

Windfall or Washout? Disaster Relief, Corruption, & Candidate Quality in Local Governments

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Abstract

Natural disasters channel vast sums of money to local officials within days, often with few restrictions. While vital for relief, these transfers also create opportunities for misuse. Why do some local governments turn disaster relief into public goods, while others divert it for private gain? I argue that disaster relief is a special type of revenue windfall: unexpected, non-tax income that expands local budgets. Unlike more permanent federal transfers, disaster relief is both temporary and costly to steal. Disasters expand local budgets but also heighten scrutiny through citizen mobilization, media coverage, and the threat of punishment. I develop a political agency model that shows how these features produce two outcomes: local politicians divert more funds in the short term, but the temporary nature of relief prevents lower-quality challengers from entering politics. I test these claims across 5,570 Brazilian municipalities from 1996–2021 using a novel measure of flood exposure that combines satellite imagery, rainfall, and streamflow data. Results show that flood exposure increases municipal revenues by approximately \$64,000 two years later, and raises detected corruption in randomized audits by about 16 percent over the same horizon. However, challenger quality remains unchanged. These findings highlight the trade-offs governments face in designing disaster relief institutions that must deliver aid rapidly while maintaining accountability.

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Around the world, natural disasters force governments to mobilize vast sums of money in days or weeks. Many countries favor institutions that prioritize getting resources to local actors quickly, often through direct transfers to local politicians with few spending restrictions. These sudden inflows can save lives, but they also bypass standard oversight, creating opportunities for misuse. Does disaster relief foster corruption? And if it alters political incentives, does it attract opportunistic individuals to seek office?

Consider the 2011 floods in Teresópolis, Brazil, which killed more than 300 people and triggered over \$420 million in federal transfers (VOA 2011). Within months the mayor, ironically elected on an anti-corruption platform, was impeached for embezzling millions in relief funds.¹ Similar scandals have followed disasters worldwide: Mayor Nagin was imprisoned for abusing Hurricane Katrina funds (Elliott 2014; Pao 2015), and Colombia's disaster management agency recently announced losses of over \$60 million from embezzlement after disasters in 2024 (Alsema 2024). With climate change increasing disaster frequency (IPCC 2023), understanding these political dynamics is critical.

I argue that disaster relief is a special type of revenue windfall (unexpected, non-tax income that expands local budgets). Like other windfalls, it raises the value of holding office, a condition tied to corruption and low-quality candidates (Brollo et al. 2013). But unlike permanent windfalls, disaster relief is temporary and arrives under intense scrutiny. These two features, relatively ignored in existing research, have distinct consequences: relief may increase short-term rent-seeking, while leaving candidate quality unchanged.

I formalize this argument by extending a standard political agency model to incorporate heterogeneity in both the duration of windfalls and the costs of rent-seeking. The model shows that when windfalls simultaneously expand budgets and raise monitoring, the net effect on corruption is ambiguous. While politicians attempt to capture as much of the windfall as possible, heightened monitoring raises the likelihood of detection and punishment. More strikingly, temporary windfalls do not attract lower-quality candidates since they fail to change the long-run value of holding office.

I test the theory in Brazil using a new measure of flood exposure that combines satellite im-

1. According to investigators, the mayor demanded 50% kickbacks from contractors during the floods, up from his usual 10% (Globo 2011).

agery, streamflow records, and precipitation data. This measure addresses a key identification problem. In Brazil, as elsewhere, disaster relief depends on emergency declarations by mayors, creating scope for strategic reporting. Brazil is notorious for its “drought industry,” where mayors use emergency relief for electoral purposes (Cooperman 2022). My objective measure avoids this selection bias.

The empirical results align with the theory. I first consider the effect of floods on local revenue and monitoring. Floods generate a short-lived revenue increase of approximately R\$373,000 (\$64,000) in the second year after the disaster, then return to baseline. Monitoring effects are mixed: floods slightly reduce voter turnout while weakly increasing non-electoral participation. Consistent with the model’s prediction that rent-seeking rises when changes to the budget dominate monitoring, I find that floods increase detected corruption by 16.1% in randomized audits. However, candidate quality, measured by education and occupation, remains unchanged, consistent with the prediction that temporary windfalls do not reshape political selection.

The magnitude of the corruption effect is roughly half that of the intergovernmental transfer policy found by (Brollo et al. 2013), which also reduced candidate quality. This comparison, while imperfect, suggests that the political consequences of windfalls depend critically on their duration and monitoring/punishment environment. Disaster relief can spur corruption, though its effect is potentially smaller and shorter-lived than other windfalls.

This paper makes three contributions. First, it develops a generalizable theory of political windfalls incorporating two dimensions ignored in prior work: duration and rent-seeking costs. Existing research shows windfalls raise the value of office and dampen electoral accountability (Torvik 2002; Ross 2012; Brollo et al. 2013; Chen and Kung 2016), but treats these shocks as uniform. My framework explains variation in political consequences across windfall types.

Second, this work extends theories of political selection by showing that the effect of windfalls on candidate quality depends critically on their duration. This is a straightforward, but important point. Permanent revenue expansion attracts opportunistic, low quality candidates, but short-lived windfalls do not create the same incentives, even when they increase the temptation for incumbents to divert funds.

Third, I introduce an objective flood measure addressing measurement problems in disaster research. Official declarations, used by major datasets like EM-DAT, create selection bias when

politicians strategically declare emergencies.² My measure combines satellite imagery, streamflow data, and precipitation records to identify floods independently of political reporting.

This paper connects three fragmented literatures: disaster politics, corruption, and the political resource curse. A growing body of work documents the adverse effects of disaster relief on corruption (Leeson and Sobel 2008; Yamamura 2014; Nikolova and Marinov 2017; Nguyen 2017; Wenzel 2021; Zafar, Rahman, and Ammara 2023), while other research highlights how disasters can inspire political participation and pro-social behavior (Fair et al. 2017; Bai and Li 2021; Melville-Rea 2022). I draw on the political accountability literature to connect these findings, showing how disasters and relief interact to produce varied outcomes depending on windfall characteristics.

My work also builds on research framing disasters within electoral accountability. Previous studies emphasize that disasters provide voters with information about incumbent behavior (Gasper and Reeves 2011; Bechtel and Hainmueller 2011; Ashworth, Bueno de Mesquita, and Friedenberg 2018; Masiero and Santarossa 2021; Blankenship et al. 2021), or that politicians favor visible relief over efficient prevention (Healy and Malhotra 2009; Gailmard and Patty 2019). I extend this work by noting that disasters do more than reveal incumbent behavior, they also heighten monitoring of that behavior. This aligns with evidence that disasters influence turnout (Fair et al. 2017; Kosec and Mo 2017; Marsh 2023), increase non-electoral participation (Tatsuki 2007; Rydén et al. 2024), and raise interest in climate policy (Constantino et al. 2022; Arias and Blair 2024).

Finally, I contribute to the literature on windfalls and political agency (Persson and Tabellini 2002; Brollo et al. 2013; Rueda and Ruiz 2020). By identifying two understudied dimensions, duration and rent-seeking costs, I provide a framework for understanding why different windfalls produce different political consequences.

2 Theory

Revenue windfalls are unexpected, non-tax sources of income. Whether triggered by natural resource discoveries, policy changes, or foreign aid,³ such windfalls often alter political incentives. Most research emphasizes adverse consequences, finding windfalls exacerbate rent-seeking and re-

2. One of EM-DAT's main inclusion criteria is an official state of emergency declaration (CRED 2023).

3. One recent report on auctioning wireless broadband frames the revenue as a “windfall” (Kane 2022).

duce candidate quality (Torvik 2002; Caselli and Cunningham 2009; Ross 2012; Brollo et al. 2013; Chen and Kung 2016). However, existing theory overlooks how windfalls differ along two dimensions that may alter these effects: 1) the costs of rent-seeking, and 2) the duration of the revenue increase.

Consider disaster relief. At face value, it should encourage rent-seeking. Like other windfalls, relief increases local revenue, making it easier for politicians to divert funds. When budgets grow, “a dollar stolen has a smaller impact on voters’ inferences about the incumbent’s unobserved ability,” which “diminishes the incentive of political incumbents to please the voters” (Brollo et al. 2013, p.1765). Given the often large scale of disaster transfers, this logic suggests relief may substantially increase corruption.

Yet disasters may simultaneously raise the costs of rent-seeking, the first key dimension. Beyond federal aid, disasters bring media attention and public scrutiny. Fair et al. (2017) find that after major floods in Pakistan, voters became more engaged in local politics. Economic hardship made “salient the importance of government action and policies that ameliorate economic harm” (p.101). Citizens participated more, increasing their ability to monitor politicians. This aligns with findings that hardship raises participation when “people see politics as the only route to solving their problems” (Ojeda, Michener, and Haselwerdt 2024, p.2616). In 2024 after Hurricane Helene, North Carolina mountain communities broke early voting records. One voter explained, “a lot of people were pretty unhappy with the way things were handled, so it might not be so good for incumbents... I think people are invested in what goes on, and then when something like this happens, I think it might get that civic involvement even higher” (Michels 2024). If disasters increase monitoring, they raise the costs of rent-seeking by making abuse easier to detect.

Disasters may also induce harsher punishments. When windfalls are earmarked to alleviate suffering, abuse may provoke severe consequences. COVID-19 provides striking examples. A US legal analytics firm notes that authorities “vigorously investigated and prosecuted” pandemic-related fraud even for “relatively small” amounts (Kingman 2023). Indonesian Minister Juliari Batubara received 12 years for embezzling COVID assistance (A. B. D. Costa 2021); Vietnamese Health Minister Nguyen Thanh Long received 18 years for hiking test-kit prices (Lam and Du 2024); Spanish official Koldo García faces a 50-year sentence for inflating face-mask prices (Iranzo 2024). These cases suggest that misuse of relief funds may be punished more harshly than ordinary

corruption.

A second key difference is duration. Disaster relief is typically temporary and hard to anticipate. A primary mechanism linking windfalls to lower candidate quality is the anticipation of future rents (Brollo et al. 2013). Since rents are more valuable for low-quality challengers, permanent budget increases inspire them to run. But challengers base entry decisions on expected future conditions, not past one-off payments. If disaster relief is temporary, it should primarily affect incumbent incentives rather than challenger entry.

These considerations suggest that disaster relief may occupy a different position in the space of possible windfalls than natural resource revenues or permanent transfer policies. But where exactly? The answer depends on empirical magnitudes. The size of budget changes, real changes in monitoring, and the duration of the revenue shock. The model I develop below formalizes these intuitions and shows how different combinations of these parameters yield different predictions for corruption and candidate quality.

2.1 A Modified Career Concerns Model

I modify Brollo et al. (2013)'s political agency model to allow windfall characteristics to vary.⁴ In the baseline version, an incumbent allocates the budget between rents and public goods. Voters observe these public goods to infer competence, and make a voting decision. Re-election depends on appearing more capable than a challenger. Brollo et al. (2013) show that windfalls incentivize rent-seeking and attract lower-quality candidates.

I introduce two changes. First, I allow the budget and rent-seeking costs to vary across periods. In the standard model these are constant. Second, I examine how optimal rents respond when budget and monitoring costs change simultaneously. These adjustments allow the model to accommodate different types of windfalls and generate conditional predictions.

Consider a two-period game with an incumbent politician, a challenger, and a representative voter. Each period, the politician allocates a budget τ_t between personal rents r_t and public goods g_t . Unlike previous work, I allow the budget to vary across periods, reflecting that budget changes are often temporary.

4. Their model extends Persson and Tabellini (2002)'s career concerns framework.

All politicians are rent-seeking, but differ in competence θ . Highly competent politicians provide more public goods from any given budget. The realized level of public goods is:

$$g_t = \theta(\tau_t - r_t) \quad (1)$$

I impose an upper bound rents in each period, $r_t \leq \psi\tau_t \equiv \bar{r}_t$.

Competence θ is drawn from a uniform distribution depending on politician type $J \in \{H, L\}$. The distribution has mean $1 + \sigma^J$, where $\sigma^H = \sigma = -\sigma^L$ and $0 < \sigma < 1$. High-quality politicians ($J = H$) are on average more competent, though specific low-quality politicians ($J = L$) may exceed specific high-quality ones. The support of θ is $[1 + \sigma^J - \frac{1}{2\xi}, 1 + \sigma^J + \frac{1}{2\xi}]$.

Politician type also influences rent-seeking costs. Rent-seeking is detected with probability q_t . I allow this to vary across periods, unlike Brollo et al. (2013) who treat it as fixed. These authors frame q_t as a fixed audit technology, but it is helpful to think of this more broadly as a general monitoring mechanism. If detected, rent-seeking incurs utility loss λ_t^J , where $0 < \lambda_t^L < \lambda_t^H < 1$. High-quality types find rent-seeking more costly because they have better non-political outside options (p.6).⁵

The incumbent chooses a period-1 policy r_1 knowing her type but not her competence or the challenger's type. She believes the challenger is high type with probability π and low type with probability $1 - \pi$, implying expected challenger competence $\hat{\sigma} = \pi\sigma^H + (1 - \pi)\sigma^L$.⁶ After r_1 , period-1 public goods g_1 are realized and observed, and the challenger's type is revealed before the election.

Voters observe public goods and politician types, but not competence or rents, and re-elect the incumbent if her expected competence conditional on g_1 and J exceeds that of the challenger. The model therefore reflects both moral hazard and adverse selection.

The election winner then chooses a period-2 policy r_2 , determining period-2 public goods g_2 and the probability of detecting rent-seeking. Payoffs are realized at the end of period 2.

To summarize:

5. An opportunity-cost logic. High-quality individuals typically have more to lose from corruption scandals.
 6. Let $O = \{H, L\}$ indicate the type of challenger and note that $P(O = H) = \pi$.

1. The incumbent chooses r_1 . Type and budget are common knowledge, while competence is hidden.
2. Public goods are realized and the challenger's type is revealed.
3. Voters choose between candidates based on g_1 and types. They do not observe r_1 .
4. Rent-seeking is detected with probability q_1 . If detected, the incumbent pays a penalty.
5. The winner chooses r_2 .
6. With probability q_2 , the period 2 politician pays a penalty.
7. Payoffs are realized and the game ends.

2.1.1 Player Payoffs

The expected cost from rent-seeking in period t for type J is $\alpha_t^J = 1 - \lambda_t^J(q_t)$. Notice this term depends on both the probability of detection q_t and the cost of punishment λ_t^J . Lower values represent higher costs. Since $\lambda_t^J < 1$, then $\alpha_t^J > 0$. A two-period incumbent's utility is:⁷

$$U^J = \alpha_1^J r_1 + R + p^J [\alpha_2^J r_2 + R] \quad (2)$$

where R represents ego-rents from holding office and p^J is the re-election probability. Voters derive utility directly from public goods.

2.2 Optimal Rents and Vote-Choice in Equilibrium

In period 2, the optimal policy is simply the maximum \bar{r} . Politicians do not anticipate future rents or elections, and $\alpha_2^J > 0$ means punishment never deters maximum extraction.

Knowing this, voters want to elect the more competent candidate. They re-elect whenever $E(\theta|g_1, J) \geq E(\theta)$. The incumbent, aware of this, chooses r_1 to maximize utility across both periods.⁸ The optimal period 1 policy is:

7. A challenger who wins receives $U_2^O = U_2^J = \alpha_2^J r_2 + R$.

8. The full derivation is in Appendix A.

$$r_1^* = \tau_1 - \xi(1 + \hat{\sigma}) \left(\frac{\alpha_2^J \psi \tau_2}{\alpha_1^J} + \frac{R}{\alpha_1^J} \right) \quad (3)$$

2.3 Windfalls and Rents

Having characterized optimal period 1 rents, I describe two comparative static results.

