# REVEALED CORRUPTION AND ELECTORAL ACCOUNTABILITY IN BRAZIL: HOW POLITICIANS ANTICIPATE VOTING BEHAVIOR

BY

GUSTAVO A. DIAZ DIAZ ROMERO

DISSERTATION

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Doctoral Committee:

Associate Professor Jacob W. Bowers, Chair
Assistant Professor Avital Livny
Associate Professor Gisela Sin
Professor Matthew S. Winters

# Abstract

Governments, civil society organizations, and scholars spend considerable resources implementing and evaluating the effect of anti-corruption interventions. However, decades of cumulative evidence suggest that these interventions rarely lead to the removal of corrupt elected officials from their positions. A recent interpretation of this gap suggests that corrupt politicians often go unpunished because they react to the knowledge of themselves or others being investigated for corruption in unanticipated ways. This dissertation uses data from a long-running anti-corruption program in Brazil to expand on the unintended consequences of anti-corruption interventions that stem from politicians' strategic behavior. The first chapter shows that mayors randomly selected for auditing in the context of this program reduce public spending, particularly in highly visible budget categories, in years close to an election. I argue that this happens because mayors attempt to preserve their reelection chances by signaling fiscal responsibility. The second chapter shows how mayors that are not directly audited, but are in municipalities close to those with mayors exposed as corrupt, tend to seek reelection under different parties more often. As previous accounts of party switching in Brazil suggest, I argue that this occurs because incumbent politicians expect their constituency to react to the news of nearby corruption with increased scrutiny on their own performance in office, which in turn leads them to switch parties in an attempt to secure a better platform for reelection. The question of the effect of exposure to information about nearby corruption opens the door to a broader methodological question of how to capture this type of effect, which is the focus of the third chapter. Research questions in the social sciences usually suggest spillover or interference effects, but rarely provide guidelines on how to model those effects. In fact, theory often suggests many different plausible operationalizations along the same hypothetical pathway. To overcome this difficulty, I propose and illustrate the properties of a model selection approach that uses tools from supervised machine learning to select among alternative operationalizations. As a whole, this dissertation makes two key contributions. First, it shows how politicians' reaction to anti-corruption interventions can stem from an attempt to avoid electoral accountability. Second, by proposing a model selection approach to interference, it expands the applicability of current tools to analyze interference effects to a broader set of research questions.

*To Noah.*

# Acknowledgments

As a first-generation scholar, I constantly question whether I belong in academia. This is not because I doubt my skills, but because I am aware of everything that went exactly the right way, and sometimes exactly the wrong way, so that I could be here. My brother tried to convince me to study economics, but I ended up choosing political science (which in my head was like economics but with less math, I was wrong). A conversation with Julieta Suárez Cao led me to apply to grad school earlier than usual. Gisela Sin happened to be around when I was choosing what programs to apply to. If Valeria Palanza had not introduced us at that time, I would not have even considered applying to UIUC.

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# Table of Contents

# Chapter 1

# Mayors Reduce Spending to Counter the Electoral Consequences of Increased Monitoring

## 1.1 Introduction

Cross-national studies suggest that corruption limits economic development and growth (Mauro 1995; Rose-Ackerman 1999). Case studies zooming into this relationship show how corruption creates market inefficiencies that increase the cost of government activity and harm the provision of public goods and services (see Olken and Pande 2012 for a review). Moreover, corruption is more prevalent in countries facing poverty, resource dependency, limited access to information, and challenges to democracy (Montinola and Jackman 2002; Tavits 2007; Treisman 2000, 2007; Uslaner 2017).[1]

Research on the effect of anti-corruption interventions suggests that increased monitoring on politicians' behavior in office reduces inefficiencies in project implementation Olken 2007 and improves the provision of goods and services (Björkman and Svensson 2009; Funk and Owen 2020; Reinikka and Svensson 2005). The literature suggests two explanations for this effect. First, increased monitoring assists authorities in detecting corruption and implementing top-down sanctions (Avis, Ferraz, and Finan 2018; Brollo 2011). Second, increasing monitoring can facilitate bottom-up accountability by sharing information with voters or by incorporating citizens in the monitoring process itself (see De Vries and Solaz 2017 and Pande 2011 for reviews).

Recent work on the electoral consequences of corruption casts doubt on the second explanation. While survey reports reveal voters' distaste for corruption, sharing information about corruption rarely harms the performance of corrupt politicians in elections (Boas, Hidalgo, and Melo 2018; Incerti 2020). One interpretation of this gap is that incumbents react to increased monitoring by updating their behavior in office, thus mitigating or preventing potential electoral sanctions (Fisman and Golden 2017). Evidence from survey experiments and observational studies hints at this mechanism by suggesting that voters forgive corruption among politicians who exhibit positive economic outcomes (Fernández-Vázquez, Barberá, and

1. The literature defines corruption broadly as the use of public office for private gain (Svensson 2005). In practice, most empirical work uses the term in reference to bribes or malfeasance (Olken and Pande 2012). Here I focus on corruption as malfeasance, understood as the misappropriation of public resources, for example, through theft or over-invoicing.

Rivero 2016; Konstantinidis and Xezonakis 2013; Muñoz, Anduiza, and Gallego 2016; Pereira and Melo 2015). If incumbents are aware that voters tolerate corruption in exchange of good performance, then they may react to increased monitoring by adjusting their behavior in office accordingly.

This chapter provides direct evidence for the mechanism suggesting that incumbents react to increasing monitoring by adapting their behavior in office. Moreover, I argue that they do so in a pattern that reveals incentives to anticipate electoral accountability. Using data from an anti-corruption program in Brazil that randomly selects municipalities to audit their use of federal funds, I find that audits lead mayors to decrease overall public spending, and to concentrate their spending on a smaller number of budget categories. This effect is more pronounced when mayors are eligible for reelection and audits happen close to or during an election-year. By disaggregating spending across budget categories, I also find that the reduction in spending occurs primarily in highly visible budget areas. Taken together, these findings suggest that incumbent mayors adapt their behavior in office in reaction to increased monitoring to signal fiscal responsibility, which serves the purpose of preserving incumbents' reelection chances.

This chapter makes two contributions. First, the main contribution is to further our understanding of the electoral consequences of corruption by showing that incumbents adapt their behavior in office in reaction to increased monitoring in an attempt to protect their reelection chances. This means that elected officials may still be responsive to their constituencies even if they are not held accountable for corruption in elections. However, this also suggests that the menu of options for politicians wanting to get away with corruption is more diverse than initially thought of. This follows from a recent call in the literature to focus on the unintended consequences of anti-corruption interventions that arise from politician's strategic responses (Fisman and Golden 2017).

Second, this chapter also contributes to the literature on political budget cycles (Aaskoven and Lassen 2017) by showing than increased monitoring close to an election, especially if unexpected, can change politicians' assessment about the effective of different fiscal policies. Previous work suggests that politicians face a trade-off between pleasing voters preferring more targeted spending and those who prioritize fiscal responsibility Drazen and Eslava 2010. This chapter suggests that increased monitoring brings attention to incumbents' performance in office as a whole and, in doing so, may tilt the balance in favor of signaling fiscal responsibility as a viable strategy for reelection.

## 1.2 Monitoring, Spending, and Electoral Accountability

### 1.2.1 Anti-corruption interventions to reduce inefficiency

Cross-national studies examining the consequences of corruption show that corruption limits economic development and growth (Mauro 1995; Rose-Ackerman 1999). Zooming into this relationship, case studies suggest that corruption, in its different forms, creates market inefficiencies that raise the cost of government activity and harm the provision of public goods and services (Olken and Pande 2012).[2] For example, a government that over-invoices a company to build a road can create inefficiencies in two ways. First, by raising the cost of the infrastructure project, it reduces the resources available for the delivery of other public goods and services. Second, if the project diverts resources to a politician's pocket, this distortion itself can introduce inefficiencies in program implementation, since the involved parties would have to make sure that theft goes undetected.

Because corrupt politicians have incentives to hide their illicit activities (Gambetta 2002; Rose-Ackerman 1978), the first order of business in the fight against corruption is to find effective strategies to uncover it. Research on the effect of anti-corruption interventions suggests that increased monitoring reduces inefficiencies in project implementation. For example, Olken (2007) shows that government audits reduce missing expenditures in road construction projects in Indonesia. By bringing attention to politicians' performance, increased monitoring can also induce positive outcome in public goods and service delivery. For example, Reinikka and Svensson (2005) show how a newspaper campaign with information about how local officials handle the implementation of an education grant program in Uganda reduced resource misappropriation and improved student enrollment and learning outcomes. More recent work shows that this effects can be long-lasting. Most relevant to this project, Funk and Owen (2020) show that Brazilian municipalities audited in 2004 improve the delivery of health, sanitation, and education services up to 6 years after an audit.[3]

What explains the reduction in inefficiencies and improvement in the delivery of public goods and services? The literature identifies two explanations. First, by uncovering corruption, increased monitoring gives information to the authorities in charge of investigating and sanctioning illicit activities, which reduces opportunities for resource misappropriation and updates politicians' belief of the probability of getting caught. For example, Avis, Ferraz, and Finan (2018) show that anti-corruption audits in Brazilian municipalities increase the probability of legal action against corrupt politicians, while also reducing the extent of subsequent corruption in nearby municipalities. Focusing on the same program, Brollo (2011) shows that municipalities

2. Some accounts of corruption suggest the opposite. In countries with restrictive institutions, corruption may facilitate investment and provision opportunities that would not be available otherwise (Méon and Weill 2010).

3. Yet note that Zamboni and Litschig (2018) find no short-run effect of audits conducted in 2009 on the quality of healthcare services.

where corruption was uncovered experience a reduction in transfers from the federal government.

Second, increased monitoring can help citizens to hold providers and politicians accountable by incorporating citizens in the monitoring process itself, or by publicizing information about politicians' performance in office. As an example of incorporating citizens in the monitoring process, Björkman and Svensson (2009) show that village meetings encouraging citizen involvement in the monitoring of health service provision in Uganda led to improvements in infant weight and mortality. On publicizing performance information, research on the Brazilian audit program shows that mayors exposed as corrupt are less likely to win reelection (Ferraz and Finan 2008) and collect less revenue in local property taxes (Timmons and Garfias 2015), which suggests that voters react to information sharing by sanctioning corrupt politicians.

### 1.2.2 The electoral consequences of corruption

Recent evidence casts doubt on the second explanation of why increased monitoring affects politicians' behavior. While Björkman and Svensson (2009) find that citizen involvement affects health outcomes positively, Olken (2007) finds no effect of this type of encouragement of missing expenditures, suggesting that politicians are not as responsive as providers to the incorporation on citizens in the involvement process. In parallel, while Ferraz and Finan (2008) find an effect of exposing corruption on incumbent vote shares, subsequent work shows that this effect disappears after the 2004 local election in Brazil (Rundlett 2018). Moreover, evidence from a separate set of audits suggests that the incentives for rent-extraction often offset the reelection incentives that would mitigate the negative consequences of corruption (Pereira, Melo, and Figueiredo 2009). Beyond electoral accountability, Timmons and Garfias (2015) acknowledge that the effect of corruption on property tax collection is short-lived.

The cumulative evidence in the broader study electoral accountability points in a similar direction. Coordinated randomized controlled trials around the world find no evidence of an effect of information campaigns sharing incumbent performance information on vote choice (Dunning, Grossman, Humphreys, Hyde, McIntosh, and Nellis 2019; Dunning, Grossman, Humphreys, Hyde, McIntosh, Nellis, et al. 2019). A meta-analysis that focusing on survey and field experiments on the effect of sharing information about corruption on incumbent vote shows that voters express strong anti-corruption norms in surveys, but their aversion does not translate to a change in election results (Incerti 2020). Simultaneous survey and field experiments in the state of Pernambuco, Brazil, also exhibit the same pattern (Boas, Hidalgo, and Melo 2018).

These findings imply that the prospect of bottom-up sanctions is an unlikely explanation for the reduction of inefficiencies through increased monitoring. However, an alternative interpretation of the gap between self-reported and actual voter behavior is that politicians react to increasing monitoring by updating their behavior in office in anticipation of the potentially negative consequences (Fisman and Golden 2017). For example, research on electoral fraud shows that the presence of election observes does not eradicate irregularities, but rather motivates politicians and parties to displace irregularities to places without monitoring (Asunka et al. 2019; Ichino and Schündeln 2012).

In the case of the electoral consequences of corruption, research shows indirect evidence of incumbents trying to anticipate electoral accountability in two ways. First recent work on corruption scandals in Italy shows that political parties avoid including legislators investigated for corruption in their proportional representation lists (Asquer, Golden, and Hamel 2019). On the flip side of the coin, also in Italy, mayors abandon their affiliation with parties involved in corruption scandals after securing reelection (Daniele, Galletta, and Geys 2020). These findings suggest that parties and elected officials try to preserve their reputations and reelection chances.

Second, evidence from survey experiments and observational studies suggests that voters forgive corruption when politicians satisfy expectations in other areas. The literature refers to this phenomenon as implicit trading (Rundquist, Strom, and Peters 1977), which occurs when voters prefer to have a corrupt politicians from their preferred party over a clean politician from the opposition (Anduiza, Gallego, and Muñoz 2013; Eggers 2014) or when voters tolerate corruption when it brings positive economic externalities (Fernández-Vázquez, Barberá, and Rivero 2016; Konstantinidis and Xezonakis 2013; Muñoz, Anduiza, and Gallego 2016).[4]

Closer to the topic of this chapter, research from Brazil suggests that incumbents can mitigate the electoral consequences of corruption through public spending. Pereira and Melo (2015) use data from the state of Pernambuco to show that the negative effect of uncovered corruption on the probability of incumbent reelection disappears among mayors with higher public spending. This finding suggests that elected officials with high public spending may counteract the electoral consequences of corruption, but does not show direct evidence of incumbents using public spending in reaction to increased monitoring as a strategy to preserve the chances of reelection.

4. Although note that the survey experiments in Breitenstein (2019) and Winters and Weitz-Shapiro (2013) find no evidence of implicit trading between corruption and economic performance.

## 1.3 Electoral Incentives to Reduce Spending in Reaction to Increased Monitoring

### 1.3.1 Mechanism: Increased attention and disruptions in the political business cycle

The previous section suggests that elected officials may adjust public spending in reaction to increased monitoring in an effort to mitigate its electoral consequences. Why would incumbents expect electoral consequences from increased monitoring? Formal theoretical models of electoral accountability suggest that voters judge politicians' performance in office through observable outputs (Barro 1973; Fearon 1999; Ferejohn 1986). When increased monitoring also involves sharing new information about incumbents' performance in office with voters, then politicians should expect voters to update their beliefs about incumbent type. In the case of audits seeking to uncover corruption, as it occurs with the Brazilian audit program discussed in this chapter, the new information may also make the issue of corruption salient in voters' minds. Previous research using public opinion data suggests that anti-corruption voting is possible only if the issue of corruption becomes salient in voters' minds (Klašnja, Tucker, and Deegan-Krause 2016). Even when audits do not reveal considerable corruption, the news of increased monitoring may bring voters' attention to the issue of corruption, which may lead elected officials to expect heightened scrutiny on their performance.

Why would incumbents use public spending in reaction to heightened scrutiny? The literature on political budget cycles shows how elected officials structure public finances during their term to improve their reelection chances (Aaskoven and Lassen 2017). While the specific way in which incumbents structure spending varies across institutional settings, research from Brazil suggests that mayors with reelection incentives either increase spending during election years (Sakurai and Menezes-Filho 2008) or keep spending constant, but restructure it towards more visible areas while simultaneously reducing local tax revenue (Klein and Sakurai 2015). From a broader perspective, local level incumbents change fiscal policy as elections approach trying to please two different audiences: voters who value targeted spending and those who value fiscal responsibility (Drazen and Eslava 2010). The reason why incumbents focus on election-year fiscal policy is because voters tend to use election-year information to infer incumbent performance throughout the term (Healy and Lenz 2014).

In what direction do incumbents update public spending in reaction to increased monitoring? A core assumption in the political business cycle is that incumbents plan ahead and structure spending with reelection in mind before the election year comes. For example, mayors in Brazil set budgets a year ahead, which means that unexpected increased monitoring may leave them incapable of adjusting fiscal policy beyond adjustments to an already allocated budget. Therefore, I argue that the electoral considerations that arise from increased monitoring lead to incumbents to focus on the incentives to signal fiscal responsibility. In other words, I expect increased monitoring to close to elections to decrease public spending.

### 1.3.2 Alternative explanations: Central government transfers and local tax revenue

The presence of bottom-up incentives to react to increased monitoring by decreasing spending does not preclude other mechanisms. However, a decrease in public spending as a consequence of increased monitoring may be respond to two alternative explanations different to the one proposed in this chapter. First, elected officials at the local level may experience a decrease in central government transfers if increased monitoring uncovers corruption, which would result in an overall reduction in public spending (Brollo 2011). If this is true, one would observe this effect across all incumbents, regardless of their reelection incentives. Since Brazilian mayors can only serve up to two consecutive terms, I can address the merit of this alternative explanation by comparing the effect of increased monitoring on spending between term-limited and reelection-eligible mayors (Besley and Case 1995).

Second, the reduction in public spending can arise from a decrease in local tax revenue. This could happen because incumbents choose to reduce the tax burden within their constituency to improve their reelection chances, which is unlikely considering the previous evidence suggesting that a reduction on local tax revenue does not come at the expense of public spending (Klein and Sakurai 2015). Another avenue could be citizens choosing to sanction a potentially corrupt administration by not paying taxes (Timmons and Garfias 2015). If this is true, one would observe the effect of increased monitoring on year $t$ affect spending on year $t + 1$, which I can address by leveraging the timing of increased monitoring and its effects on spending outcomes at different points during the mayoral term.

## 1.4 Research Design

### 1.4.1 Background and data

I examine the effect of increased monitoring on public spending using data from a long-running anti-corruption program in Brazil. As of 2020, Transparency International classifies Brazil as a moderately corrupt country, ranking 94 out of 180 in the list of least corrupt countries, and a below average Corruption Perceptions Index of 38 over 100 (with a global average of 43).[5] According to the Global Corruption

5. See https://www.transparency.org/en/countries/brazil.

Barometer from 2019, 54% of survey respondents in Brazil thought corruption had increased in the last year and 11% of public service users report paying a bribe within the same time frame (Pring 2019). At the local level, corruption occurs most commonly through over-invoicing or misappropriation of federal funds destined to the delivery of public goods and services or the implementation of public works (Ferraz and Finan 2011). Previous research shows that corruption is more common in municipalities with larger transfers from the federal government (Brollo et al. 2013).

To fight corruption at the local level, the federal government mandated the country's supreme audit institution, *Controladoria Geral da União* (CGU), to implement an anti-corruption program between 2003-2015. The program periodically selected municipalities with population under 500 thousand inhabitants by lottery to audit their use of federal funds.[6] Across 13 years, the program conducted 40 lotteries, translating into 2,187 audits across 1,918 municipalities.[7]

Before each lottery, the CGU determines the number of municipalities to audit within each state. I consider the CGU audit program as a natural experiment (Dunning 2012), since audits are assigned to municipalities at random within each state and lottery round but I do not control the assignment process. Once a municipality is selected for auditing, the CGU also selects at random a number of service orders that become the focus of the audit. Service order is the term used by the CGU to identify different items associated with federal transfers in a municipality's budget. For example, the delivery of conditional cash transfer payments under the *Bolsa Família* program (Zucco 2013) is a service order.

Once an audit is concluded, the CGU compiles a report for each audited municipality and shares it with the media and relevant authorities. Reports include a detailed account of irregularities found by the auditors and the associated monetary value.[8] Previous research examining the consequences of publicizing the results of audits shows that exposing corruption leads to electoral sanctions against audited mayors (Ferraz and Finan 2008), although this effect disappears after the 2004 election, which is the first after the introduction of the program (Rundlett 2018). Increased monitoring under this program lead to both short and long-term reductions in corruption (Avis, Ferraz, and Finan 2018; Zamboni and Litschig 2018), as well as long term improvements in the delivery of health, sanitation, and education services (Funk and Owen 2020).

6. About 92% of the 5,570 municipalities in Brazil have a population under 500 thousand.

7. The program continued after 2015, but was reformulated to include random and non-random audits, as well as audits to sectors of the economy, as opposed to municipal governments. Given the magnitude of the changes, I do not include post-2015 audits in the analysis.

8. Digitized versions of the reports are available at https://auditoria.cgu.gov.br/. For an example the types of irregularities uncovered, see https://www.gov.br/cgu/pt-br/assuntos/noticias/2008/01/cgu-encontra-muitasirregularidades-na-23a-edicao-do-programa-de-sorteios (in Portuguese).

### 1.4.2 Explanatory variables: Audit selection and timing

Since the CGU program ran from 2003 to 2015, I analyze the effect of increased monitoring on public spending across four mayoral terms encompassing the elections in 2004, 2008, 2012, 2016. Therefore, the unit of analysis is the municipality-term. The main explanatory variable is a binary indicator of whether a municipality was audited in a given term.

Figure 1.1 shows the distribution audits across lotteries over time. During the first two lotteries, the program audited only a few municipalities. Starting with the third lottery, the number of municipalities selected by lottery grew to 50, and then 60 in the tenth lottery. Sometimes, audits do not take place due to implementation issues. The number of canceled audits is usually small, with the exception being the 36$^{\text{th}}$ lottery in which most of the audits where canceled because of a CGU employee strike.

To confirm that audits are assigned at random, Table 1.1 compares the means of non-audited and audited municipalities across selected covariates. Since the CGU first determines the number of municipalities to audit in each state and the selects which municipalities to audit within states, I assume that treatment is assigned within state-term strata.[9] That is, for each covariate I calculate and compare the means of both non-audited and audited municipalities within each state for each election year. Then I report the weighted averages based on the number of non-audited and audited municipalities within each strata. The table suggests that the only covariate in which we have enough evidence against the null of equal means is population. On average, non-audited municipalities have about seven thousand more inhabitants than audited municipalities ($p = 0.059$). This is expected since only municipalities with a population smaller than 500 thousand can be audited. However, a $\chi^2$ test shows little evidence against the null hypotheses of overall balance ($\chi^2 = 14.37$, $df = 13$, $p = 0.35$), so we can analyze the effect of audits on public spending without adjusting for potential confounders (see Hansen and Bowers 2008 for details).

