Political Resource Curse: An Econometric Re-Examination

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1 The Research Question

A central topic within political economy is the relationship between government revenues and political corruption, especially in the context of exogenous resource inflows. There is a growing body of literature that examines the unintended consequences of resource windfalls within the political process (Tornell and Lane 1999, Rodrik and Velasco 1999, Caselli and Coleman 2013). Building on this literature, Brollo et al. 2013, in the paper "The Political Resource Curse", explore how increases in government revenues - specifically through federal transfers - affect political integrity.

The paper extends the discussion on the 'resource curse' by addressing key outcome of these revenue increases: political corruption, the educational qualifications of political candidates, and the likelihood of the incumbent mayors securing reelection. Through this inquiry, the research aims to evaluate how additional federal transfers exacerbate governance challenges, highlighting the adverse effects of increased resources on political processes, candidate quality, and institutional integrity.

2 The Causal Challenge and Research Design

The key challenge in addressing the relationship between federal transfers and corruption is endogeneity, primarily due to reverse causality. While increases in government revenues can potentially lead to higher levels of corruption, it is also plausible that corrupt leaders may be more adept at securing additional resources from higher levels of government. This dynamic complicates causal inference, as simply comparing regions with higher transfers to those with lower transfers would yield biased estimates due to systematic differences.

Beyond reverse causality, omitted variable bias poses another significant problem. Factors such as regional economic conditions, the quality of institutions, geographic location, political stability, and educational attainment can influence both the amount of federal transfers and levels of corruption. Failure to account for these variables results in biased estimates, as the effects of the omitted variables become conflated with the observed relationship between transfers and corruption.

To clarify this issue, consider the following baseline equation:

$$\text{Corruption}_i = \alpha + \beta(\text{revenues}_i) + \epsilon_i \tag{1}$$

Here, coefficient of revenues does not give the desired causal interpretation because the control group (municipalities receiving smaller/no increases in federal transfers) and treatment group (municipalities receiving larger increases in federal transfers) have systematic differences i.e. there are factors other than the assignment of the treatment (increases in transfers) that differentiate the control group from the treatment group. Furthermore, the error term ϵ, which should be uncorrelated with revenues, instead reflects the influence of unobserved factors such as local economic strength or institutional quality.

To deal with the aforementioned issues, the authors make use of the federal transfer system in Brazil. These FPM (Fundo de Participação dos Municípios) transfers are determined exogenously based on cut-off rules which are a deterministic function of population i.e. amount of transfers received by a municipality in a state changes discontinuously as the municipality's population passes a specific

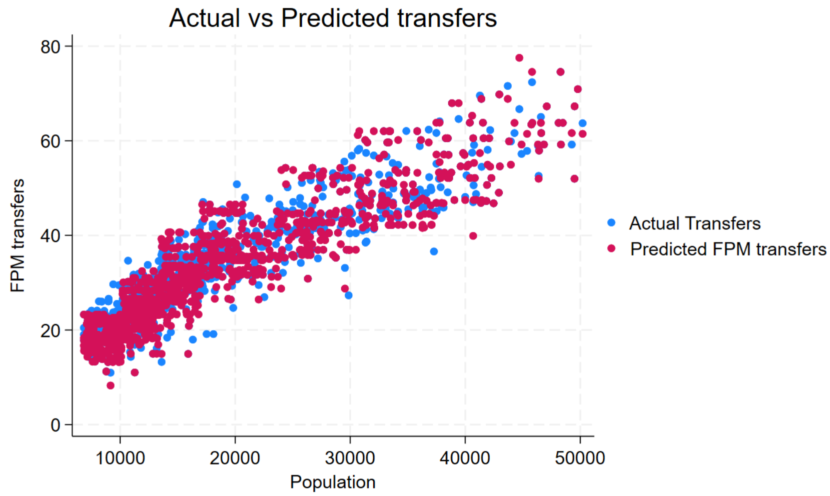

Figure 1: Visual Representation of the Non-Compliance

cutoff [1]. As the population crosses a threshold, the amount of transfers received by the municipality increases discontinuously. The total state-level revenues allocated to the municipality determine the size of these transfers, make the population threshold a key exogenous instrument for identifying the effect of transfers on political outcomes.

