searching for and contacting vulnerable families before the reform may have been better positioned to translate new quota authorizations into realized enrollment and, in turn, into electoral returns. Municipalities without a CRAS are coded as not performing outreach, so the estimation uses the full sample and the omitted group comprises all municipalities without active outreach, whether or not they had a CRAS in place.[12]

Table 9 reports the results. The first stage is clear and robust across all specifications. The quota shock has a positive and significant effect even for municipalities without active outreach, but the interaction with *busca ativa* is positive and precisely estimated throughout. In the main specification, the base effect is (0.135), the differential is (0.251), and the implied total for municipalities with outreach is (0.386). Active outreach substantially amplified the conversion of federal quota margin into realized beneficiary growth.

The IV evidence speaks to both channels in the model. In the more parsimonious specification without electoral controls, the base IV effect is (1.514) and the interaction is (-0.891), implying a total return of (0.624) percentage points for municipalities with

[12]In the Appendix D, we replicate this exercise using two continuous measures of pre-reform operational intensity – the weekly volume of individual and family interviews and the weekly volume of home visits – restricted to municipalities with a CRAS in 2008. Both variables produce the same qualitative pattern: positive and precisely estimated first-stage interactions, with less stable IV decompositions. The results confirm that the implementation-margin finding does not depend on the specific measure of frontline engagement.

active outreach. The pattern is consistent with inference dilution: municipalities whose welfare apparatus was already engaged in outreach before the reform earned a lower electoral return per standard deviation of beneficiary expansion than those without outreach, plausibly because voters in such municipalities already expected effective delivery and updated less upon observing it. In the main specification with electoral controls and party fixed effects, the differential is (-0.197) and no longer statistically significant, while both the base and joint effects remain positive and significant. The evidence for inference dilution is therefore present but sensitive to specification — robust without controls, attenuated once party composition and the number of candidates are absorbed.

Taken together, *busca ativa* speaks to both margins of the mechanism. On the implementation side, it robustly amplifies the first stage across all specifications. On the electoral-return side, it provides suggestive evidence that pre-existing operational engagement lowered the informational value of program expansion to voters – consistent with Proposition 2(ii), though this result does not survive the most demanding specification.

Taken together, the mechanism results reveal a consistent hierarchy across the two panels of evidence. The implementation margin – captured by the first stage – is where the evidence is strongest and most uniform. Baseline CRAS presence is a necessary condition for the quota shock to generate realized coverage growth at all: municipalities without a welfare center display no first-stage response and no electoral return to the federal authorization. Among municipalities with the infrastructure in place, active outreach further amplifies this translation, with interaction terms that are positive and precisely estimated across specifications and treatment definitions. The message is unambiguous: what mattered was not administrative existence but administrative engagement.

The reduced-form results broadly corroborate this picture. The total reduced-form effect for municipalities with a CRAS is 1.215 percentage points in the main specification, closely mirroring the full-sample estimate. For active outreach, the joint reduced-form effect is 1.231 percentage points, with the base effect alone – capturing municipalities without outreach, including those without a CRAS – positive and precisely estimated throughout.

The evidence on the electoral return per standard deviation of beneficiary expansion – the second-stage interaction – is more limited and warrants a careful reading. The CRAS interaction in the IV is mechanically unstable, a direct consequence of dividing the reduced form by a near-zero first stage in the no-CRAS subgroup rather than

Table 9: Heterogeneity by Active Outreach in 2008

	$\text{Quota}_m^{2009} - \text{CountQuota}_m^{2009}$		$\text{Quota}_m^{2009} - \text{Quota}_m^{2008}$	
	Alt. 1	Main Spec.	Alt. 2	Alt. 3
Panel A. First Stage				
$\rho_1 : \Delta\text{Quota}_m$	0.1217^{***}	0.1347^{***}	0.1333^{***}	0.1372^{***}
	(0.0265)	(0.0198)	(0.0104)	(0.0075)
$\rho_2 : \Delta\text{Quota}_m \times \mathbb{1}(\textit{busca ativa})$	0.3221^{***}	0.2512^{***}	0.4096^{**}	0.3229^{*}
	(0.0509)	(0.0438)	(0.1269)	(0.1357)
$\rho_1 + \rho_2$	0.4438^{***}	0.3859^{***}	0.5429^{***}	0.4601^{**}
	(0.0546)	(0.0492)	(0.1361)	(0.1409)
Panel B. Reduced Form				
$\phi_1 : \Delta\text{Quota}_m$	1.0169^{***}	0.8689^{***}	0.6860^{***}	0.6340^{***}
	(0.1272)	(0.1033)	(0.1225)	(0.1111)
$\phi_2 : \Delta\text{Quota}_m \times \mathbb{1}(\textit{busca ativa})$	$-0.2115^{\cdot}$	0.3623	0.0333	0.4820
	(0.1171)	(0.3151)	(0.3830)	(0.6542)
$\phi_1 + \phi_2$	0.8054^{***}	1.2311^{***}	$0.7193^{\cdot}$	$1.1161^{\cdot}$
	(0.1171)	(0.2850)	(0.3986)	(0.6717)
Panel C. IV				
$\delta_1 : \Delta\widehat{\text{Beneficiaries}}_m$	1.5144^{***}	1.4049^{***}	1.1979^{***}	1.1188^{***}
	(0.2725)	(0.2203)	(0.2299)	(0.2063)
$\delta_2 : \Delta\widehat{\text{Beneficiaries}}_m \times \mathbb{1}(\textit{busca ativa})$	-0.8905^{***}	-0.1971	-0.7622^{**}	-0.2717
	(0.1870)	(0.2073)	(0.2301)	(0.3341)
$\delta_1 + \delta_2$	0.6239^{***}	1.2077^{***}	0.4357^{**}	0.8471^{*}
	(0.1106)	(0.2375)	(0.1585)	(0.3466)
Controls: N. of Candidates	No	Yes	No	Yes
Fixed Effects: Party	No	Yes	No	Yes
Observations	4,384	4,384	4,384	4,384