In additional analyses, I distinguish the year within the mayoral term in which the audit happened. The colors in Figure 1 indicate this distinction, which also helps to visualize how lotteries became less common over time, although with enough variation to capture the effect of being audited at different times during the term. The timing of the audits is recorded based on the date in which the CGU announces the results of a lottery.[10] This means the results reported in this chapter correspond to mayors' reaction to the news that their administration will be monitored. Section A.3 of appendix A show that results point in the same direction when using the extent of corruption uncovered by the audits as an explanatory variable, which suggests that mayors react to both increased monitoring and revealed corruption in a pattern that

9. Actual treatment assignment happens within state-lottery strata, but since the unit of analysis is the municipality-term, this is as close as I can get.

10. The lottery dates are available in https://www.gov.br/cgu/pt-br/assuntos/auditoria-e-fiscalizacao/programa-de-fiscalizacao-em-entes-federativos/edicoes-anteriores/municipios

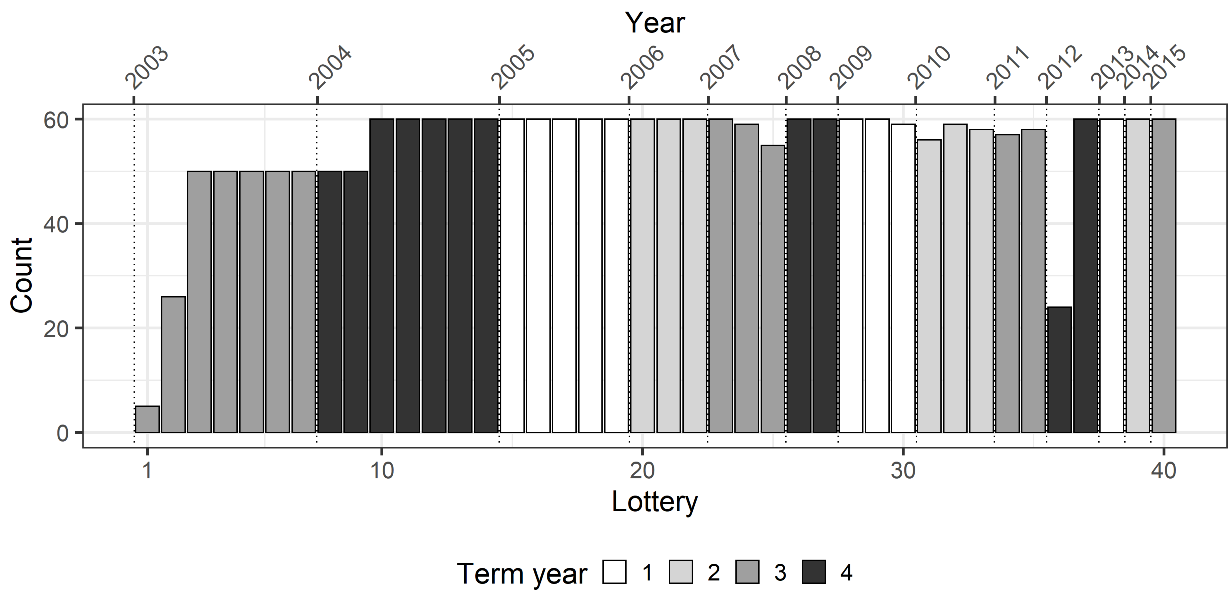


**Figure 1.1: Distribution of audits across lotteries over time**

*Note:* Vertical dotted lines denote the beginning of a year. Colors indicate the year within the mayoral term in which the audit happens.

reveals incentives to preserve their reelection chances. The results using the extent of corruption as the explanatory variable also show the findings in this chapter can coexist with other explanations for top-down and bottom-up sanctioning (Brollo 2011; Timmons and Garfias 2015).

### 1.4.3 Outcome variables: Public spending and budget concentration

Public spending data comes from the *Insituto de Pesquisa Econômica Aplicada*.[11] I focus on two outcome variables. The first outcome is the total spending per capita (in Brazilian reais), which I calculate by summing the reported spending in each municipality across 21 budget categories. Table A.1 in appendix A shows the list of budget categories.

Second, I measure how concentrated public spending is across budget categories by calculating the effective number of budget categories. More formally, the effective number of budget categories in a given municipality-year is

$$Concentration = \frac{1}{\sum_{i=1}^{21} s^2} \tag{1.1}$$

with $i$ being the index for each of the 21 budget categories and $s$ the share out of the total public spending in each category. This is analogous to the formula for the effective number of political parties (Laakso and

11. Available at http://www.ipeadata.gov.br/.

| | Non-audited | Audited | Adj. diff. | Std. diff. | p-value |
|---|---|---|---|---|---|
| Population (thousands) | 30.821 | 23.809 | -7.012 | -0.036 | 0.059 |
| Female population (%) | 0.493 | 0.493 | 0.000 | 0.010 | 0.641 |
| Rural population (%) | 0.384 | 0.378 | -0.006 | -0.029 | 0.161 |
| Human Development Index | 0.691 | 0.690 | -0.001 | -0.011 | 0.384 |
| GDP per capita | 12.537 | 12.730 | 0.193 | 0.011 | 0.597 |
| Welfare recipients per capita | 0.104 | 0.104 | 0.001 | 0.002 | 0.905 |
| Illiteracy (%) | 0.231 | 0.232 | 0.001 | 0.011 | 0.395 |
| Post-secondary education (%) | 0.031 | 0.031 | -0.000 | -0.004 | 0.838 |
| Has local media | 0.696 | 0.706 | 0.009 | 0.021 | 0.374 |
| Mayor term limited | 0.314 | 0.314 | 0.000 | 0.000 | 0.993 |
| Previous incumbent vote (%) | 0.143 | 0.146 | 0.003 | 0.020 | 0.394 |
| PT incumbent | 0.079 | 0.076 | -0.002 | -0.008 | 0.720 |
| PSDB incumbent | 0.153 | 0.140 | -0.014 | -0.038 | 0.093 |

**Table 1.1: Comparing non-audited and audited municipalities along selected covariates within state-term strata**

*Note:* Weighted means calculated based on the number of non-audited and audited municipalities within strata. An omnibus $\chi^2$ test shows no evidende against the null hypothesis of overall balance ($\chi^2 = 14.37$, $df = 13$, $p = 0.35$).

Taagepera 1979), with lower values indicating more concentration in public spending.

Since the unit of analysis is the municipality-term and the goal of this chapter is to capture incumbents' attempt to preserve their reelection chances, I focus primarily on the effect of audits on total per capita spending and budget concentration in the fourth year of the mayoral term, which corresponds to the election year. Figure 1.2 shows the distribution of both outcomes, distinguishing across election years. Albeit with a relatively small deviation in 2004, total spending tends to be lower and budgets less concentrated, the distributions of the outcomes are relatively similar over time. The figure also shows that total spending has a long right tail, which means observations with high values may leverage the results. I address this complication by estimating effects on the natural logarithm transformation of this outcome.

In subsequent analyses, I examine the effect of audits on both outcome variables in all mayoral term-years separately. I also examine the effect of audits on public spending in each budget category separately to examine whether the changes in spending in reaction to increased monitoring follow a systematic pattern across municipalities.[12]

I estimate the effect of audits on spending outcomes using OLS regression with term fixed-effects and clustered standard errors by term. Ideally, I would also account for the fact that audits are randomized within states. However, with no more than 60 audits per lottery across 26 states, many of the state-term blocks would have too few audited municipalities to calculate a meaningful treatment effect, so this is as close as I can get to the ideal estimation strategy. The next section reports results using figures. Section A.2 of Appendix A shows the underlying numerical results.

12. In this case the transformation is $\ln(y + 1)$ with $y$ being the outcome in question.

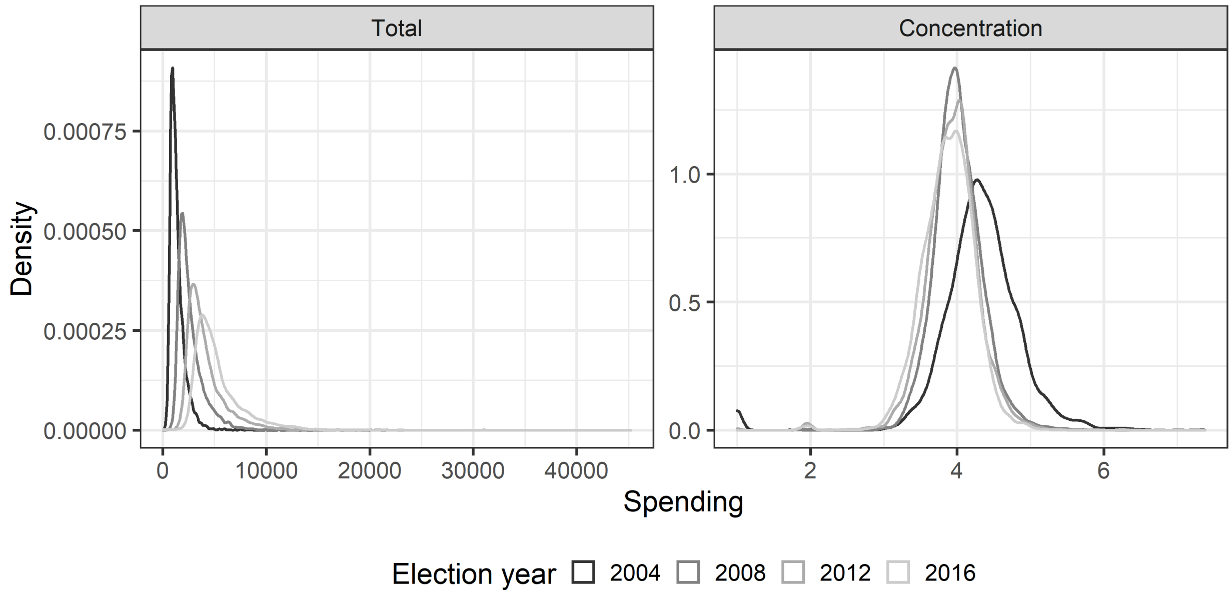


**Figure 1.2: Distribution of total spending per capita (left) and budget concentration (right)**
*Note:* Colors denote election years. Panels have different scales in both axes.

## 1.5 Results

### 1.5.1 Main results

Figure 1.3 shows the effect of audit selection on both total spending per capita (logged) and budget concentration during the election year, showing separate effects depending on whether the incumbent mayor faces a term-limit. The effect of auditing on both outcomes is different from zero among reelection-eligible mayors but indistinguishable from zero among term-limited mayors. Reelection eligible mayors in audited municipalities spend, on average, 6% less than non-audited mayors with reelection incentives. In terms of budget concentration, reelection-eligible mayors that are audited concentrate their spending in about 0.4 fewer budget areas, on average, than non-audited municipalities. As a reference, the latter effect corresponds to a change of almost 9% of a standard deviation in the distribution of the effective number of budget areas in the entire sample.

These results suggest that increased monitoring has an effect on spending amount and composition only in municipalities with incumbents that reelection incentives. I interpret this finding as evidence in favor of the argument that mayors investigated for potential corruption try to signal fiscal responsibility through reduced spending. The effect on budget concentration also suggests that this reduction comes at the expense of some budget categories but not others, presumably those that are more visible. The next sub-section addresses possible gaps in this interpretation.

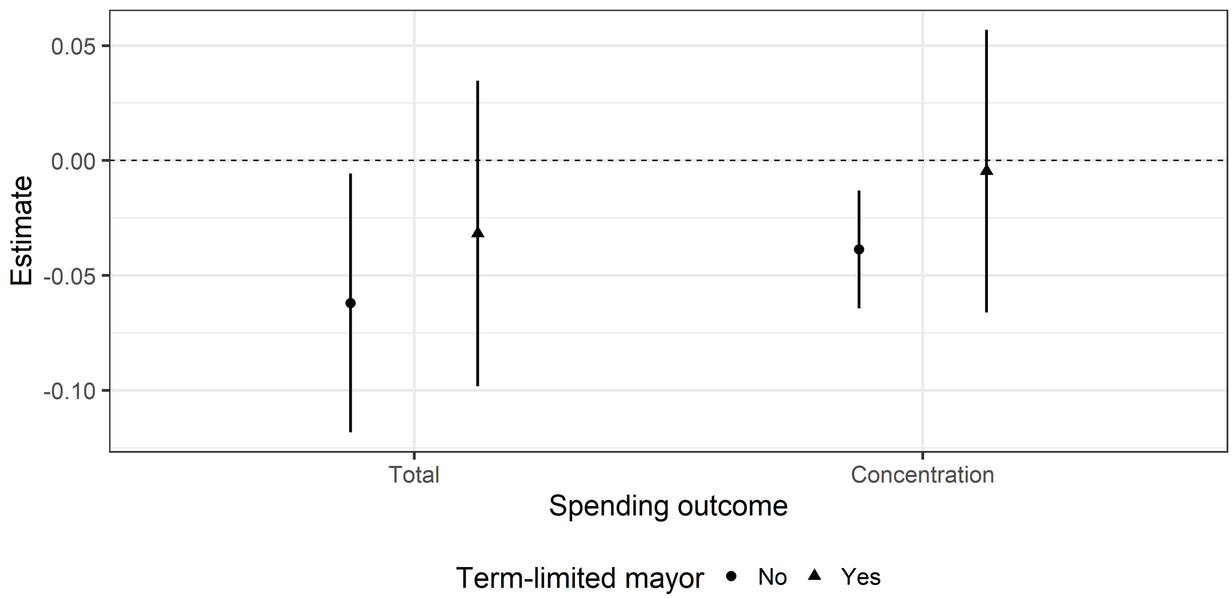


**Figure 1.3: Effect of audit selection on election-year (logged) total spending per capita (left) and budget concentration (right) by incumbent term-limit status**

*Note:* Outcomes are not scaled. Based on OLS regression with term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals.

### 1.5.2 Additional results

#### Audit timing

The main argument in this chapter is that audited mayors with reelection incentives reduce spending close to an election in anticipation to electoral sanctions from voters. Two alternative explanations also follow from the same empirical pattern. First, the reduction in spending may not come from incumbents' own devices, but rather as punishment from the federal government. Previous work shows that municipalities wherein corruption was uncovered in the context of the CGU program receive lower transfers from the federal government afterwards (Brollo 2011), which in turn may lead to an overall decrease in spending. This critique is already addresses in part in Figure 1.3, since we would observe a negative effect of auditing on spending among both reelection-eligible and term-limited mayors.

I focus on the timing of audits to provide further evidence for the interpretation in this chapter. The political budget cycles literature suggests that elected officials structure the spending so that figures look more attractive come election year (Aaskoven and Lassen 2017). The underlying logic is that voters pay more attention to incumbent behavior in office as elections approach, and tend to use election-year information to make judgments about an incumbent's performance throughout the term (Healy and Lenz 2014). In the context of this chapter, this implies that increased monitoring should have a more pronounced effect on spending outcomes as audits happen closer to the election, since incumbents expect their constituencies to be more receptive to information about their performance in office.

Figure 1.4 addresses this implication by estimating the effect of auditing timing (against the baseline of no audit) on election year spending outcomes, once again distinguishing whether the incumbent is term-limited. Auditing has non-zero effects on total spending per capita among reelection eligible mayors only when they occur in year 3 or 4 in the mayoral term, while the effect remains indistinguishable from zero among term-limited mayors. For budget concentration, auditing has non-zero effects among reelection-eligible mayors in year 3. Although the confidence interval covers in year 4, the p-value of the test against the null hypotheses of zero effect is close to the conventional cutoff for statistical significance ($p \approx 0.08$). Auditing in year 4 of the term also has a nearly non-zero effect on total spending per capita ($p \approx 0.07$) and a non-zero effect on budget concentration among term-limited mayors. I attribute this to the possibility that some term mayors choose to run as candidates to city council elections, which happen concurrently with mayoral elections. In this case, incumbents may still try to anticipate backlash from voters, but only after they have committed to participate in elections, which explains why the effects appear only when audits occur in the last year of the term.[13]

**Spending across the term**

The second alternative explanation the reduction in spending does not come from incumbents anticipation of voter sanctions, but from voter sanctions themselves. Because some Brazilian municipalities have limited capacity to enforce local tax collection, citizens may choose to retaliate against audited incumbents found as corrupt in audits by not paying local property taxes (Timmons and Garfias 2015). This alternative explanation also aligns with the pattern in Figure 1.4, since voters pay more attention to incumbent performance, and therefore are more likely to avoid paying taxes, as elections approach.

I address this alternative explanation by estimating the effect of audit timing across the mayoral term on spending outcomes throughout the term. Mayors in Brazil set budgets a year in advance, so citizen sanctioning through tax collection in year $t$ can only affect outcomes starting on $t + 1$. In turn, audits on the first year of the mayoral term can affect spending outcomes on years 1 through 4, and on the opposite side, audits on year 4 can only affect spending outcomes in that same year. By examining the effect of audit timing on spending outcomes across the mayoral term, I can determine whether incumbents' response to increased monitoring has the delay implied by this alternative explanation.

Figure 1.5 shows the effect of audit timing on spending outcomes across the term among municipalities

13. Elections in Brazil usually happen in October, candidates must announce their decision to run a year in advance. Therefore, a mayor audited in year 4 of the term knows for sure whether they will participate in the upcoming election.

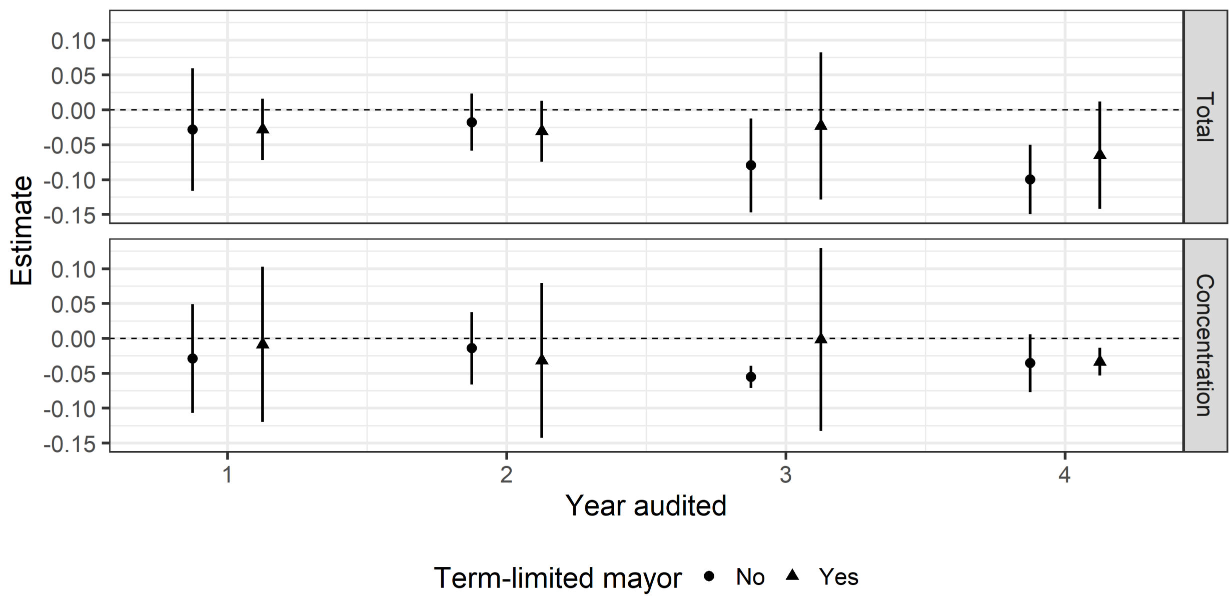


**Figure 1.4: Effect of audit timing on election-year (logged) total spending per capita (top) and budget concentration (bottom) by incumbent term-limit status**

*Note:* Outcomes are not scaled. Based on OLS regression with term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals.

with reelection-eligible mayors.[14] Audits have a non-zero effect on total spending per capita only in year 4 and only when they happen in year 3 or 4. While this pattern does not fully discard the alternative explanation of citizen sanctioning, it suggests that at least part of the effect of audits can be attributed the incumbents' attempt to mitigate electoral sanctions, since citizen sanctioning through tax avoidance in year 4 can only affect spending on the first year of the subsequent term.

The picture is not as clear for the effect of audit timing on budget concentration. I only find clear non-zero effects of audits on budget concentration in the same year during the first year of the term, and only suggestive evidence in the remaining years ($p \approx \{0.1, 0.08, 0.07\}$, respectively). This hints at the prospects of top-down sanctioning. I also find non-zero effects of audits on budget concentration in the next year in years 1 and 3. The point estimate suggest a larger effect size of auditing in year 3 on budget concentration in year 4, which hints at the possibility that, when timing allows changes in the budget, mayors adapt the composition of spending during election years. The confidence intervals suggest that the effect of auditing in year 3 on budget concentration in the next year is different from the analogous quantity in year 1, which suggests that some of the effect can be attributed to an attempt to anticipate electoral sanctions.

14. After an audit, a municipality can only be selected for auditing again after one year. In the entire data, there are 14 instances of municipalities being audited twice during the same mayoral term. I exclude those from this part of the analysis.

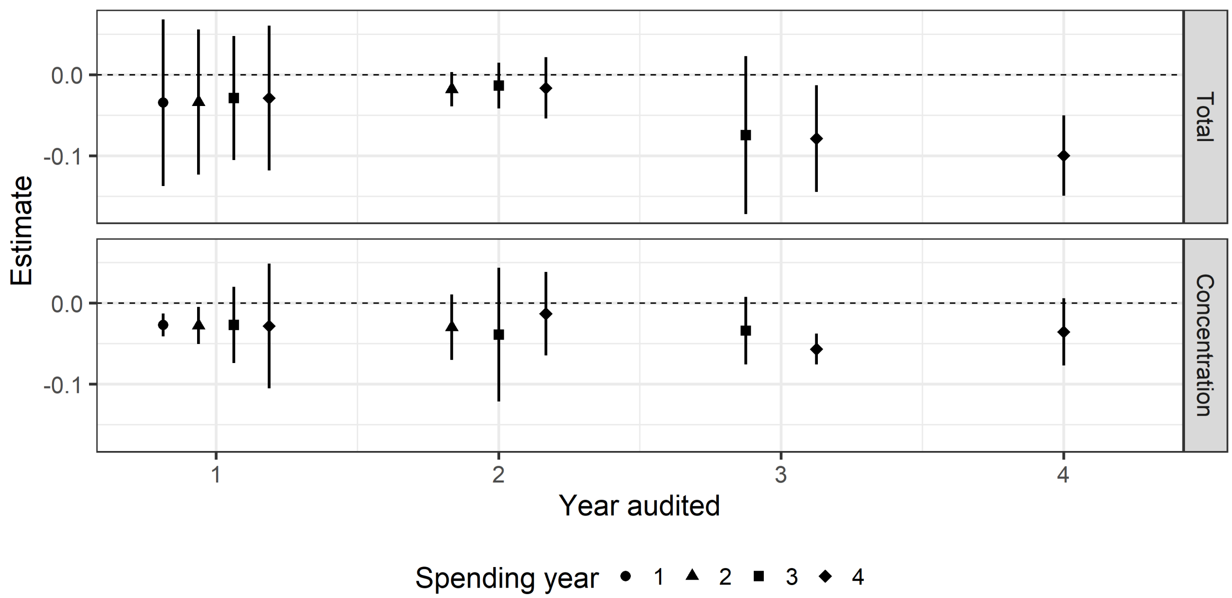


**Figure 1.5: Effect of audit timing on (logged) total spending per capita (top) and budget concentration (bottom) across the mayoral term**

*Note:* Outcomes are not scaled. Results restricted to municipalities with reelection-eligible mayors. Based on OLS regression with term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals.

