Revealing Nearby Corruption Drives Party Switching: Evidence from Local Level Audits in Brazil*

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Abstract

Anti-corruption interventions that seek to bridge the gap between voters and politicians' performance in office should improve electoral accountability. However, the cumulative evidence suggests that voters rarely punish corrupt politicians. Recent work explains this puzzle by suggesting that politicians anticipate electoral sanctions and adjust their behavior in office to minimize electoral backlash. Focusing on exposure to nearby corruption information, this paper argues that politicians without direct association with corruption also engage in this behavior. This is because nearby corruption leads politicians to expect increased scrutiny on their performance in office, which creates incentives to secure a better platform for reelection. Using data from a long running anti-corruption program in Brazil, I show that increasing nearby corruption encourages incumbent mayors to seek reelection under a different party, a strategy that helps in securing campaign resources. Additional analyses suggest that they do so in an attempt to counter potential electoral backlash. This means anti-corruption efforts trigger unintended strategic responses among politicians who are not the target of these interventions.

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1 Introduction

Governments, civil society organizations, and scholars in the developing world spend considerable resources implementing and evaluating information-sharing interventions that seek to bridge the gap between voters and politicians' performance in office. The expectation is that these interventions would help voters hold politicians accountable. However, the cumulative evidence suggests that citizens rarely use this information to update their vote choice (De Vries and Solaz 2017). For example, a series of coordinated trials across five different countries suggests that performance information does not affect incumbent votes, even when voters receive, understand, and believe the information (Dunning, Grossman, Humphreys, Hyde, McIntosh, Nellis, Adida, et al. 2019). In the context of corruption, a recent meta-analysis shows that the strong anti-corruption norms documented in survey self-reports does not translate to changes in the frequency with which incumbents win elections (Incerti 2020). Even in cases where exposing corrupt politicians leads to electoral sanctions (e.g. Ferraz and Finan 2008), bottom-up punishment tends to be short lived (Rundlett 2018; Timmons and Garfias 2015). If anything, politicians seem more responsive to the prospects of legal sanctions than to expected changes in voter behavior in response to corruption (Avis, Ferraz, and Finan 2018).

While most research on the electoral consequences of corruption focuses on understanding the circumstances under which voters do not sanction corruption (see De Vries and Solaz 2017 for a review), recent work turns toward politicians' strategic behavior in reaction to corruption investigations. These reactions aim to counter or prevent the negative electoral consequences that may follow from increased monitoring. Some reactions fall within the expected consequences of exposing corruption. For example, in the context of the Clean Hands scandal in Italy, parties chose not to renominate legislators investigated for corruption for reelection (Asquer, Golden, and Hamel 2019). Some reactions are unintended consequences that emerge because politicians are aware of monitoring efforts and adjust their behavior be-

fore the general public hears about them (Fisman and Golden 2017). For example, corrupt politicians in Romania respond to anti-corruption efforts by engaging in electoral manipulation more often (Klašnja and Pop-Eleches 2022).

In this paper, I argue that exposing corruption also triggers strategic responses among those not directly connected to corruption. So far, the study of the unintended consequences of monitoring corruption has focused on the strategic behavior of those connected with potential wrongdoing, be it individuals investigated for corruption or parties with members being investigated. I advance this argument by focusing on exposure to corruption information from nearby localities. When corruption is revealed in a nearby locality, incumbent politicians expect their constituencies to hear about it and to pay more attention to their performance in office. In turn, incumbents who expect increased scrutiny on their performance have incentives to seek alternatives to protect their reelection chances.

One such alternative in the context of local elections in Brazil is seeking reelection under a different party. In many countries, elected officials resort to party switching to achieve ideological, policy, and electoral goals (Desposato 2006). In a context where parties are key actors in shaping candidate selection but party labels convey little information to citizens in terms of deciding who to vote for, incumbents with increased incentives to protect their reelection chances will seek party switching more actively.

I evaluate the effect of exposure to nearby corruption on party switching using data from a long running anti-corruption program in Brazil, created using a combination of text-as-data and supervised learning tools to overcome limited data availability. The program randomly selected municipalities to audit the use of federal funds, releasing reports to the relevant authorities and the media. I show that more nearby corruption infractions increase the rate at which incumbent mayors seek reelection under a different party. The effect appears only in the subset of municipalities exposed to nearby corruption that are not audited themselves, which suggests that switching is a viable strategy to secure reelection only when voters do

not have access to their own incumbent's corruption record. [EXPLAIN WHY]

I also evaluate second order implications to provide additional evidence in favor of the re-election incentives mechanism. First, I show that effects are similar across municipalities exposed to varying proportions of same-party corruption. Considering the high number of parties and the weakness of party brands in local level elections in Brazil (Klašnja and Titiunik 2017; Novaes 2017), this result suggests that the findings in this paper are distinct from the potential of top-down sanctions. Second, the effect appears after mayors learned about the possibility of anti-corruption electoral backlash for the first time in the 2004 election. Third, the effect is more pronounced in places without access to local media, which suggests that mayors only engage in party switching when doing so will not be seen as an admission of association with corruption.