Proposition 1. *Comparative statics on optimal period 1 rents with respect to the budget and cost of rent-seeking:*

1. *Period 1 rents are increasing in the period 1 budget:*

$$\frac{\partial r_1}{\partial \tau_1} = 1 > 0.$$

2. *Period 1 rents are increasing in α_1^J :*

$$\frac{\partial r_1}{\partial \alpha_1^J} = \xi(1 + \hat{\sigma}) \left(\frac{1 + \alpha_2^J \psi \tau_2}{(\alpha_1^J)^2} \right) > 0$$

where $\alpha_1^J = 1 - \lambda_1^J q_1$, so lower α_1^J represent higher rent-seeking costs.

The first result mirrors Brollo et al. (2013): budget increases (from a type windfall) raise corruption. The second captures the intuition that higher monitoring and punishment costs reduce corruption. Crucially, these forces may operate simultaneously. A disaster that increases the budget while also raising scrutiny creates countervailing pressures:

Corollary 1. *A windfall that increases 1) the budget, t_1 , and 2) the costs of rent-seeking (by decreasing α_1^J), has an ambiguous effect on rents, r_1 . The net effect depends on relative magnitudes.*

This yields a clear empirical implication. The corruption consequences of disaster relief depend on which effect dominates. If budget effects are large and monitoring effects are small, corruption should increase. If monitoring effects dominate, corruption should decrease.

2.4 Temporary Windfalls & Candidate Entry

In Appendix B, I extend the model to allow endogenous candidate entry. The key result concerns windfall duration:

Proposition 2. *A temporary windfall that raises only the period 1 budget τ_1 has no effect on candidate composition:*

$$\frac{\partial \pi}{\partial \tau_1} = 0$$

This differs sharply from permanent windfalls. When budgets increase permanently, low-quality candidates, who have lower opportunity costs and find rents more valuable, are disproportionately attracted to office.⁹ But challengers base entry on expected future conditions, not past payments. If disaster relief affects only the current budget, candidate composition should be unchanged.¹⁰

2.5 Empirical Implications & Validity of Assumptions

The model does not predict a single outcome for disaster relief. Instead, it defines a space of possibilities depending on three empirical quantities:

1. **Budget effects** (τ): Does disaster relief meaningfully expand revenues?
2. **Monitoring effects** (α): Do disasters change the costs of rent-seeking through increased scrutiny or punishment?
3. **Duration** ($\tau_1 = \tau_2?$): Is the revenue shock temporary or persistent?

The empirical analysis proceeds in two stages. First, I estimate these quantities for flood disasters in Brazil. Second, using these results, I derive expectations for corruption and candidate quality and test them against the data. This approach allows the theory to drive the empirical work.

9. This is a key result in Brollo et al. (2013).

10. This is a reasonable assumption since the scale, timing, and frequency of disasters are unpredictable.

3 Brazil: Institutional Setting & Data

Brazil provides an ideal setting for studying disaster windfalls. Existing research documents that windfalls increase rent-seeking and lower candidate quality in Brazilian municipalities (Brollo et al. 2013; Baragwanath 2020), offering a benchmark for comparison. Brazil also provides high-quality data on corruption through randomized audits and detailed candidate information through electoral records.

3.1 Municipal Government and Mayoral Power

Brazil's 26 states are subdivided into 5,570 municipalities, each governed by a mayor and city council. Municipal elections occur every four years in October. Mayors are elected by simple majority and limited to two consecutive terms (Klašnja and Titiunik 2017; Boas, Hidalgo, and Toral 2021).¹¹ Candidates must be Brazilian nationals, at least 21 years old, a resident in the municipality for one year, and affiliated with a party for six months prior to the election (Law No. 9,504/1997). In practice, Brazil's party system is “weakly institutionalized” with “low levels of party identification” and “frequent party switching” (Klašnja and Titiunik 2017, p.134), making municipal races largely candidate-centered (Anagol and Fujiwara 2016).

Despite the non-partisan nature of local elections, partisan alignment dictates the flow of federal resources. Mayors sharing a party with the president or influential federal deputies receive preferential discretionary transfers (Ferreira, Alves, and Caldeira 2021; Slough, Urpelainen, and Yang 2017; Brollo and Nannicini 2012), a pattern that persists in the allocation of disaster relief (Cooperman 2022) and non-governmental funding (Bueno 2018).

Mayors hold substantial power over municipal resources. Finan and Nelson (2009) describe the position as a “major avenue to economic capture” (p.340), and Hollyer, Klašnja, and Titiunik (2022) note that Brazilian politicians consider it “the most valuable prize” in politics (p.90). Decentralization reforms in the 1980s gave municipalities substantial autonomy over public goods provision (Ferraz and Finan 2011). With local taxes providing less than 5.5% of revenue, municipalities rely heavily on federal transfers to fund public goods (Brollo and Nannicini 2012). Although the local council approves budgetary actions, mayors exert substantial control over the

11. Cities over 200,000 use runoff elections (Bueno and Dunning 2017).

actual allocation of these resources, discretion that extends to disaster relief (Cooperman 2022).

Evidence of abuse is widespread. Ferraz and Finan (2011) document mayors fraudulently managing procurement, diverting funds, and over-invoicing for goods and services. In one case, a mayor in Itapetinga used his control over a school lunch program to direct federal funds to his brother's firm, posting the call for bids just one hour before deadline. This discretionary power creates opportunities for corruption, particularly when oversight is limited.

3.2 Disaster Relief

Municipal disaster relief depends on official declarations. When disasters strike, mayors report damages and declare states of emergency. If approved by state or federal authorities, municipalities become eligible for relief transfers (Marchezini et al. 2020; Cooperman 2022). Relief is available for up to 180 days, and mayors can request additional transfers based on reported needs (C. E. Kuhn et al. 2022).¹² Crucially, unlike standard federal transfers, disaster relief carries few spending restrictions (Cooperman 2022).

This system creates scope for strategic behavior. Brazil is notorious for its “drought industry,” where mayors in the Northeast use emergency declarations for electoral purposes (Cooperman 2022). Since relief amounts depend on mayor-reported damages, information asymmetries make abuse difficult to detect. This motivates my use of an objective flood measure that does not depend on declarations.

3.3 CGU Randomized Audits

Brazil’s Office of the Comptroller General (CGU) conducts randomized municipal audits, providing an unusually reliable measure of corruption. The CGU’s audit program (PFSP) ran from 2003-2015, randomly selecting municipalities each lottery round and auditing federal transfer expenditures from preceding years (Ferraz and Finan 2008).¹³ Auditors documented instances of mismanagement and corruption, distinguishing between administrative irregularities and clear malfeasance.

12. For a more detailed explanation of the legal background see Appendix D.

13. Cities under 500,000 were eligible, excluding state capitals. The CGU sampled with replacement and stratified by state (Zamboni and Litschig 2018). Procedural details are in Appendix C.

The CGU publicly releases all audit reports and forwards findings to federal prosecutors and media. These audits have real consequences. Avis, Ferraz, and Finan (2018) document hundreds of prosecutions and convictions stemming from audit findings between 2004 and 2012. This enforcement gives the audits credibility as a corruption measure.

4 Research Design

4.1 Measuring Floods

I construct a binary, objective measure of flood exposure that is independent of political declarations. Floods are defined as 1) the overflow of water from a stream channel onto normally dry land, 2) higher-than-normal levels of water along coasts, lakes, and reservoirs, and 3) the pooling of water at or near where rain falls (Liu et al. 2024). Based on this, a municipality i is coded as flooded in year t if it meets at least one of three criteria: 1) its maximum Standardized Precipitation Index (SPI) exceeds 2, indicating extreme rainfall, 2) a streamflow gauge within its boundaries records flow at or above the historic 99th percentile, or 3) the municipality falls within the boundary of a satellite-identified flood event.

This objective measure addresses several concerns with using declared disasters. First, declarations are endogenous to political incentives. Mayors may strategically declare emergencies to access federal resources regardless of actual conditions. Second, the objective measure captures floods that occur, but are never declared, either because mayors lack political connections or because they are unaware of available relief. Third, it provides a cleaner test of the theoretical mechanism. Objective floods create exogenous shocks to municipal resources, whereas declarations conflate the shock itself with the political response to it. Figure 1 displays the geographic distribution of observed versus declared floods. Full details on the measure appear in Appendix E.

4.2 Key Estimand

The main estimand is the average treatment effect on the treated (ATT) for treated municipality-years,

$$\text{ATT} \equiv E[Y_{it}(1) - Y_{it}(0) \mid \text{Flood}_{it} = 1],$$

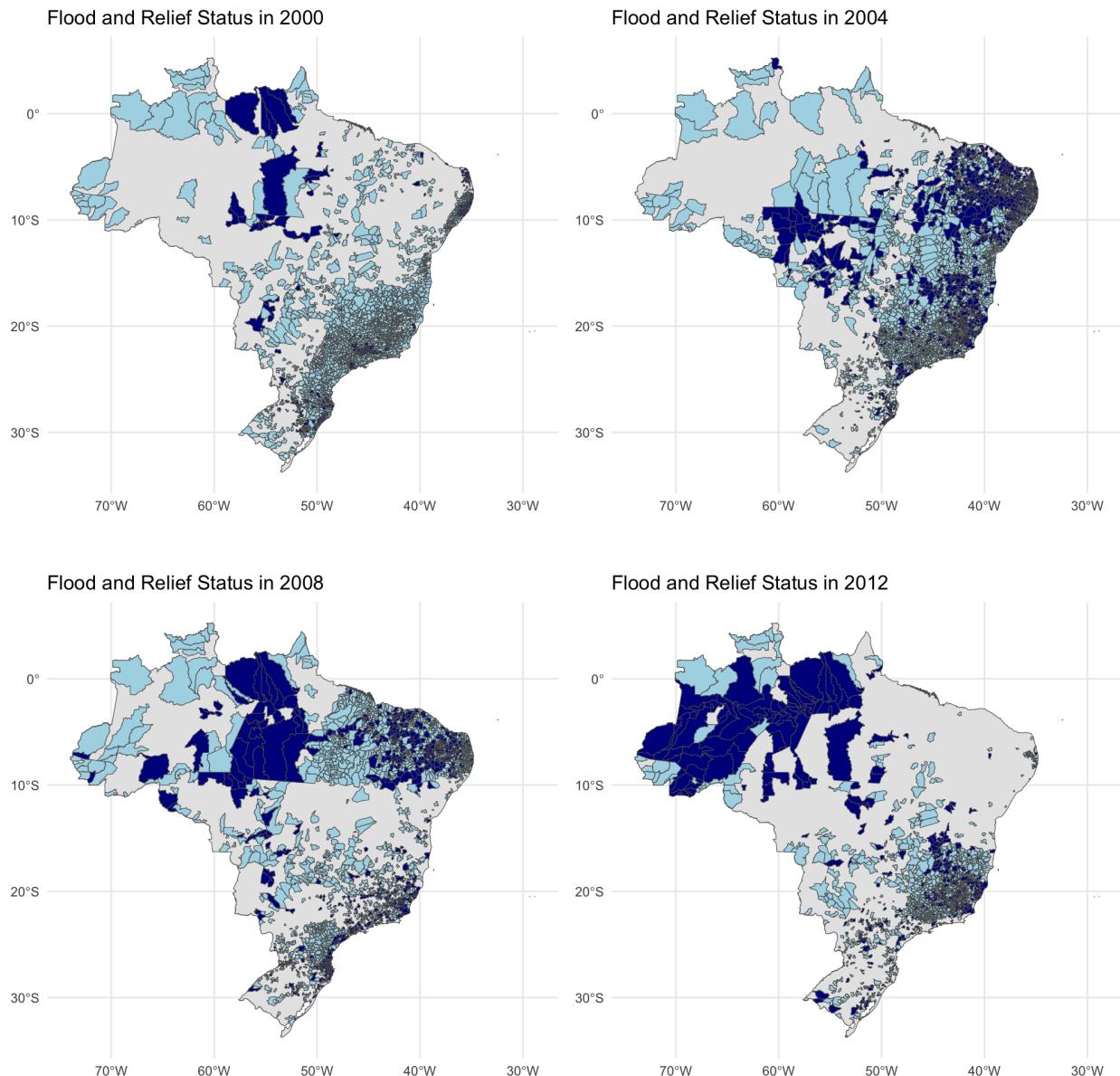


Figure 1: A Sample of Observed & Declared Floods

The light blue regions indicate observed floods, and the dark blue indicate declared floods.

where $Y_{it}(1)$ denotes the potential outcome of interest in the presence of a flood in year t for municipality i , and $Y_{it}(0)$ denotes the counterfactual outcome absent a flood in that same municipality-year.

I also estimate dynamic treatment effects, ATT_s , which capture how effects evolve over time relative to flood onset. Specifically, ATT_s is the average treatment effect s periods after a flood begins, where $s = 0$ denotes the flood year itself. Because flood exposure is temporary and irregularly timed, any estimated effects in years following a flood are interpreted as persistence (or carryover) rather than effects under sustained treatment.¹⁴

4.3 Estimation

Flood exposure in Brazil is non-absorbing: municipalities may experience floods in multiple, non-consecutive years, with treatment switching on and off over time. To accommodate this structure, I use the fixed effects counterfactual (FEct) estimator proposed by Liu, Wang, and Xu (2024). This approach addresses well-known problems with conventional two-way fixed effects (TWFE) models when treatments are heterogeneous or staggered (De Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Callaway and Sant'Anna 2021).

The estimation proceeds in three steps. First, outcomes are modeled using only untreated municipality-years.¹⁵ Second, this model is used to impute counterfactual outcomes $\widehat{Y}_{it}(0)$ for treated observations. Third, treatment effects are computed as,

$$\widehat{\delta}_{it} = Y_{it} - \widehat{Y}_{it}(0),$$

and averaged over treated municipality-years to recover the ATT. Liu, Wang, and Xu (2024) also show that event-study estimates are then the mean of $\widehat{\delta}_{it}$ across all unit-years that are s periods into a treatment spell. Standard errors are block bootstrapped and clustered at the municipality level. I report TWFE estimates for transparency in Appendix H.

14. Details on the timing of treatment, and missingness in the data, are in Appendix G.

15. The model includes unit and time fixed effects, as well as a covariate vector including lagged SPI, log population, log GDP, sanitation, and party alignment with the federal government. See Appendix F.2 for a description of covariate sources.

4.4 Identification

Identification relies on the parallel trends assumption. This means that in the absence of flooding, treated and control municipalities would have followed similar outcome trends (Angrist and Pischke 2009). This assumption is not directly testable, but several diagnostics provide indirect evidence. Under parallel trends, estimated treatment effects in pre-treatment periods should be close to zero. I report these pre-treatment estimates in the event-study figures and conduct an F-test for their joint significance. When pre-trends are detected, I conduct sensitivity analysis following Rambachan and Roth (2023), which characterizes how estimates change under varying degrees of parallel trends violations.¹⁶

An additional threat is the potential for anticipation effects. Municipalities might change behavior before floods if they can predict them. However, while seasonal rainfall patterns are somewhat predictable, the precise timing and severity of floods are not. The objective flood measure, based on extreme threshold values, captures events that are difficult to anticipate.

5 Results

The theoretical framework generates predictions conditional on three windfall characteristics: the magnitude and direction of budget effects, changes in rent-seeking costs, and duration. I organize the results accordingly, first establishing that floods produce meaningful but temporary revenue increases with ambiguous monitoring effects, then examining the downstream consequences for corruption and candidate quality.