### Disaggregating budget categories

Taken together, the results in Figures 1.3-1.5 suggest that mayors reduce spending in an attempt to anticipate the electoral consequences of increased monitoring, and that this decrease comes at the expense of some budget categories, but not others. If, as this chapter argues, mayors reduce spending in some areas to signal fiscal responsibility, then one should expect the decrease to be more pronounced in areas that are more visible to voters.

Figure 1.6 addresses this implication by estimating the effect of auditing on the election-year spending across 20 budget categories in municipalities with reelection-eligible incumbents.[15] Since some municipalities may report zero spending in some categories, in this case the transformation is $\ln(y+1)$ where $y$ is the total spending in each budget category. Moreover, since I estimate the effect of audits on 20 outcomes simultaneously, I need to account for the possibility of non-zero results emerging by chance. For each estimate, I calculate false discovery rate (FDR) adjusted p-values (Benjamini and Hochberg 1995). The figure indicates in black color which estimates have FDR-adjusted p-values smaller than the conventional significance cutoff of 0.05, in which case I interpret that the corresponding non-zero estimate did not appear by chance.

15. I report estimate effects on 20 out of a total of 21 budget categories because I omit the category of regional development since all municipalities record zero spending in this outcome.

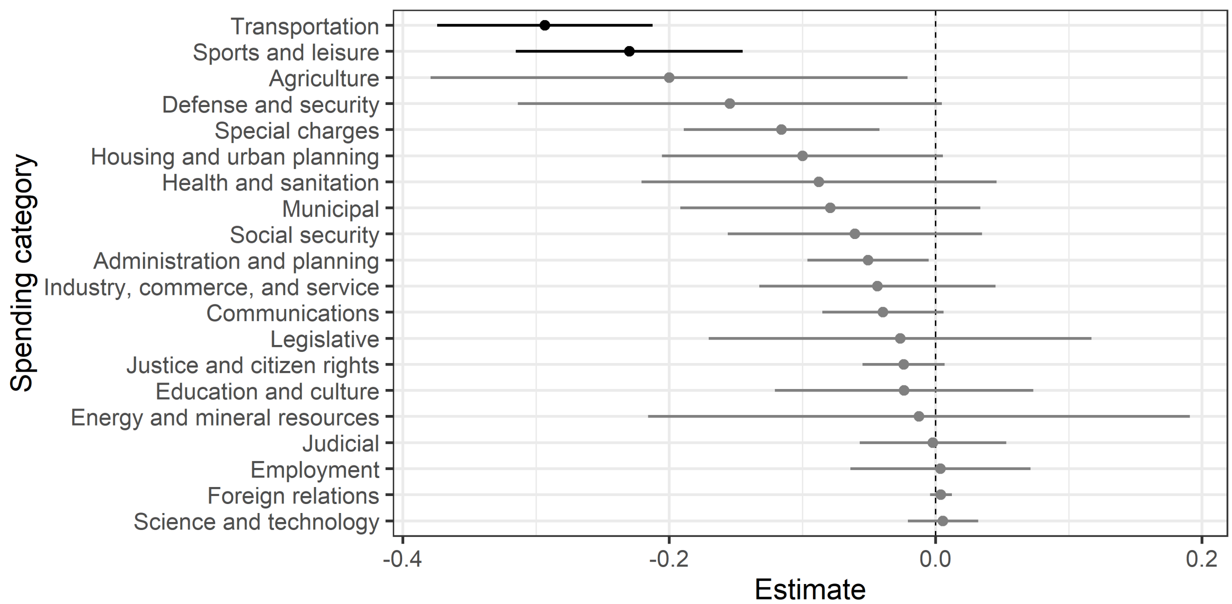


**Figure 1.6: Effect of audit timing on election-year spending per capita across budget categories**
*Note:* Outcomes transformed by $\ln(y+1)$ where $y$ is the value of each outcome. Results restricted to municipalities with reelection-eligible mayors. Based on OLS regression with term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals. Black color indicates estimates with false-discovery rate adjusted p-values smaller than the conventional 0.05.

The figure sorts estimates in decreasing order and shows negative non-zero effects of auditing on the spending categories of transportation and sports and leisure. Note that this does not imply that auditing does not affect spending in other categories, only that transportation and spending are visible enough to detect an overall effect. The extent to which other areas can be considered visible may vary across municipalities. Transportation is a feasible candidate for a visible spending category since it includes projects on the construction, maintenance, and improvement of roads and related infrastructure. The same applies to sports and leisure, Brazil hosted five major international sports event in the period under study, which involved considerable investment in infrastructure across the country (Carneiro et al. 2019).[16] In addition, under this category also fall infrastructure projects aimed at increasing the everyday accessibility to sports and leisure.

As previous work on the CGU program shows, corruption is common in the delivery of public goods and services and in public works (Ferraz and Finan 2011), yet while spending in public goods and services affects only some population subgroups (e.g. spending in social security is only visible to welfare program recipients), infrastructure projects can reach a larger audience. Therefore, the most likely way in which mayors with incentives to mitigate the negative electoral consequences of increase monitoring is through

16. The 2007 Pan American Games, 2011 Military World Games, 2013 FIFA Confederations Cup, 2014 FIFA World Cup, and the 2016 Summer Olympics.

spending in budget categories in which infrastructure projects are common and visible.

## 1.6 Conclusion

This chapter argues that elected officials with reelection incentives react to increased monitoring by reducing public spending and concentrating it in fewer budget areas in an attempt to preserve their reelection chances. I argue that this occurs because increased monitoring brings attention to incumbents' performance in office, which in turn creates incentives for incumbents to signal fiscal responsibility to voters. I show evidence in favor of this argument with data from an anti-corruption program in Brazil that randomly selects municipalities to audit their use of federal funds. Mayors audited in the context of this program reduce public spending and concentrate it into fewer budget categories as elections approach. Moreover, these effects are more pronounced in municipalities with mayors eligible for reelection (as opposed to term-limited) and when audits happen close to or during an election year. This reduction in spending comes primarily from the budget areas of transportation and sports and leisure, both featuring infrastructure projects that affect voters across all municipalities, which suggests that the incentive is to signal fiscal responsibility in highly visible budget areas.

Taken together, these results further our knowledge on the electoral consequences of corruption by suggesting that incumbents adapt their behavior in office in reaction to increased monitoring to anticipate potential electoral sanctions from their constituencies. This means elected officials can still be responsive to voters even if previous research shows mixed evidence on the effect of information about corruption on incumbent vote shares (Incerti 2020; Boas, Hidalgo, and Melo 2018). However, the findings in this chapter also imply that corrupt politicians have yet another strategy to get away with corruption. More generally, this chapter highlights the importance of considering the unintended consequences of anti-corruption interventions that may arise from politicians strategic reaction (Fisman and Golden 2017). This does not imply that other forms of top-down (Brollo 2011) and bottom-up (Timmons and Garfias 2015) sanctioning do not exist. However, the results in this chapter in combination with section A.3 of Appendix A suggest that increased monitoring is sufficient to trigger the anticipatory behavior described in this chapter, while other avenues through which increased monitoring can affect public spending depend on the level of corruption uncovered.

This chapter also has implications for the literature on political budget cycles (Aaskoven and Lassen 2017). By showing that the incentive in reaction to increased monitoring is to reduce spending, this chapter highlights how exogenous events, such as an unexpected audit, can shift the equilibrium in how politicians balance different electoral considerations through fiscal policy. While previous work suggests that Brazilian mayors with reelection incentives either increased spending (Sakurai and Menezes-Filho 2008) or reduce the tax burden on their constituencies (Klein and Sakurai 2015), research in other contexts suggests that politicians face a trade-off between pleasing voters who prefer more targeted spending and those who prioritize fiscal responsibility (Drazen and Eslava 2010). By bringing attention to incumbents' overall performance in office, increased monitoring (especially when unexpected) can tilt the balance in favor of signaling fiscal responsibility as a viable strategy to win reelection.

# Chapter 2

# How Politicians Mitigate the Electoral Consequences of Nearby Corruption

## 2.1 Introduction

Governments, civil society organizations, and scholars spend considerable resources implementing and evaluating anti-corruption interventions that seek to bridge the gap between voters and politicians' performance in office. However, the cumulative evidence suggests that voters rarely punish corrupt politicians (Incerti 2020). Even in the 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 electoral behavior in response to corruption Avis, Ferraz, and Finan 2018.

While most research on the electoral consequences of corruption focuses on understanding why voters fail to sanction corruption (see De Vries and Solaz 2017 for a review), recent work suggests that politicians adjust their behavior in anticipation of expected electoral sanctions, suggesting that lack of sanctioning does not imply lack of accountability. Parties choose not to renominate politicians implicated in corruption scandals in elections (Asquer, Golden, and Hamel 2019), whereas elected officials at the local level avoid association with corrupt parties by switching parties or choosing not to seek reelection (Daniele, Galletta, and Geys 2020). These findings illustrate how politicians avoid association with corruption, yet because the connection is the political party, they cannot disentangle whether this behavior is a response to expected electoral sanctions or an attempt to avoid involvement in future legal investigations, since the same empirical pattern could respond to either electoral accountability or top-down sanctioning mechanisms.

To disentangle between these two explanations, this chapter focuses on the electoral consequences of exposure to information about nearby corruption at the local level. I argue that exposing corruption has spillover effects on the behavior of incumbents eligible for reelection. Exposure to nearby corruption (as opposed to same-party corruption) creates an opportunity to evaluate whether (1) incumbents with reelection incentives are more likely to try to avoid association with corruption and (2) whether they do so at differential rates depending on the distribution of same-party nearby corruption.

One challenge to the study of spillover effects in this context is how to define what "nearby" means. Most of the tools to study spillovers assume that the researcher observes how units are connected, so there is a clear pathway to model spillovers (for reviews, see Aronow et al. 2021 and Halloran and Hudgens 2016). For the purposes of this chapter, that implies making a statement about how municipalities are connected so that exposing corruption in one place affects the outcome in others. One cannot make that connection without additional assumptions that do not follow from one's theory. Therefore, I approach the operationalization of "nearby" as a model selection problem in supervised machine learning (see Chapter 3 for details). Using cross-validation, I find a plausible range for the upper bound at which exposure to information about corruption politicians' strategic decisions in nearby localities.

I evaluate the effect of exposure to nearby corruption using data from a long running anti-corruption program in Brazil, created using a novel combination of text-as-data and supervised learning tools to overcome researcher bias in the coding of corruption. 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 infractions increase the probability that the incumbent mayor will seek reelection under a different party. These effects only appear in the subset of municipalities exposed to nearby corruption that are not audited themselves, which suggests that this behavior is a viable strategy only when voters do not have access to their own incumbent's corruption record.

I also evaluate second order implications to provide additional evidence in favor of the electoral accountability mechanism. 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 chapter are distinct from the potential of top-down sanctions. I also show that nearby corruption does not affect the rates at which incumbents seek or win reelection, which suggests an incentive to anticipate electoral sanctions, as opposed to mere opportunistic behavior. Finally, I describe how reelection-eligible mayors choosing not to switch parties win elections less often as nearby corruption increases.

This chapter 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 chapter extends on recent work suggesting this mechanism but not testing it directly (Asquer, Golden, and Hamel 2019; Daniele, Galletta, and Geys 2020).

Second, this chapter applies a novel approach to study spillovers in observational studies, which I describe in more detail in Chapter 3. Approaching spillovers as a model selection problem in supervised learning has the advantage of avoiding modeling assumptions that do not follow from theory and minimizes the bias that emerges from not knowing the true pathway through which spillovers occur. The researcher still has to make decisions about the pathways through which spillovers travel (in this chapter, I assume spillovers travel through geography), but instead of committing to a specific model, an algorithm suggests a range of plausible models based on the data.

Third, this chapter 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 documents 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.

## 2.2 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.

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

Current explanations for the limited electoral sanctions emphasize the circumstances in which voters choose not to punish corruption. One prominent explanation is that voters forgive corruption when politicians satisfy their expectations in other areas.[1] Evidence from survey experiments and observational studies suggests that voters forgive corruption among co-partisans (Anduiza, Gallego, and Muñoz 2013; Eggers 2014) or when it comes with positive economic outcomes (Fernández-Vázquez, Barberá, and Rivero 2016; Konstantinidis and Xezonakis 2013; Muñoz, Anduiza, and Gallego 2016; Pereira and Melo 2015).[2] Another explanation is that, in the absence of viable alternatives to replace corrupt politicians, corruption demobilizes voters (Boas, Hidalgo, and Melo 2018; Chong et al. 2015; Pavão 2018).

Recent work proposes another alternative explanation, politicians anticipate electoral sanctions and manipulate these factors to counteract voter punishment. Research on local elections in Brazil evaluates this mechanism indirectly, showing how parties in municipalities where audits reveal high corruption present more-educated candidates (a proxy for candidate quality) in city council elections (Cavalcanti, Daniele, and Galletta 2018). Recent work on Italy's Clean Hands scandal also suggests that politicians avoid association with corruption, arguing for (but not proving) an electoral accountability mechanism. Asquer, Golden, and Hamel (2019) show how parties attempt to protect their public brand by avoiding the renomination of legislators who face extensive media coverage around corruption. Daniele, Galletta, and Geys (2020) show how local politicians from parties implicated in the scandal are less likely to seek reelection and more likely to switch parties.

Research showing that politicians avoid association with corruption suggests electoral accountability as the underlying mechanism. Parties and individual politicians avoid the connection with corruption to safeguard their reputations and future electoral chances. However, research from Brazil suggests that, if anything, anti-corruption interventions succeed in reducing subsequent corruption because politicians are responsive to the prospect of future investigation or legal sanction (Avis, Ferraz, and Finan 2018). Conversely, bottom-up punishment through voting behavior or local tax revenue tends to be short-lived (Rundlett 2018; Timmons and Garfias 2015).

In other words, research that focuses on the party as the connection between corrupt politicians and those trying to avoid association with them cannot disentangle whether between the bottom-up and top-down

1. Another prominent explanation that I do not discuss here is voters ignoring information about corruption when the source is not credible (Botero et al. 2015; Weitz-Shapiro and Winters 2017; Winters and Weitz-Shapiro 2018).

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

sanctioning. To illustrate this limitation, consider case of Rodrigo Neves, former mayor of the municipality of Nitéroi in the state of Rio de Janeiro. In the leadup to the 2016 local election in Brazil, Neves switched allegiance from the Worker's Party (PT) to the opposition Green Party (PV). As media coverage suggested at the time, the purpose of this move was to improve his electoral chances by avoiding association with president Dilma Rousseff's administration in the midst of corruption allegations that affected public opinion negatively and eventually lead to her impeachment.[3]

Neves would go on to secure reelection under the new party. However, halfway into his term he was detained and prosecuted as part of the investigation around the *Lava Jato* (Operation Car Wash) scandal.[4] This can be interpreted in two ways, either Neves originally tried and failed to avoid investigation, or the fact that he was investigated anyway suggests that improving electoral performance was the original goal because party switching does not protect politicians from legal consequences.

## 2.3 The Effect of Exposure to Nearby Corruption

To overcome the difficulties of using the political party as the connection, I focus on exposure to information about nearby corruption. I argue that politicians react to nearby corruption in anticipation of electoral sanctions. Why would politicians expect their constituencies to hold them accountable for corruption exposed in other places? The broader literature on electoral accountability suggests that voters hold politicians accountable for events that are outside their control (Achen and Bartels 2016; Gasper and Reeves 2011; Healy and Malhotra 2009, 2010, 2013). Findings in the domain of performance-based voting in Latin America also point in this direction. Voters hold local governments accountable for the national economic performance (Remmer and Gélineau 2003), and sanction elected officials for the performance of their staff members (Winters and Weitz-Shapiro 2016). While this line of work cannot produce a definitive judgment on voter rationality Gailmard and Patty 2018, it does imply that voters rely on informational shortcuts to infer incumbent performance.

This suggests that politicians would expect voters hearing about nearby corruption to update their priors about the likelihood of corruption in their own locality and pay more attention to their own incumbent's performance, yet it does not guarantee that voters will hold incumbents accountable for nearby corruption. Public opinion data from Slovakia suggests that anti-corruption voting is only possible when personal experience or sociotropic perceptions make the issue salient in voters' minds (Klašnja, Tucker, and Deegan-

3. For details, see https://oglobo.globo.com/rio/bairros/rodrigo-neves-decide-deixar-pt-pdt-pmdb-saopossiveis-destinos-18915753 and http://g1.globo.com/rio-de-janeiro/eleicoes/2016/noticia/2016/10/rodrigoneves-do-pv-e-reeleito-prefeito-de-niteroi-rj.html (in Portuguese).

4. See https://g1.globo.com/rj/rio-de-janeiro/noticia/2018/12/10/forca-tarefa-no-rj-faz-operacao-para-prender-rodrigo-neves-por-desvio-de-dinheiro.ghtml for details (in Portuguese).

Krause 2016). Exposure to nearby corruption may serve a similar purpose by priming voters about their own incumbent's corruption record. However, nearby corruption may also contribute to the perception that corruption is widespread. Previous work using survey data and focus groups in Brazil suggests that the perception of corruption being pervasive leads voters to believe that all politicians are implicated with it, which in turn prevents them from identifying credible alternatives to replace corrupt politicians and makes them less likely to sanction corruption (Pavão 2018). A survey experiment in Spain supports this argument by showing how voters only sanction corruption when a clean alternative is available (Agerberg 2020). However, survey experiments in Argentina, Chile, and Uruguay suggest that perceptions of widespread corruption do not mitigate voters' intention to sanction it (Klašnja, Lupu, and Tucker 2020). For the purposes of this chapter, I assume that nearby corruption, at least in average, leads politicians to believe that voters in their constituency are more likely to hold them accountable.

If incumbents expect accountability for nearby corruption, then they should update their behavior in office to counter potential electoral sanctions. While incumbents revealed as corrupt can only achieve this by improving their performance in other areas, for example, through better economic performance (Fernández-Vázquez, Barberá, and Rivero 2016; Konstantinidis and Xezonakis 2013; Muñoz, Anduiza, and Gallego 2016; Pereira and Melo 2015), incumbents exposed to nearby corruption may resort to party switching as a more cost-effective alternative. At the federal level in Brazil, legislators switch parties to further their policy and career goals (Desposato 2006). While mayoral candidates must run under a party brand, weak parties and poor accountability at the local level combine to produce anti-incumbent party bias among voters (**Klasnja20**), which in turn creates incentives for incumbent mayors to switch parties in search for more resources to secure reelection (Novaes 2017). Therefore, incumbent mayors who experience higher scrutiny from voters as a result of exposure to nearby corruption have higher incentives to switch parties, as opposed to devoting their own resources, to secure reelection.

Under what circumstances would politicians react to nearby corruption? I argue that party switching is not a viable strategy among incumbents that are already being investigate for corruption, since allegations or ongoing investigations will not disappear just because the incumbent switched parties, and as previous research shows (Asquer, Golden, and Hamel 2019), party organizations avoid association with corrupt politicians to protect their brand. In other words, I expect exposure to nearby corruption to have an effect only among those mayors who are not exposed as corrupt themselves.

## 2.4 Research Design

### 2.4.1 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 varying range of budget areas 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 effect at its early stage. Timmons and Garfias (2015) remark 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$^{th}$ lottery in 2006, the CGU included explicit corruption categories in the reports, classifying each infraction as mismanagement, moderate infraction, or 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, Ferraz, and Finan (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 presence of local media (Ferraz and

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 changed to include both random and non-random audits.
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 B.1 of Appendix B replicates the findings from Rundlett (2018) with the data used in this chapter, showing a similar trend.
9. In Portuguese: *falha formal*, *falha média*, and *falha grave*.

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$^{\text{th}}$ 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 B.1 in Appendix B 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 outliers with a large number of infractions. 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 B.9 in Appendix B disaggregates results by election year and shows that findings do not depend on machine-coded corruption.

### 2.4.2 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 2.1 shows the cross-tabulations 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 suggests that party mayors seeking reelection under a different party tend to do so to improve their electoral chances.

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 2.1: Distribution of mayors eligible for reelection that seek reelection, switch party, and win reelection**

### 2.4.3 Explanatory Variables

#### 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 an audited municipality, but still are close to one. I could expand the definition of nearby to include more municipalities, but without a standard to determine the appropriate range I would not know where to stop.

To overcome this difficulty, I approach the operationalization of nearby as a model selection problem in supervised learning (see Chapter 3 for details). The task is to identify an upper bound within which information about nearby corruption has an effect on party switching rates. I count the number of infractions per audited neighbor using increases contiguity order upper bounds.The most parsimonious operationalization counts corruption among those neighbors with which a municipality shares a border (first order), the second order includes infractions among first order neighbors and the neighbors or the neighbors, and so on up to the tenth order of contiguity.[11]

I choose to count nearby infractions per audited neighbor up to the tenth order of contiguity as a reasonable upper bound for spillovers. Figure B.2 in Appendix B shows the distribution of neighbors and audited neighbors by contiguity order. The average number of neighbors at the first order of contiguity is 5.8, out of which an average of 1.4 are audited. At the tenth level of contiguity, the average number of neighbors is about 93.2 and the average number of audited neighbors is 8.5. Figure 2.1 shows the distribution of nearby corruption under the different operationalizations of nearby.