This paper makes three contributions. First, it expands on the literature on the electoral consequences of corruption by showing how anti-corruption efforts have effects beyond the immediate locales where they are implemented. This puts previous findings in perspective, as the limited evidence in favor of voter sanctioning outside of the survey framework (e.g. Boas, Hidalgo, and Melo 2018; Incerti 2020) may arise because politicians respond strategically to avoid punishment (Fisman and Golden 2017), and not necessarily because voters are not inclined to sanction corruption. In that regard, this paper extends on recent work suggesting this mechanism but not testing it directly (Asquer, Golden, and Hamel 2019; Daniele, Galletta, and Geys 2020).

Third, this paper overcomes the limitations in previous work using data from the aforementioned anti-corruption program in Brazil. Previous research relies on human coding to measure corruption in a subset of the data, without a measure of coding reliability and potentially ignoring general trends over time (Brollo et al. 2013; Cavalcanti, Daniele, and Galletta 2018; Ferraz and Finan 2008, 2011; Timmons and Garfias 2015). I overcome these difficulties using a text-as-data approach to code corruption. I use the audit report docu-

ments as a bridge between labeled and unlabeled cases, creating a measure of corruption that reproduces the official coding for the entirety of the program's duration.

2 The Effect of Exposure to Nearby Corruption

2.1 Previous Evidence on the Electoral Consequences of Corruption

Formal theoretical models of electoral accountability highlight voters' adverse selection problem. Voters prefer to have good over bad politicians in office, but they can only infer an incumbent's type from observable outputs (Barro 1973; Fearon 1999; Ferejohn 1986). Therefore, politicians have incentives to hide corrupt activities from voters (Gambetta 2002; Rose-Ackerman 1978, 1999), so voters do not have enough information to link experiences with and perceptions of corruption with those responsible for it.[1]

The literature on electoral accountability suggests that information plays a key role in minimizing voters' adverse selection problem (Adsera, Boix, and Payne 2003; Dunning, Grossman, Humphreys, Hyde, McIntosh, and Nellis 2019; Dunning, Grossman, Humphreys, Hyde, McIntosh, Nellis, Adida, et al. 2019; Tavits 2007). Both observational studies (e.g. Chang, Golden, and Hill 2010; Ferraz and Finan 2008; Welch and Hibbing 1997) and field experiments (e.g. Buntaine et al. 2018; Chong et al. 2015; Green, Zelizer, and Kirby 2018) in various contexts suggest that exposing corrupt politicians leads to electoral sanctions. However, the cumulative evidence suggests that the electoral consequences of corruption are limited (see De Vries and Solaz 2017 for a review). Recent work highlights the discrepancy between self-reported and actual political behavior in reaction to corruption. While respondents in survey experiments consistently report their inclination to sanction corrupt politicians, evidence from field experiments suggests that preferences do not translate to

[1] Corruption is generally understood as the use of office for private gain (Svensson 2005). In this paper, I focus on corruption as the misallocation or misappropriation of public resources.

votes (Boas, Hidalgo, and Melo 2018; Incerti 2020).

Current explanations for the limited electoral sanctions emphasize the circumstances under which voters choose not to punish corruption. These fall under three categories. First, voters may tolerate corruption when politicians satisfy their expectation in other areas. For example, a corrupt politician may be favored over a clean alternative if they come from a preferred political party (Anduiza, Gallego, and Muñoz 2013; Eggers 2014) or provide good economic outcomes (Fernández-Vázquez, Barberá, and Rivero 2016; Konstantinidis and Xezonakis 2013; Muñoz, Anduiza, and Gallego 2016; Pereira and Melo 2015).[2] Second, voters may ignore corruption information when the source is not credible (Botero et al. 2015; Weitz-Shapiro and Winters 2017; Winters and Weitz-Shapiro 2018). Third, in the absence of viable alternatives to replace corrupt politicians, corruption information leads voters to prioritize other elements of politicians' performance in office or to not participate at all (Boas, Hidalgo, and Melo 2018; Chong et al. 2015; Pavão 2018).

While current explanations focus on the behavior of voters, recent work shifts toward politicians' strategic behavior. Information-sharing interventions require increased monitoring of politicians' performance in office, meaning that officeholders become aware of monitoring efforts well before they uncover and disseminate any instance of corruption. This creates a gap that allows officeholders to take anticipatory measures to minimize the potential electoral backlash of being exposed as corrupt (Fisman and Golden 2017). In other words, one reason why we do not observe voters punishing corruption more often is because politicians beat citizens to the punch.

Some of these strategic reactions can be considered intended or desirable. For example, recent work on Italy's Clean Hands scandal shows how parties attempt to protect their public brand by avoiding the renomination of legislators who face extensive media coverage around corruption scandals (Asquer, Golden, and Hamel 2019). In local elections in Brazil, parties

[2]See Breitenstein (2019) and Winters and Weitz-Shapiro (2013) for counterpoints. Both pieces suggest that voters do not trade corruption for economic performance.

in municipalities where audits reveal high corruption present more educated candidates (a proxy for quality) in city council elections with the aim of countering increased public scrutiny (Cavalcanti, Daniele, and Galletta 2018). These reactions are intended or desirable in that they force politicians or parties to avoid association with those exposed as corrupt, which serves the purpose of curbing corruption.