5.1 Windfall Characteristics

5.1.1 Revenue

The first key empirical test involves the effect of disasters on municipal revenue. For disaster relief to shape political behavior, it must meaningfully expand municipal budgets. Recall that floods are potentially impactful through: 1) their effect on period one budgets τ_1 , and 2) the duration of this effect (if $\tau_1 = \tau_2 = \tau$). If floods create significant revenue increases, incumbent politicians may

16. Found in Appendix J.

seek to appropriate part of the additional funds rather than channel them fully into public goods. At the same time, temporary resources are less likely to reshape long-run political incentives. I use official data from Brazil's National Treasury (SICONFI 2024) to test the effect of floods on revenue from 1998 to 2021.¹⁷

Figure 2 displays the estimated ATT_s for log federal transfer revenue. The results show that floods produce a positive but delayed increase in municipal revenues. In the flood year itself ($s = 0$) and the year immediately after ($s = 1$), the effects are close to zero, suggesting delays in fund disbursements. The effect peaks at $s = 2$, with an estimated increase of 0.83% in federal transfers ($\delta = 0.008, p < 0.001$). Given mean annual transfers of R\$44.9 million, this corresponds to roughly R\$373,408 ($\approx \$64,000$). By $s = 3$ and $s = 4$, the effect returns to zero, confirming the temporary nature of disaster-related windfalls.

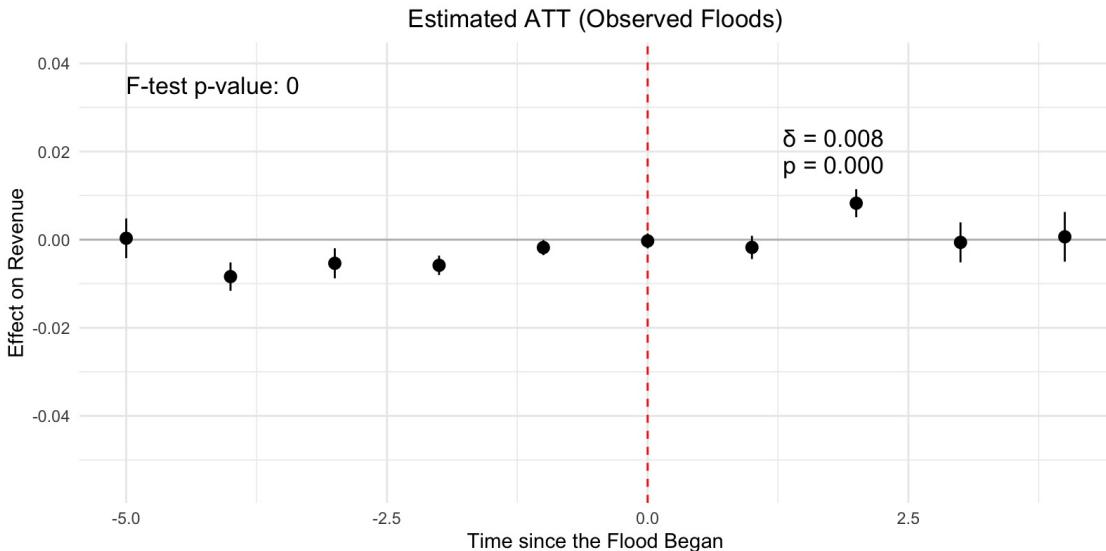


Figure 2: Effect of Floods on Federal Transfer Revenue. Event time 0 denotes the flood year. Bars show 95% confidence intervals. The F-test evaluates the null hypothesis that all pre-treatment effects are jointly zero.

This magnitude is substantively meaningful. For comparison, the average annual salary of mayors in state capital cities is approximately R\$245,000 (Aguiar 2024), thus the typical flood-induced revenue boost exceeds a mayor's yearly salary. The magnitude is also comparable to the policy windfall studied by Brollo et al. (2013), who found that a transfer increase of approximately R\$550,000 produced substantial increases in corruption.

17. See Appendix F.3 for a detailed description of the data.

The F-test of pre-treatment effects rejects the null of no pre-trends ($p < 0.01$). Figure 2 shows a slight dip in the pre-treatment estimates around $s = -4$ to $s = -2$ before recovery. This pattern raises concerns about parallel trends, though the temporary dip followed by recovery before treatment is inconsistent with simple confounding stories where flooded municipalities are on systematically different trajectories.

5.1.2 Monitoring

The theory predicts that disasters attenuate corruption effects if they increase monitoring and thereby raise the costs of rent-seeking. I examine two proxies for bottom-up monitoring: voter turnout in municipal elections and non-electoral political participation.

Data on turnout come from the *Tribunal Superior Eleitoral* (TSE), which maintains records on all municipal elections.¹⁸ Because elections occur every four years, the event-study is measured in election cycles rather than years. Figure 3 displays the results. In the election immediately following a flood ($s = 0$), turnout slightly rises. However, turnout declines in subsequent election cycles: by approximately 1.3 percentage points at $s = 2$ (two election cycles, or eight years, after the flood) and 1.7 percentage points at $s = 3$.

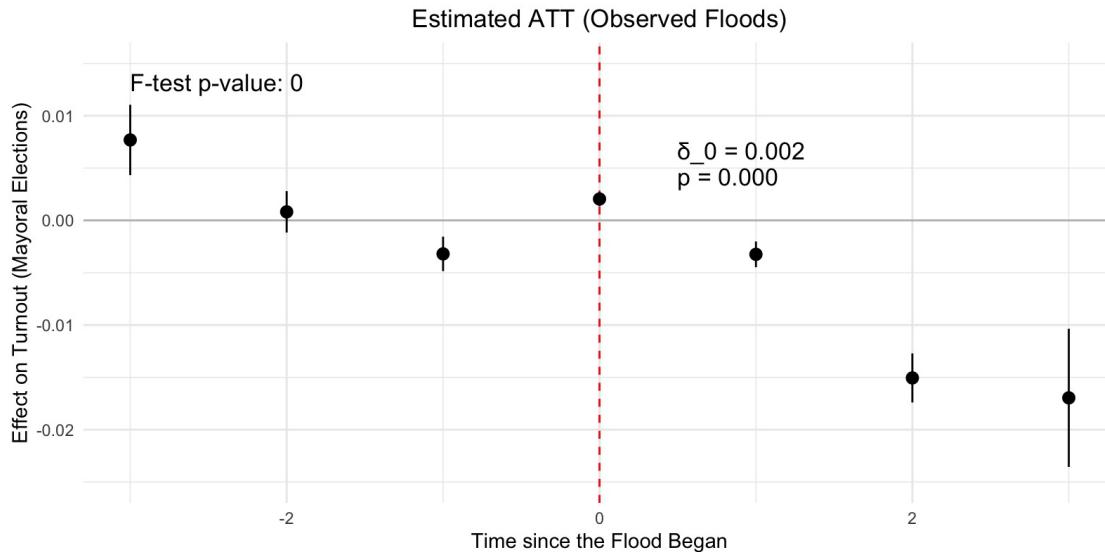


Figure 3: Effect of Floods on Voter Turnout in Mayoral Elections. Time is measured in election cycles (4 years each). Event time 0 denotes the first election after a flood.

18. A cleaned version of the data is available through Dahis et al. (2022)'s Base dos Dados.

This delayed negative effect may reflect flood-induced out-migration that compounds over time, or lasting damage to local civic infrastructure. The pattern somewhat contrasts with Fair et al. (2017), who find positive short-run participation effects after floods in Pakistan. The F-test rejects the null of no pre-trends, with some evidence of declining turnout in treated municipalities even before flood exposure, which complicates causal interpretation.

I supplement the turnout results with survey data from LAPOP's AmericasBarometer, which covers various forms of political engagement including attendance at community meetings, political party meetings, and protests (LAPOP 2024). The results, reported in full in Appendix H.2, are mixed. Some forms of non-electoral participation increase modestly after floods. For example, attendance at political meetings rises by roughly 7 percentage points and the likelihood of joining a protest increases by around 2 percentage points. However, other outcomes are sensitive to specification and often statistically insignificant.

Overall, the monitoring evidence is ambiguous. Electoral participation shows no immediate effect but declines over subsequent election cycles, while non-electoral engagement shows mixed patterns. This suggests that floods do not clearly raise the costs of rent-seeking through citizen oversight, and may even weaken accountability over time. That said, the results also suggest that floods may shift the form of political participation rather than suppress it outright. Given these mixed results, combined with the substantial (though delayed) revenue increase, the theory predicts that floods should increase corruption.

5.2 Corruption

Data on corruption come from Brazil's Office of the Comptroller General (CGU), which conducted randomized audits of municipal governments between 2003 and 2015 as part of the Public Oversight of Public Resources Program (CGU 2024). Auditors coded irregularities into categories of varying severity. Following Avis, Ferraz, and Finan (2018) and Brollo et al. (2013), I count moderate and severe irregularities as corruption.¹⁹ The primary outcome is the number of irregularities per audited service order in a given municipality-year. The audit sample includes 1,134 municipalities across 15 years, with an average of 2.36 irregularities per service order. Additional details appear in Appendix F.5. Figure 4 displays the sample of audited municipalities.

19. Both include actions that may classify as rent-seeking.

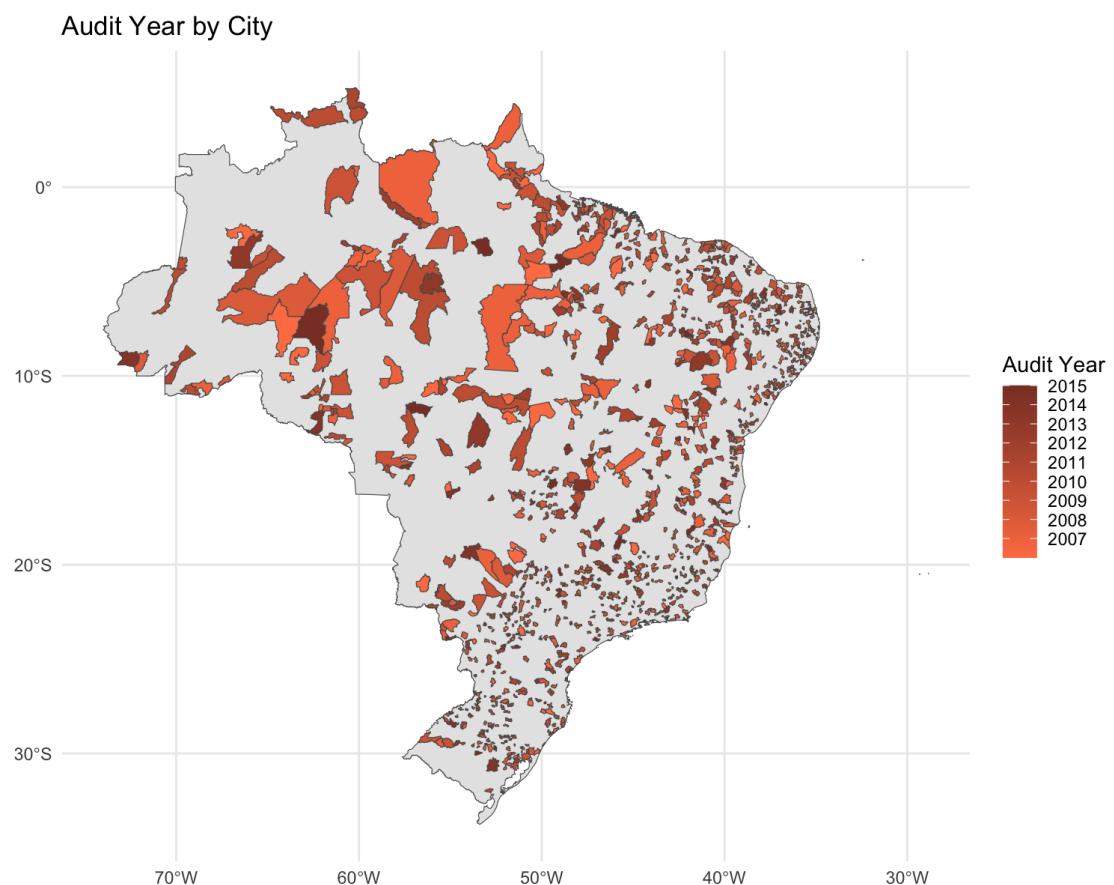


Figure 4: Municipalities Audited between 2006 and 2015

Figure 5 displays the estimated effects of floods on corruption. Consistent with theoretical expectations, floods increase detected corruption. The effect emerges at $s = 2$, with treated municipalities experiencing approximately 0.38 additional irregularities per service order ($\delta = 0.379$, $p = 0.041$). Given the baseline mean of 2.36, this represents a 16.1% increase in corruption two years after a flood.

The timing aligns with the revenue results. Corruption increases when the additional transfers arrive, not immediately upon flood exposure. This pattern is consistent with the theoretical mechanism in which budget expansion enables rent extraction.

The F-test for pre-treatment effects yields $p = 0.032$, marginally rejecting the null of no pre-trends. Figure 5 shows imprecise pre-treatment estimates, including a positive estimate at $s = -4$ that declines toward zero by $s = -2$. This pattern is potentially consistent with sampling variability in corruption outcomes, which are observed only for audited municipalities. Sensitivity analyses appear in Appendix J.

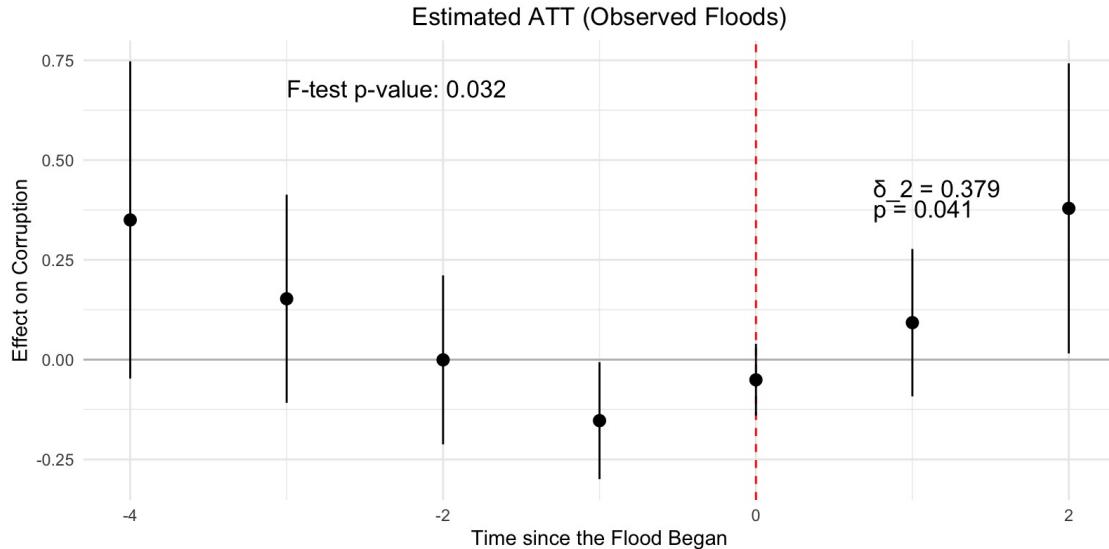


Figure 5: Effect of Floods on Corruption. Corruption is measured as irregularities per audited service order. Baseline mean is 2.36.

To benchmark these results, I compare with Brollo et al. (2013), who study a similarly sized policy windfall. Using their measure of corruption (percent of service orders with irregularities), I find that floods increase corruption by 3.4 percentage points, compared to their 7.3 percentage point effect (the full results are in Appendix I). The flood effect is roughly half the magnitude

despite comparable transfer amounts. I return to possible explanations in the conclusion.