Each one of these operationalizations implies a different OLS regression model of nearby corruption on party switching, also including an interaction with a municipality's own audit status and election-year fixed effects to account for time variance in the outcome. I select among models by computing their root mean

11. I create each of these operationalizations using queen contiguity, which implies municipalities are neighbors if they share a border in any direction.

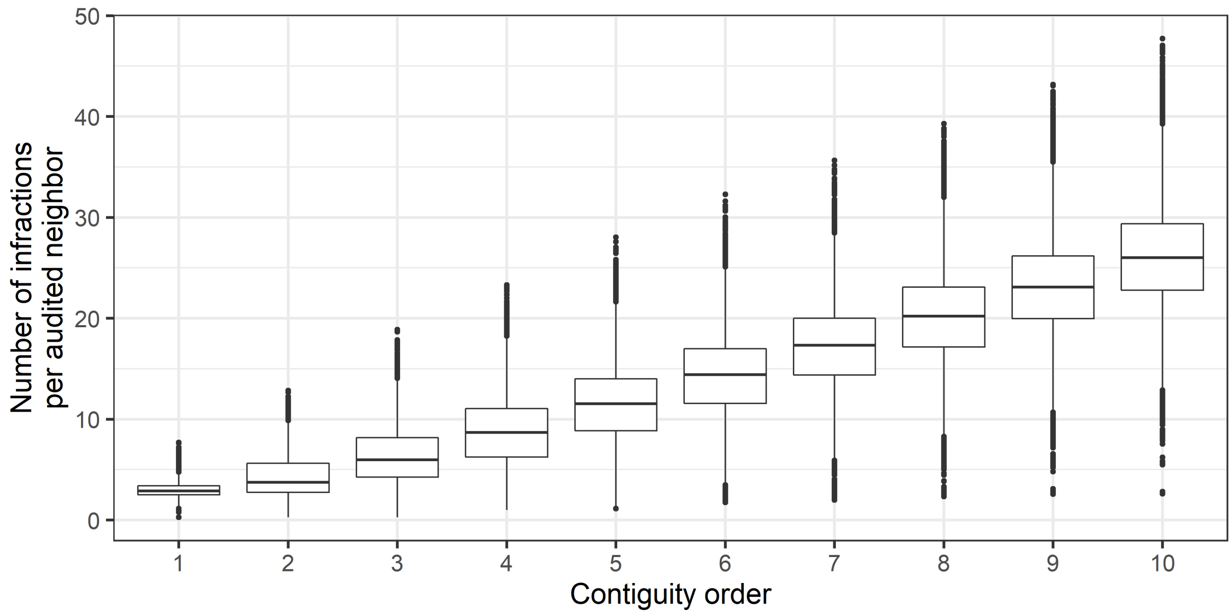


**Figure 2.1: Distribution of nearby corruption infractions at different orders of contiguity**

squared error (RMSE) via 10-fold cross validation. Table B.2 in Appendix B summarizes the result of this procedure. The model that minimizes RMSE is a plausible candidate for an appropriate operationalization of nearby. A standard practice to in supervised learning to avoid overfitting is to choose the most parsimonious model with an RMSE within one standard deviation from the minimum (Hastie, Tibshirani, and Friedman 2009). In this application, the model that minimizes RMSE counts nearby corruption up to fifth contiguity order, and the most parsimonious model with RMSE within one standard deviation from the minimum counts nearby corruption up to the second. I consider all models within this range as plausible definitions of nearby. I interpret results based on this range, yet for transparency I report the main results using all the pre-specified operationalizations of nearby.

### Audit status

I argue that exposure to nearby corruption does not affect party switching among mayors in municipalities where voters have access to credible information about their own incumbent's corruption record. To capture this heterogeneous effect, I use a binary indicator of whether a municipality had an audit report released before the election. In the sample of 16,917 municipality-election years with at least one audited neighbor within ten neighbors apart, 1,708 (about 10%) are audited themselves.

Despite the difference in proportions, since audits are randomly assigned within audit waves, audited and non-audited municipalities are not different from each other in expectation.[12] Table 1.1 in Chapter

12. Moreover, once audited, a municipality cannot be audited within the next year.

1 compares non-audited and audited municipalities along selected covariates. In short, audited and non-audited municipalities are balanced in most of the observed covariates, and in the cases where they are not, the differences are negligible.

## 2.5 Results

### 2.5.1 Main results

Figure 2.2 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. The shaded region indicates the optimal operationalizations of nearby based on the model selection process described in the previous section.[13]

Figure 2.2 illustrates the importance of avoiding a narrow definition of nearby. 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 wide confidence interval in the subset of audited municipalities. Within the range of plausible upper bounds, the effect of nearby corruption is different from zero among non-audited municipalities, but indistinguishable from zero among audited municipalities. In average, and using the most parsimonious definition of nearby within the optimal range, a one standard deviation increase in nearby corruption increases the party switching rate by two percent.[14]

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 in the cases 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. Rather than addressing every possible unobserved confounder individually, I choose

13. Section B.2 in Appendix B shows tables with the numerical results underlying the results in this chapter. Table B.8 in the same appendix shows that results are similar when using logistic regression.

14. As a benchmark, the validation exercise in Table B.1 of Appendix B suggests that one standard deviation increase in corruption in 2004 (the only year that exhibits a non-zero effect) decreases incumbent party vote shares by about 2.7 percent.

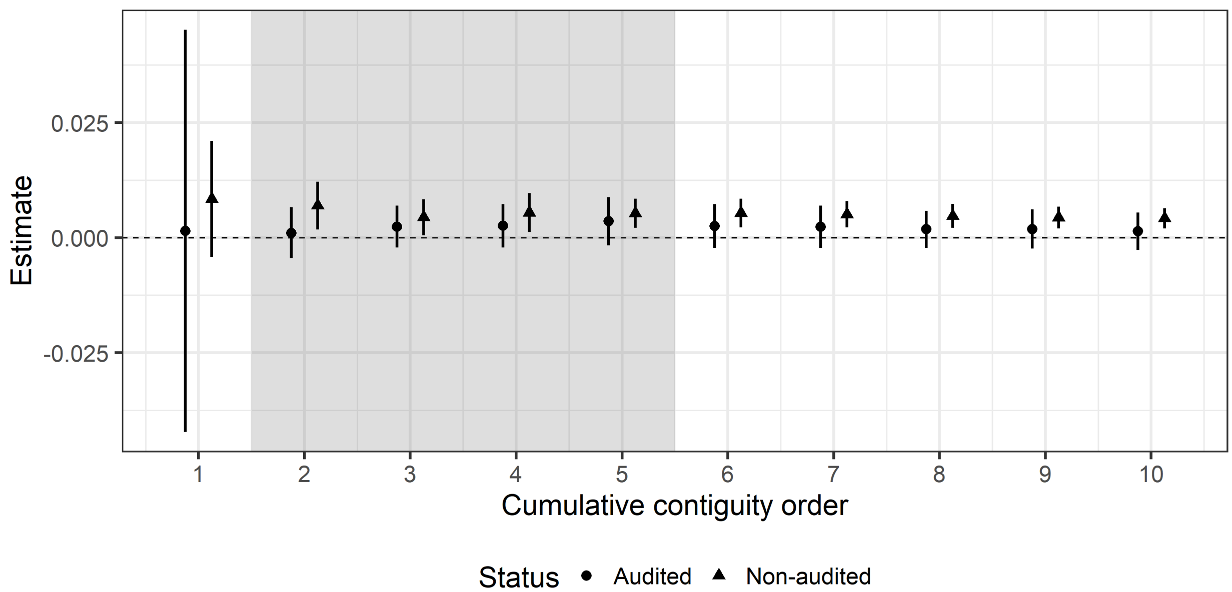


**Figure 2.2: Effect of nearby corruption on incumbent mayor party switching**
*Note:* Based on OLS regression with election year fixed effects and clustered standard errors by election year. The shaded region denotes the optimal range suggested by cross-validation. Vertical lines denote 95 percent confidence intervals.

to address the omitted variable bias critique in general (Cinelli and Hazlett 2020). Figure B.3 in Appendix B shows that, given the current model specification, 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 in any of the optimal operationalizations of nearby into zero.

In the next sub-section, I explore the merit of alternative interpretations of the findings and a second order implication of the electoral accountability mechanism.

### 2.5.2 Additional results

#### Effects on seeking and winning reelection

An alternative explanation for Figure 2.2 is that exposure to information about nearby corruption creates a favorable public opinion environment for incumbents, especially when they are not audited themselves, which is the opposite of the argument in this chapter and implies that incumbents either seek or win reelection more often. If this is true, nearby corruption may affect party switching by construction, since the outcome is measured as whether the incumbent seeks reelection under a different party.

Figure 2.3 explores this possibility by estimating the effect of nearby corruption in interaction with audit status on whether the incumbent mayors seeks and wins reelection as separate outcomes. Within the range suggested by cross-validation, the effect of nearby corruption is indistinguishable from zero regardless of audit status. I interpret this as evidence against the argument that nearby corruption promotes incumbents' reelection chances.

This finding also lends support to the interpretation that mayors switch parties more often as nearby corruption increases in an attempt to avoid the consequences of increased scrutiny on their own performance. If the relationship between nearby corruption and party switching responded to strategic behavior from incumbents seeking to take advantage of the misfortune of others by securing a better platform for reelection, then one would also observe a positive effect of nearby corruption on how often incumbents win elections.

#### Exposure to same-party nearby corruption

Another alternative interpretation of the main results is that politicians react to nearby corruption not in anticipation of electoral punishment, but rather to avoid top-down sanctions. This interpretation has grounds on previous research suggesting that politicians are more reactive to the prospect of police crackdowns or a reduction in federal transfers, than to voter sanctioning (Avis, Ferraz, and Finan 2018; Brollo 2011). To address this possibility, I focus on non-audited municipalities and analyze whether the proportion of audited neighbors from the same party as the incumbent moderates the effect on party switching.

In the 2016 local election, 31 different parties secured at least one mayoral seat. The high number of parties, along with their relative weakness at the local level, suggests that voters focus primarily on individual candidates rather than parties when it comes to local elections (Klašnja and Titiunik 2017; Novaes 2017)).[15] Since political parties convey little information to voters in local elections, if incumbents are more likely to react to nearby corruption when exposed to same-party corruption, then the top-down sanctions mechanism has more merit that the electoral accountability mechanism. Conversely, if the primary mechanism is politicians anticipating voter sanctions, then effects should not vary with the party affiliation of audited mayors nearby, especially because most candidates are supported by coalitions of parties with considerable variation across municipalities. If the top-down sanctions mechanism holds, and considering that a municipality can be exposed to different proportions of audited neighbors from the same party as the incumbent mayor, an increasing proportion of same-party audited neighbors should lead to a larger effect of exposure to nearby corruption on party switching.

I evaluate this implication by zooming in on non-audited municipalities. I replicate the models reported in Figure 2.2, introducing an interaction term for the proportion of the nearby audited municipalities with mayors that share party with the incumbent. Figure 2.4 reports the simulated average marginal effects of

15. This contrasts with the general pattern at the national level in the period under study, which is characterized by patterns of positive and negative partisanship towards the Worker's Party (Samuels and Zucco 2013, 2018).

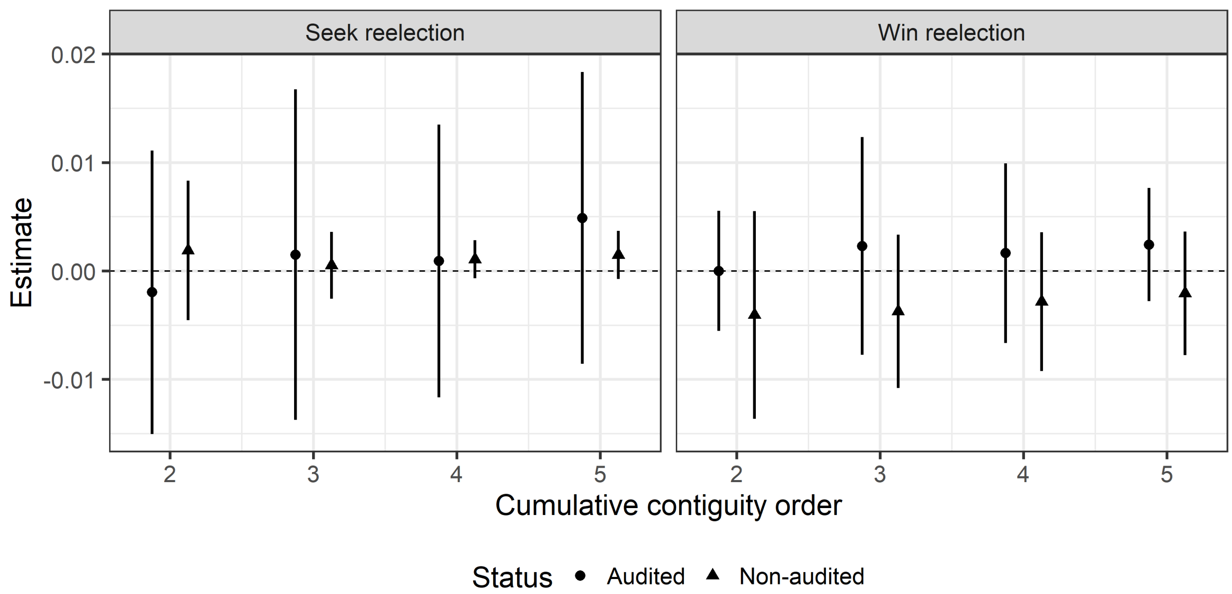


**Figure 2.3: Effect of nearby corruption on seeking and winning reelection**

*Note:* Based on OLS regression with election year fixed effects and clustered standard errors by election year. The figure includes only the operationalizations or nearby suggested by cross-validation. Vertical lines denote 95 percent confidence intervals.

nearby corruption at different proportions of same-party audited mayors in the neighborhood.

The point estimates at the optimal range suggested by cross-validation suggest, if anything, increasing the proportion of same-party audited neighbors either reduces the marginal effect of nearby corruption or does not change it. Moreover, Table B.6 in Appendix B shows that the interaction effect between the two variables is indistinguishable from zero, meaning that we slope of the effect of nearby corruption does not change with the proportion of same-party audited neighbors. I interpret this as evidence against the alternative explanation that the observed main result arises from incumbents' attempt to avoid top-down sanctions.

**The consequences of party switching**

A sufficient but not necessary condition that follows from the electoral sanctioning mechanism is that incumbent mayors who do not switch parties experience worse electoral fates as nearby corruption increases. This condition is not necessary because mayors not switching parties may be in a position where party switching would not improve their reelection chances, either because they are too weak or secure enough to not need it. Given the coding of the main outcome variable, mayors who switch parties always run for reelection. However, those who do not switch may be less (or more) likely to seek reelection as nearby corruption increases. Figures 2.2 and 2.3 suggest that nearby corruption does not affect the tendency to

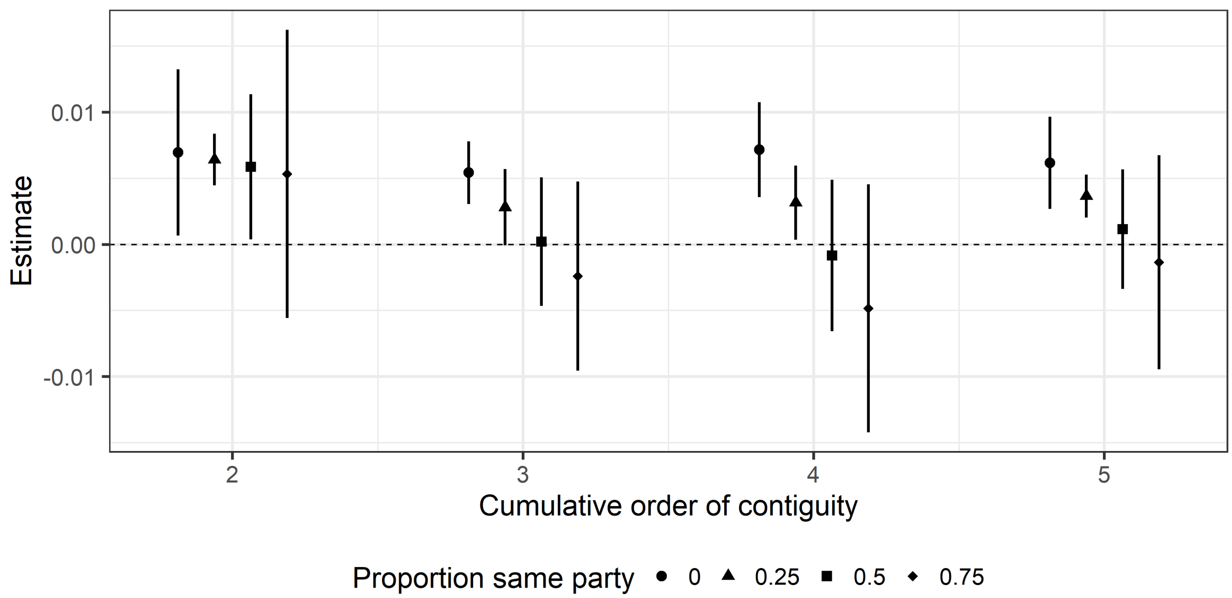


**Figure 2.4: Simulated marginal effect of nearby corruption on incumbent mayor party switching conditional on different proportions of same-party audited neighbors**

*Note:* Based on OLS regression interacting nearby corruption with the proportion of same-party audited neighbors in the sample of non-audited municipalities. Estimation includes election year fixed effects and clustered standard errors by election year. The figure only includes the optimal range suggested by cross-validation. Vertical lines denote 95 percent confidence intervals.

seek reelection among those who do not switch parties. Absent self-selection, one should expect voters to sanctions incumbents that fail to update their behavior in office for nearby corruption.

Figure 2.5 shows the effect of nearby corruption on whether the incumbent mayor wins the election in the subset of non-audited municipalities, further dividing the data on whether the mayor runs with a different party. In the subset of mayors who switch parties, nearby corruption does not affect their reelection chances, which implies that party switching is a viable strategy to avoid electoral sanctions. However, those who do not switch parties lose elections more often as nearby corruption increases, this estimate is indistinguishable from zero across the optimal ranges suggested by cross validation, but the associated p-value is around 0.06, which is close to the usual rule of thumb used to determine statistical significance. In short, the figure suggests that mayors exposed who do not switch parties experience worse electoral fates as nearby corruption increases, which reinforces the argument for the electoral accountability mechanism.

This finding can only be interpreted descriptively since conditioning on whether the mayor seeks reelection under a different party can induce post-treatment bias. Moreover, previous work suggests that incumbents who switch parties are more likely to be reelection-oriented, as opposed to policy oriented Peterlevitz 2019, which implies that those who switch parties are not comparable to those who choose not to. Still, nearby corruption having a negative effect on reelection among those who do not switch parties, who are more likely

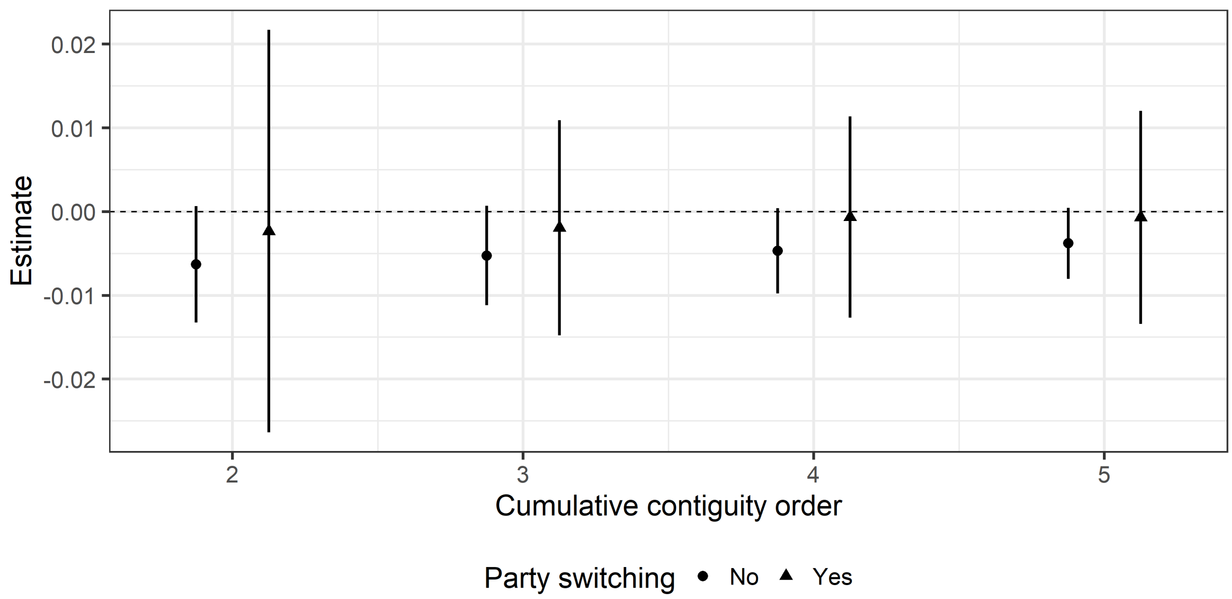


**Figure 2.5: Effect of nearby corruption on whether the incumbent wins reelection in interaction with party switching**

*Note:* Based on OLS regression with election year fixed effects and clustered standard errors by election year. The figure includes only the operationalizations or nearby suggested by cross-validation. Vertical lines denote 95 percent confidence intervals.

to be policy-oriented, underscores the negative electoral consequences of nearby corruption for those who are unable or unwilling to adjust their behavior in office.

## 2.6 Conclusion

This chapter 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 corruptionAsquer, Golden, and Hamel 2019; Daniele, Galletta, and Geys 2020, this chapter 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 chapter 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 chapter 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 chapter 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 chapter 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 chapter 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.

Second, this chapter illustrates the importance of taking a modular approach to the study of spillover effects. Recent advances in methodology allow researchers to make valid inferences while relaxing the non-interference assumption, yet they still require the researcher to make modeling assumptions that often do not follow from theory. By adopting a model selection approach, this chapter shows an example of how supervised learning can help researchers to study spillovers in applications where the underlying pathway that connects observations remains unobserved.