This discontinuous nature of the treatment (increases in federal transfers) allows for a localized experiment around the cutoffs. When municipalities cross a population threshold, they experience a change in treatment, facilitating the use of a regression discontinuity design (RDD). In theory, the RDD should produce a share distinction in outcomes at threshold. However, due to non-compliance, actual transfers often do not align perfectly with the predicted transfers based on the cut-offs.

Figure 1 illustrates this non-compliance by plotting actual versus predicted transfers. If compliance were perfect, the scatter plot would display complete overlap between actual and predicted transfers. The divergence between these points shows the extent of non-compliance in the data. such that the treatment turns on upon crossing the threshold and therefore a regression discontinuity design (RDD) can be set up. Such a situation would have been a sharp RD if there were a deterministic jump in the treatment at the cutoffs. However, when observing actual transfers received by a municipality and comparing it to the expected transfers, based on the rules, there are differences that arise i.e. there is non-compliance. The figure below shows a plot of actual and predicted FPM transfers. If there were no non-compliance, the points on the scatter plots for transfers would overlap. To account for this non-compliance, the authors employ a fuzzy RD, utilizing instrumental variables. The variable fpm_hat (theoretical transfers) serves as an instrument for the actual transfers in the fuzzy RDD setup. The model controls for population using a high-order polynomial to account for the relationship between population size and transfers, while also incorporating state and time fixed effects, with standard errors clustered at the municipality level. However, the choice of large bandwidths, extending toward the midpoints of population thresholds, introduces potential issues. This decision makes it challenging to satisfy the fixed covariate assumption, as municipalities just below and just above the cutoff are assumed to have similar properties apart from treatment status. Additionally, the high-order polyno-

[1] The Fundo de Participação dos Municípios (FPM) is a federal transfer system in Brazil where municipalities receive funds based on their population size, according to predetermined population brackets. The amount of FPM transfers received by a municipality is a step function of its population, with larger municipalities receiving higher coefficients. The formula used to calculate FPM transfers for each municipality, FPM_i^k, is:

$$FPM_i^k = \frac{FPM_k \cdot \lambda_i}{\sum \lambda_i}$$

where FPM_k is the total amount allocated to state k, and λ_i is the coefficient assigned to municipality i based on its population bracket. These transfers create exogenous variations in municipal revenues, which can be used for policy and econometric analyses related to corruption and political outcomes mials used to control for population can lead to over-fitting near the thresholds, amplifying variance and reducing precision.

In the next section, I propose improvements to the specifications and discuss the implications of these findings.

3 Regression Discontinuity Design

3.1 The RD Specification

A **fuzzy RD** is conceptually close to an IV approach and can be understood as a local-to-the-cutoff IV. In both approaches, the instrument (population cutoff) induces variation in treatment (FPM transfers) that is unrelated to potential outcomes (corruption), making it possible to isolate the causal effect. The fuzzy RD design leverages exogenous variation introduced by population thresholds, with municipalities just above the cutoff acting as the treatment group and those below serving as the control. Being above the population cutoff strongly predicts receiving larger FPM transfers, making the population threshold a suitable instrument for the treatment. Essentially, municipalities near the cutoff receive differing treatment intensities based solely on their population.