Notes: This table reports heterogeneous first-stage, reduced-form, and IV estimates by whether the municipality reported performing active outreach (*busca ativa*) in 2008. The omitted group consists of municipalities that did not report active outreach, including municipalities without a CRAS. In each panel, the first row reports the base effect for municipalities without active outreach, the second row reports the differential effect for municipalities with active outreach, and the third row reports the total effect for municipalities with active outreach, computed as the sum of the first two coefficients. The standard errors reported in the third row are obtained from the estimated variance-covariance matrix of the corresponding linear combination. All regressions include year and municipality fixed effects, standard errors clustered at the state level, and weights by population in 2000. Significance: *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\cdot}$ $p < 0.1$.

evidence of a large negative effect. For active outreach, the picture is more informative: without electoral controls, municipalities with outreach earned a significantly lower return per additional beneficiary than those without ((δ_2 = -0.891)), consistent with the inference-dilution channel in the model. This differential does not survive the specification with party fixed effects ((δ_2 = -0.197)), leaving the evidence suggestive rather than conclusive.

Two interpretations are consistent with this pattern. The first is structural: the facilitation and inference-dilution effects predicted by Proposition 2 roughly offset each other in the data, so that operationally active municipalities generate more beneficiaries per quota unit but earn a smaller electoral return per standard deviation of beneficiary expansion, leaving the aggregate second-stage effect broadly stable across the capacity distribution. The second is statistical: the preferred instrument does not generate sufficient within-margin variation to detect the heterogeneous return once party composition is absorbed, and the null reflects limited power rather than a zero effect. The evidence from *busca ativa* without controls lends some support to the first interpretation, but distinguishing between the two conclusively is beyond what the current design can deliver. We leave this as a natural direction for future research.

6 Conclusion

This paper asks whether local politicians can extract electoral gains from a federally administered cash transfer program designed to minimize local discretion. The answer is yes – but only under specific institutional conditions, and only through a specific channel.

We exploit a 2009 federal reform that revised the formula used to compute municipal *Bolsa Família* quotas, generating large and heterogeneous changes in authorized beneficiary slots across municipalities. Because the reform expanded ceilings without automatically enrolling anyone, the translation of federal authorization into realized coverage depended entirely on local governments' decisions to identify, locate, and register eligible families. Using the difference between actual and counterfactual 2009 quotas as an instrument for realized beneficiary expansion, we estimate that a one-standard-deviation increase in instrumented coverage raises the incumbent's vote share by 1.09 percentage points. To put this in context: between the 2008 and 2012 elections, the median incumbent in our sample lost approximately 3 percentage points of vote share. The estimated effect offsets roughly one third of that typical inter-election decline, and for the incumbents who won re-election by a margin of less than 1.09 percentage

points, it would have been sufficient to determine the outcome.

Three features of the evidence discipline this finding. First, the electoral return is not an artifact of federal authorization alone. The reduced form confirms a positive association between the quota shock and incumbent vote share, but the second stage shows that this return materializes only where authorization was converted into realized local delivery. The federal government created the margin; local governments determined whether that margin became actual program expansion.

Second, this conversion depended sharply on pre-existing administrative capacity. The first-stage relationship between quota shocks and realized coverage is concentrated entirely among municipalities that already had a CRAS before the reform. Municipalities without a CRAS display no first-stage response and no electoral return to the quota shock. Among municipalities with a CRAS, the relevant margin is not formal presence but operational engagement: active outreach substantially amplifies the translation of quota authorization into realized enrollment, with interaction terms that are positive and precisely estimated across specifications. What mattered was not whether a welfare center existed but whether it was already substantively engaged with the community it served.

Third, the evidence on the electoral return per standard deviation of beneficiary expansion is more nuanced. Without electoral controls, municipalities with active outreach earned a significantly lower return per additional beneficiary than those without, consistent with voters expecting effective delivery from already-engaged administrations and updating less upon observing it. This differential does not survive the most demanding specification, leaving the evidence suggestive rather than conclusive. Two interpretations remain consistent with the data: either facilitation and inference-dilution effects roughly offset each other across the capacity distribution, leaving the aggregate return per standard deviation of beneficiary expansion broadly stable; or the preferred instrument simply lacks sufficient within-margin variation to detect the heterogeneity once party composition is absorbed. Distinguishing between these interpretations would require either richer variation in capacity or a larger sample with adequate first-stage power in every subgroup. We leave this as a natural direction for future research.

Taken together, these findings speak to a broader question in the political economy of redistribution: under what conditions can politicians claim credit for policies they did not design and cannot manipulate? Our evidence suggests that the relevant margin is not the program's architecture but its last mile. When eligibility rules, benefit values, and payment flows are centralized, politicians lose control over who receives what

and how much. But when enrollment depends on local administrative effort, the implementation process itself becomes a source of political differentiation. Voters reward incumbents not for the existence of the federal program but for the effort invested in making it locally accessible.

These findings speak to a broader class of institutional designs. Many large-scale social programs in middle- and low-income countries combine centralized eligibility and payment with decentralized registration and outreach, creating administrative margins structurally analogous to the one we study. Our evidence from Brazil establishes that, in at least one such setting, centralized program architecture did not eliminate the political returns to local implementation – and that those returns depended on inherited administrative capacity rather than on any manipulation of program rules. Whether this pattern generalizes depends on features we cannot test here: the salience of registration effort to voters, the degree of bureaucratic autonomy at the local level, and the extent to which digital infrastructure has displaced face-to-face enrollment.

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