# Chapter 3

# A Model Selection Approach to Interference

## 3.1 Introduction

In the causal inference framework, interference or spillovers occur when a unit's potential outcome depends on the treatment assignment of other units (Cox 1958). This is a violation of the stable unit treatment value assumption (SUTVA) used to justify the implementation of common design-based estimators (Rubin 1990). Some scholarship aims at detecting interference (Aronow 2012) or recovering the ability to identify causal effects in its presence (Sävje, Aronow, and Hudgens 2019). However, in the social sciences, interference can be a phenomenon of interest by itself. For example, research on electoral fraud suggests that the presence of election observers reduces irregularities in treated units, but increases them in nearby localities (Asunka et al. 2019; Ichino and Schündeln 2012).

Detecting and estimating interference in the causal inference framework is an active research agenda (see Halloran and Hudgens 2016; Aronow et al. 2021; and Ogburn and VanderWeele 2014 2014 for overviews). Current approaches assume the researcher knows the pathway(s) through which interference occurs. For example, Aronow and Samii (2017) propose a general estimator of the average unit-level causal effect of experiencing different exposure regimes, which requires knowledge of what units are exposed to treatment.[1] Similarly, Hudgens and Halloran (2008) develop estimators for several quantities of interest under the assumption that interference occurs within, but not across, groups or strata. Beyond estimation, Bowers, Fredrickson, and Panagopoulos (2013) introduce a framework to test hypotheses of counterfactual causal effects under a theoretically informed model of interference. As with any application of mathematical models, this approach assumes a correct model specification.[2] These type of assumptions are not verifiable, but researchers can justify them with research designs meant to capture interference, such as multilevel experiments (Sinclair, McConnell, and Green 2012), saturation designs (Baird et al. 2018), or natural experiments (Keele and Titiunik 2018).

1. I use "exposure to treatment" as a shorthand for "being exposed to a treated unit." This means we suspect that the outcome of the unit in question may be affected by the treatment status of those it is exposed to.
2. See also Toulis and Kao 2013; Athey, Eckles, and Imbens 2018; and Basse, Feller, and Toulis 2019 for alternative approaches with similar assumptions.

In many social science applications, these research designs are not feasible. If the known pathways assumption is not met, the resulting estimates may be biased (Aronow et al. 2021). This chapter focuses on estimating interference effects in the case in which researchers' domain expertise suggests a pathway for interference, but does not indicate how units connect to each other. For example, we may know units' geographic location, but not how far away a treated unit interferes with the outcome of others. To make a statement of what units are exposed to treatment, we must convert observed pairwise distances into exposures. This conversion is a decision that follows from theory, but is not directly informed by it, and the discipline currently lacks standards to determine whether a specific conversion is more appropriate than alternative operationalizations.

I argue that we can approach this challenge as a model selection task in supervised learning. We can think of alternative conversions from pairwise distances into exposures as a set of alternative models, or variables within a model, among which the researcher can select the one that best satisfies a performance criterion, given an algorithm and resampling strategy of choice. While this chapter focuses primarily on inverse probability weighted difference in means (Aronow and Middleton 2013; Aronow and Samii 2017) and the lasso-family methods for model selection (see Ratkovic and Tingley 2017 for an overview), I propose a general protocol that researchers can adapt to fit their theoretical and estimation needs.

The choice of algorithm depends primarily on two other decisions. First, the researcher must decide whether the purpose is to estimate a categorical (e.g. Aronow and Samii 2017) or a marginal exposure effect (e.g. Hudgens and Halloran 2008). Second, whether the task is to estimate a single global effect or multiple local effects at different distance ranges. I illustrate how to navigate these decisions, as well as the protocol's effectiveness, with simulations based on hypothetical experiments on a road network in Ghana (based on Bowers et al. 2018) and a reproduction of the analysis of an experiment designed to capture the spillover effect of election observers on voter registration irregularities in the same country (Ichino and Schündeln 2012).

The main contribution of this chapter is to develop standards to assist researchers in making informed decisions while modeling interference in experiments and observational studies that use experimental logic when the known pathways assumption is not satisfied by design. The model selection approach described here is helpful in identifying the most appropriate model among alternatives stemming from one or more theoretically relevant pathways. It extends recent work on analyzing interference when the assumption of known pathways is not sustainable. For example, Egami (2021) develops sensitivity analysis for interference in the presence of unobserved networks and Sävje (2019) replaces the assumption of known exposure in Aronow and Samii (2017) with the weaker condition of sufficiently controlled (and hence estimable) specification errors. It also extends on recent literature using statistical learning to improve causal inference (see Blakely et al. 2020 for a general treatment), especially in the context of using supervised learning model selection algorithms prior to estimation to increase efficiency (e.g. Belloni, Chernozhukov, and Hansen 2013; Bloniarz et al. 2016).

## 3.2 Causal Inference Approaches to Interference

### 3.2.1 Setting

The technical aspects of this section follow Aronow et al. (2021) closely. Consider an experiment conducted in a sample of $N$ units indexed by $i = \{1, 2, ..., N\}$. Let $\boldsymbol{Z}$ denote a transposed treatment assignment vector so that $\boldsymbol{Z} = \{Z_1, ..., Z_N\}^\top$. For simplicity, assume a binary treatment variable, which implies unit $i$'s possible treatment status is $Z_i = \{0, 1\}$. The logic in this chapter still applies to multiple and continuous treatments. Based on the experimental design, the probability of treatment assignment $Pr(\boldsymbol{Z} = \boldsymbol{z})$ is known for all possible treatment vectors $\boldsymbol{z} \in \{0, 1\}^N$.

Assuming no interference, unit $i$'s potential outcome $Y_i(\boldsymbol{z})$ depends on its treatment assignment only, which means its observed outcome is $Y_i = z_i Y_i(1) + (1 - z_i) Y_i(0)$, where $z_i$ is the observed treatment status of unit $i$. To make the absence of interference more explicit, let $\boldsymbol{z_{-i}}$ and $\boldsymbol{z'_{-i}}$ denote two different treatment vectors that exclude unit $i$'s treatment status. If potential outcomes depend on treatment assignment only, we can claim that $Y_i(z_i, \boldsymbol{z_{-i}}) = Y_i(z_i, \boldsymbol{z'_{-i}})$.

Interference implies that $Y_i(z_i, \boldsymbol{z_{-i}}) \neq Y_i(z_i, \boldsymbol{z'_{-i}})$, so we need to account for treatment status of other units to fully characterize unit $i$'s potential outcome. With a binary treatment, accounting for every unit's treatment status leads unit $i$ to have $2^N$ possible potential outcomes. Since this is intractable in most applications, researchers need to impose some structure. When manipulation is feasible, researchers can implement saturation (Baird et al. 2018) or multilevel (Sinclair, McConnell, and Green 2012) designs so that both treatment assignment and exposure to treatment are known. Natural experiments can also provide such a setting (e.g. Keele and Titiunik 2018. In these cases, the research can assume that interference occurs only within the structure imposed by the research design.[3] When manipulation is not feasible, structure can come from theoretically informed assumptions. For example, we could assume that interference is more likely among legislators with similar ideology (Coppock 2014), or that the presence of election observers affects electoral irregularities within a given distance radius (Ichino and Schündeln 2012).

3. Note that, even with a research design meant to capture interference, this is still an assumption that may not be credibly satisfied in some applications.

Different assumptions about the nature of interference inform different empirical strategies. If the researcher observes how units connect with each other in a network and assumes that said network is sufficient to capture all relevant information about how treatments propagate, then they can estimate interference as the contrast across different exposures. Alternatively, if the researcher is willing to assume that interference happens within observed groups only, they can estimate the marginal or saturation effect of being exposed to treatment within the group.

### 3.2.2 Interference as contrast across exposures

Consider the case of estimating interference in randomized experiments where the researcher knows how units connect with each other. Aronow and Samii (2017) introduce the concept of exposure mapping to characterize all the possible treatment-exposure combinations that may emerge depending on what the researcher assumes about interference. Under this setting, $Y_i(d_k)$ denotes the potential outcome of unit $i$ under exposure $k$.[4] With this, we can express the unit-level average exposure effect as

$$\tau(d_k, d_{k'}) = \frac{1}{N}\sum_{i=1}^{N} Y_i(d_k) - \frac{1}{N}\sum_{i=1}^{N} Y_i(d_{k'}) \tag{3.1}$$

which is the difference in means between the potential outcomes under exposures $k$ and $k'$, with $k'$ denoting any exposure different from $k$. For example, if we want to express the effect of being exposed to treatment among control units, we would write

$$\tau(d_{01}, d_{00}) = \frac{1}{N}\sum_{i=1}^{N} Y_i(d_{01}) - \frac{1}{N}\sum_{i=1}^{N} Y_i(d_{00}) \tag{3.2}$$

with $Y_i(d_{01})$ denoting the potential outcome of unit $i$ when assigned to control and exposed to treatment, and $Y_i(d_{00})$ denoting the potential outcome of $i$ when assigned to control and not exposed to treatment.

Per the fundamental problem of causal inference (Holland 1986), we cannot observe unit $i$'s potential outcome in more than one exposure at a time. Moreover, the probability of experiencing different exposures varies across units. Aronow and Samii (2017) propose a Horvitz-Thompson inverse probability estimator of $\tau(d_k, d_{k'})$ that accounts for both issues.

$$\widehat{\tau_{HT}}(d_k, d_{k'}) = \frac{1}{N}\left[\sum_{i=1}^{N} \mathbf{I}(D_i = d_k)\frac{Y_i}{\pi_i(d_k)} - \sum_{i=1}^{N} \mathbf{I}(D_i = d_{k'})\frac{Y_i}{\pi_i(d_{k'})}\right] \tag{3.3}$$

This is the difference in inverse probability weighted means between two exposures, with $\boldsymbol{I}(D_i = d_k)$

4. Note that a different treatment status of unit $i$ implies a different exposure. See Aronow and Samii (2017) for an extensive formalization of this approach.

denoting the units for which we observe exposure $k$, and $\pi_i(d_k)$ denoting the probability of unit $i$ experiencing exposure $k$. The notation is analogous for $k'$. For a randomized experiment, $\pi_i(d_k)$ is the expected proportion of treatment assignments that induce unit $i$ to experience exposure $k$, which can be computed exactly in relatively small samples or approximated through simulation otherwise.

See Aronow and Samii (2017) for a subsequent discussion of variance estimation. For the purposes of this chapter, the important part is that the estimator $\widehat{\tau_{HT}}(d_k, d_{k'})$ is unbiased under the known pathways assumption. Therefore, when this assumption is not satisfied by design, any operationalization of interference, even if informed by theory, may inadvertently introduce bias. From a model selection perspective, the task to find an operationalization that minimizes this bias.

### 3.2.3 Stratified interference

Another way to impose structure under interference is to assume that it occurs only within observed groups or strata. For example, Get Out the Vote campaigns may affect the voting behavior of untreated individuals living in treated households (Nickerson 2008), or that of individuals in other households in the neighborhood (Sinclair, McConnell, and Green 2012). With this restriction, unit $i$ does not have $2^N$ different potential outcomes, but rather $2^{n_g}$, with $n_g$ denoting the number of units in group $g$.[5] Capturing exposure-specific effects under this assumption may still be intractable in applications with large groups, so Hudgens and Halloran (2008) focus on marginal exposure effects instead.

They define four quantities of interest at the individual, group, and population level: the direct, indirect, total, and overall causal effects. For simplicity, I only describe individual level effects.[6] The average direct causal effect for individual $i$ in group $g$ is

$$\tau_{ig}^{D}(\psi) = \overline{Y}_{ig}(0;\psi) - \overline{Y}_{ig}(1;\psi) \tag{3.4}$$

which is the difference in means between treatment conditions, holding exposure regime $\psi$ constant. The individual average indirect causal effect is

$$\tau_{ij}^{I}(\phi,\psi) = \overline{Y}_{ij}(0;\phi) - \overline{Y}_{ij}(0;\psi) \tag{3.5}$$

which is the difference in means between exposures regimes $\phi$ and $\psi$ under the control condition. The individual average total causal effect is

5. This notation is different from the one in Hudgens and Halloran (2008). They denote the group as $i$ and the individual as $j$.

6. In general, the group and population level causal effects are the aggregation of their individual level counterparts. See Hudgens and Halloran (2008) for details.

$$\tau_{ig}^{T}(\phi,\psi) = \overline{Y}_{ig}(0;\phi) - \overline{Y}_{ig}(1;\psi) \tag{3.6}$$

which is the same as the sum of direct and indirect effects on individual $i$. Finally, the individual average overall causal effect is:

$$\tau_{ig}^{O}(\phi,\psi) = \overline{Y}_{ig}(\phi) - \overline{Y}_{ig}(\psi) \tag{3.7}$$

which is equivalent to the difference in means across two different treatment regimes.

Under Bernoulli random assignment with probability $\psi$,[7] an unbiased estimator of unit $i$'s outcome in group $g$ under treatment assignment $z = \{0,1\}$ and exposure regime $\psi$ is

$$\widehat{Y}_g(z;\psi) = \frac{\sum_{i=1}^{n_g} Y_{ig}(Z_g) I[Z_{ig} = z]}{\sum_{i=1}^{n_g} I[Z_{ig} = z]} \tag{3.8}$$

which is the average of observed outcomes in a group under the same treatment condition and treatment regime $\boldsymbol{Z_g}$. This informs the population-level estimators of direct, indirect, total, and overall causal effects. For example, the estimator for the population average indirect effect is $\hat{\tau}^I(\phi,\psi) = \widehat{Y}(0;\phi) - \widehat{Y}(0;\psi)$.

These estimators are unbiased under the partial interference assumption, which states that unit $i$'s potential outcomes depend only on the treatment assignment of units within, but not across, groups Sobel (2006). The assumption of partial interference, which states that the potential outcomes of unit $i$ depend on the proportion or number of treated units in group $g$, but not on which units within the group are treated, is sufficient but not necessary to identify causal effects.[8]

Aronow et al. (2021) show that violations of partial interference introduce bias in marginal exposure effect estimates. This violation is plausible in social science settings since individuals can interact through unobserved networks. Egami (2021) considers sensitivity analysis for the case in which the researcher observes a causally relevant network but has concerns over whether interaction in unobserved networks may violate partial interference. For example, the researcher may observe how treatments propagate in online social networks but may not be able to account for treatment propagation through face-to-face interaction.

More generally, bias may emerge in this setting because groups themselves are not properly defined. In experiments with individuals as the unit of analysis, the challenge is to identify the right reference group. For example, in an experiment that assigns treatments to individuals in their households, interference can occur within a household, or across households in a neighborhood. In experiments with administrative units,

7. Hudgens and Halloran (2008) suggest that the estimator is unbiased with any randomization schedule, Sävje, Aronow, and Hudgens (2019) clarify that this is only true for Bernoulli random assignment.

8. However, Hudgens and Halloran (2008) show that stratified interference is necessary for variance estimation.

the task is to identify an upper bound that determines which localities can be considered part of the same group. This chapter focuses on the latter case.

## 3.3 A Supervised Learning Model Selection Protocol

Both approaches to interference, contrast across exposures and stratified interference, may suffer bias when the known pathways assumption is not satisfied. Also in both cases, a supervised learning model selection approach involves identifying which of the many plausible theoretically informed operationalizations minimizes this bias. Table 3.1 summarizes the proposed protocol to accomplish this task. The first step is to identify one or more relevant pathways through which interference occurs. This choice comes from theory or domain expertise. In the social sciences, common pathways include connections between peers (Paluck, Shepherd, and Aronow 2016), ideological similarity (Coppock 2014), or geographical proximity (Ichino and Schündeln 2012).

Second, the researcher expresses the chosen pathway as pairwise distances between units along a distance metric. These pairwise distances are straightforward when the theoretically relevant pathway is a continuous measure indicating units' placement in multidimensional space (e.g. location, ideology scores), in which case we can express distance as the difference between the values of any two units. When pathways indicate how units connect to each other in a discrete manner, as in a peer network, the distance can be expressed as the geodesic (the lowest number of edges connecting two nodes).

As an illustration, let $\boldsymbol{W} \in [0, +\infty)$ be an $M \times T$ distance matrix where $M$ is the number plausibly exposed units and $T$ is the number of treated units.

$$W = \begin{bmatrix} \infty & w_{12} & \cdots & w_{1T} \\ w_{21} & \infty & \cdots & w_{2T} \\ \vdots & \vdots & \ddots & \\ w_{M1} & w_{M2} & \cdots & \infty \end{bmatrix} \tag{3.9}$$

The dimensions of $\boldsymbol{W}$ depend on the assumptions we make about the nature of interference. If exposure to treatment affects both treated and control units, then $M = N$, if only control units can be affected by the treatment assignment of others, then $M = (N - T)$.

The task is to convert the matrix $\boldsymbol{W}$ into a matrix $\boldsymbol{K}$ of binary indicators of whether unit $i$ can be considered as exposed to treated unit $j$. Let $\kappa$ denote a distance threshold so that

1. Identify relevant pathway(s) through which interference occurs
2. Express the pathway as pairwise distances between (plausibly) exposed and treated units along a distance metric
3. Use pairwise distances to inform plausible models of interference
4. Use supervised learning algorithm of choice to perform model/variable selection
5. Estimate interference using the model(s) that satisfy the performance criterion

**Table 3.1: A supervised learning model selection protocol for interference**

$$k_{ij} = \begin{cases} 1 & \text{if } w_{ij} \leq \kappa \\ 0 & \text{otherwise} \end{cases} \tag{3.10}$$

When $k_{ij} = 1$, we say that unit $i$ is exposed to unit $j$. With this information, the researcher can compute the predictors of interest based on the desired estimation target. If the goal is to estimate categorical exposure effects, then they should record unit $i$ is exposed to at least one treated unit. If the goal is marginal exposure effects, the predictors should indicate the number or proportion of treated units $i$ is exposed to.

The third step is to use the information about pairwise distances to identify multiple plausible models of interference. Different values of $\kappa$ imply different versions of $\boldsymbol{K}$. Once again, this decision depends on the estimation target. If the researcher seeks to identify a single global effect, then different values of $\kappa$ inform alternative upper bounds, and hence different models, for which units can be considered as exposed to treatment. For example, Paluck, Shepherd, and Aronow (2016) argue that the effects of anti-conflict interventions in school spread to those students who spend time with treated students. However, the intervention can also reach those who spend time with those who spend time with treated students. In this case, $\kappa$ can record network degrees as plausible upper bounds for interference.[9]

If the goal is to identify local exposure effects at different distance ranges, then different values of $\kappa$ inform the construction of different predictors among which an algorithm will select to identify the most appropriate model. For example, Ichino and Schündeln (2012) estimate the marginal effect of election observer visits on voter registration irregularities within the 0-5 km and 5-10 km distance ranges. From a model selection perspective, each one of this brackets is a different predictor that responds to a different value of $\kappa$.

In the fourth step, the researcher uses the supervised learning algorithm of choice to perform variable selection. As in the previous step, the choice of algorithm depends on the estimation target. Off the

9. Aronow and Samii (2017) overcome this challenge theoretically by allowing an arbitrary number of exposure mappings. In their approach, the possibility of second degree effects implies an increase in the number of distinct exposures.

shelf variable selection algorithms are appropriate when the task is to select among multiple plausible local exposure effects. This chapter focuses on the lasso-family of algorithms (see Ratkovic and Tingley 2017 for a review) since they are a straightforward extension of the regression models that researchers usually estimate in this setting. However, the researcher may also use tree-based methods (Breiman 2001; Bleich et al. 2014; Montgomery and Olivella 2018; Speiser et al. 2019) or any other feature selection algorithm of choice.

When the target is a single global effect, different values of $\kappa$ inform a different version of the estimator, so the algorithm is the estimator itself, and the research can select the most appropriate model by searching over a range of values for $\kappa$ using the resampling strategy and performance criterion of choice. For regression, a common strategy is to compute mean squared error via k-fold cross-validation (Hastie, Tibshirani, and Friedman 2009). In this context, $\kappa$ is a hyper-parameter, which value the researcher can tune searching over a manual grid. If the application suggests a computationally restrictive grid, random search is also an option (Bergstra and Bengio 2021).

The final step is to estimate the optimal model(s) suggested by the supervised learning protocol. The researcher can use the estimator of choice, including those discussed in the previous two sub-sections. Depending on the performance metric and resampling strategy, the protocol may suggest multiple models that satisfy the performance criteria. For example, several models can be within one standard deviation from the model that minimizes mean squared error, in which case the researcher can consider all of them as plausible definitions of nearby.[10]

## 3.4 Simulation

### 3.4.1 Setup

This simulation study illustrates the properties of the supervised learning model selection approach in the context of estimating interference as the contrast across exposures, with the goal of identifying a single global effect.

Consider a hypothetical experiment conducted in a network. I use the network in Bowers et al. (2018) since it reflects a realistic layout. Each node in this network is an electoral area in Ghana, a subdivision of a legislative district ($N = 868$). Edges in this network denote whether a direct road connects two areas. The median number of direct connections is 22 (mean: 38, standard deviation: 41).

Bowers et al. (2018) focus on the case where interference occurs between adjacent nodes only. In this

10. When supervised learning algorithms use current data to predict new data, the common practice to reduce overfitting is to select the model with the highest mean squared error within one standard deviation from the minimum (Hastie, Tibshirani, and Friedman 2009). Since the task here is within-sample model selection, any model within a reasonable range away from the minimum can be considered viable.

simulation exercise, interference may also occur at higher degrees. Therefore, the distance metric is the geodesic, or the number of edges in the shortest path between two nodes.

$Z$ is a treatment assignment vector. $Z_i \sim \text{Bernoulli}(\alpha)$, which implies two possible treatment conditions $Z_i = \{0, 1\}$, which I refer to as control and treatment, respectively. One of the main conclusions in Bowers et al. (2018) is that experiments have higher power to detect interference when the proportion of units assigned to treatment is smaller than the usual even split across conditions. With this in mind, I set $\alpha \in [0.1, 0.4]$. Figure 3.1 shows one realization of the treatment assignment in the network with $\alpha = 0.1$.