The IV setup involved two stages. In the **first stage**, the treatment, i.e., actual FPM transfers, is instruments by the variable *treatnorm*, a binary variable with value equal to 0 from the midpoint of a threshold till the cutoff, and equal to 1 from cutoff till the midpoint of the next threshold. As population increases, crossing the cutoff causes *treatnorm* to turn on. The first and second stages of the IV setup are given as:

$$\text{fpm}_i = \alpha_1 + \beta_1(\text{treatnorm}_i) + \gamma_1(\text{population}_i) + \delta_i + \epsilon_i \tag{2}$$

$$\text{narrow}_i = \alpha_2 + \beta_2(\hat{fpm}_i) + \gamma_2(\text{population}_i) + \delta_i + \epsilon_i \tag{3}$$

In these equations, *fpm* represents the actual transfers received by a municipality, and $\hat{fpm}$ is the predicted value of FPM obtained from the first stage regression. The outcome of interest, **narrow corruption**, is a binary variable equal to 1 if there was at least one instance of narrow corruption[2] reported and 0 otherwise. Using a binary outcome simplifies interpretation, allowing us to assess whether receiving higher FPM transfers increases the likelihood of severe corruption. Additionally, **broad corruption** also included irregularities that can be interpreted as bad administration and hence serves as a less reliable indicator.

The key confounder in this design is **population**, which drives the assignment of treatment. To address this, the running variable (population) is controlled for using a high-order polynomial to capture any nonlinear effects of population size on transfers. Additionally, the specification includes **state fixed effects**, which account for differences in how states allocate resources, ensuring that the results are not biased by state-level variations in funding.

Given the distribution of population in Figure 2, observations around the last four cutoffs exhibit significant noise, which reduces precision. To improve the RD specification, these cutoffs are excluded, and the bandwidth is set at 1000 population units. This choice strikes a balance between ensuring a sufficient number of observations and reducing variation. Subsequent robustness checks verify that the results remain consistent across different bandwidth choices.

3.2 Validity and Results of the RD

The validity of the above fuzzy RD specification is contingent on three assumptions. **The existence of a first stage** requires that the instrument must have a strong predictive power over the treatment. Table 1 reports the first stage statistics, with a statistically significant outcome at the 1% level ($p<0.01$) and accompanied t-statistic of 5.03. This high significance indicates that the population cutoff is a robust predictor of FPM transfers, confirming the strength of the first stage.

[2] Narrow corruption refers to more serious violations, such as over-invoicing, fraudulent contracts, and favoritism, which represent direct misuse of public funds

Table 1: First Stage Results

VARIABLES	FPM transfers
Treatnorm (instrument)	1.713***
	(0.340, *5.03*)
Population	0.002***
	(0.000, *23.23*)
Observations	372

Note: Standard errors clustered at the city level and t-statistic (italicized) in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Secondly, for any RD to be valid, it is necessary that there is **no manipulation of the running variable**. If manipulation occurs, the exogenous variation in the treatment breaks down, leading to the unaccounted-for systematic differences between treatment and control groups. A frequency histogram in Figure 2 is used to visually inspect for manipulation of the running variable. Although minor jumps in frequency levels are observed at certain cutoffs, these jumps are not systematic enough to invalidate the assumption. Therefore the no manipulation assumption holds.

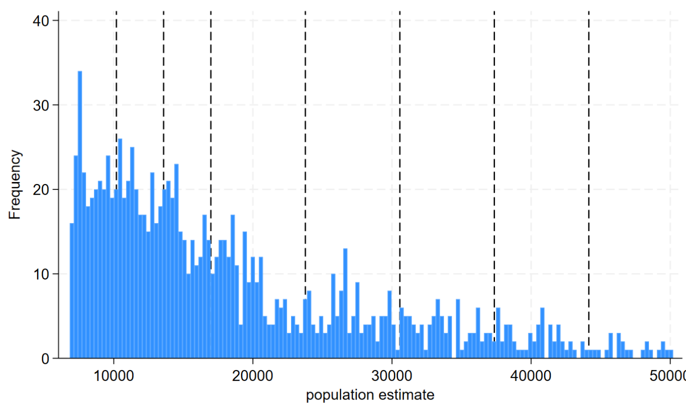

Figure 2: Frequency Distribution of Population

Lastly, **the fixed covariates assumption** implies that municipalities on either side of the cutoff do not systematically differ except the presence of the treatment. This means that the treatment (FPM transfers) does not affect any other covariates. The balance checks test whether these covariates - income per capita in reais (2009 prices), fraction of houses in the city urban areas, and the literacy rate - are evenly distributed across the cutoff.