Other reactions have unintended consequences. For example, in the context of election monitoring efforts, the presence of domestic observers creates incentives to relocate irregularities rather than eliminating them (Asunka et al. 2019; Ichino and Schündeln 2012). In Romania, corrupt politicians engage in electoral manipulation more often in retaliation to anti-corruption efforts (Klašnja and Pop-Eleches 2022). These are unintended consequences in that they allow politicians to avoid or counter accountability while still engaging in or benefiting from illicit activities.

The common theme to both intended and unintended reactions is that politicians try to avoid association with corruption when they are connected to it directly. A direct connection can be an individual politician investigated for corruption, or a political party whose members are being investigated. However, the scope of these strategic reactions may also extend to those without a direct connection to corruption. This paper focuses on exposure to nearby corruption to show that elected officials in localities close to areas in which corruption is revealed also have incentives to anticipate potential electoral backlash.

2.2 Exposure to Nearby Corruption

Proximity is one way in which revealing corruption in one locality can affect the strategic decisions of elected officials in other localities. This can happen for two reasons. First, when revealed corruption leads to legal investigation and prosecution, elected officials in neighboring localities may update their beliefs about the likelihood and severity of top-down punishment. Recent work in Brazil argues that this is the reason why mayors close to

municipalities audited in the past exhibit fewer irregularities when they are audited in the future (Avis, Ferraz, and Finan 2018).

This paper focuses on a second explanation driven by incentives to avoid bottom-up accountability. Previous work on the electoral consequences of corruption suggests that anti-corruption voting is only possible if voters believe that corruption is an important issue that needs to be addressed (De Vries and Solaz 2017). For example, Klašnja, Tucker, and Deegan-Krause (2016) argue that direct experiences with corruption are sufficient to trigger anti-corruption votes, but that receiving information about corruption in society only leads to updates in voting behavior when elites mobilize to highlight its salience.

I argue that elected officials are aware that certain events increase the salience of corruption, and that they expect the reveal of corruption in nearby localities to have this effect. This means that elected officials exposed to nearby corruption will expect increased scrutiny on their performance in office. Notice that expecting increased scrutiny is the only necessary condition. Actual increased scrutiny from nearby corruption is sufficient but not necessary. This means that politicians' strategic reactions to nearby corruption can just be reverse wishful thinking, in that they react to nearby corruption because of the possibility of electoral backlash, even if electoral backlash does not occur. As I discuss in the results, the case of the anti-corruption program in Brazil is one in which politicians experienced electoral backlash in the early stages of the program, but not in subsequent rounds, meaning that politicians may base their decision to react to nearby corruption in these early experiences.

If it is true that elected officials expect increased scrutiny from nearby corruption, then those exposed to nearby corruption will have incentives to invest in strategies to improve their reelection chances. The most obvious strategy would work to improve performance by any metric that matters to their constituency. However, in the context of local elections in Brazil, a more cost-efficient strategy is to seek reelection under a different party.

2.3 Party switching to improve reelection chances

Why would politicians switch parties in reaction to nearby corruption? Party switching is generally conceived as a strategy for politicians to maximize policy, ideology, and electoral goals (Desposato 2006). In the case of local level elections in Brazil, parties do not convey much information to citizens when it comes to choosing who to vote for. While candidates need the endorsement of a party participate in elections, campaigns tend to emphasize individual candidates and their connections to recognizable political figures (Ames 2001). To illustrate, as of the 2016 election in Brazil, the last covered in this paper, 31 distinct parties elected at least one mayor.[3]

These weak but necessary attachments between elected officials and parties create a situation in which incumbents can switch parties with relatively minimal reputation costs. In fact, previous research suggests that incumbent mayors, in average, find themselves at an electoral disadvantage by staying with the same party (Klašnja and Titiunik 2017). Research on the behavior of Brazilian legislators suggest that those who switch parties are more likely to engage in reelection-seeking strategies, such as clientelism, at the expense of programmatic platforms (Peterlevitz 2021). At the local level, incumbents can act as brokers between parties and the core supporters in their constituencies, so that a party may have incentives to bring an incumbent into the fold in exchange for support in a reelection campaign (Novaes 2017).

This means that officeholders who expect increased scrutiny as a result of exposure to nearby corruption may prefer to counter the potential electoral backlash by seeking reelection under a different party instead of improving performance in office. In a context with weak party labels, switching parties is more cost-efficient than improving performance, since the cost is shared between the incumbent and the new party, as opposed to the incumbent alone. Since party labels are weak, this comes with relatively minimal reputation cost. The exception is

[3]This is based on the party under which candidates are registered. In practice, candidates can be endorsed by a coalition of parties, which would make this number higher.

circumstances under which voters are likely to interpret party switching in reaction to corruption as an indication that the incumbent is somehow connected to the uncovered corruption. I address this possibility in the Results section by distinguishing between municipalities with and without access to local media.[4]

The main empirical expectation from this argument is that increasing nearby corruption is associated with an increase in the frequency with which incumbents seek reelection under a different party. I expect this effect to be more evident among mayors in municipalities with neighbors investigated for corruption that are not being investigated themselves. This is because switching parties in reaction to nearby corruption does not change the fact that an investigation is revealing a politician as either clean or corrupt. If the incumbent is corrupt, potential new parties may not be interested in investing in their campaign, even if politicians have incentives to do so. If the incumbent is clean, then the electoral boost of the information alone may be sufficient to secure reelection, so that switching parties is not necessary.