5.3 Candidate Quality

Given the results on revenue, the theory predicts that temporary windfalls should not alter candidate quality. This is because floods do not create long-run changes in municipal revenue (and thus the value of holding office). Data on candidates come from the TSE, which maintains records on all individuals running for mayor, including their education and occupation. I aggregate these characteristics at the municipality-election level, excluding incumbents seeking reelection. Because elections occur every four years, the event-study is again measured in election cycles. Details on data collection appear in Appendix F.6.

I rely on three proxies of candidate quality, all designed to reflect differences in the opportunity costs of rent-seeking. The first two measures are based on education: the proportion of challengers with a high school degree and the proportion of challengers with a college degree. I follow many other scholars in using education as a proxy for quality (Besley, Pande, and Rao 2005; Brollo et al. 2013; Galasso and Nannicini 2011; Beath et al. 2016). The connection between education and rent-seeking depends on the assumption that more educated candidates have access to higher-paying, stable jobs, which increases the opportunity cost of engaging in corruption. While plausible, there are recent critiques of this approach. For example, Carnes and Lupu (2016) finds that, in the Brazilian context, lower-educated incumbent politicians are no more likely to engage in corruption than their highly educated counterparts.²⁰ With this in mind, I propose an additional measure of quality.

The final measure is the proportion of challenger candidates with white-collar jobs, which more directly aligns with the theoretical link between opportunity costs and rent-seeking. White-collar candidates may face higher opportunity costs compared to blue-collar workers due to their employment prospects outside of politics.

Figure 6 displays the estimated effects on challenger education: the proportion with at least a high school diploma (top panel) and the proportion with a college degree (bottom panel). Consis-

²⁰ It is worth noting that Carnes and Lupu (2016) relies on a close-elections regression discontinuity, which has recently faced criticism (Marshall 2024). It may be the case that lower-quality candidates who win close elections are fundamentally different from those who lose.

tent with the theoretical prediction, floods show no meaningful effects on candidate education. The estimates are close to zero across all event-time periods, and the F-tests fail to reject the null of no pre-trends ($p = 0.82$ for high school; $p = 0.23$ for college). The clean pre-treatment dynamics and null post-treatment effects provide strong support for the theoretical prediction.

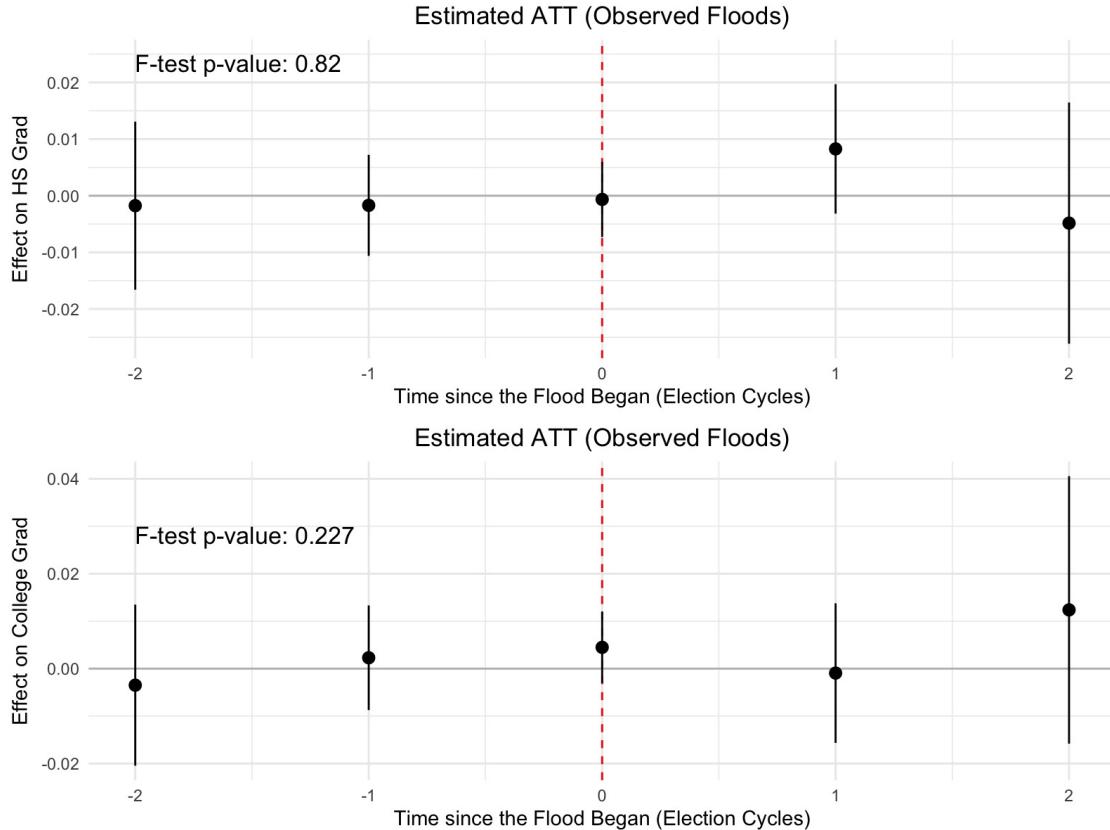


Figure 6: Effect of Floods on Candidate Education. Top panel: proportion of challengers with high school diploma. Bottom panel: proportion with college degree. Time is measured in election cycles.

Results for occupational background are similar. Figure 7 shows effects on the proportion of white-collar candidates. The F-test fails to reject the null ($p = 0.72$), and post-treatment estimates remain close to zero. If anything, floods are associated with, at most, a minor increase in the proportion of challengers with white-collar backgrounds, though this is less than two percentage points. Given that, on average, roughly 80% of candidates already come from white-collar professions, this shift is substantively minor.

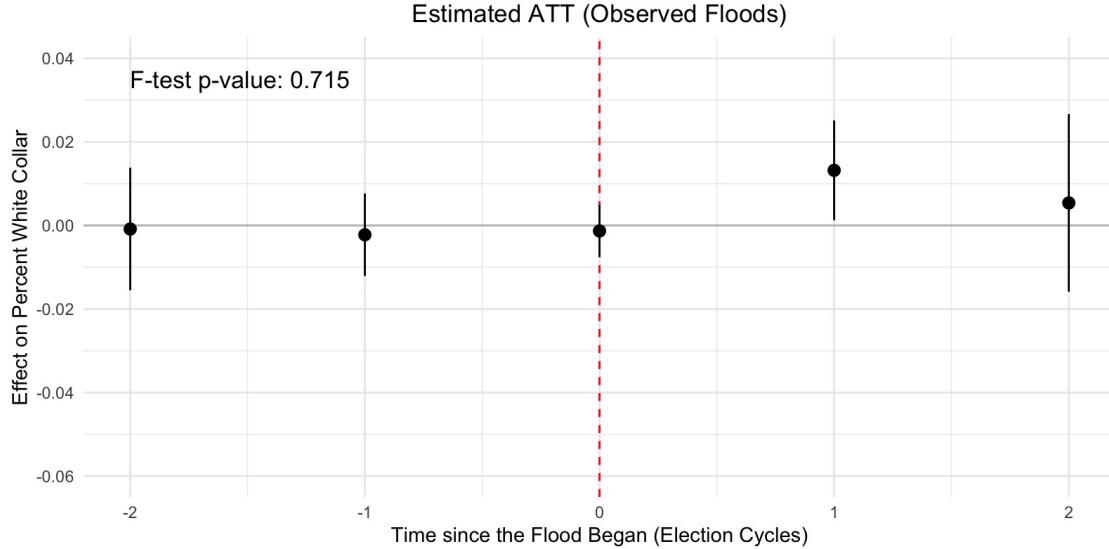


Figure 7: Effect of Floods on Candidate Occupation. Outcome is the proportion of challengers in white-collar occupations. Time is measured in election cycles.

These null results contrast sharply with Brollo et al. (2013), who find that permanent windfalls reduce the proportion of college-educated candidates by 2.7 percentage points. The difference aligns with the theoretical prediction. Temporary windfalls do not attract lower-quality candidates because they do not alter expected future rents.

The overall empirical pattern supports the theory. Floods generate temporary revenue windfalls that peak two years after exposure and dissipate by year three. Monitoring effects are ambiguous at best and may be negative over time. Consistent with budget effects dominating, corruption increases by 16% at the same lag as revenue. Yet, consistent with the prediction that temporary windfalls do not reshape political selection, candidate characteristics show precisely estimated null effects with clean pre-treatment dynamics.

6 Discussion & Conclusion

This paper asked whether natural disasters, by generating temporary windfalls, foster corruption and alter the quality of political candidates. The question matters because disaster relief is designed to save lives, yet it also provides local politicians with sudden revenue inflows that can be exploited as private rents. With climate change increasing the frequency and severity of disasters, understanding these political consequences is increasingly urgent.

I developed a theoretical framework that introduces two dimensions of windfalls largely ignored in prior work: their duration and the costs of capturing rents. Standard models treat windfalls as permanent budget expansions that both increase corruption and attract lower-quality candidates. My extension shows that these effects can be decoupled. Budget effects and monitoring costs jointly determine corruption, while candidate quality depends specifically on whether windfalls are permanent or temporary. Temporary windfalls may increase corruption without degrading the candidate pool.

The empirical results from Brazil support these predictions. Floods generate meaningful but short-lived revenue increases, approximately R\$373,000 in additional federal transfers, arriving with a two-year lag and dissipating by the third year. Monitoring effects are ambiguous: voter turnout declines modestly while non-electoral participation shows mixed patterns. Given substantial budget effects and unclear monitoring changes, the theory predicts increased corruption, which is what I find: a 16.1% increase in detected irregularities in randomized audits. Yet consistent with the prediction that temporary windfalls do not reshape political selection, candidate characteristics remain unchanged.

The comparison with Brollo et al. (2013) is instructive. Their study of a permanent policy windfall of similar magnitude found both higher corruption (7.3 percentage points) and lower candidate quality (2.7 percentage point reduction in college-educated candidates). The flood effect on corruption is roughly half this size despite comparable transfer amounts. Three factors may explain this difference. First, the Brollo et al. windfall was somewhat larger (by approximately R\$169,000). Second, disaster contexts may impose higher monitoring costs that partially offset budget effects. While I only investigate monitoring, future work should consider the role of punishment. Third, and most important theoretically, the temporary nature of floods prevents adverse selection effects. In the Brollo et al. setting, permanent windfalls attracted lower-quality candidates, which indirectly increased corruption by improving incumbents' reelection prospects. With floods, the absence of selection effects means corruption increases only through the direct budget channel.

These findings have implications for both theory and policy. For scholars of political economy, the results demonstrate that windfall duration belongs alongside windfall size as a key determinant of political consequences. Models that treat all revenue shocks equivalently miss important

variation. These new dimensions are important for building more general theories of how fiscal shocks affect governance. For policymakers concerned about disaster relief, the results offer cautious reassurance. While corruption risks are real, these temporary windfalls appear less politically damaging when compared to permanent revenue expansions. The absence of candidate quality effects is particularly notable given concerns that disaster-prone regions might attract lower-quality politicians.

This paper has several limitations. Pre-treatment dynamics in some specifications raise concerns about parallel trends, though the main results are robust across alternative specifications. The evidence on monitoring should be interpreted as suggestive. More direct measures of oversight and sanctioning would strengthen the analysis. Finally, while Brazil offers unusually rich institutional detail and audit data, the single-country setting limits external validity.

Future research could extend this framework in several directions. Testing whether similar dynamics arise following other temporary windfalls, such as commodity price shocks or one-time fiscal transfers, would help assess the generalizability of the mechanism. Exploring heterogeneity by political competition or institutional capacity could clarify where temporary fiscal expansions are most damaging. As climate change increases the frequency of natural disasters, an open question is whether repeated temporary shocks eventually resemble permanent revenue increases in their political effects. Others may also be interested in applying the framework to other types of disasters with different features (for example, droughts, which are longer lasting and harder to measure).

Disaster relief saves lives, but it also creates political temptations. The theoretical framework developed here clarifies when those temptations are likely to be most severe, and optimistically shows that temporary assistance does not necessarily undermine democratic governance.

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Supplementary Material for Windfall or Washout? Disaster Relief, Corruption, & Candidate Quality in Local Governments

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A Proofs for the Baseline Model

A.1 Optimal Rents

The proofs for optimal rents in the baseline model closely resemble Brollo et al. (2013).

A.1.1 Period 2 Rents

The period 2 utility for any type politician is

$$U_2^J = \alpha_2^J r_2 + R$$

where $\alpha_2^J = 1 - \lambda^J q_2$. Since $\lambda^J < 1$, and $0 \leq q_2 \leq 1$, it is always true that $\alpha_2^J > 0$. This means that politicians will only ever improve their period 2 utility by raising r_2 . The upper bound of r_2 is $\bar{r}_2 \equiv \psi r_2 < \tau_2$. Thus, the optimal choice for period 2 rents is

$$r_2^* = \bar{r} = \psi r_2$$

A.1.2 Period 1 Rents

The optimal period 1 policy decision is given by

$$r_1^* = \tau_1 - \xi(1 + \hat{\sigma}) \left(\frac{\alpha_2^J \psi \tau_2}{\alpha_1^J} + \frac{R}{\alpha_1^J} \right).$$

In order to find this, the incumbent maximizes

$$U_1^J = \alpha_1^J + R + p^J V_2^J$$

with respect to r_1 .