Table 2 presents the results of the balance checks. The coefficients for FPM transfers on all three covariates are not statistically significant:

- **Income per capita** has a coefficient of -1.866, with a large standard error of 2.698, making this results statistically significant.
- **Urbanization rate** shows a coefficient of 0.011 with an insignificant t-statistic, meaning there is no discernible difference in urbanization levels between municipalities above and below the cutoff.
- **Literacy rate** has a coefficient near zero (0.000) with no statistical significance.

With the validity of the fuzzy RD specification confirmed, we can interpret the results causally. Table 3 presents the outcomes of the RD specification for narrow corruption, the education qualifications of

Table 2: BalanceChecksResults

VARIABLES	Income	Urban rate	Literacy
FPM Transfers	-1.866	0.011	0.000
	(2.698)	(0.011)	(0.003)
Observations	372	372	372

Note: Standard errors clustered at the city level in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

the oppositional pool, and the probability of reelection.

The coefficient for narrow corruption is 0.061, which is statistically significant at the 5% level, implying that a one-unit increase in FPM transfers leads to a 6.1 percentage point increase in the probability of observing narrow corruption. This result has both statistical and economic significant, suggesting that federal transfers have a substantial impact on corruption practices.

The coefficients for educational qualifications of the political challengers[3] are not statistically significant. This suggests that federal transfers do not significantly alter the educational composition of challengers. This result can be understood intuitively; the education levels of political opponents are typically shaped by longer-term societal and institutional forces that federal transfers are unlikely to influence over a short period.

Finally, the coefficient for reelection is also not statistically significant, indicated that increased FPM transfers do not directly influence the reelection probability of incumbents. Authors of The Political Resource Curse (Brollo et al. 2013) suggest that this result may be driven more by the fact that incumbents are facing less capable opponents rather than by the increased transfers themselves. Although the transfers fuel corruption, they do not appear to significantly improve electoral prospects for incumbents.

Table 3: Specification Results

VARIABLES	Narrow	Opp_yschool	Opp_college	Reelected
FPM Transfers	0.061**	-0.006	0.017	-0.025
	(0.029)	(0.149)	(0.019)	(0.038)
Observations	372	372	372	181

Note: Standard errors clustered at the city level in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.3 Robustness of the RD

It is essential to assess the robustness of the results obtained from the RD specification to ensure that the findings are not driven by specific assumptions or data specifications, but can be generalised. Table 4 presents the findings of these checks.

First, slope changes at the three cutoffs were allowed to verify whether the observed treatment effect is consistent or an artifact of the RD strategy. The results demonstrate robustness to slope changes around the first and second cutoffs but not at the third, suggesting that while the treatment effect holds in most cases, additional scrutiny is required at the third cutoff.

Next, the sensitivity of the results to the choice of functional form was tested by incorporating higher-order polynomials of the running variable (population). The specification was not robust to the inclu-

[3]Measured by the proportion of opponents with high school and college education

sion of pop_3[4] or the combination of pop_2[5] and pop_3. The inclusion of pop_2 yielded a statistically significant coefficient of 0.063 at the 5% level. This indicates that the quadratic specification captures the relevant non-linearity in the data, while the higher-order terms introduce instability.

Finally, the robustness of the results was assessed by varying the bandwidth used in the RD specification. The initial bandwidth was set at 1200 population units, producing a coefficient for FPM of 0.090, statistically significant at the 1% level. When the bandwidth was reduced to 800 units, the coefficient dropped to 0.056, significant at the 5% level. The slight reduction in statistical significance is likely due to the smaller number of observations, but the effect size remains economically meaningful. These findings confirm that the observed effect of FPM transfers on narrow corruption is robust across different bandwidth choices, functional forms, and slope adjustments.