I account for this distinction by analyzing the effect of nearby corruption on party switching rates in two groups. First, mayors who are not selected for auditing but have audited neighbors. Second, mayors who are audited and also have audited neighbors. I expect my argument to hold for the first group, but not the second.

[4]As previous research suggests, the direction of party switching does not systematically favor bigger or smaller parties. While a bigger party can offer more resources and access to more recognizable campaign partners, a smaller party may be able to distribute resources to fewer candidates (Cheibub et al. 2022). Therefore, I do not draw any empirical expectations in terms of the relative size of the parties that incumbents switch to.

3 Research Design

3.1 Background and Data

Between 2003 and 2015, the Brazilian government implemented an anti-corruption program through the *Controladoria Geral da União* (CGU), the country's supreme audit institution. The program randomly selected municipalities with less than 500 thousand inhabitants to audit their use of federal funds.[5] The auditors' task is to identify irregularities in the implementation of public services and welfare programs. The audits cover a range of budget areas that varies over time, focusing on program implementation in education, health, welfare, and public works.[6]

After inspection, the CGU reports the findings from each audited municipality to authorities and the general public. Reports include a detailed account of the findings and monetary amounts involved.[7] In its duration, the program organized 40 lotteries, encompassing 2,187 audits across 1,918 municipalities. Previous research highlights the effectiveness of this program in helping voters hold politicians accountable. Exposing corruption in the context of the CGU audit program led voters to sanction corrupt incumbents (Ferraz and Finan 2008) and to a reduction in local tax revenue (Timmons and Garfias 2015). Both findings reflect the program's effects in its early stage. Timmons and Garfias (2015) note how the effects on local tax revenue are short-lived. Moreover, recent work finds no evidence for electoral sanctions beyond the 2004 local election (Rundlett 2018).[8]

Starting with the 20$^{\text{th}}$ lottery in 2006, the CGU included explicit labels in the reports,

[5] Municipalities with less than 500 thousand inhabitants comprise about 92% of the 5,570 municipalities in Brazil.

[6] After 2015, the CGU was incorporated into the transparency ministry and the program reformulated in a way that makes audits after 2015 incomparable to those studied here. For example, municipalities are now selected based on priority rather than by lottery and a parallel set of audits focus on budget areas instead of geographic units.

[7] Reports are available from https://auditoria.cgu.gov.br/. For an example of the type of information that becomes public, see https://www.gov.br/cgu/pt-br/assuntos/noticias/2008/01/cgu-encontra-muitas-irregularidades-na-23a-edicao-do-programa-de-sorteios (in Portuguese).

[8] Table A1 of the appendix replicates the findings from Rundlett (2018) with the data from this paper, showing a similar trend.

classifying each finding as an instance of mismanagement, a moderate infraction, or a severe infraction.[9] Following previous research using similar data (Avis, Ferraz, and Finan 2018), I code corruption as the sum of the number of moderate and severe infractions, divided by the number of service orders. Service order is the term used by the CGU to identify different municipality budget items associated with federal transfers (e.g. a conditional cash transfer program is a service order). For each municipality selected for auditing, the CGU chooses a random sample of service orders in the last three or four years.

The motivation behind this coding decision is twofold. First, as Avis et al (2018) argue, moderate and severe infractions are hard to distinguish from each other in intensity, especially since the effects of exposing corruption through these audit reports depend on the role of local media (Ferraz and Finan 2008). Second, the coverage of the audit reports, both in terms of number and types of service orders, varies over time and across municipalities. Dividing the number infractions by the number of service orders makes audits comparable over time.

The audit reports before the 20^{th} lottery do not include corruption categories. To reproduce the CGU's coding on this subset of the data, I leverage text data extracted from the original audit report documents. Following a bag-of-words approach, I train a random forest on the labeled cases, using word frequencies as predictors, to predict the corruption variable in unlabeled cases. Section A in the appendix outlines this protocol in more detail and reports its predictive performance. The algorithm performs well for most cases, but it tends to underestimate corruption among high outliers. This implies that models including data from the 2004 election (where most of the machine-coded categories are) will underestimate the effect on the outcomes of interest. Table B6 in the appendix disaggregates results by election year and shows that findings do not depend on machine-coded corruption.

[9] In Portuguese: *falha formal*, *falha média*, and *falha grave*.

3.2 Outcome Variables

I construct the outcome variables using data from the Brazilian electoral court (*Tribunal Superior Eleitoral*, TSE).[10] The main outcome is a binary indicator of whether the incumbent mayors seeks reelection under a different party. In additional analyses, I also focus on a binary indicator denoting whether the incumbent mayor is reelected.

I analyze the mayoral elections in 2004, 2008, 2012, and 2016, since these are the years that overlap with the CGU audit program. Mayors in Brazil can only serve for up to two consecutive terms, so I focus on municipalities where the incumbent mayor is not term-limited.