To start, consider p^J , the probability of re-election. Equation ?? states

$$E(\theta | g_1, J) \geq 1 + \sigma^O, \quad J, O \in \{H, L\}$$

In addition, equation 1 demonstrates that

$$\begin{aligned} \theta(\tau_t - r_t) &= g_t \\ \theta &= \frac{g_t}{(\tau_t - r_t)} \end{aligned} \tag{4}$$

Letting r_1^{eJ} equal the voters' expectation of period 1 rents from an incumbent of type J ,

$$E(\theta|g_1, J) = \frac{g_1}{\tau_1 - r_1^{eJ}} \quad (5)$$

Since the incumbent knows the true value of r_1 , then substituting g_1 gives,

$$\begin{aligned} E(\theta|g_1, J) &= \frac{g_1}{\tau_1 - r_1^{eJ}} \\ E(\theta|g_1, J) &= \frac{\theta(\tau_t - r_t)}{\tau_1 - r_1^{eJ}}. \end{aligned} \quad (6)$$

Thus, the incumbent wins the election if

$$\begin{aligned} \frac{\theta(\tau_t - r_t)}{\tau_1 - r_1^{eJ}} &\geq 1 + \sigma^O \\ \theta &\geq \left(\frac{\tau_1 - r_1^{eJ}}{\tau_t - r_t} \right) (1 + \sigma^O). \end{aligned} \quad (7)$$

This means that the probability of re-election for an incumbent of type J facing a challenger of type O is,

$$p^{J,O} = P \left(\theta \geq \left(\frac{\tau_1 - r_1^{eJ}}{\tau_t - r_t} \right) (1 + \sigma^O) \right) \quad (8)$$

To find this probability, note that in general, the probability distribution function $f_\theta(\theta)$ gives the following:

$$\begin{aligned} P(\theta > x) &= \int_x^{1+\sigma^J+1/2\xi} \xi \, d\theta \\ &= \xi \left(1 + \sigma^J + \frac{1}{2\xi} - x \right) \\ &= \frac{1}{2} + \xi \left(1 + \sigma^J - x \right) \end{aligned}$$

Therefore,

$$\begin{aligned} p^{J,O} &= \frac{1}{2} + \xi \left(1 + \sigma^J - \left(\frac{\tau_1 - r_1^{eJ}}{\tau_t - r_t} \right) (1 + \sigma^O) \right) \\ &= \frac{1}{2} + \xi \left(1 + \sigma^J \right) - \xi \left(\frac{\tau_1 - r_1^{eJ}}{\tau_t - r_t} \right) (1 + \sigma^O) \end{aligned} \quad (9)$$

At the time of the period 1 policy choice, the incumbent does not know the type of challenger,

but knows that the expected competence of any challenger (regardless of type) is equal to $\hat{\sigma}$, giving

$$p^J = \frac{1}{2} + \xi(1 + \sigma^J) - \xi \left(\frac{\tau_1 - r_1^{eJ}}{\tau_t - r_t} \right) (1 + \hat{\sigma}) \quad (10)$$

All of this makes it possible to check the first order condition for period 1 utility.

$$\frac{\partial U_1^J}{\partial r_1} = \alpha_1 + \frac{\partial p^J}{\partial r_1} U_2^J = 0$$

Here, the partial derivative of the re-election probability with respect to r_1 is:

$$\begin{aligned} \frac{\partial p^J}{\partial r_1} &= -\xi(1 + \hat{\sigma})(\tau_1 - r_1^{eJ})(-1)(\tau_1 - r_1)^{-2}(-1) \\ &= -\xi(1 + \hat{\sigma}) \frac{\tau_1 - r_1^{eJ}}{(\tau_1 - r_1)^2} \end{aligned}$$

Imposing the equilibrium condition that $r_1^{eJ} = r_1$ gives,

$$\frac{\partial p^J}{\partial r_1} = -\frac{\xi(1 + \hat{\sigma})}{\tau_1 - r_1} < 0 \quad (11)$$

This entire term is less than 0 because (all terms besides the negative 1 out front are positive). With all of this, I can now find the optimal period 1 rents.

$$\begin{aligned} \frac{\partial U_1^J}{\partial r_1} &= \alpha_1 + \frac{\partial p^J}{\partial r_1} U_2^J = 0 \\ \alpha_1 - \frac{\xi(1 + \hat{\sigma})}{\tau_1 - r_1} (\alpha_2^J \psi \tau_2 + R) &= 0 \\ \alpha_1 &= \frac{\xi(1 + \hat{\sigma})}{\tau_1 - r_1} (\alpha_2^J \psi \tau_2 + R) \\ \alpha_1 (\tau_1 - r_1) &= \xi(1 + \hat{\sigma}) (\alpha_2^J \psi \tau_2 + R) \\ \alpha_1 r_1 &= \alpha_1 \tau_1 - \xi(1 + \hat{\sigma}) (\alpha_2^J \psi \tau_2 + R) \\ r_1^* &= \tau_1 - \xi(1 + \hat{\sigma}) \left(\frac{\alpha_2^J \psi \tau_2}{\alpha_1^J} + \frac{R}{\alpha_1^J} \right) \end{aligned}$$

B Endogenous Candidate Entry

In addition to all of the baseline model's parameters, consider the following setting where π becomes endogenous. Let $2N$ represent the number of individuals in a population of possible challengers. In line with Brollo et al. (2013), N is a large integer. Just like the politicians, individuals in the population are one of two types, $J \in \{H, L\}$, where $J = H$ indicates a high-quality type, and $J = L$ a low-quality type.

Potential candidates differ in their opportunity costs of running. Specifically, person i of type J has a cost $\beta_i y^J$, where $\beta_i = i$. Assume that $y^H > y^L > 0$, so that the opportunity cost of the high quality types is larger than the low quality types. [rationale for doing this]. Type, like in the baseline model, also influences the distribution of θ , and the punishment cost from rent-seeking, λ^J .

At the start of the first period, individuals in the population decide whether or not to run for political office. Let n^J represent the number of high and low quality individuals in the candidate pool, where $n = n^H + n^L$. An individual's utility from staying out of politics is

$$W_i^J(\text{Stay out}) = iy^J \quad (12)$$

If the individual enters politics, they are selected as the single challenger with uniform probability $\frac{1}{n}$. Additionally, the selected individual of type J wins the election with probability $(\frac{1}{2} + \xi\sigma^J)$. If an individual enters the pool, and either remains unselected or loses the election, they receive zero utility. Therefore, the utility from entering the candidate pool is

$$W_i^J(\text{Enter}) = \frac{1}{n} \left(\frac{1}{2} + \xi\sigma^J \right) V_2^J \quad (13)$$

This means that individual i in group J enters whenever

$$iy^J \leq \frac{\left(\frac{1}{2} + \xi\sigma^J \right)}{n} V_2^J \quad (14)$$

We can then solve for n by first considering the indifference condition

$$n^J y^J = \frac{\left(\frac{1}{2} + \xi\sigma^J \right)}{n} V_2^J \quad (15)$$

where

$$n^J = \frac{\left(\frac{1}{2} + \xi\sigma^J\right)}{n} \frac{V_2^J}{y^J} \quad (16)$$

And

$$n = \sqrt{\frac{V_2^H}{y^H} \left(\frac{1}{2} + \xi\sigma\right) + \frac{V_2^L}{y^L} \left(\frac{1}{2} - \xi\sigma\right)} \quad (17)$$

This means that the proportion of low quality types in the candidate pool is the following

$$\pi = \frac{n^L}{n^H + n^L} = \frac{1}{1+x} \quad (18)$$

where

$$x = \frac{V_2^H}{V_2^L} \frac{y^L \frac{1}{2} + \xi\sigma}{y^H \frac{1}{2} - \xi\sigma} \quad (19)$$

And

$$\frac{V_2^H}{V_2^L} = \frac{\alpha_2^H \psi \tau_2 + R}{\alpha_2^L \psi \tau_2 + R} \quad (20)$$

From this, it is straightforward to show the result in proposition 2:

$$\begin{aligned} \frac{V_2^H}{V_2^L} &= \frac{\alpha_2^H \psi \tau_2 + R}{\alpha_2^L \psi \tau_2 + R} \\ \frac{\partial V_2^H / V_2^L}{\partial \tau_1} &= 0 \end{aligned}$$

Since τ_1 never shows up in the equations that determine the value of π , the value of π does not change based on the value of τ_1 . A special case of this is where $\tau_1 = \tau_2 = \tau$ (the version of the model in Brollo et al. (2013)). In this case,

$$\frac{V_2^H}{V_2^L} = \frac{\alpha_2^H \psi \tau + R}{\alpha_2^L \psi \tau + R} \quad (21)$$

And

$$\frac{\partial V_2^H/V_2^L}{\partial \tau} = \frac{\psi R}{(V_2^L)^2}(\alpha_2^H - \alpha_2^L) < 0 \quad (22)$$

Which means that $\frac{\partial \pi}{\partial \tau} > 0$, the proportion of low quality candidates is increasing in constant budget, τ .

C CGU Randomized Audits

Given the municipal revenue structure and record of corruption, the Brazilian government regularly holds randomized audits of municipalities. This began in 2003 with the restructuring of the Office of the Comptroller General (CGU). As part of his anti-corruption banner, President Lula da Silva expanded the scope of the CGU, and explicitly mandated its formal role as a transparency and anti-corruption agency (Odilla and Rodriguez-Olivari 2021). The CGU was given both a high degree of political autonomy, and sufficient resources to carry out its mandate (Bersch, Praça, and Taylor 2017). One of its first and most prominent anti-corruption initiatives was the Programa de Fiscalização por Sorteios Públicos (PFSP), a program of randomized audits (Ferraz and Finan 2008). The program ran from 2003-2015, auditing over 4,000 municipalities. In August 2015, the CGU replaced the PSFP with the Programa de Fiscalização em Entes Federativos (FEF). The new program still audits municipalities and reports on corruption, but with different selection criteria for municipalities (CGU 2024).

During the PFSP, the GCU would regularly and randomly select between 26 and 60 municipalities for audits. The CGU sampled with replacement, and stratified geographically by State (Zamboni and Litschig 2018). Cities with up to 500,000 citizens were eligible for selection, excluding state capitals (Zamboni and Litschig 2018). Once selected, the CGU would gather a list of all federal transfers from the previous several years to create a list of inspection orders. Each order detailed audit tasks for specific government project, like the purchase of school lunches, or construction of roads, and included details on financial management, contract procurement, wages, and the quality of services (Brollo et al. 2013; Gonzales 2021). After issuing the inspection orders, a team of 10-15 auditors would then spend around 2 weeks conducting the audits, and documenting all instances of mismanagement and corruption (Zamboni and Litschig 2018). The auditors themselves are not compensated extra for finding irregularities, are paid a competitive wage, and work closely in teams, all contributing to their independence (Avis, Ferraz, and Finan 2018).

The CGU publicly releases the audit results, and forwards each report to several government agencies, federal prosecutors, the municipality, and the media. Many of these have led to high-profile prosecutions and impeachments of mayors. For example, the councilors in Caracaraí voted to remove Enildo Júnior from office after he was found guilty of over-invoicing contractors meant to install vegetable gardens in public schools (E. Costa 2015). Between 2004 and 2012, Avis, Ferraz, and Finan (2018) document hundreds of police crackdowns and convictions of mayors stemming from the audit reports.

D Disaster Declarations

Municipal level disaster relief is contingent upon official disaster declarations. When a disaster strikes, local level officials are responsible for providing details on the scale of damages, and declaring a state of emergency. If the federal or state government recognizes the declaration, then the municipality becomes eligible for disaster relief. The relief itself comes mostly in the form of emergency services²¹ and transfers to the municipality²² (Marchezini et al. 2020; Cooperman 2022). This general process for municipal disaster relief has been in place since Decree-Law No. 950 established the Special Fund for Public Disasters (FUNCAP) in 1969 (C. E. Kuhn et al. 2022). A number of laws and policies have reshaped the natural disaster response framework, notably Law No. 12.608, 10 April 2012, but the general process has remained the same. Additionally, while FUNCAP exists in name, the government has never drawn on it for federal disaster relief. Instead, money comes from a number of different agencies, with the Civil Defense agency taking a lead role in the coordination.

The mayor's office can declare one of two types of emergencies: an emergency situation (SE), or a state of public calamity (CP). The type of declaration depends on the severity of the disaster, which is summarized in Table 1. In either case, once the state or federal government approves the declaration, relief transfers become available for the duration of the disaster, typically set to 180 days (C. E. Kuhn et al. 2022). Throughout this period, mayors can continue to request additional transfers based on the needs of the municipality. Importantly, the amount of relief money depends on the details of the disaster, which the mayors also report. Mayors submit details of disasters through the Disaster Information Form (FIDE). The FIDE captures existing municipal finances, and details on human, environmental, and economic losses from the disaster (C. E. S. Kuhn et al. 2023). Unlike typical federal transfers, there are very few spending restrictions with federal disaster relief (Cooperman 2022).

21. For example, the federal government will often send in water trucks during droughts

22. In 2017, the federal government began to release funds immediately after approving declarations through the Civil Defense Payment Card (CPDC)

Level	Description	Classification
Level 1: Disaster of low intensity	<ul style="list-style-type: none"> • Considerable human damage • The situation of normality can be reestablished with state and federal resources 	Situation of Emergency
Level 2: Disaster of medium intensity	<ul style="list-style-type: none"> • In addition to (1) • Characterized by the occurrence of at least two types of damages, one of them being, mandatorily, human damage that entails public or private economic losses that affect the public administration's capacity to respond and manage the crisis. 	Situation of Emergency
Level 3: Disaster of high intensity	<ul style="list-style-type: none"> • In addition to (1, 2) • Characterized by the simultaneous occurrence of deaths, population isolation, interruption of essential services, interdiction or destruction of housing units, damage or destruction of public facilities providing essential services and public infrastructure works. 	State of Public Calamity

Table 1: Disaster Levels and Classifications

Adapted from (Ramos et al. 2020)

E Flood Measure Details

The following are descriptions for the 3 criteria used to identify floods in Brazilian municipalities from 1996-2021. I code a municipality as flooded if meets at least one of these criteria: 1) its SPI value exceeds 2 in a given year, 2) a streamflow gauge indicates flow levels at or above the historic 99th percentile for a given gauge in a given year, or 3) the municipality falls within the boundary of a known and mapped flood.

Extreme Precipitation (SPI > 2): First, I consider a municipality flooded if its maximum Standardized Precipitation Index (SPI) value exceeds 2 during the year (indicating extreme rainfall and possible flash-flood conditions). SPI measures precipitation deviations from a historic, standardized norms.²³ McKee, Doesken, and Kleist (1993) first proposed the SPI as a way to detect droughts, but others have explained its utility as a flood detection tool (Seiler, Hayes, and Bressan 2002; Olanrewaju and Reddy 2022). I calculate municipal SPI using monthly rainfall data from the Climate Hazards Group InfraRed Precipitation with Station data (CHIRPS) dataset, which provides global rainfall estimates in 5x5 km square grids (Funk et al. 2015). CHIRPS is particularly useful, as not all municipalities have rainfall gauges, and has been validated as a strong predictor of true precipitation in Brazil (Paredes-Trejo, Barbosa, and Lakshmi Kumar 2017).

Extreme Streamflow (>99th Percentile): Second, I consider municipalities flooded whenever streamflow gauges within their boundaries exceed the historic 99th percentile of daily flow. Both Lun et al. (2020) and Zhang et al. (2021) explain that this threshold is highly predictive of floods. To do this, I gather daily data on over 3,000 streamflow gauges across Brazil from 1980 to 2021. The data come from the Catchment Attributes and Meteorology for Large-sample Studies - Brazil (CAMELS-BR) dataset (Chagas et al. 2020). Figure 8 displays the location of each gauge.

Satellite Imagery (Flood-Affected Boundaries): Finally, I consider a municipality flooded if it falls within boundaries of known floods. The Dartmouth Flood Observatory (DFO) uses news reports and satellite imagery to map flood-affected areas (Brakenridge 2024). I use these images to intersect municipality and flood-extent boundaries. However, given limited coverage in Brazil, I rely on this measure as a complement to the other two.

23. To calculate the SPI, researchers gather monthly precipitation, preferably at least 30 years worth of historical data. The data are first fit to a Gamma function, and then transformed into a standard normal distribution with mean 0 (McKee, Doesken, and Kleist 1993). Thus positive values above 2 are considered extreme precipitation events.



Figure 8: Streamflow Gauge Locations

F Other Data Sources & Descriptions

F.1 Flood Declarations

All data on natural disasters in Brazil are housed in the Integrated Disaster Information System (S2ID). The S2ID platform is Brazil's hub for disaster management. Mayors submit FIDE forms and official declarations through the S2ID, and the federal and state governments use it to streamline the relief approval process (Marchezini et al. 2020). In addition to making S2ID forms publicly available, the Civil Defense, World Bank, and Federal University of Santa Catarina recently published an online atlas of all declarations from 1991 to 2023 (Brasil 2023).

F.2 Municipality Covariates

Additional municipality characteristics, including population, GDP, and sanitation, come from the Brazilian Institute of Geography and Statistics (IBGE) and the Ministry of Health (MoH). The IBGE and MoH regularly collect and report geographic, economic, and health data for various administrative divisions in Brazil. Fernandes Santos Alves et al. (2022) compile this data into a comprehensive dataset for researchers, from which I gather yearly GDP, population, and sanitation levels. Sanitation measures the proportion of individuals with inadequate water supply and sanitation, serving as a proxy for disaster vulnerability.²⁴

F.3 Municipal Revenue

Data on municipal revenue comes from the National Treasury of Brazil (Tesouro Nacional, TN). The TN maintains information on federal transfers to municipalities through its Public Sector Accounting and Fiscal Information System Project (SICONFI 2024). Disaster relief transfers are distributed across various federal programs and agencies, making it difficult to measure the full extent of relief (Cooperman 2022). However, the relief itself is included in aggregated measures of municipal revenue. As such, I gather data on municipalities' total yearly revenue and federal transfers from 1998 to 2021. I use a measure of revenue to most closely evaluate the changes in municipal resources following disasters. Notably, federal transfers account for on average 88% of total municipal revenue.