$\boldsymbol{Y(d_{00})}$ is the vector of potential outcomes under control and without exposure, $\boldsymbol{Y(d_{10})}$ under treatment without exposure, $\boldsymbol{Y(d_{01})}$ under control with exposure, and $\boldsymbol{Y(d_{11})}$ under both treatment and exposure. I assume $\boldsymbol{Y(d_{00})} \sim U(0, 1)$ and $\boldsymbol{Y(d_{10})} = \boldsymbol{Y(d_{01})} = \boldsymbol{Y(d_{11})} = \lambda \boldsymbol{Y(d_{00})}$ with the multiplicative treatment effect $\lambda \in \{0.26, 0.63\}$, which is about one and two standard deviations of the uniform distribution of the outcome, respectively. Under this setup, we can restrict our attention the effect of being exposed to treatment within control units, which is the contrast between $\boldsymbol{Y(d_{00})}$ and $\boldsymbol{Y(d_{01})}$.

To capture the idea that interference may occur within different distance ranges, I set $\kappa = \{1, 2, 3\}$ as the true interference upper bound. For example, with $\kappa = 2$ a node is exposed to treatment if it is within two or fewer edges away from a treated unit. Within this range, a node is infected with probability $\gamma \in [0.5, 1]$. Infected units exhibit the outcome under exposure $\boldsymbol{Y(d_{01})}$, otherwise they exhibit $\boldsymbol{Y(d_{00})}$. This reflects the scenario where treatments propagate at different rates.

For each combination of the parameters $\alpha$, $\lambda$, $\kappa$, and $\gamma$, I consider plausible upper bounds $k = \{1, 2, 3, 4\}$. $k$ is the researcher's guess of what the value of the true $\kappa$ may be, with higher values reflecting a broader range within which control units are considered exposed to treatment. For each value of $k$, I consider the inverse probability weighted difference in means described in Equation 3 as both the algorithm and the estimator. I use 5-fold cross validation to compute the root mean squared error (RMSE) for each model and choose the one with the largest RMSE within one standard deviation of the mean. I denote the value of $k$ in the chosen model as $\bar{k}$, and estimate the corresponding model.[11]

I repeat this process 1,000 times for each parameter combination and compare the protocol's performance with an oracle that knows the value of $\kappa$ ahead of time. I assess performance with bias, mean absolute deviation (as a measure of consistency), power at a 0.05 false positive rate, and the mean value of $\bar{k}$.[12]

11. This decision rule implies erring on the side of selecting a smaller $\bar{k}$.

12. In some settings, treatment spreads too fast, which leads to implausible or rank-deficient estimates. This occurs in roughly 3% of the simulations (out of a total of 288,000) and happens more often as $\alpha$ and $\kappa$ increase. I exclude these from the analysis.

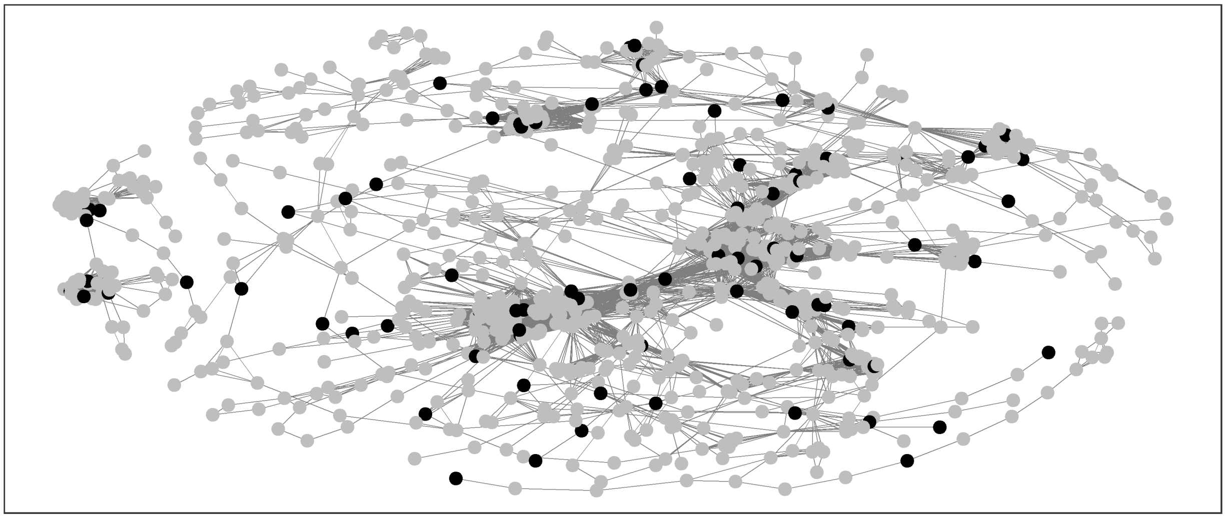


**Figure 3.1: Hypothetical experiment in a road network in Ghana**
*Note:* Colors denote treatment assignment. Units are assigned to treatment with probability $\alpha = 0.1$.

### 3.4.2 Results

To facilitate exposition, Figures 3.2-3.5 report performance at select parameter values $\alpha = \{0.1, 0.4\}$, $\gamma = \{0.5, 1\}$, and $\tau = 0.26$. Section C.1 of Appendix C reports simulation results in full. Figure 3.2 shows the bias of the estimator selected by the proposed supervised learning protocol against an oracle that knows the true value of the upper bound $\kappa$. Each value in the figure is based on 1,000 simulations. The protocol has similar bias than the oracle when treatment assignment probability $\alpha$ is low. When $\alpha$ is close to a coin flip ($\alpha = 0.4$), the protocol increases in bias with $\kappa$. In general, bias is higher when the infection probability $\gamma$ is lower, but the difference between the protocol and the oracle as $\kappa$ increases under high $\alpha$ is more pronounced when the infection probability $\gamma$ is deterministic ($\gamma = 1$).

Figure 3.3 shows mean absolute deviation as a measure of consistency, with higher values suggesting more variation in the distribution of estimates. As with bias, the protocol has similar consistency to the oracle with low $\alpha$, and in general estimates are more inconsistent when $\gamma$ is a coin toss. When alpha is high, mean absolute deviation increases with $\kappa$ for both the protocol and the oracle, but the chance is more pronounced for the protocol.

Figure 3.4 shows power as the proportion of the simulations in which the test that follows from the inverse probability weighted difference in means rejects the null hypothesis of no effect at a significance level of 0.05. Power is high when $\gamma$ is deterministic and either $\alpha$ or $\kappa$ are low. Similar to bias and consistency, power tends to decrease as $\kappa$ increases under $\alpha = 0.4$ and the change is more pronounced for the protocol

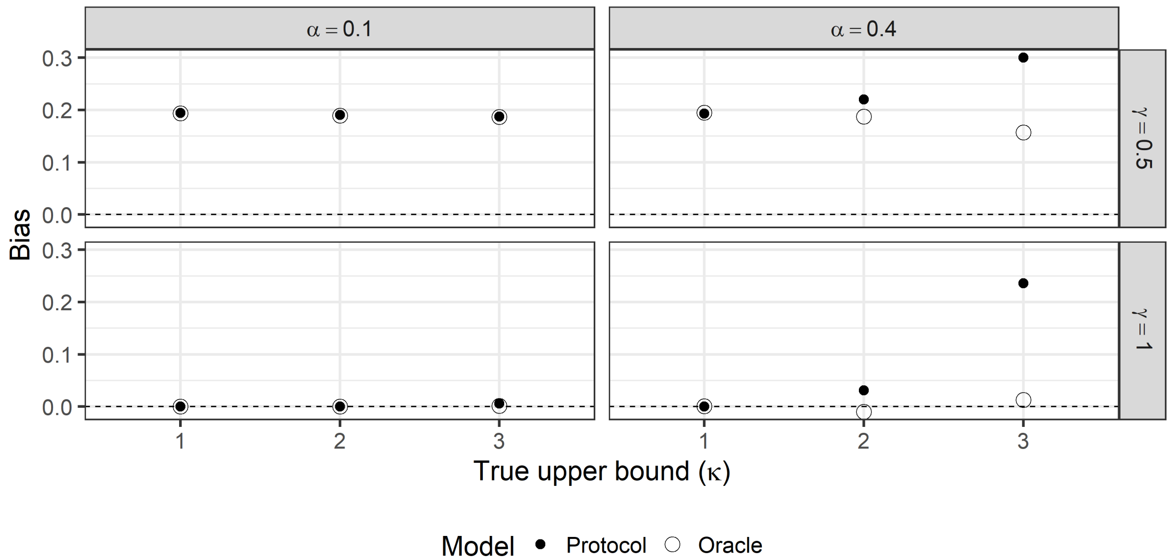


**Figure 3.2: Comparing bias of the supervised learning model selection protocol against the oracle**

*Note:* Each value is based on 1,000 simulations. When the treatment assignment probability $\alpha$ is low, the protocol and oracle have similar bias. When $\alpha$ is high, the bias of the protocol increases with the true upper bound $\kappa$. Bias is higher when the infection rate $\gamma$ is a coin toss.

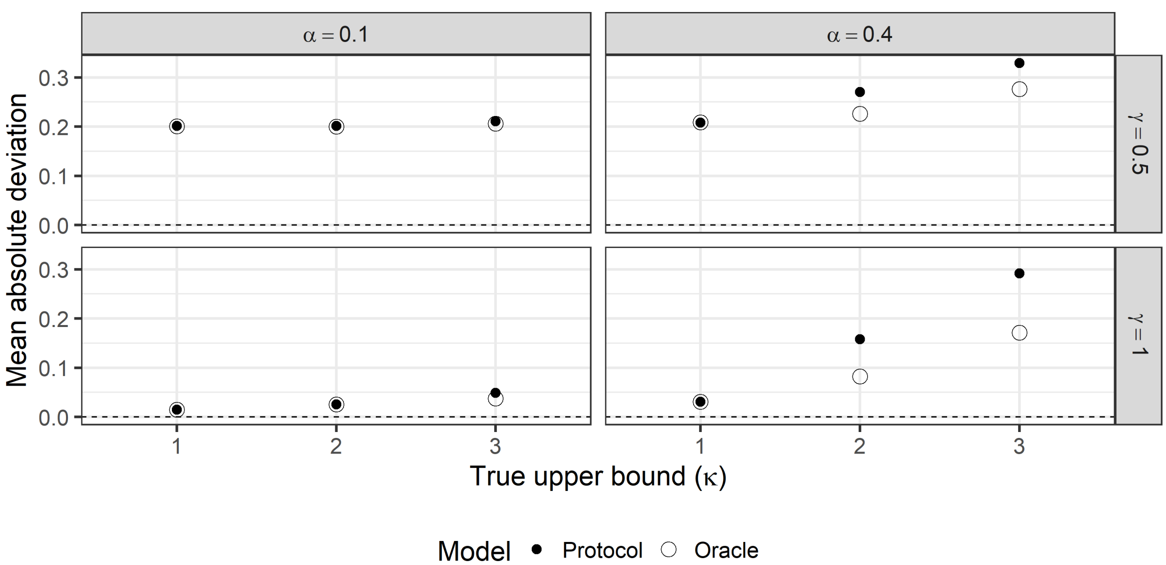


**Figure 3.3: Comparing the consistency of the supervised learning model selection protocol against the oracle**

*Note:* Each value is based on 1,000 simulations. When the treatment assignment probability $\alpha$ is low, the protocol and oracle have similar mean absolute deviation. When $\alpha$ is high, the mean absolute deviation of the protocol increases with the true upper bound $\kappa$ faster than the oracle. Mean absolute deviation is higher when the infection rate $\gamma$ is a coin toss.

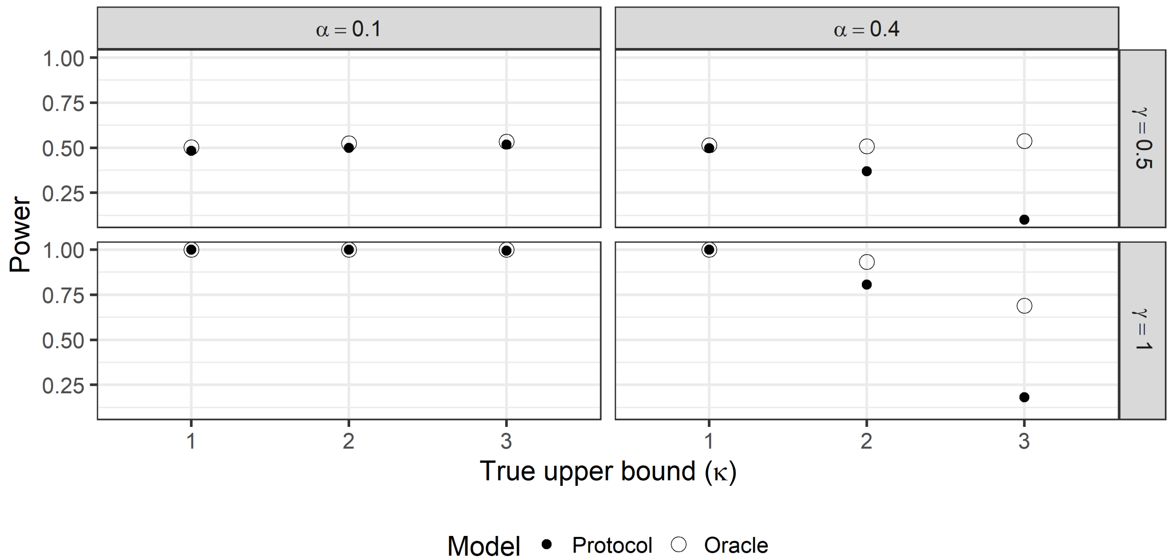


**Figure 3.4: Comparing the power of the supervised learning model selection protocol against the oracle**

*Note:* Each value is based on 1,000 simulations. Power is the proportion of the simulations in which the test rejects the null of no effect at a significance level of 0.05 when $\tau = 0.26$. Power is high when the infection rate $\gamma$ is deterministic and the treatment assignment probability $\alpha$ or the true upper bound $\kappa$ are low. As $\kappa$ increases, the power of the protocol decreases faster than the oracle.

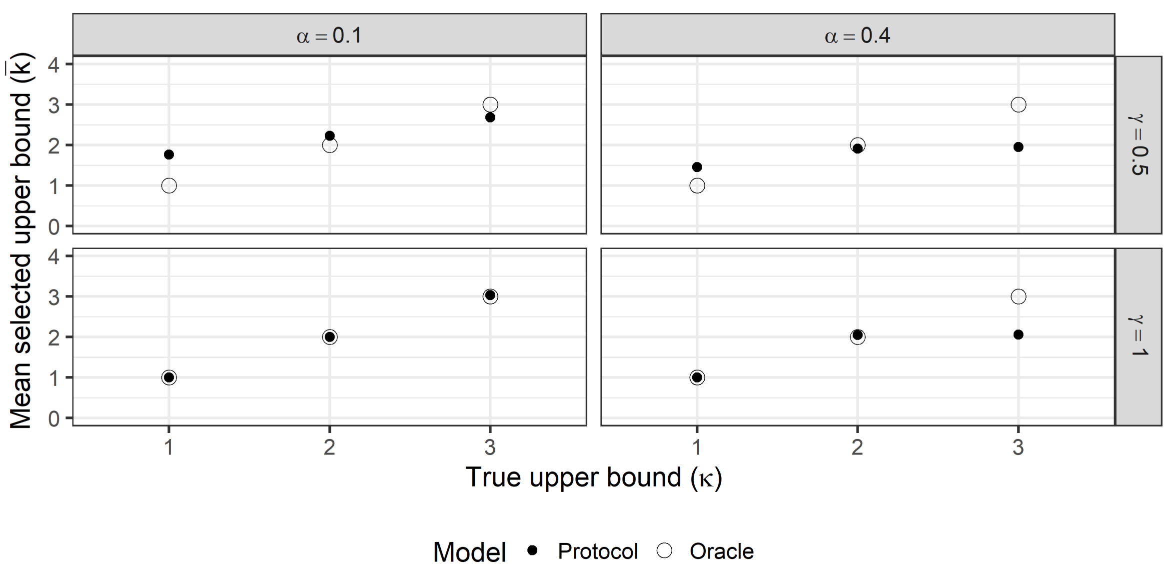


**Figure 3.5: Comparing the mean selected upper bound of the supervised learning model selection protocol against the oracle**

*Note:* Each value is based on 1,000 simulations. By definition the oracle always selects the right value of the true upper bound $\kappa$. In average, the protocol chooses the correct upper bound when the treatment assignment probability $\alpha$ is low and the infection rate $\gamma$ is deterministic. Otherwise, the protocol tends to select a broader upper bound when $\kappa$ is low and a narrower upper bound when $\kappa$ is high.

than the oracle. This is more evident in the case of $\gamma = 1$.

Finally, Figure 3.5 shows the mean selected upper bound $\bar{k}$, this statistic reflects the protocol's average guess for $\kappa$ across simulations. By definition, the oracle always selects the correct interference upper bound, so the protocol performs better when it resembles the oracle. This is the case under $\alpha = 0.1$ and $\gamma = 1$. In other scenarios, the protocol tends to overestimate the upper bound when $\kappa$ is low, and underestimate it when $\kappa$ is high.

To summarize, the simulation exercise suggests that the protocol performs better when a relatively small proportion of units are assigned to treatment. This finding also appears in the simulations in Bowers et al. (2018). The underlying intuition is that when too many units are treated there is not enough information about the outcome of control units to assess the difference between those exposed to and isolated from treatment. For the same reason, performance tends to be worse when treatments travel too far, which is captured by $\kappa$. If too many units are exposed, then the researcher does not have enough information about those isolated.

Finally, performance also suffers when treatments spread in a probabilistic way. This is true for both the protocol and the oracle, which suggests that the problem does not lie in the supervised learning model selection approach, but rather in the theoretical decision to commit to the same upper bound across units. Future work may alleviate this problem by implementing more flexible distance metrics and algorithms that allow the interference upper bound to vary across units or clusters thereof.

## 3.5 Application

### 3.5.1 Election observers and voting registration irregularities in Ghana

The simulation exercise in the previous section illustrates the supervised learning model selection approach to interference in the case of a single global categorical effect. This section illustrates the case of multiple marginal effects by reanalyzing an experiment on the spillover effect of election observers on voter registration irregularities in Ghana (Ichino and Schündeln 2012). Table 3.2 summarizes the two-stage research design. Before randomization, the authors group constituencies (legislative districts) into blocks according to the difference in vote shares between the two main parties in the country in the 2004 legislative election. At the first stage, one constituency per block was randomly assigned to treatment and two were assigned to control. In the second stage, roughly 25% of the electoral areas (ELAs), subdivisions of constituencies, were assigned to receive the visit of an election registration observer in the wake of the 2008 election.

The main finding in this experiment is that election observers do not deter but rather displace irregular-

| Constituencies | Electoral areas (ELAs) | Observations |
|---|---|---|
| Control | Control | 592 |
| Treatment | Control | 199 |
| Treatment | Treatment | 77 |

**Table 3.2: Research design in Ichino and Schündeln (2012)**

*Note:* Two-stage design. Constituencies are randomly assigned to treatment and control, then ELAs within treatment constituencies are randomly assigned to the visit of an election observer.

ities, one additional ELA assigned to treatment within 5 kilometers leads to a 3% increase in irregularities, measured as the percent change in voter registration between 2004 and 2008, in control ELAs.[13] The effect is indistinguishable from zero for the number of ELAs assigned to treatment in the 5-10 kilometer range.

### 3.5.2 Lasso model selection

The exercise from a model selection perspective is to determine whether the proposed protocol can recover the same findings as an experiment designed to capture interference. I assume that the researcher believes that geographic distance is a relevant distance metric and, along with the original authors' decision, count the number ELAs assigned to treatment at 5 kilometer intervals up to 50 kilometers. This translates to 10 predictors among which to select.

I select among these predictors using lasso-family algorithms. The logic of lasso (Least Absolute Shrinkage and Selection Operator) methods is to add a penalty term to the the objective function of OLS regression, which is the residual sum of squares (see Tibshirani (1996) for mathematical details). As the penalty term increases, the resulting regression coefficients shrink toward zero, a coefficient that reaches zero is excluded from the estimation step. The size of the penalty term is determined by a tuning parameter $\lambda$, which for the frequentist lasso is tuned via cross-validation or another resampling strategy.

A shortcoming of the standard lasso is that it tends to select irrelevant predictors that correlate with relevant predictors. Figure 3.6 shows the correlation matrix for the variables involved in the reanalysis. Many predictors correlate positively with each other, which may create complications for variable selection in the lasso. Subsequent refinements to the lasso attempt to minimize the tendency to over-select predictors (see Ratkovic and Tingley 2017 for a review).

In this application, I focus on the following variants of the lasso:

1. Lasso (Tibshirani 1996)

2. Adaptive lasso (Zou 2006)

13. This is the intent-to-treat effect, which is the focus of this chapter, the local average treatment effect for those ELAs that were visited by an election observer is similar. See Ichino and Schündeln (2012) for details.

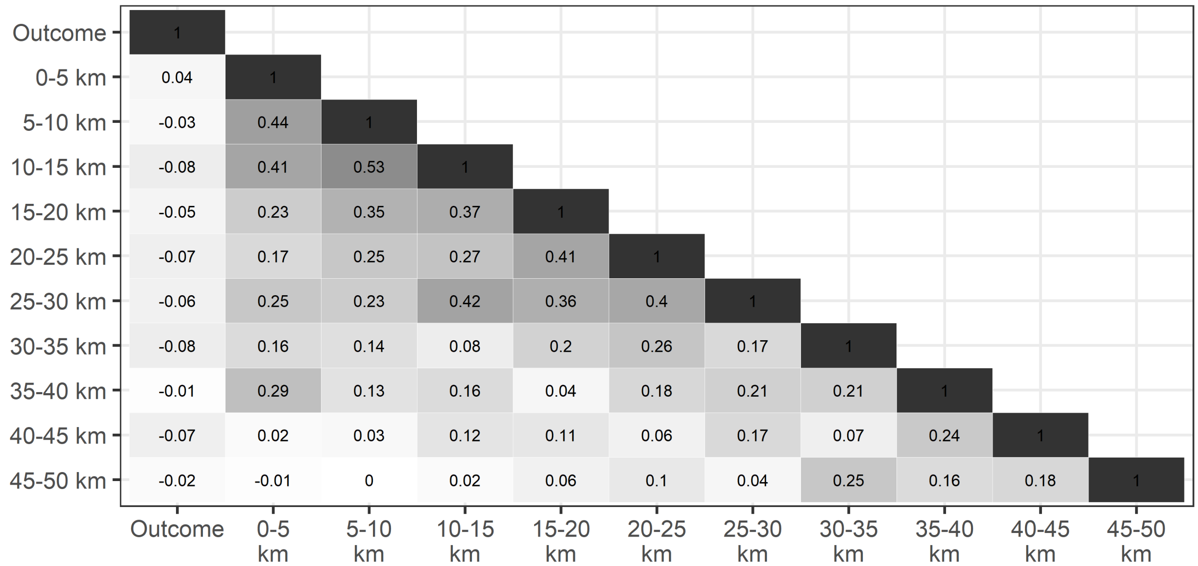


**Figure 3.6: Correlation matrix for outcome and predictors**

*Note:* The outcome is the 2004-2008 percent change in voter registration. The predictors count the number of ELAs assigned to treatment in the corresponding range. Numbers indicate the Pearson correlation coefficient between the intersecting variables. Darker shades of gray indicate more extreme values.