Table 1 shows the cross-tabulation of the aforementioned variables in this sample. In the period under study, about 60% of the mayors eligible for reelection do not seek reelection. About 13% of the total seek reelection under a different party, which corresponds to roughly 32% (2116/6539) of those seeking reelection. Roughly 28% (1258/4423) of the mayors who seek reelection without switching parties win the election, while 32% (680/2116) of those who seek reelection under a different party win. This aligns with previous work suggesting that incumbents seek reelection under a different party to improve their electoral chances (Novaes 2017; Peterlevitz 2021).

3.3 Explanatory Variables

3.3.1 Defining nearby

The main explanatory variable is the number of nearby corruption infractions, which requires an operationalization of "nearby". The most parsimonious model considers a municipality as exposed to information about nearby corruption if it shares borders with at least one audited municipality. That decision excludes municipalities that do not share a border with

[10] These are available from the TSE website: http://www.tse.jus.br/. An API alternative is also available from the *Centro de Política e Economia do Setor Público* (CEPESP) at *Fundação Getulio Vargas* (FGV): http://cepespdata.io/.

Seeks reelection	Switches party	Wins reelection	N	%
No	No	No	9761	59.88
Yes	No	No	3165	19.42
Yes	No	Yes	1258	7.72
Yes	Yes	No	1436	8.81
Yes	Yes	Yes	680	4.17

Table 1: Distribution of mayors eligible for reelection that seek reelection, switch party, and win reelection

an audited municipality, but are still close to one, which risks false negative findings. To overcome this difficulty, I consider ten different operationalizations of nearby corruption based on increasing contiguity order. The first operationalization only counts as neighbors those municipalities that share a border. Every subsequent operationalization increases the contiguity upper bound, so that the most generous operationalization counts municipalities as neighbors if they are within ten degrees of separation.

I choose the tenth contiguity order as the upper limit as a reasonable upper bound for spillovers. To illustrate the magnitude, Figure C1 in the appendix shows the distribution of neighbors and audited neighbors by contiguity order. The median number of neighbors at the first order of contiguity is 6, whereas the median number of audited neighbors is 1. At the tenth level of contiguity, the medians are 85 and 8, respectively. The main results show that effects stabilize once one moves beyond the operationalization that only considers immediate neighbors.

Each one of these operationalizations implies a different version of the nearby corruption variable. Figure 1 shows the distribution of these variables. I only include in the analysis municipalities with at least one audited neighbor within range since one cannot observe corruption in municipalities that are not audited. This implies that sample size increases along with the contiguity upper bound.

In the most parsimonious operationalization, the median number of infractions per audited neighbor is 2.88 (mean = 3.03). In the most generous operationalization, the median is

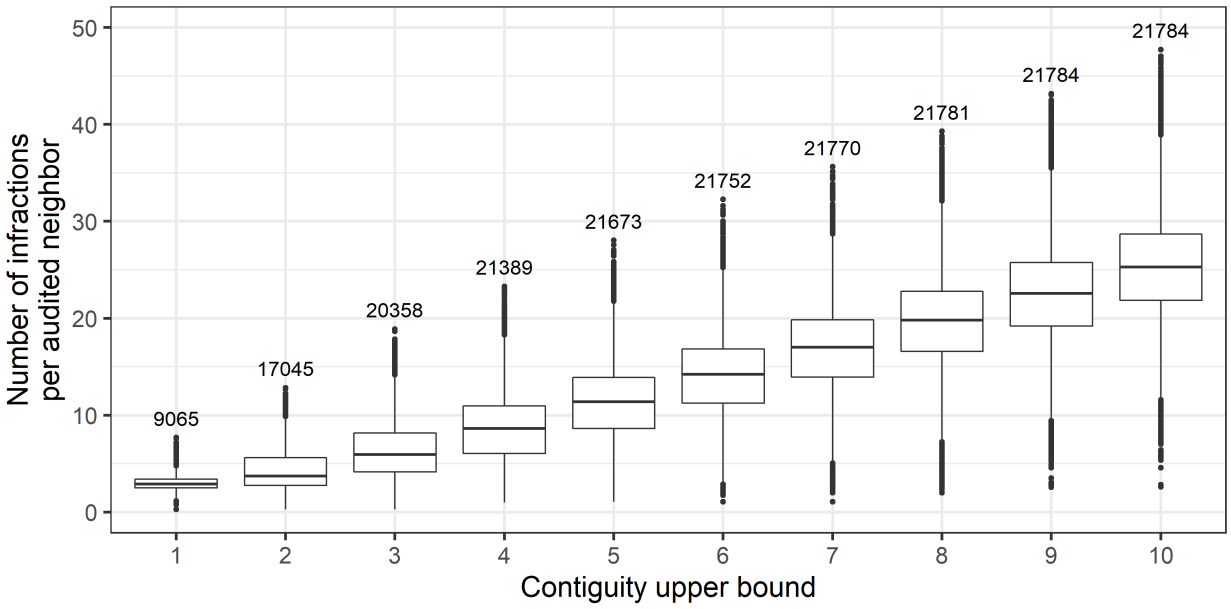

Figure 1: Distribution of nearby corruption infractions at different contiguity upper bounds
Note: Labels denote sample size.

around 25.27 (mean = 25.16). While Figure C1 shows that the number of audited neighbors grows slower than the number of total neighbors, Figure 1 suggests the number of infractions per audited neighbor grows faster than the total number of neighbors. This is because the same municipalities with multiple infractions contribute to the value of nearby corruption of more municipalities as the upper bound increases. In general, the move from one contiguity upper bound to the next is marginal, but considerable as one contrasts the most parsimonious and more generous operationalizations.