F.4 LAPOP Outcomes

LAPOP is a part of Vanderbilt University's Center for Global Democracy, and regularly surveys countries throughout the Americas using a common questionnaire core (LAPOP 2024). The survey data cover Brazil, and are publicly available on the AmericasBarometer website. I collect data on the following outcomes from 2004 to 2021:

1. cp7: Attended a meeting of a parents association
2. cp8: Attended a community improvement meeting

²⁴. I gather data on vulnerability to improve the strength of my empirical design, which uses imputation methods.

3. cp13: Attended a political party or organization meeting
4. pol1: Interest in politics
5. gi0n: Frequency of watching the news
6. prot3: Participated in a protest
7. vb20: Plan to vote in the next election

F.5 Corruption

My measure of corruption is based on data from the CGU's municipal audits. While all audit reports are publicly available online, the CGU maintains a database of results covering the 20th through the 40th lottery rounds (CGU 2024, 2006–2015,). Staff code three general types of irregularities: 1) acts of mismanagement (Falha Formal), 2) acts of moderate corruption (Falha Média), and 3) acts of severe corruption (Falha Grave). Table 2 outlines the specific actions included in each category. Following (Avis, Ferraz, and Finan 2018), I include both moderate and severe irregularities in my measure of corruption, as the distinction between the two is not always clear, and both involve activities that classify as "rent-seeking." This approach closely aligns with Brollo et al. (2013)'s version of "narrow corruption."

Table 2: CGU Coding Rules

Category	Description
Severe Corruption (Falha Grave)	Omission of the duty to render accounts (including withholding information necessary for the actions of Internal Control); Damage to the treasury due to illegitimate or uneconomical management acts; Embezzlement or misappropriation of public funds, assets, or values; Illegal, illegitimate, or uneconomical management acts or violations of legal or regulatory norms that have the potential to cause losses to the treasury or represent a severe deviation from the principles governing Public Administration.
Moderate Corruption (Falha Média)	Illegal, illegitimate, or uneconomical management acts or violations of legal or regulatory norms that do not fall under the definitions of "Falha Grave."
Acts of Mismanagement (Falha Formal)	Formal failures that do not result in damage to the treasury but have the potential to lead to the nonobservance of public administration principles or violations of legal and regulatory norms, such as deficiencies in internal management controls, violations of clauses, abuse, recklessness, or incompetence.

Using this database, I construct a panel dataset of municipality-year observations. Each year includes an aggregate measure of corruption based on the coded findings of audited inspection

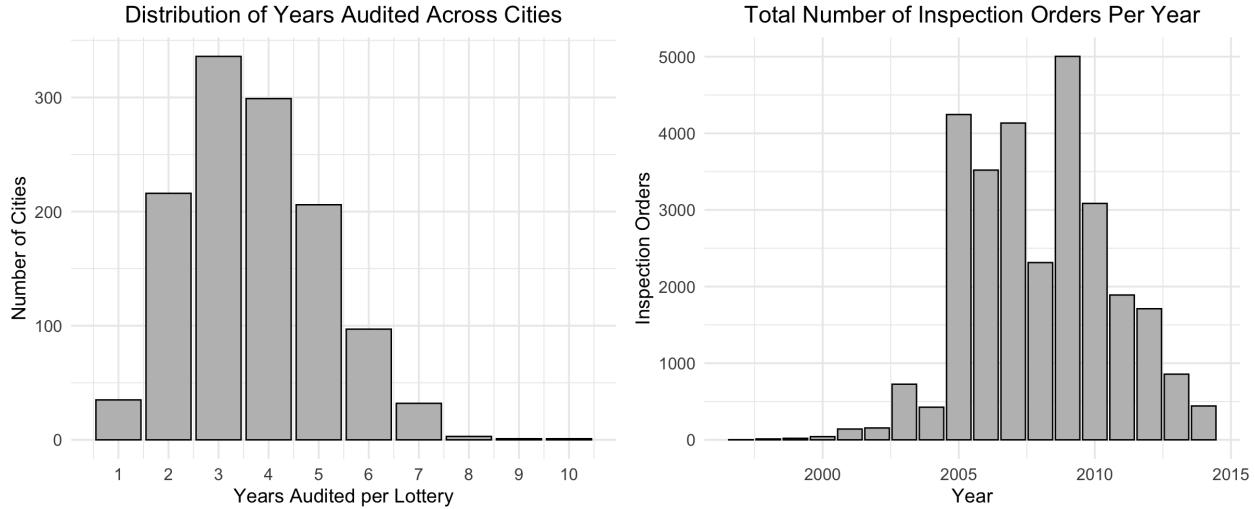


Figure 9: Years Audited and Inspection Orders

orders. For example, Macapá was audited in 2006 during the 20th lottery, covering inspection orders for the previous six years (2000–2005). Consequently, Macapá appears in the dataset for the years 2000 to 2005, and has a measure of corruption in each year.

I use two measures of corruption: 1) the number of irregularities per service order in a given year and 2) the percentage of service orders in a year with at least one irregularity.²⁵ The resulting panel includes 1,134 municipalities across 15 years. Figure 9 presents the distribution of inspection orders per year and the range of years covered by each audit. While most audits span 3–4 years, some cover as few as 1 year and others as many as 10. Figure 4 provides a map of the municipalities included in the dataset. On average, the number of irregularities per inspection order is 2.36, and 70.2% of orders have either a moderate or severe finding.

F.6 Candidate Quality

Data on Brazilian candidates comes from the *Tribunal Superior Eleitoral* (TSE). The TSE keeps detailed, publicly available records on all individuals running for mayor. I obtain the data from *Bancos dos Dados*, a non-profit research organization dedicated to compiling and distributing data from the Brazilian government (Dahis et al. 2022). I use this to compile a list of mayoral candidates from 1996 to 2020 across all 5,570 municipalities.²⁶

25. To clarify, only severe and moderate acts of corruption count as irregularities here.

26. I exclude incumbents running for re-election in the aggregation of candidate characteristics. To do this, I match candidate names and unique ID numbers from previous election results with candidates running for office.

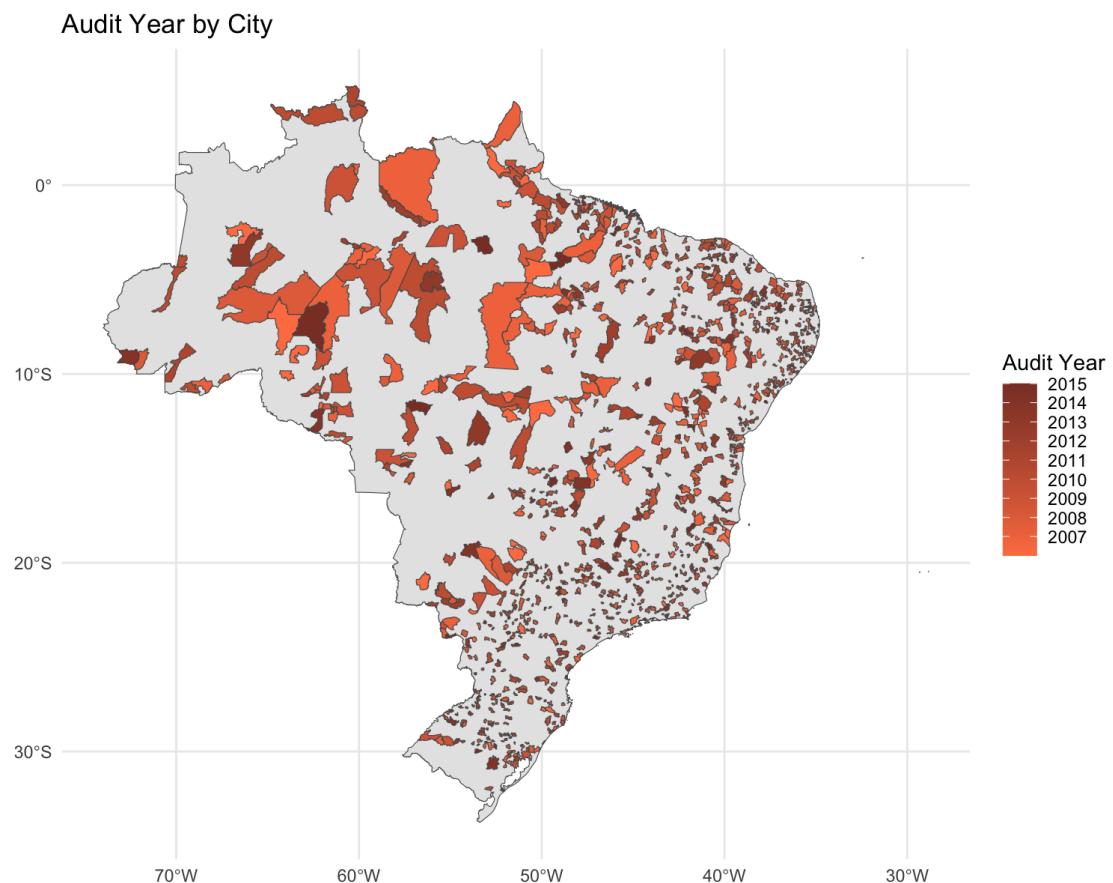


Figure 10: Municipalities Audited between 2006 and 2015

G Treatment Timing & Missing Data

Appendix G provides a visualization of both the treatment timing and missing-value status of each observation across different panel datasets used for analysis.

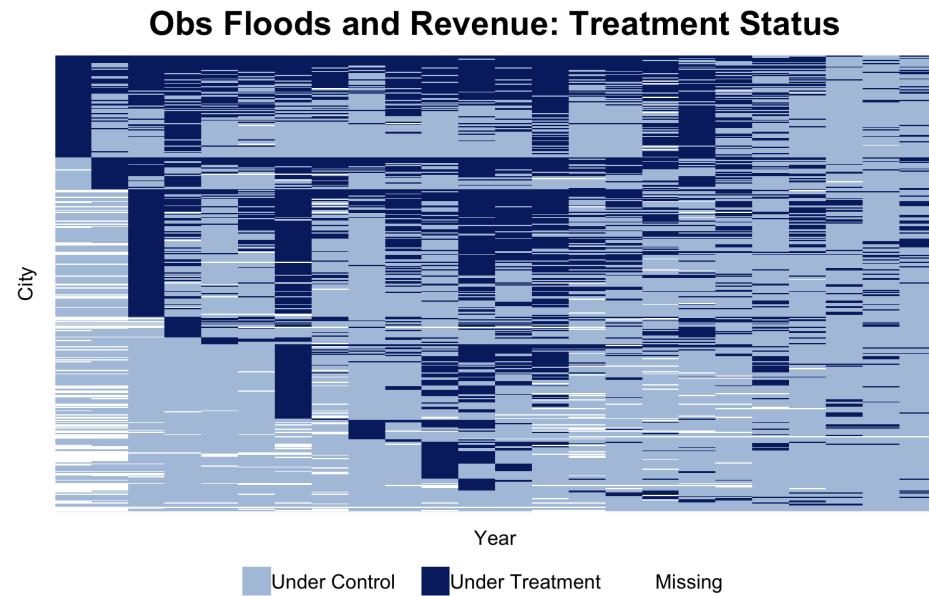


Figure 11: Treatment Status: Observed Floods & Transfer Revenue

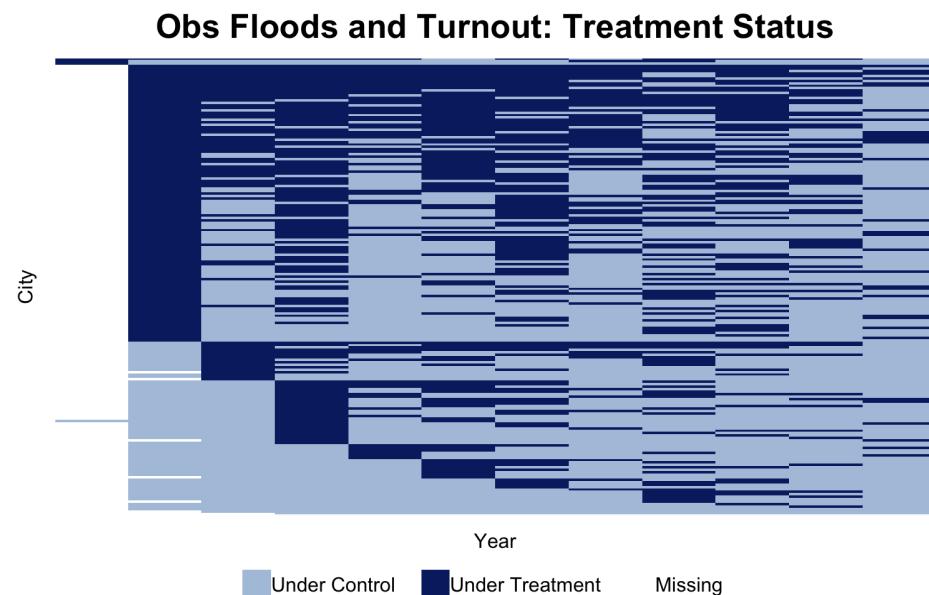


Figure 12: Treatment Status: Observed Floods & Voter Turnout

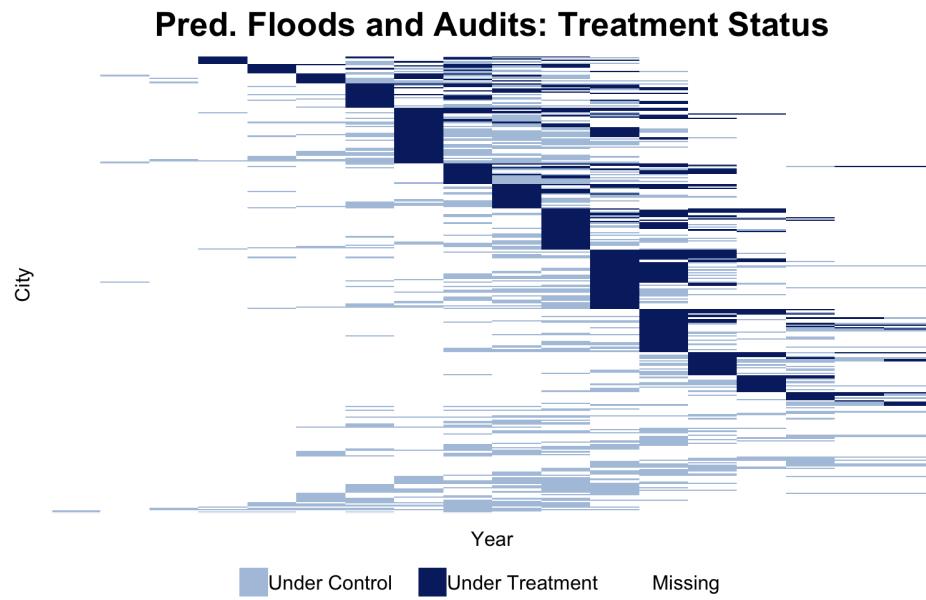


Figure 13: Treatment Status: Observed Floods & Corruption

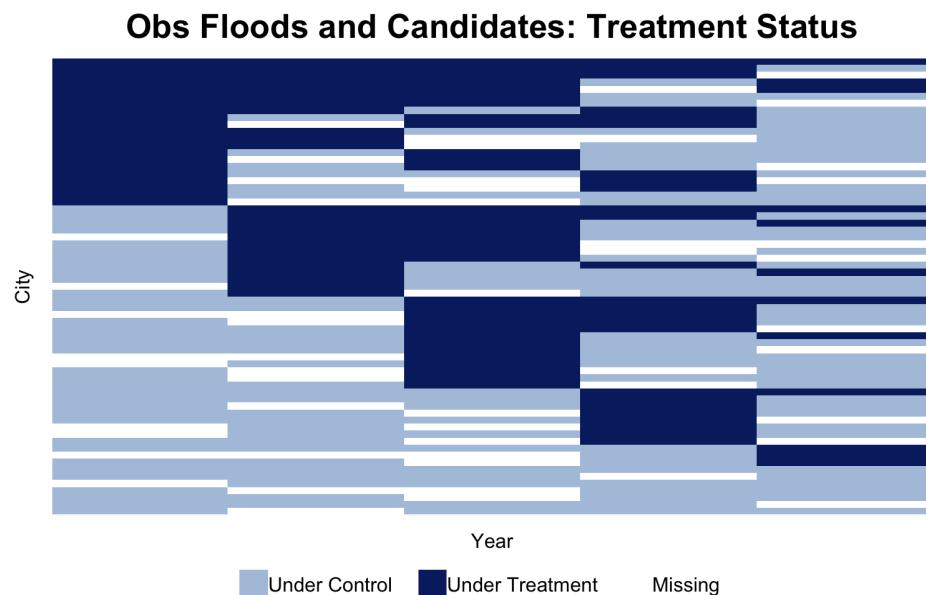


Figure 14: Treatment Status: Observed Floods & Candidate Education

H Two-Way Fixed Effects Estimation

This section provides results from the two-way fixed effects estimation for each set of results. The tables include results for the main flood measure, as well as those with flood declarations as the treatment. The models generally take the following form:

$$Outcome_{it} = \alpha_i + \gamma_t + \delta Flood_{it} + \beta X + \epsilon_{i,t} \quad (23)$$

where $Outcome_{it}$ represents the outcome of interest in municipality i in year t , α_i and γ_t are municipality and year fixed effects, $Flood_{it}$ is an indicator for a flood in municipality i in year t , and X_{it} is a vector of covariates. The covariates include lagged Standardized Precipitation Index (a continuous measure of potential flood exposure), lagged and log transformed municipal GDP, lagged and log transformed population, lagged municipal sanitation (a measure of vulnerability), and party alignment between the mayor and president.²⁷ The parameter of interest, δ , captures the treatment effect, where a positive and significant value indicates an increase in the outcome of interest.

27. Previous research finds that party alignment can influence the distribution of federal resources, which may influence rent-seeking (Brollo and Nannicini 2012; Slough, Urpelainen, and Yang 2017; Bueno 2018; Ferreira, Alves, and Caldeira 2021; Cooperman 2022).

H.1 Revenue

Table 3: Municipal Transfer Revenue

Dependent Variable:	log(Transfer Revenue)			
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Obs Flood	0.0074*** (0.0014)		0.0011 (0.0013)	
Dec Flood		0.0065*** (0.0020)		0.0041*** (0.0016)
SPI _{t-1}			0.0042*** (0.0008)	0.0042*** (0.0008)
log(Pop) _{t-1}			0.3151*** (0.0143)	0.3150*** (0.0143)
log(GDP) _{t-1}			0.1956*** (0.0095)	0.1956*** (0.0095)
Party Alignment			0.0015 (0.0026)	0.0014 (0.0026)
Sanitation _{t-1}			0.0001 (0.0002)	0.0001 (0.0002)
<i>Fixed-effects</i>				
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	128,045	128,045	113,666	113,666
R ²	0.97842	0.97841	0.98383	0.98383
Within R ²	0.00026	0.00011	0.11563	0.11568

Clustered (City) standard-errors in parentheses

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

H.2 Monitoring

Voter Turnout

Table 4: Voter Turnout

Dependent Variable:	Voter Turnout (Percent of Eligible Voters)			
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Obs Flood	-0.0013** (0.0005)		-0.0017*** (0.0005)	
Dec Flood		-0.0072*** (0.0007)		-0.0067*** (0.0007)
SPI _{t-1}			0.0044*** (0.0003)	0.0043*** (0.0003)
log(Pop) _{t-1}			-0.0433*** (0.0029)	-0.0430*** (0.0029)
log(GDP) _{t-1}			0.0153*** (0.0012)	0.0150*** (0.0012)
Sanitation _{t-1}			-0.0001*** (3.36 × 10 ⁻⁵)	-0.0001*** (3.33 × 10 ⁻⁵)
<i>Fixed-effects</i>				
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	33,420	33,420	33,309	33,309
R ²	0.71749	0.71844	0.72655	0.72733
Within R ²	0.00022	0.00356	0.03060	0.03337

Clustered (City) standard-errors in parentheses

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

Non-Electoral Political Outcomes

Table 5: LAPOP: Non-Electoral Political Participation

Dependent Variables: Model:	Parent Meeting (1)	Community Board Meeting (2)	Political Meeting (3)	Political Meeting (4)	Protest (5)	Protest (6)	Protest (7)	Protest (8)
<i>Variables</i>								
Obs Flood	0.0698** (0.0302)	-0.0050 (0.0338)	0.0363 (0.0249)	-0.0151 (0.0254)	0.0673*** (0.0247)	0.0308 (0.0245)	0.0169* (0.0100)	0.0055 (0.0084)
SPI _{t-1}	0.0420** (0.0192)	0.0185 (0.0209)	0.0002 (0.0135)	-0.0009 (0.0134)	0.0204 (0.0160)	0.0240 (0.0161)	0.0073 (0.0058)	0.0073 (0.0064)
log(Pop) _{t-1}	0.0387 (0.0445)	1.006** (0.4874)	-0.0634 (0.0419)	1.167*** (0.3114)	0.0571** (0.0267)	-0.1274 (0.3814)	0.0220** (0.0110)	-0.2472** (0.1070)
log(GDP) _{t-1}	-0.0335 (0.0408)	-0.0455 (0.1382)	0.0510 (0.0390)	0.2091** (0.0900)	-0.0555** (0.0236)	-0.0054 (0.0726)	-0.0222** (0.0091)	0.0097 (0.0389)
Sanitation _{t-1}	-0.0053*** (0.0019)	-0.0026 (0.0023)	-0.0033*** (0.0017)	-0.0033*** (0.0014)	-0.0033*** (0.0004)	-0.0008** (0.0017***)	0.0018*** (0.0002)	0.0018*** (0.0002)
Age	0.0064*** (0.0006)	0.0065*** (0.0007)	-0.0032*** (0.0006)	-0.0032*** (0.0006)	0.0011* (0.0005)	0.0010* (0.0005)	0.0017*** (0.0002)	0.0018*** (0.0002)
Constant	2.986*** (0.1465)	3.807*** (0.1616)	3.807*** (0.1616)	3.823*** (0.1013)	3.823*** (0.1013)	3.823*** (0.1013)	1.908*** (0.0338)	1.908*** (0.0338)
<i>Fixed-effects</i>								
City	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>								
Observations	5,918	5,918	5,929	5,929	5,922	5,922	5,953	5,953
R ²	0.01747	0.06829	0.01051	0.08361	0.00587	0.06549	0.01096	0.04772
Within R ²		0.01376		0.00943		0.00207		0.01014

Clustered (City) standard-errors in parentheses

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

Table 6: LAPOP: Political Interest

Dependent Variables: Model:	Interest in Politics		Attention to News		Intention to Vote	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Variables</i>						
Obs Flood	0.0027 (0.0328)	-0.0081 (0.0369)	0.0137 (0.0582)		-0.1036* (0.0545)	-0.0413 (0.0408)
SPI _{t-1}	-0.0072 (0.0204)	-0.0060 (0.0210)	0.0282 (0.0409)		-0.0606** (0.0265)	-0.0410* (0.0218)
log(Pop) _{t-1}	0.0683 (0.0444)	-0.8232 (0.6469)	-0.0207 (0.0809)		-0.1640*** (0.0510)	-0.3148 (0.6192)
log(GDP) _{t-1}	-0.0586 (0.0382)	-0.2347* (0.1310)	-0.0138 (0.0715)		0.1647*** (0.0438)	0.2976* (0.1602)
Sanitation _{t-1}	-0.0034* (0.0020)		0.0005 (0.0039)		0.0075*** (0.0020)	
Age	0.0015* (0.0009)	0.0017* (0.0009)	-0.0036** (0.0016)	-0.0036** (0.0016)	-0.0074*** (0.0010)	-0.0079*** (0.0010)
Constant	2.840*** (0.1329)		1.812*** (0.2448)		2.713*** (0.1547)	
<i>Fixed-effects</i>						
City	No	Yes	No	Yes	No	Yes
Year	No	Yes	No	Yes	No	Yes
<i>Fit statistics</i>						
Observations	5,185	5,185	1,481	1,481	5,407	5,407
R ²	0.00558	0.04719	0.02246	0.09948	0.02323	0.12308
Within R ²		0.00536		0.01525		0.01894

Clustered (City) standard-errors in parentheses

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

H.3 Corruption

Table 7: Floods and Corruption Audits

Dependent Variable: Model:	Corruption			
	(1)	(2)	(3)	(4)
<i>Variables</i>				
Obs Flood	0.1679** (0.0757)		0.1466** (0.0739)	
Dec Flood		0.1256 (0.1072)		0.1369 (0.1085)
SPI _{t-1}			0.0613 (0.0549)	0.0521 (0.0547)
log(Pop) _{t-1}			1.253** (0.4978)	1.317*** (0.4998)
log(GDP) _{t-1}			-0.4620* (0.2375)	-0.4801** (0.2394)
Party Alignment			-0.1468 (0.1544)	-0.1453 (0.1556)
Sanitation _{t-1}			-0.0066 (0.0113)	-0.0069 (0.0113)
<i>Fixed-effects</i>				
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	4,550	4,550	4,500	4,500
R ²	0.50772	0.50722	0.51104	0.51077
Within R ²	0.00157	0.00054	0.00451	0.00397

Clustered (City) standard-errors in parentheses

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

Mean level of corruption in the sample: 2.36 irregularities per service order

H.4 Candidate Quality

Education Results

Table 8: Percent of Candidates with High School Diploma

Dependent Variable: Model:	Percent High School Grad			
	(1)	(2)	(3)	(4)
<i>Variables</i>				
Obs Flood _{t-1}	0.0098** (0.0045)		0.0095** (0.0045)	
Dec Flood _{t-1}		-0.0079 (0.0064)		-0.0084 (0.0064)
SPI _{t-2}			0.0023 (0.0029)	0.0030 (0.0029)
log(Pop) _{t-2}			-0.0437** (0.0222)	-0.0434* (0.0222)
log(GDP) _{t-2}			0.0108 (0.0100)	0.0112 (0.0100)
Sanitation _{t-2}			-0.0002 (0.0004)	-0.0002 (0.0004)
<i>Fixed-effects</i>				
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	27,053	27,053	27,043	27,043
R ²	0.36902	0.36893	0.36923	0.36915
Within R ²	0.00021	6.77 × 10 ⁻⁵	0.00051	0.00039

Clustered (City) standard-errors in parentheses

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

Table 9: Percent of Candidates with College Degree

Dependent Variable: Model:	Percent College Grad			
	(1)	(2)	(3)	(4)
<i>Variables</i>				
Obs Flood _{t-1}	0.0122** (0.0056)		0.0119** (0.0056)	
Dec Flood _{t-1}		0.0056 (0.0077)		0.0051 (0.0077)
SPI _{t-2}			0.0010 (0.0034)	0.0012 (0.0034)
log(Pop) _{t-2}			-0.0135 (0.0241)	-0.0140 (0.0242)
log(GDP) _{t-2}			0.0243** (0.0116)	0.0244** (0.0116)
Sanitation _{t-2}			0.0003 (0.0004)	0.0003 (0.0004)
<i>Fixed-effects</i>				
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	27,053	27,053	27,043	27,043
R ²	0.39271	0.39259	0.39289	0.39277
Within R ²	0.00023	2.43 × 10 ⁻⁵	0.00046	0.00026

Clustered (City) standard-errors in parentheses

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

Occupation Results

Table 10: Percent of Candidates with White Collar Jobs

Dependent Variable: Model:	Percent White Collar			
	(1)	(2)	(3)	(4)
<i>Variables</i>				
Obs Flood _{t-1}	0.0117** (0.0047)		0.0119** (0.0047)	
Dec Flood _{t-1}		0.00006 (0.0062)		0.0005 (0.0062)
SPI _{t-2}			-0.0041 (0.0029)	-0.0037 (0.0029)
log(Pop) _{t-2}			0.0032 (0.0225)	0.0030 (0.0225)
log(GDP) _{t-2}			0.0112 (0.0102)	0.0115 (0.0102)
Sanitation _{t-2}			0.0002 (0.0004)	0.0002 (0.0004)
<i>Fixed-effects</i>				
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	26,608	26,608	26,598	26,598
R ²	0.36018	0.36000	0.36026	0.36007
Within R ²	0.00029	3.74 × 10 ⁻⁹	0.00046	0.00016

Clustered (City) standard-errors in parentheses

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

I Additional Corruption Results

Results after changing the outcome to the probability of corruption (the number of service orders with any corrupt findings divided by the total number of orders in a given year). Model 3 demonstrates a 2.35% percentage point increase in the probability of corruption. This is not significant at $\alpha = 0.05$. The mean probability of corruption in the sample is approximately 70%, which is quite high. Thus, a 2.35 percentage point increase does not really induce a substantive increase in visible corruption, but does nonetheless suggest more rent-seeking behavior. The effect size actually matches the logic in the model relatively well. Since disaster relief only modestly increases the budget, there is not much room to grab extra rents without public notice.