3. Lasso + OLS (Belloni and Chernozhukov 2013)

4. LASSOplus (Ratkovic and Tingley 2017)

The logic of the original lasso is described above. The adaptive lasso introduces more flexibility by allowing predictor-specific weights, resulting in a penalty term that varies across predictors (Zou 2006). Following standard practice, I weight each predictor by the multiplicative inverse of the absolute value of its coefficient in a multivariate OLS regression of the outcome against the predictors, so for predictor $p$ the weight is $1/|\beta_p|$.

The LASSO + OLS variant seeks to improve upon the initial lasso fit by excluding non-zero predictors that exceed a threshold. In this application, I focus on the goodness of fit thresholding (also knows as OLS post-fit Lasso) procedure proposed by Belloni and Chernozhukov (2013), which consists of fitting an OLS regression for all the subsets of the model suggested by the lasso, and then choosing the subset with a residual variance that resembles the residual variance of the suggested lasso fit.

Lastly, the LASSOplus proposed by Ratkovic and Tingley (2017) is a Bayesian procedure that streamlines model selection and estimation. This method has three advantages. First, as a Bayesian method, it produces credible intervals analogous to the confidence intervals obtained by OLS regression, so it does not need an additional step for inference like the frequentist variants do. The second advantage of LASSOplus is that it is a thresholding function that zeroes out small coefficients. Bayesian variants of the lasso tend to have better predictive performance (Casella et al. 2010), but are not sparse estimators in the sense that they do not automatically zero-out coefficients. Third, unlike standard lasso methods, LASSOplus can incorporate prior knowledge about the data generating process, which in the context of this chapter translates to research design features. For example, LASSOplus can automatically incorporate the blocked structure of the Ichino and Schündeln (2012) experiment and adjust the posterior distribution accordingly.

For the frequentist variants of the lasso (standard, adaptive, and lasso + OLS), I tune $\lambda$ via 10-fold cross-validation using the `glmnet` R package and choose the most generous model among those implied by the range within one standard deviation from the value of $\lambda$ that minimizes RMSE.[14] I follow the default parameters of the `sparsereg` R package for LASSOplus, which estimates the model via MCMC via Gibbs sampling with 200 burn-in iterations, 200 posterior samples, and thinning every 10 samples.[15]

### 3.5.3 Results

Figure 3.7 summarizes the variables selected by each lasso variant. Figures C.9 and C.10 in Appendix C show additional details for the frequentist lasso variants. The horizontal axis lists every predictor and the dark blocks denote whether a predictor was selected by each also variant. For reference, the first row denotes the original model selection in Ichino and Schündeln (2012), including the number of ELAs assigned to treatment within 0-5 kilometers and 5-10 kilometers, of which only the first has a non-zero effect. All the frequentist variants of the lasso correctly select this predictor, but also overselect every other predictor. This pattern emerges because continuous predictors tend to correlate with each other, and the lasso chooses to penalize one heavily and to leave the other untouched. LASSOplus, in turn, correctly selects the number of ELAs assigned to treatment within 0-5 kilometers as the only non-zero predictor, which aligns with the original findings.

Figure 3.8 shows the estimates from the models suggested by each lasso variant in separate frames. The first frame reproduces the original result in Ichino and Schündeln (2012). For the frequentist variants, estimation occurs after model selection following the original specification, which includes block fixed effects, a control for whether an ELA is in a treatment constituency, and clustered standard errors at the block level. Because in the frequentist variants of the lasso estimation occurs after selection, I compute 97.5% confidence intervals to control for the possibility of false coverage-statement rate (Benjamini and Yekutieli 2005). Although all of the frequentist variants select irrelevant predictors, the estimation for each returns an effect similar to the original for the number of ELAs assigned to treatment in the 0-5 kilometer range.

14. In this case, more generous implies selecting the model with the most predictors.
15. This is a relatively small number of iterations yet, as the next sub-section shows, sufficient to yield satisfactory results.

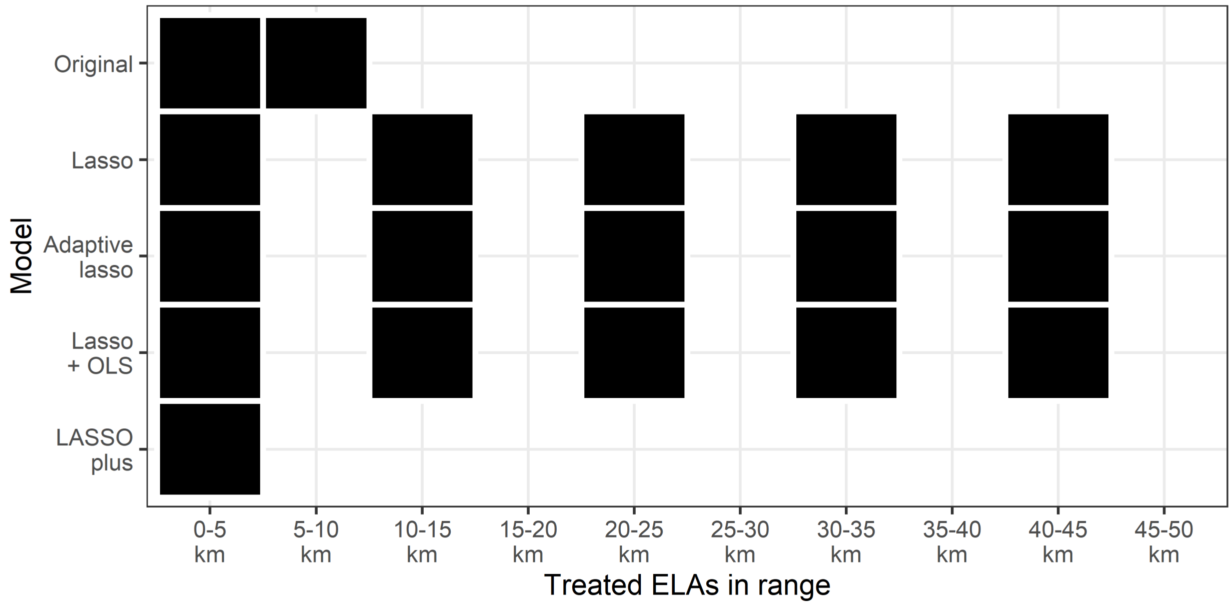


**Figure 3.7: Predictors selected by each lasso variant**

*Note:* Each column in the horizontal axis denotes a different predictor counting the number of ELAs assigned to treatment in the corresponding range. Blocks denote whether each predictor has a nonzero coefficient in the model chosen by the corresponding lasso method. For reference, the first row denotes the model selection in Ichino and Schündeln (2012).

The remaining estimates are indistinguishable from zero, which still aligns with the conclusion that spillover effects do not occur beyond five kilometers.

For LASSOplus, the point estimate is the posterior mean of the same model used for variable selection, which includes the original research design features as prior information. Unlike the frequentist variants, LASSOplus only selects the number of ELAs assigned to treatment in the 0-5 kilometer range, and while the estimate is smaller than the original, the 95% credible interval is narrower than in the frequentist counterparts.

To summarize, both the frequentist variants of the lasso and LASSOplus arrive at the same substantive conclusion as an experiment intended to capture interference. As documented in previous work, the frequentist variants tend to select irrelevant effects that correlate with true non-zero effects (Ratkovic and Tingley 2017), a problem that LASSOplus overcomes in this application. Frequentist lasso-family methods arrive at the correct substantive conclusion despite over-selecting because, under regular conditions, the frequentist lasso and its variants satisfy the oracle inequality, which is the idea that, asymptotically, the lasso selects at least a subset of the true model (Candès 2006).[16] Therefore, both frequentist and Bayesian sparse estimation models are suitable for a model selection approach to interference.

16. The oracle inequality exists in contrast with the oracle property, which requires a variable selection procedure to asymptotically select the right model (Fan and Li 2001).

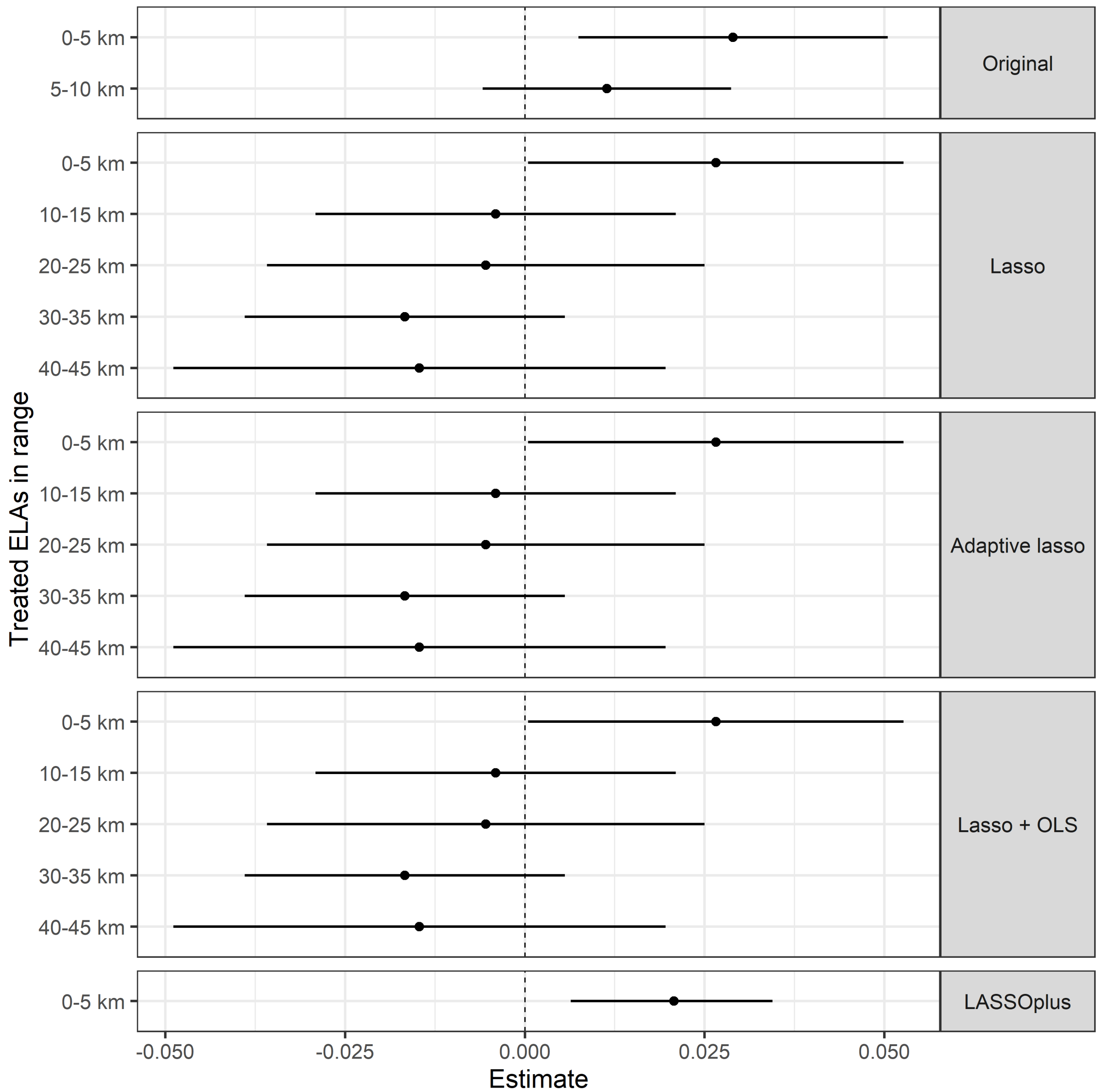


**Figure 3.8: Estimates for the marginal effect of the number of ELAs assiged to treatment within selected distance ranges on voter registration irregularities**

*Note:* Each frame denotes a different model. The first frame reproduces the original results. For the frequentist variants of lasso, the estimates come from post-selection OLS regression with block fixed effects and a control for whether an ELA is in a treatment constituency, with bars representing 97.5% false coverage-statement rate adjusted confidence intervals from block clustered standard errors. For LASSOplus, the point estimate is the posterior mean and the bars denote the 95% credible interval.

## 3.6 Conclusion

This chapter proposes a supervised learning model selection approach to study interference in contexts in which the identification assumptions of current design-based causal inference approaches cannot be satisfied. In such case, theory still guides modeling decisions, but it often implies multiple ways to operationalize interference, and the discipline currently lacks a standard to determine which model is more appropriate. This approach still puts the weight of justifying causal inference on what the researcher is willing to assume about the pathway through which interference occurs, but it provides tools to connect theoretical justification with modeling decisions.

The key intuition of the supervised learning model based approach is that, if the researcher can express a theory about interference in terms of a distance metric, then the metric can help in identifying plausible alternative operationalizations, among which the researcher can select using the algorithm, resampling strategy, and performance metric of choice.

Context-specific considerations aside, the choice of model selection approach depends primarily on two decisions about the estimation target. First, the researcher must determine whether the goal is to estimate categorical or marginal exposure effects. This informs whether to record if a unit is exposed to a treatment condition or not, or to count the number of units assigned to a treatment condition within a certain range.

Second, the researcher must determine whether the task is to estimate a single global effect, or multiple local effects at different distance ranges. This informs the type of algorithm. In the case of a single global effect, the distance metric informs the different values of the upper bound of a range within which interference occurs, which in turns assists in identify alternative plausible operationalizations. For multiple local effects, the distance metric informs the construction of predictors among which the researcher can select using off-the-shelf algorithms for variable selection.

This chapter illustrates the supervised learning model selection approach with simulations and the reanalysis of an experiment intended to capture interference. Focusing on the case of estimating the contrast across two exposures in a road network in Ghana, the simulations suggest that the proposed protocol performs better when a relatively small proportion of units is treated, when the probability of infection conditional on exposure is deterministic (as opposed to probabilistic), and when treatments do not spread too far along the theoretically relevant pathways. These conclusions align with previous simulations exercises (e.g. Bowers et al. 2018), which highlights the appropriateness of the approach. Similarly, the reanalysis of an experiment on the spillover effect of election observers on voter registration irregularities in Ghana (Ichino and Schündeln 2012) shows how the lasso-family of algorithms select models that lead to estimates that resemble the original findings, which also validates the usefulness of the approach in cases where the experimental structure was not designed to detect interference.

The simulations and reanalysis focus on model selection using cross-validation and the lasso family of methods, respectively. Similarly, the focus on the estimation side is on inverse probability weighted difference-in-means (Aronow and Samii 2017) and conventional design-based estimators for experimental data. However, the ideas in this chapter are general enough to accommodate for other model selection algorithms and estimation approaches.

A limitation of this approach is that it does not suit hypothesis testing approaches to interference. For example, the key intuition in Bowers, Fredrickson, and Panagopoulos (2013) is that we can test hypotheses about a range of parameter values in a theoretical model of interference. Supervised learning algorithms for variable selection operate under the assumption that a true effect exists and can be detected, even if unknown to the researcher. Testing approaches follow the idea that we can compute uncertainty for a range of hypothetical parameter values.

Future work should acknowledge this difference and develop standards to select among competing models in the context of hypothesis testing approaches to interference. Another area for future development is to develop guidelines on how to choose the appropriate distance metric. For example, the reanalysis of an experiment on the effect of election observers on voter registration irregularities in Ghana follows the authors' decision to count the number of treated units within 5 kilometer bins. However, in novel applications the choice of the number and size of bins (or whether to use bins at all) might be consequential for both the applicability and performance of a supervised learning algorithm.

Finally, future work should apply the ideas of the supervised learning model selection approach to the task of comparing the appropriateness of multiple plausible theoretical pathways. In such a case, the challenge is not only to determine which one is more appropriate, but also to assess and interpret whether alternative pathways are complementary.

# Appendix A

# Supplementary Information for Chapter 1

## A.1 List of Spending Categories

Table A.1 shows the list of spending categories based on the data in http://www.ipeadata.gov.br/, both in the original Portuguese and their English translation. These are used to calculate the main outcome variables and appear in Figure 6 in the main text. See http://www.portaltransparencia.gov.br/pagina-interna/603315-orcamento-da-despesa for a brief description of each category. The definition of most categories is straightforward, but some are not. Special charges (*Encargos especiais*) are expenses not directly related to the provision of goods and services. These include debt repayment, reimbursement, restitution, and contributions to international organizations. They are the largest expense category across municipalities, according to CGU.

The Judicial, Legislative, and Municipal categories are expenses related to the basic operation of the corresponding government branch. These are usually fixed expenses and vary little over time.

In the current data, all municipalities report zero spending in the Regional development (*Desenvolvimento regional*) category. Therefore, this is implicitly excluded in the calculation of total spending per capita and budget concentration, and explicitly excluded in the analysis in Figure 1.6.

## A.2 Result Tables

This section reports the numerical results underlying Figures 1.3-1.6 in the main text. All tables report estimates from OLS regression with term fixed effects and clustered standard errors by term.

- Table A.2 corresponds to Figure 1.3
- Table A.3 corresponds to Figure 1.4
- Table A.4 corresponds to Figure 1.5
- Table A.5 corresponds to Figure 1.6

| | Original | Translation |
|---|---|---|
| 1 | Administração e planejamento | Administration and planning |
| 2 | Agricultura | Agriculture |
| 3 | Assistência e previdência | Social security |
| 4 | Ciência e tecnologia | Science and technology |
| 5 | Comunicações | Communications |
| 6 | Segurança nacional e defesa pública | Defense and security |
| 7 | Desportes e lazer | Sports and leisure |
| 8 | Desenvolvimento regional | Regional development |
| 9 | Educação e cultura | Education and culture |
| 10 | Encargos especiais | Special charges |
| 11 | Energia e recursos minerais | Energy and mineral resources |
| 12 | Habitação e urbanismo | Housing and urban planning |
| 13 | Indústria, comércio e serviços | Industry, commerce, and service |
| 14 | Essencial a justiça e direito da cidadania | Justice and citizen rights |
| 15 | Judiciária | Judicial |
| 16 | Legislativa | Legislative |
| 17 | Municipal | Municipal |
| 18 | Relações exteriores | Foreign relations |
| 19 | Saúde e saneamento | Health and sanitation |
| 20 | Trabalho | Employment |
| 21 | Transporte | Transportation |

**Table A.1: List of spending categories**

*Note:* Regional development excluded from analysis since all municipalities record zero spending in this category

## A.3 Using Corruption as Explanatory Variable

This section reports the effect of the level of corruption uncovered by audits on spending outcomes. Corruption is measured as the number of moderate and severe infractions per service order (Avis, Ferraz, and Finan 2018). The CGU labels infractions with this criteria starting in 2006. I predict the values of the corruption variable before 2006 using a random forest (see Chapter 2 for details). One can only observe corruption in audited municipalities, so all models are restricted to that subset of the data. All models include term fixed effects and clustered standard errors by term.