I choose to focus on contiguity because it aligns with the administrative jurisdictions of municipalities, which in turn determines mayoral constituencies. An alternative approach would be to operationalize nearby corruption as a function of geographic distance. However, this approach ignores considerable variation in the sizes of municipalities in Brazil and requires a model-based statement on how effects vary as a function of distance (e.g. Bowers, Fredrickson, and Panagopoulos 2013; Coppock 2014) that does not follow immediately from my argument.

3.3.2 Audit status

I argue that nearby corruption leads mayors to seek party switching more often in an attempt to preserve their reelection chances. As the previous section discusses, this empirical expectation should hold only for mayors who are not investigated for corruption themselves. Otherwise, either the mayor or the potential new party would not have incentives to switch.

To account for this possibility, I estimate the effect of nearby corruption on party switching for two different groups. The first group are non-audited municipalities with at least one audited neighbor withing range, this is the group for which I expect nearby corruption to lead to party switching. The second group are audited mayors with at least one audited neighbor in range. For this group, I do not expect my argument to hold. I use a binary indicator of whether a municipality had an audit report released before the election. In the sample of 21,784 municipality-election years with at least one audited neighbor within ten neighbors apart, 2,152 (about 10%) are audited themselves.

In principle, these two groups may not be comparable. A non-audited municipality with audited neighbors only needs lottery selection to happen once. An audited municipality with audited neighbors needs the lottery to select two nearby municipalities at the same time, which is rarer event. If this is true, then different effects between the two groups may be a function of topology, which can indirectly map onto unobserved confounders that correlate both with the distribution of both corruption and party switching over space. Therefore, I can only attribute the effect of nearby corruption on party switching to the reelection incentives argument if I can rule out the role of topology as a potential confounder.

Fortunately, one feature of the CGU audit program facilitates sustaining this claim. In each lottery, the CGU chooses the number of municipalities that will be selected for auditing in each state with the goal of ensuring even coverage. This means random selection is independent across states, akin to a block randomized experiment. Therefore, after accounting for variation in time and across states, the number of neighbors a municipality has should

not affect whether which group it corresponds to. Table X in the appendix shows this is the case using logistic regression for each operationalization of nearby corruption. Furthermore, Figure X in the appendix also shows audited and non-audited municipalities with at least one neighbor are balanced across selected covariates. The only exception is that audited municipalities tend to have a smaller population, which is expected since only municipalities with less than five hundred thousand inhabitants can be audited. Both pieces of evidence suggest that topology does not play a mayor factors in the distribution of audit status across municipalities with audited neighbors at different ranges.

4 Results

4.1 Main results

Figure 2 shows the effect of one unit increase in nearby corruption on seeking reelection under a different party in the subset of municipalities where the mayor is not term-limited. Each value in the horizontal axis denotes a separate OLS regression model, including an interaction with a municipality's own audit status, election year fixed effects, and clustered standard errors by election year. Increasing values in the horizontal axis indicate a more inclusive definition of nearby, based on cumulative contiguity order. For example, when the cumulative contiguity order equals 1, the model considers a municipality as exposed to corruption if they share borders with at least one audited neighbor. At a value of 10, the model considers a municipality as exposed to corruption if they have at least one audited neighbor within 10 degrees of separation.[11]

Figure 2 illustrates the importance of avoiding a narrow operationalization of nearby corruption. Focusing only on immediate neighbors suggests a positive effect of nearby corruption on party switching, albeit indistinguishable from zero. A narrow definition also leads to a

[11]Section B in the appendix shows tables with the numerical results underlying the results in this paper. Table C2 shows that results are similar with logistic regression.