Model:	Dependent Variable: Corruption			
	(1)	(2)	(3)	(4)
<i>Variables</i>				
Obs Flood	0.0241*	0.0235*		
	(0.0134)	(0.0134)		
Dec Flood		0.0207	0.0199	
		(0.0165)	(0.0168)	
SPI _{t-1}		0.0151	0.0136	
		(0.0093)	(0.0092)	
log(Pop) _{t-1}		0.1216	0.1316	
		(0.0935)	(0.0940)	
log(GDP) _{t-1}		-0.0110	-0.0139	
		(0.0426)	(0.0424)	
Party Alignment		-0.0555*	-0.0552*	
		(0.0319)	(0.0319)	
Sanitation _{t-1}		-0.0007	-0.0007	
		(0.0016)	(0.0016)	
<i>Fixed-effects</i>				
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Observations	4,550	4,550	4,500	4,500
R ²	0.42209	0.42177	0.42338	0.42308
Within R ²	0.00099	0.00045	0.00325	0.00274

Clustered (City) standard-errors in parentheses

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

Table 11: TWFE Results with Probability of Corrupt Orders

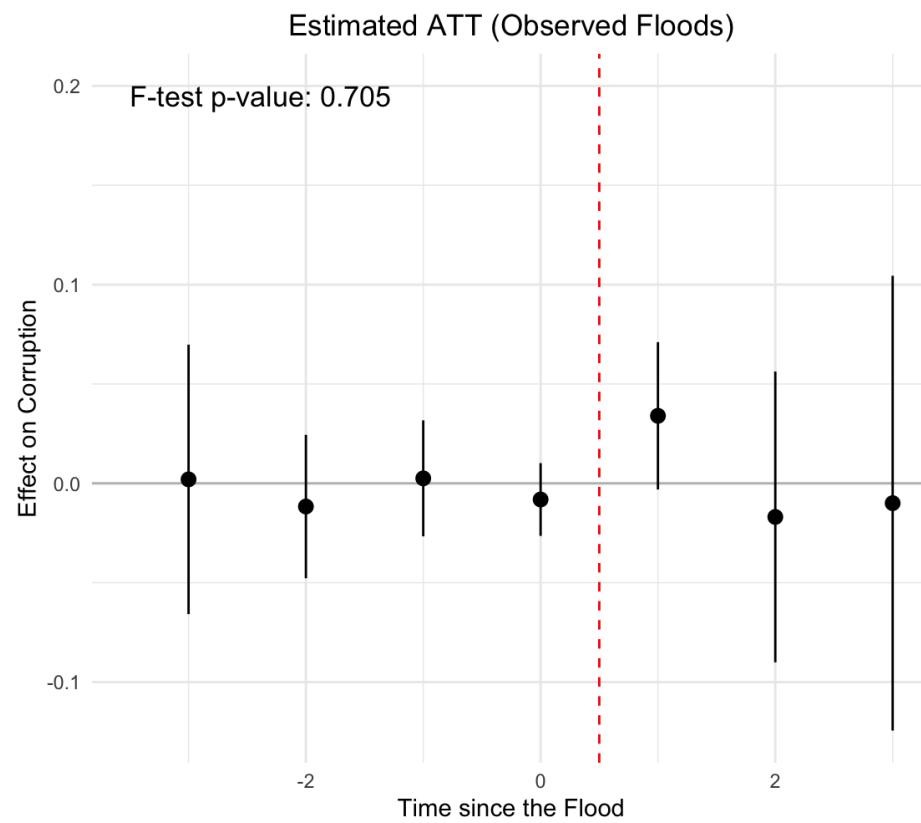


Figure 15: Probability of Corruption

J Sensitivity Analysis

Figure 18 displays sensitivity analysis for the revenue results following Rambachan and Roth (2023). The parameter \bar{M} governs the maximum deviation from parallel trends relative to the largest pre-treatment violation. At $\bar{M} = 0$ (parallel trends holds exactly), the effect is positive and statistically significant. As \bar{M} increases, confidence intervals widen; the effect remains bounded away from zero for $\bar{M} < 1$ but includes zero for larger violations.

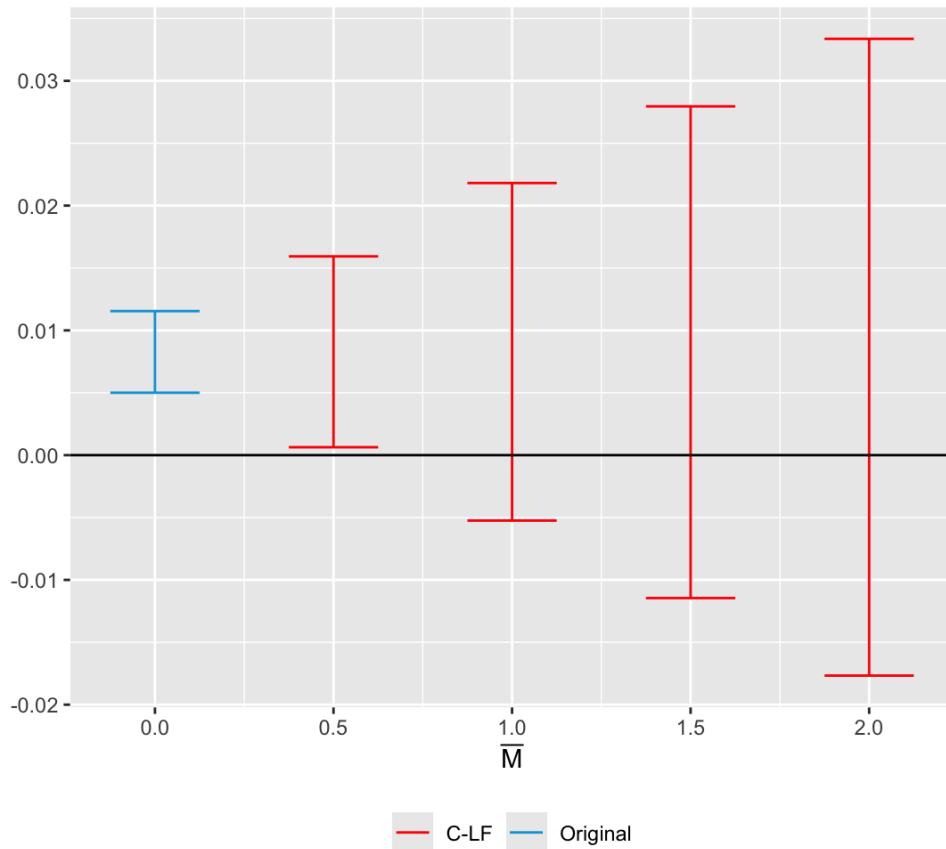


Figure 16: Sensitivity Analysis: Revenue (Rambachan and Roth 2023)

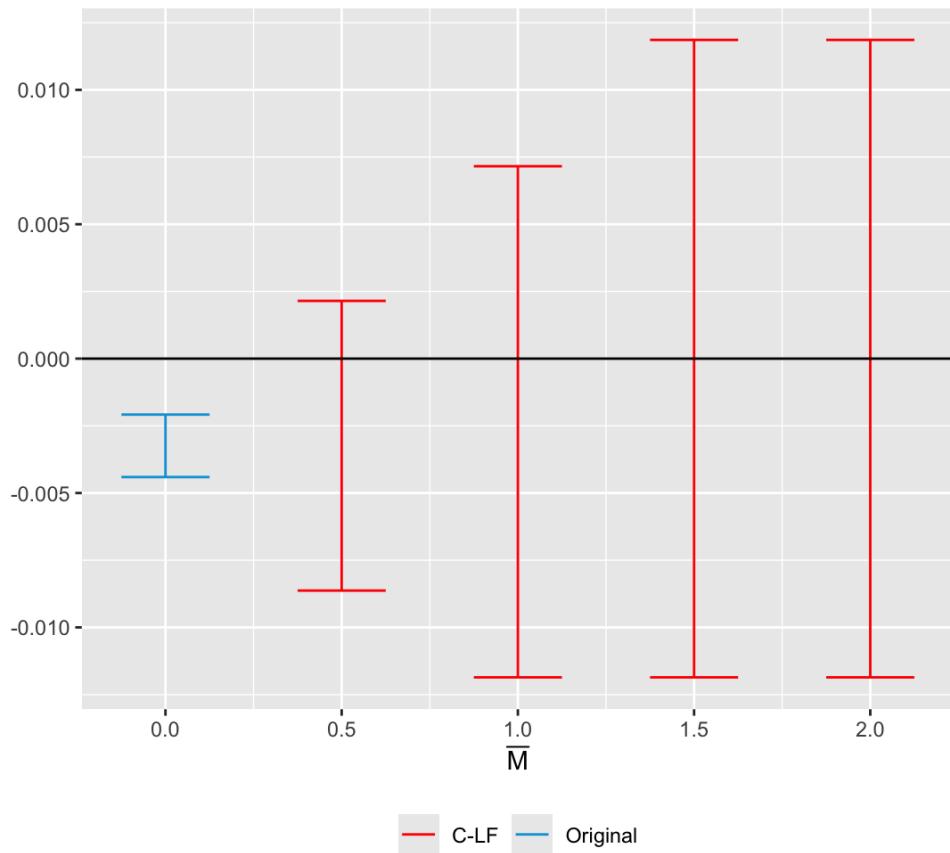


Figure 17: Sensitivity Analysis: Turnout (Rambachan and Roth 2023)

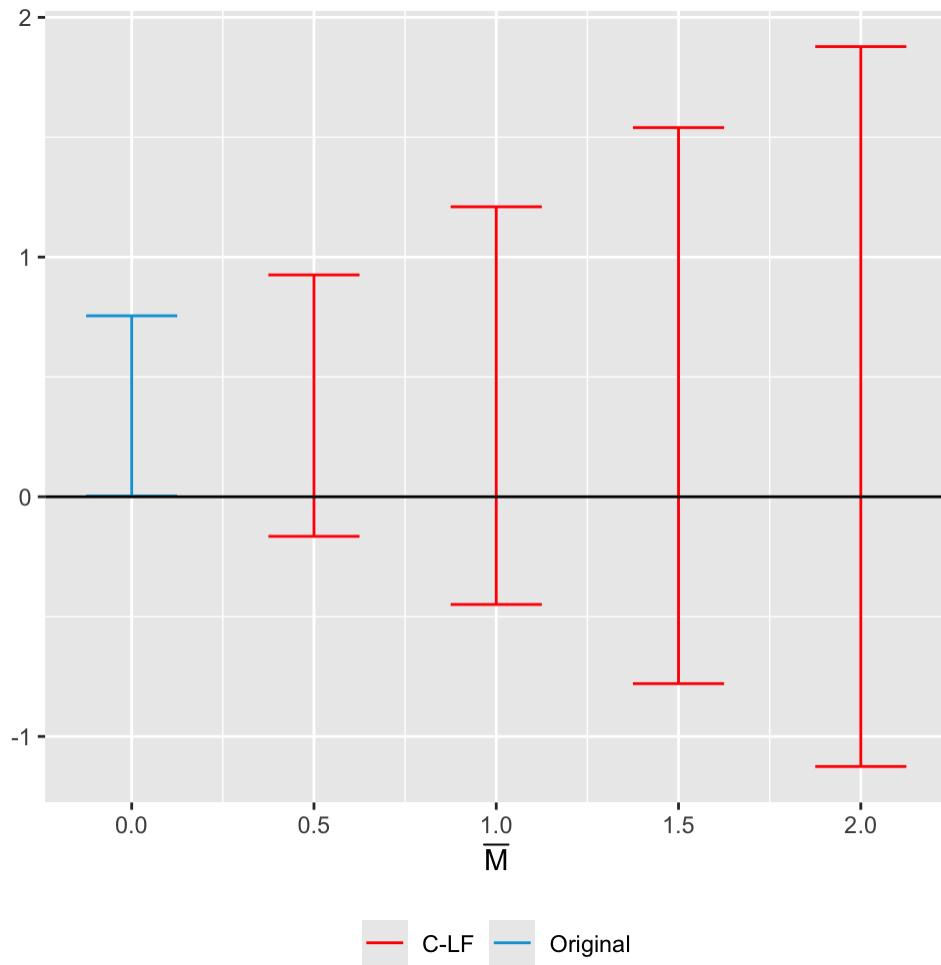


Figure 18: Sensitivity Analysis: Corruption (Rambachan and Roth 2023)

K Observed Floods & Flood Declarations

Table 12: Relationship Between Observed Floods & Declarations

Dependent Variable: Flood Declaration	
Model: (1)	
<i>Variables</i>	
Observed Flood	0.1208*** (0.0017)
Constant	0.0706*** (0.0010)
<i>Fit statistics</i>	
Observations	143,898
R ²	0.03209
Adjusted R ²	0.03208
<i>IID standard-errors in parentheses</i>	
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1	

K.1 An Example of a Known Flood

The recent May 2024 floods in Porto Alegre are captured in the objective flood measure (2024 is outside of the study, but provides a recent and identifiable example).



Figure 19: Floods in Porto Alegre: SCMP

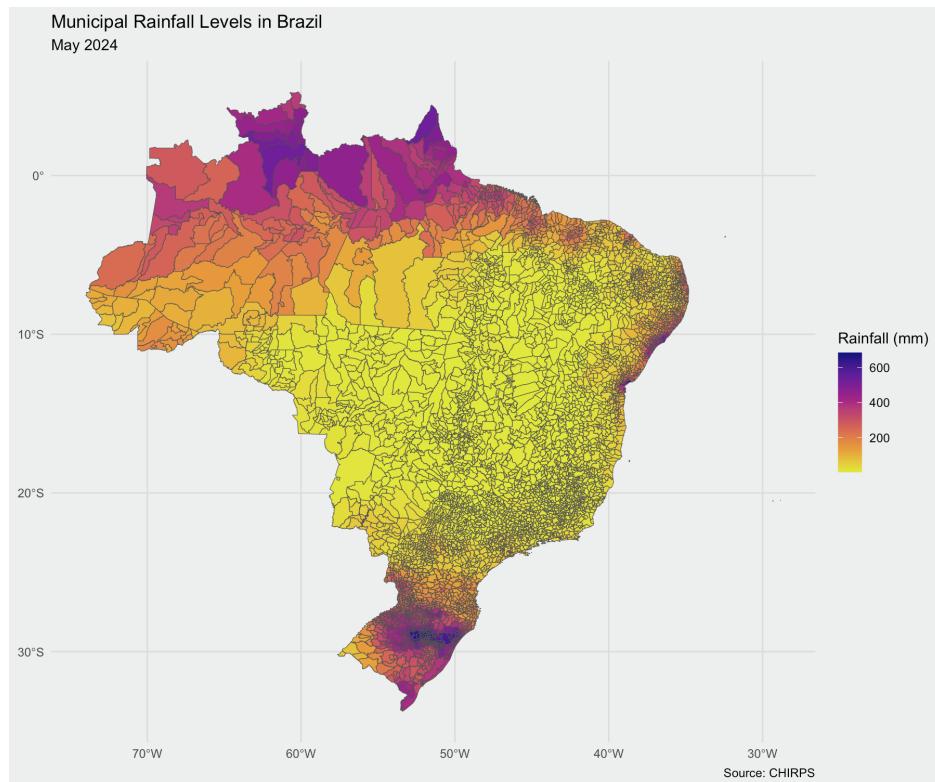


Figure 20: CHIRPS Rain Data (Porto Alegre is in the South)

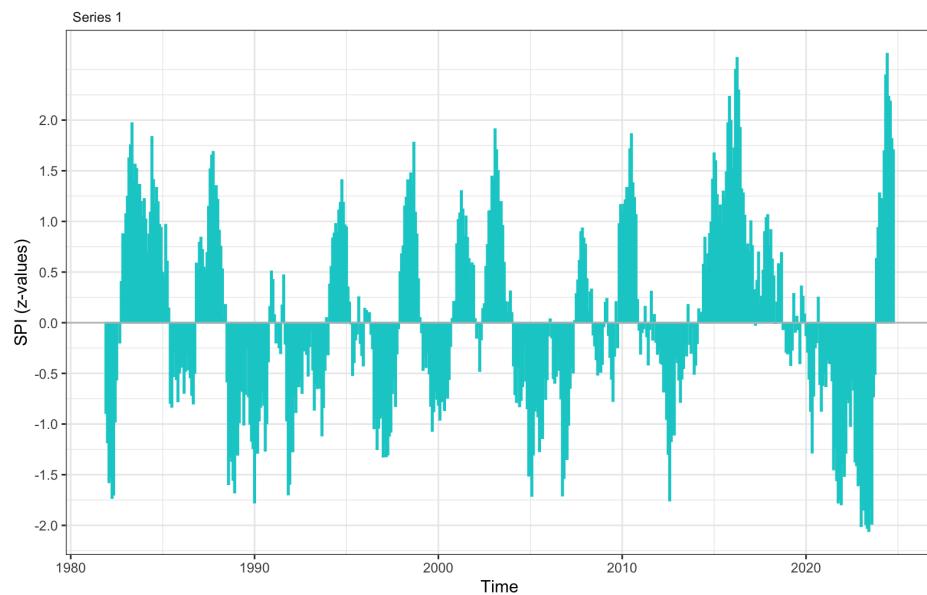


Figure 21: Porto Alegre SPI (Exceeds 2.0 in May 2024)