Table 4: Robustness Results

VARIABLES	**inter_1**	**inter_2**	**inter_3**	**bw =1200**	**bw =800**	**pop$\hat{2}$**
fpm	0.065**	0.075**	0.044	0.090***	0.056*	0.063**
	(0.032)	(0.033)	(0.031)	(0.034)	(0.030)	(0.029)
Observations	372	372	372	450	300	372

Note: Standard errors clustered at the city level in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4 Differences-in-Differences

4.1 The DiD Specification

The differences-in-differences approach is an identification strategy useful to analyze the impact of policy changes. It makes use of panel data from treatment and control groups to obtain a counterfactual to estimate the causal effect. It is an important strategy when randomization on the individual level is not possible. The DiD specification has been described below.

$$\text{narrow}_i = \alpha + \beta(\text{fpm}_i) + \delta_i + \gamma_t + \epsilon_i \quad (4)$$

Here, β is the coefficient of interest. This is so because by allowing for time γ and city fixed effects δ , we are able to interpret β as the difference in slope between a municipality with a unit higher FPM transfers. Running this regression on our dataset fails to yield any statistically significant results. Table 5 presents the specification results.

Table 5: DiD Specification Results

VARIABLES	**(1) narrow**	**(2) Opp_yschool**	**(3) Opp_college**	**(4) Reelected**
FPM transfers	0.006	0.019	0.004	0.004
	(0.016)	(0.078)	(0.011)	(0.078)
Observations	1,157	1,157	1,157	547

Note: Standard errors clustered at the city level in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

4.2 Validity of DiD Specification

Despite the statistically insignificant results, it is crucial to test the validity of the DiD setup. A DiD approach requires that parallel trends assumption be fulfilled, which implies that in the absence of treatment the differences between the control and the treatment group remains constant over time.

[4]Cubic polynomial of the population
[5]Quadratic polynomial of the population

This allows our estimate to remain unbiased. This assumption can be tested through visual inspection of the time trends of the outcome of interest for the control and treatment group. However, given that the dataset has only two time periods and there is no pre-treatment time period, we are unable to verify this assumption. Therefore, parallel trends becomes a tenuous assumption and calls the validity of the DiD into question.

Like all regressions, it is important to know that the allocation of the treatment is not determined by the outcome. In the case of federal transfers and corruption, it is quite tough to argue for this case as there are chances that the noncompliance to predicted transfers might be due to corruption in the first place. However, the exogenously determined nature of Brazilian federal transfers allows us this assumption to hold for the DiD analysis. Lastly, there should also be no spillover effects i.e. one municipality receiving higher transfers should not impact the amount of transfers received by another municipality. This assumption can be intuitively understood to hold on the grounds that the FPM transfers received by a municipality are state and population dependent and therefore unlikely to cause any spillover effects.

5 Diff-in-Diff vs RDD

This report attempts to answer how increases in federal transfers impact instances of corruption through an RDD and a DiD set up; both the approaches are identification strategies to answer our causal question. While the RD yields significant results, DD fails to produce similar outcomes.

A diff-in-diff approach is beneficial in instances where there is no specific functional form for the treatment of interest, as is quite often the case. While controlling for time and individual specific trends, it also allows for systematic differences to persist between the treatment and control group, something the RD approach fails to do as it is based on close to the cutoff continuity of features data. A DiD set up would also allow us to make use of multiple time periods, however, that is something missing from our dataset.

Despite these advantages of DiD, we are unable to satisfactorily prove its validity in this set up, making RDD a better approach. The RD specification is able to exploit the presence of multiple population cutoffs and treat each of them as localized experiments. Given the nature of the data and the treatment, we are able to show the validity and the robustness of the RDD. While DiD is preferable with repeated observations where any systematic differences can be differenced out, an RDD approach would be better suited to answer this specific causal question due to the cutoff based treatment.

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