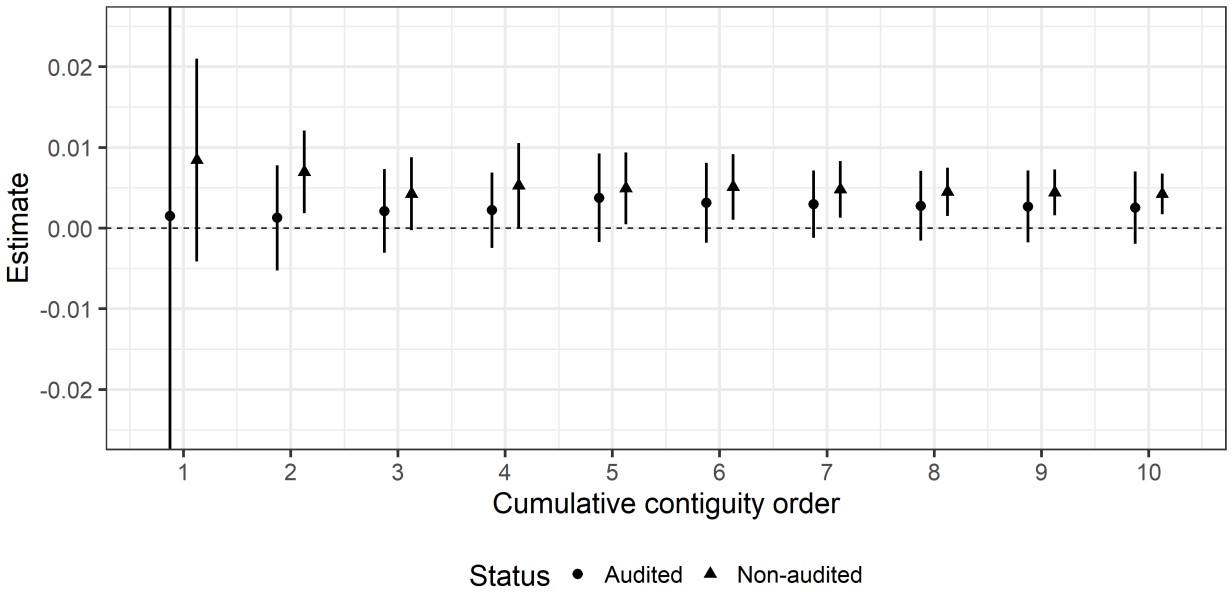

Figure 2: Effect of nearby corruption on incumbent mayor party switching
Note: Based on OLS regression with election year fixed effects and clustered standard errors by election year. The shaded region denotes the optimal range suggested by cross-validation. Vertical lines denote 95 percent confidence intervals. Vertical axis zoomed in to facilitate visualization.

wide confidence interval in the subset of audited municipalities. As one moves beyond the most parsimonious operationalization, the effect of nearby corruption becomes different from zero among non-audited municipalities, but indistinguishable from zero among audited municipalities. The effect among non-audited municipalities is more pronounced for the second contiguity upper bound and dilutes, but exhibits narrower confidence intervals, as the range increases. Using the second order contiguity upper bound as a benchmark, a one standard deviation increase in nearby corruption among non-audited municipalities leads to 1.3 percent increase in party switching rates. This effect size is considerable since in this sample the average party switching rate is around 13 percent.

These results suggest that nearby corruption encourages incumbent mayors eligible for reelection to run with a different party, but only in the subset of municipalities that are not audited themselves. This aligns with the argument that politicians react to nearby corruption only when they expect voters to hold them accountable for nearby corruption. A critique to this interpretation is that, while municipalities are randomly selected for auditing,

observed corruption infractions are not random, which opens the door for omitted variable bias. One way to address this possibility is through sensitivity analysis (Cinelli and Hazlett 2020). Figure C2 in the appendix shows that, focusing on the operationalization that uses the second contiguity order, an unobserved confounder would need to explain more than 50% of the partial R^2 in nearby corruption or party switching to turn the observed estimate into zero.

In the next sub-section, I zoom into the sample of non-audited municipalities with at least one audited neighbor at different ranges to explore the merit of three alternative interpretations of these findings.

4.2 Alternative explanations

Note: *This section is still in progress!*

4.2.1 Mayors are likely to have direct connections with nearby corruption

This paper focuses on exposure to nearby corruption because proximity is one channel through which mayors may expect accountability for corruption even if they are not connected to it. However, it may still be the case that mayors in proximate localities share partisanship. If so, the positive effect of nearby corruption on party switching rates may still reflect an attempt to avoid direct connection with nearby corruption.

To address this possibility, I focus on the proportion of audited neighbors who are from the same party as the target mayor. This proportion changes depending on the operationalization of nearby corruption. Under the narrowest operationalization, roughly 82% of the mayors do not have an audited neighbor from the same party. Under the broadest operationalization, this number is 11%. This means one cannot rule out direct connection through partisanship as a confounder.

If direct connection through partisanship plays a role, one would expect the effect of nearby

corruption on party switching to be more pronounced as the proportion of audited neighbors from the same party increases. Figure 3 evaluates this expectation in the sample of non-audited municipalities with audited through the interaction of nearby corruption and the proportion of same-party audited neighbors. The left panel shows the estimated change in the slope of nearby corruption as the proportion of audited mayors in nearby localities goes from 0 to 1. Regardless of the operationalization of nearby corruption, which also modifies the distribution of same-party audited neighbors, I do not find evidence that the effect of nearby corruption on party switching rates varies with the proportion of audited neighbors from the same party.

To further illustrate this point, the right panel of Figure 3 shows simulated marginal effects of nearby corruption on party switching rates at different proportions of audited neighbors from the same party using the narrowest definition of nearby corruption used the second contiguity border upper bound, since this is the most parsimonious operationalization that leads to non-zero effects in Figure 2. This panel suggests that the marginal effects cannot be distinguished from each other across proportions of same-party audited neighbors. Moreover, the effect is only distinguishable from zero among municipalities where the majority of audited neighbors are from different parties.

4.2.2 Mayors try to take advantage of nearby corruption

Coming soon!

From my handwritten notes:

- No effect on run/win (appendix table)
- Effects by year (learning)
- Map with direct effects (appendix and previous lit)

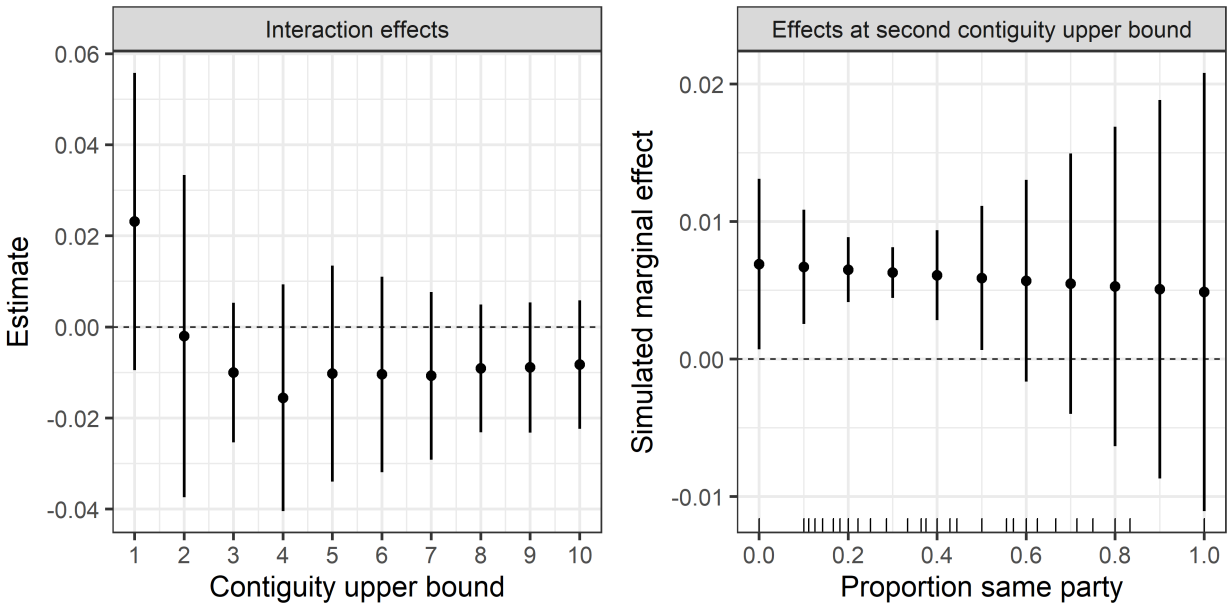

Figure 3: Interaction effect of nearby corruption and the proportion of same-party audited neighbors on party switching rates among non-audited municipalities

Note: The left panel shows interaction effect coefficients under different operationalizations of nearby corruption. The right panel shows simulated marginal effects of nearby corruption under different proportions of same-party audited neighbors using the second contiguity order upper bound

4.2.3 Party switching as an admission of guilt

Coming soon!

5 Conclusion

Note: *This section is still in progress!*

This paper argues that politicians exposed to nearby corruption react to it by updating candidate selection and entry strategies. Moreover, they do so in a pattern that suggests an attempt to avoid electoral sanctions. I show evidence in favor of this argument using data from a long running anti-corruption program in Brazil. Unlike previous work showing how politicians avoid association with corruption (Asquer, Golden, and Hamel 2019; Daniele, Galletta, and Geys 2020), this paper disentangles electoral accountability from top-

down sanctioning mechanisms. In this regard, it strengthens the case for an alternative explanation to the limited evidence in favor of voter sanctions in the corruption literature. While current explanations emphasize how surveys overestimate voters' ability to sanction and suggest more realistic vignettes (e.g. Boas, Hidalgo, and Melo 2018; Incerti 2020), this paper suggests taking into account politicians strategic behavior in reaction to corruption. While this idea is already implicit in the research that explores the circumstances under which voters choose to forgive corruption, bringing politicians' reaction to the forefront may increase our understanding of the micro-foundations underlying the electoral consequences of corruption.

The main implication for the study of the electoral consequences of corruption is that interventions aimed at reducing the informational gap between voters and politicians' performance in office may bring unintended consequences. Whether these consequences are positive or negative is a matter for future debate. On one hand, the results in this paper suggests that information campaigns to fight corruption create incentives for politicians to pay attention to voter behavior, or at least their belief of what voter behavior will be. On the other hand, they also create incentives for politicians to cloud voters' ability to attribute responsibility.

In emphasizing the unintended consequences of exposing nearby corruption, this paper also highlights how politicians respond strategically to anti-corruption efforts (Fisman and Golden 2017). In that sense, it connects the literature on corruption with accounts of how increasing election monitoring may displace, rather than deter, electoral fraud and violence Ichino and Schündeln (2012), which suggests that the mechanisms in place in this paper may extend to other countries where voters' adverse selection problem is pronounced. While the results from Brazil may not replicate directly in other settings, the underlying logic may apply to other contexts facing challenges to electoral accountability.

Methodologically, this paper makes two contributions. First, it extends previous research on the effects of the CGU anti-corruption program by creating a comprehensive data set

that puts 13 years of publicly released audit reports under the same coding scheme, avoiding biases in human coding and reproducing the official supreme audit institution's criteria.

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