- Table A.6 shows results analogous to Table A.2 here and Figure 1.3 in the main text
- Table A.7 shows results analogous to Table A.3 here and Figure 1.4 in the main text
- Table A.8 shows results analogous to Table A.4 here and Figure 1.5 in the main text
- Table A.9 shows results analogous to Table A.5 here and Figure 1.6 in the main text

In general, the results suggest that more uncovered corruption decreases both spending outcomes. However, the results that account for the timing of the audit show that uncovering corruption throughout the term affects spending in any year during the term. This suggests that, while auditing is enough to trigger

| | Total | Concentration |
|---|---|---|
| Audited | $-0.06^{*}$ | $-0.04^{*}$ |
| | (0.01) | (0.01) |
| Term-limited | 0.01 | 0.00 |
| | (0.03) | (0.03) |
| Interaction | 0.02 | 0.03 |
| | (0.03) | (0.02) |
| $R^2$ | 0.61 | 0.13 |
| Adj. $R^2$ | 0.61 | 0.13 |
| Num. obs. | 20649 | 20649 |
| RMSE | 0.42 | 0.43 |
| N Clusters | 4 | 4 |

$^{*}p < 0.05$

**Table A.2: Effect of audits on total spending per capita (logged) and budget concentration by term limit status**

the electoral anticipation mechanism suggested in the main text, uncovering corruption may also trigger other forms of top-down and bottom-up sanctioning (Brollo 2011; Timmons and Garfias 2015). This is also evident in Table A.9, since corruption affects spending in both visible and not so visible areas, suggesting multiple mechanisms at play.

| | Total | Concentration |
|---|---|---|
| Year 1 | −0.03 | −0.03 |
| | (0.03) | (0.03) |
| Year 2 | −0.02 | −0.01 |
| | (0.01) | (0.02) |
| Year 3 | −0.09* | −0.06* |
| | (0.02) | (0.00) |
| Year 4 | −0.10* | −0.04 |
| | (0.02) | (0.02) |
| Term-limited | 0.01 | −0.00 |
| | (0.03) | (0.03) |
| Year 1 × Term-limited | 0.01 | 0.03 |
| | (0.04) | (0.01) |
| Year 2 × Term-limited | −0.01 | −0.01 |
| | (0.02) | (0.05) |
| Year 3 × Term-limited | 0.08 | 0.07 |
| | (0.06) | (0.05) |
| Year 4 × Term-limited | 0.05 | 0.01 |
| | (0.03) | (0.03) |
| $R^2$ | 0.61 | 0.13 |
| Adj. $R^2$ | 0.61 | 0.13 |
| Num. obs. | 20649 | 20649 |
| RMSE | 0.42 | 0.43 |
| N Clusters | 4 | 4 |

$^*p < 0.05$

**Table A.3: Effect of audit timing on total spending per capita (logged) and budget concentration by term limit status**

| | Total | | | | Concentration | | | |
|---|---|---|---|---|---|---|---|---|
| | Year 1 | Year 2 | Year 3 | Year 4 | Year 1 | Year 2 | Year 3 | Year 4 |
| Year 1 | −0.03 | −0.03 | −0.03 | −0.03 | −0.03* | −0.03* | −0.03 | −0.03 |
| | (0.03) | (0.03) | (0.02) | (0.03) | (0.00) | (0.01) | (0.01) | (0.02) |
| Year 2 | | −0.02 | −0.01 | −0.02 | | −0.03 | −0.04 | −0.01 |
| | | (0.01) | (0.01) | (0.01) | | (0.01) | (0.03) | (0.02) |
| Year 3 | | | −0.07 | −0.08* | | | −0.03 | −0.06* |
| | | | (0.03) | (0.02) | | | (0.01) | (0.01) |
| Year 4 | | | | −0.10* | | | | −0.04 |
| | | | | (0.02) | | | | (0.01) |
| $R^2$ | 0.64 | 0.61 | 0.62 | 0.63 | 0.70 | 0.01 | 0.05 | 0.12 |
| Adj. $R^2$ | 0.64 | 0.61 | 0.62 | 0.63 | 0.70 | 0.01 | 0.05 | 0.12 |
| Num. obs. | 13990 | 14392 | 15042 | 15097 | 13998 | 14392 | 15042 | 15097 |
| RMSE | 0.42 | 0.41 | 0.41 | 0.41 | 0.32 | 0.33 | 0.34 | 0.43 |
| N Clusters | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 4 |

$^*p < 0.05$

**Table A.4: Effect of audit timing on total spending per capita (logged) and budget concentration across the mayoral term in municipalities with reelection-eligible mayors**

| | Outcome | Estimate | Std. Error | FDR p-value |
|---|---|---|---|---|
| 1 | Transportation | -0.293 | 0.025 | 0.028 |
| 2 | Sports and leisure | -0.230 | 0.027 | 0.033 |
| 3 | Agriculture | -0.200 | 0.056 | 0.153 |
| 4 | Defense and security | -0.155 | 0.050 | 0.162 |
| 5 | Special charges | -0.116 | 0.023 | 0.102 |
| 6 | Housing and urban planning | -0.100 | 0.033 | 0.162 |
| 7 | Health and sanitation | -0.088 | 0.042 | 0.227 |
| 8 | Municipal | -0.079 | 0.035 | 0.222 |
| 9 | Social security | -0.061 | 0.030 | 0.227 |
| 10 | Administration and planning | -0.051 | 0.014 | 0.153 |
| 11 | Industry, commerce, and service | -0.044 | 0.028 | 0.326 |
| 12 | Communications | -0.040 | 0.014 | 0.174 |
| 13 | Legislative | -0.027 | 0.045 | 0.700 |
| 14 | Justice and citizen rights | -0.024 | 0.010 | 0.198 |
| 15 | Education and culture | -0.024 | 0.030 | 0.658 |
| 16 | Energy and mineral resources | -0.013 | 0.064 | 0.914 |
| 17 | Judicial | -0.002 | 0.017 | 0.914 |
| 18 | Employment | 0.004 | 0.021 | 0.914 |
| 19 | Foreign relations | 0.004 | 0.003 | 0.326 |
| 20 | Science and technology | 0.005 | 0.008 | 0.697 |

**Table A.5: Effect of auditing on spending per capita (using $\ln(y+1)$ transformation) across budget categories in municipalities with reelection-eligible mayors**

| | Total | Concentration |
|---|---|---|
| Infractions | $-0.08^{*}$ | $-0.07^{*}$ |
| | (0.01) | (0.01) |
| Term-limited | 0.18 | $-0.01$ |
| | (0.08) | (0.02) |
| Interaction | $-0.04^{*}$ | 0.01 |
| | (0.01) | (0.01) |
| $R^2$ | 0.55 | 0.13 |
| Adj. $R^2$ | 0.55 | 0.13 |
| Num. obs. | 1994 | 1994 |
| RMSE | 0.41 | 0.41 |
| N Clusters | 4 | 4 |

$^{*}p < 0.05$

**Table A.6: Effect of corruption on total spending per capita (logged) and budget concentration by term limit status**

| | Reelection-eligible | | Term-limited | |
|---|---|---|---|---|
| | Total | Concentration | Total | Concentration |
| Year 1 | −0.11* | −0.05* | −0.10* | −0.04 |
| | (0.02) | (0.01) | (0.02) | (0.03) |
| Year 2 | 0.02 | −0.03 | 0.01 | −0.01 |
| | (0.04) | (0.03) | (0.01) | (0.04) |
| Year 3 | 0.03 | −0.03 | −0.07 | 0.02 |
| | (0.06) | (0.03) | (0.04) | (0.05) |
| Year 4 | 0.06 | −0.02 | 0.01 | −0.03 |
| | (0.04) | (0.05) | (0.08) | (0.03) |
| $R^2$ | 0.59 | 0.12 | 0.43 | 0.20 |
| Adj. $R^2$ | 0.59 | 0.11 | 0.42 | 0.18 |
| Num. obs. | 1347 | 1347 | 647 | 647 |
| RMSE | 0.42 | 0.43 | 0.39 | 0.36 |
| N Clusters | 4 | 4 | 4 | 4 |

$^*p < 0.05$

**Table A.7: Effect of corruption on total spending per capita (logged) and budget concentration by audit timing and term limit status (audit timing indicators omitted)**

| | Total | | | | Concentration | | | |
|---|---|---|---|---|---|---|---|---|
| | Year 1 | Year 2 | Year 3 | Year 4 | Year 1 | Year 2 | Year 3 | Year 4 |
| Year 1 | −0.08 | −0.09* | −0.09* | −0.11* | −0.07 | −0.07* | −0.06* | −0.05* |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.01) | (0.01) | (0.01) |
| Year 2 | | 0.00 | 0.02 | 0.02 | | 0.01 | −0.02 | −0.03 |
| | | (0.04) | (0.04) | (0.04) | | (0.04) | (0.02) | (0.03) |
| Year 3 | | | 0.02 | 0.03 | | | −0.01 | −0.03 |
| | | | (0.06) | (0.06) | | | (0.03) | (0.03) |
| Year 4 | | | | 0.06 | | | | −0.02 |
| | | | | (0.04) | | | | (0.05) |
| $R^2$ | 0.35 | 0.37 | 0.56 | 0.59 | 0.06 | 0.05 | 0.05 | 0.11 |
| Adj. $R^2$ | 0.35 | 0.37 | 0.55 | 0.58 | 0.05 | 0.04 | 0.05 | 0.11 |
| Num. obs. | 342 | 638 | 1042 | 1339 | 342 | 638 | 1042 | 1339 |
| RMSE | 0.42 | 0.42 | 0.43 | 0.42 | 0.32 | 0.33 | 0.34 | 0.43 |
| N Clusters | 3 | 3 | 4 | 4 | 3 | 3 | 4 | 4 |

$^*p < 0.05$

**Table A.8: Effect of corruption on total spending per capita (logged) and budget concentration across the term by audit timing in municipalities with reelection-eligible mayors (audit timing indicators omitted)**

| | Outcome | Estimate | Std. Error | FDR p-value |
|---|---|---|---|---|
| 1 | Transportation | -0.405 | 0.032 | 0.010 |
| 2 | Sports and leisure | -0.309 | 0.015 | 0.005 |
| 3 | Agriculture | -0.276 | 0.062 | 0.046 |
| 4 | Special charges | -0.196 | 0.067 | 0.099 |
| 5 | Housing and urban planning | -0.181 | 0.024 | 0.031 |
| 6 | Legislative | -0.146 | 0.041 | 0.075 |
| 7 | Social security | -0.140 | 0.023 | 0.031 |
| 8 | Industry, commerce, and service | -0.135 | 0.042 | 0.089 |
| 9 | Health and sanitation | -0.127 | 0.022 | 0.031 |
| 10 | Administration and planning | -0.101 | 0.019 | 0.033 |
| 11 | Municipal | -0.093 | 0.015 | 0.031 |
| 12 | Defense and security | -0.089 | 0.040 | 0.143 |
| 13 | Employment | -0.065 | 0.023 | 0.099 |
| 14 | Energy and mineral resources | -0.041 | 0.016 | 0.109 |
| 15 | Communications | -0.036 | 0.013 | 0.104 |
| 16 | Justice and citizen rights | -0.021 | 0.010 | 0.152 |
| 17 | Education and culture | 0.003 | 0.014 | 0.885 |
| 18 | Judicial | 0.005 | 0.031 | 0.885 |
| 19 | Science and technology | 0.007 | 0.012 | 0.630 |
| 20 | Foreign relations | 0.008 | 0.001 | 0.031 |

**Table A.9: Effect of corruption on spending per capita (using $\ln(y+1)$ transformation) across budget categories in municipalities with reelection-eligible mayors**

# Appendix B

# Supplementary Information for Chapter 2

## B.1 Coding Audits Before 2006

### B.1.1 Protocol

I use text data from the audit reports as a bridge between labeled and unlabeled cases. I use a bag-of-words approach to predict the sum of moderate and severe infractions, divided by the number of service orders. The predictors are raw word counts. I use the following protocol:

1. Match text data from the audit reports with CGU infraction labels for the 2006-2015 period. This is the period where the CGU coding is available.

2. Predictors are word counts, omitting infrequent terms (words missing in more than 99% of the documents).

3. This leaves a data set with 1226 observations and 11386 variables.

4. Randomly split data in training (75%) and test (25%) sets.

5. Fit multiple random forest on training data with a grid of tuning parameters, choose the model and tuning parameters with the lowest RMSE, create predicted variable in test set.

I chose random forests because they achieve reasonable performance with the current data. I explored including topic membership covariates from structural topic modeling to assist the algorithm, but the predictive gains are minimal. An alternative is to use algorithms from the deep-learning family, but trial runs suggest that the sample size is too small to guarantee convergence.

One way to increase predictive power dramatically would be to turn this from a regression problem into a classification task by separating documents into findings. That is, moving from predicting numbers of infractions at the document level to predicting whether each item counts as a formal, moderate, or severe infraction. This yields a larger training set with more information, and also supervised learning algorithms tend to perform better with classification tasks than with continuous outcomes. However, because audit report formats are not stable over time, dividing documents at the finding level would require prohibitively expensive human coding.

### B.1.2 Performance

Figure B.1 reports reports performance in the test set (N = 319). In average, the predicted values are off by 1.34 infractions per service order compared to the actual values. The predictions map close to a 1:1 relationship for moderate cases of corruption, but tend to underestimate it for large outliers. This implies that models using this variable will underestimate the effect of nearby corruption on the outcomes of interest, making it harder to detect non-zero estimates.

### B.1.3 Validation

As a validation exercise, I reproduce the findings in previous work using the machine coded categories. Rundlett (2018) shows that exposing corruption has a negative effect on incumbent vote only for the 2004 elections. Table B.1 replicates the same pattern using my own data set. This is different from the main analysis in that it evaluates direct effects: Whether revealing corruption in a municipality affects votes for the incumbent in that municipality. The substantive result is the same as in previous work.

## B.2 Result Tables

The main text reports results using figures. This section shows tables with numerical results that underlie those figures. The list below shows the correspondence:

- Table B.2 summarizes the output of the cross-validation procedure described in section 2.4.3 in the main text
- Table B.3 shows the results of Figure 2.2 in the main text: The effect of nearby corruption on party switching by audit status using different definitions of nearby.
- Tables B.4 and B.5 show the results in Figure 2.3: The effect of nearby corruption on seeking and winning reelection.
- Table B.6 shows the results in Figure 2.4: The effect of nearby corruption in interaction with the proportion of same-party audited mayors on party switching. Note that Figure 3 shows simulated

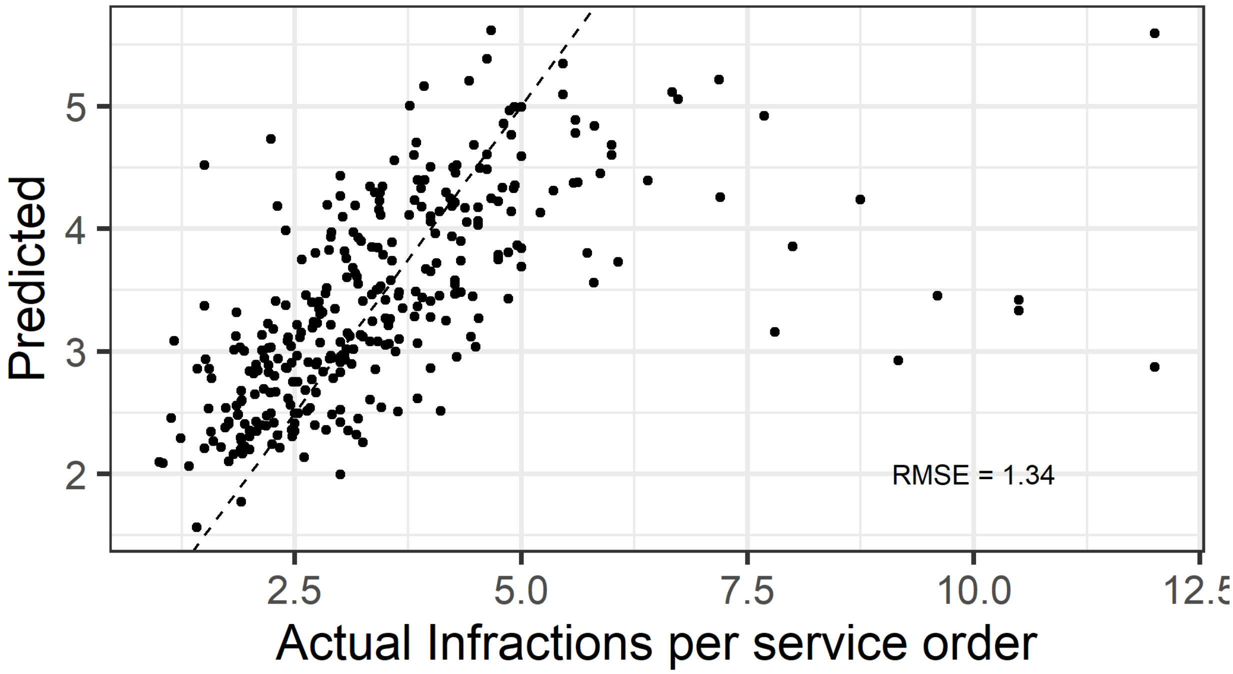


**Figure B.1: Actual vs. predicted corruption variable in the test set. The dashed line denotes the 1:1 relationship. The algorithm does performs well at predicting moderate levels of corruption, but tends to underestimate large outliers.**

marginal effects at discrete margins based on this estimation, the interaction term is indistinguishable from zero.

- Table B.7 shows the results of Figure 2.5: The effect of nearby corruption on winning reelection conditional on party switching among non-audited municipalities.

Unless otherwise specified, the columns in regression tables denote the corresponding contiguity order.

## B.3 Descriptive Statistics and Robustness Checks

- Table B.8 produces the results from Figure 2.2 in the main text and table B.2 using logistic regression.
- Table B.9 shows the effect of nearby corruption using a specification similar to Figure 2.2, focusing on the second contiguity order, but separating the analysis by election year. Results do not depend on the inclusion of machine-coded corruption before the 2004 election.
- Figure B.2 shows the distribution of audited and total number of neighbors by contiguity order, as a complement to Figure 1 in the main text

| | 1. Pooled | 2. 2004 | 3. 2008 | 4. 2012 | 5. 2016 |
|---|---|---|---|---|---|
| Intercept | | 0.54* | 0.49* | 0.48* | 0.44* |
| | | (0.06) | (0.03) | (0.02) | (0.06) |
| Infractions | 0.00 | −0.03 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.02) | (0.01) | (0.01) | (0.02) |
| $R^2$ | 0.01 | 0.01 | 0.00 | 0.00 | 0.00 |
| Adj. $R^2$ | 0.01 | 0.00 | −0.00 | −0.00 | −0.01 |
| Num. obs. | 1133 | 193 | 463 | 341 | 136 |
| RMSE | 0.16 | 0.14 | 0.17 | 0.14 | 0.21 |
| N Clusters | 4 | | | | |

$^*p < 0.05$

**Table B.1: Replication of Rundlett (2018) with my data. The first column includes election year fixed effects and clustered standard errors by election year. The remaining columns include robust (HC1) standard errors.**

| Contiguity | RMSE | SD |
|---|---|---|
| 1 | 0.3449 | 0.0176 |
| 2 | 0.3392 | 0.0107 |
| 3 | 0.3373 | 0.0079 |
| 4 | 0.3374 | 0.0125 |
| 5 | 0.3348 | 0.0078 |
| 6 | 0.3354 | 0.0077 |
| 7 | 0.3357 | 0.0100 |
| 8 | 0.3359 | 0.0096 |
| 9 | 0.3359 | 0.0060 |
| 10 | 0.3378 | 0.0080 |

**Table B.2: Results of model selection via 10-fold cross-validation**

- Figure B.3 shows sensitivity analyses for the effect of nearby corruption on party switching among non-audited municipalities (cf. Figure 2 in the main text) following the partial $R^2$ approach of Cinelli and Hazlett (2020). The logic is to entertain how much of the residual variance, in terms of partial $R^2$ in a regression model, an unobserved confounder would need to explain in either the outcome or explanatory variable to bring the observed effect towards zero. The figure suggests that, given the model specification and choice contiguity upper bound, an unobserved confounder would have to explain more than 50% of the partial $R^2$ in either party switching or nearby corruption to eliminate the effect reported in Figure 2 of the main text.

| Contiguity | Infractions | SE | p-value | Audited | SE | p-value | Interaction | SE | p-value | N | Adj. R-squared |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | 0.008 | 0.004 | 0.121 | 0.022 | 0.045 | 0.655 | -0.004 | 0.016 | 0.792 | 6246 | 0.025 |
| 2 | 0.007 | 0.002 | 0.024 | 0.027 | 0.009 | 0.061 | -0.005 | 0.002 | 0.106 | 12025 | 0.021 |
| 3 | 0.004 | 0.001 | 0.044 | 0.004 | 0.014 | 0.804 | -0.001 | 0.002 | 0.753 | 14400 | 0.019 |
| 4 | 0.005 | 0.001 | 0.031 | 0.010 | 0.013 | 0.486 | -0.001 | 0.001 | 0.450 | 14873 | 0.020 |
| 5 | 0.005 | 0.001 | 0.017 | 0.001 | 0.012 | 0.906 | -0.000 | 0.001 | 0.810 | 14657 | 0.020 |
| 6 | 0.005 | 0.001 | 0.016 | 0.016 | 0.010 | 0.196 | -0.001 | 0.001 | 0.197 | 14269 | 0.021 |
| 7 | 0.005 | 0.001 | 0.014 | 0.020 | 0.009 | 0.110 | -0.001 | 0.000 | 0.066 | 13798 | 0.021 |
| 8 | 0.005 | 0.001 | 0.012 | 0.034 | 0.007 | 0.017 | -0.002 | 0.000 | 0.004 | 13246 | 0.021 |
| 9 | 0.004 | 0.001 | 0.012 | 0.028 | 0.008 | 0.045 | -0.001 | 0.000 | 0.071 | 12592 | 0.021 |
| 10 | 0.004 | 0.001 | 0.012 | 0.037 | 0.017 | 0.119 | -0.001 | 0.001 | 0.163 | 11890 | 0.021 |

**Table B.3: The effect of nearby corruption on party switching by audit status using different operationalizations of nearby**

| | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Infractions | 0.00 | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Audited | −0.01 | −0.03 | −0.03 | −0.07 |
| | (0.02) | (0.02) | (0.02) | (0.03) |
| Interaction | −0.00 | 0.00 | 0.00 | 0.00 |
| | (0.01) | (0.00) | (0.00) | (0.00) |
| $R^2$ | 0.01 | 0.02 | 0.02 | 0.02 |
| Adj. $R^2$ | 0.01 | 0.02 | 0.02 | 0.02 |
| Num. obs. | 12025 | 14400 | 14873 | 14657 |
| RMSE | 0.49 | 0.49 | 0.49 | 0.49 |
| N Clusters | 4 | 4 | 4 | 4 |

$^*p < 0.05$

**Table B.4: Effect of nearby corruption on whether the incumbent mayor seeks reelection**

| | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Infractions | −0.00 | −0.00 | −0.00 | −0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Audited | −0.01 | −0.04 | −0.04 | −0.05 |
| | (0.02) | (0.03) | (0.02) | (0.02) |
| Interaction | 0.00 | 0.01 | 0.00 | 0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| $R^2$ | 0.04 | 0.04 | 0.04 | 0.04 |
| Adj. $R^2$ | 0.04 | 0.04 | 0.04 | 0.04 |
| Num. obs. | 12025 | 14400 | 14873 | 14657 |
| RMSE | 0.31 | 0.31 | 0.32 | 0.31 |
| N Clusters | 4 | 4 | 4 | 4 |

$^*p < 0.05$

**Table B.5: Effect of nearby corruption on whether the incumbent mayor wins reelection**

| | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Infractions | 0.01 | 0.01* | 0.01* | 0.01* |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Prop. same party | −0.06 | −0.05 | −0.02 | −0.08 |
| | (0.04) | (0.03) | (0.04) | (0.05) |
| Interaction | −0.00 | −0.01 | −0.02 | −0.01 |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| $R^2$ | 0.03 | 0.02 | 0.03 | 0.03 |
| Adj. $R^2$ | 0.02 | 0.02 | 0.03 | 0.03 |
| Num. obs. | 10805 | 13032 | 13477 | 13306 |
| RMSE | 0.33 | 0.33 | 0.33 | 0.33 |
| N Clusters | 4 | 4 | 4 | 4 |

$^*p < 0.05$

**Table B.6: Effect of nearby corruption on party switching among non-audited municipalities in interaction with the proportion of audited municipalities with mayors from the same party as the incumbent**

| | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Infractions | −0.01* | −0.01* | −0.00* | −0.00 |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Party switch | 0.23* | 0.21* | 0.20* | 0.20* |
| | (0.03) | (0.02) | (0.03) | (0.05) |
| Interaction | 0.00 | 0.00 | 0.01 | 0.00 |
| | (0.01) | (0.00) | (0.00) | (0.00) |
| $R^2$ | 0.11 | 0.10 | 0.11 | 0.11 |
| Adj. $R^2$ | 0.11 | 0.10 | 0.11 | 0.11 |
| Num. obs. | 10808 | 13035 | 13480 | 13311 |
| RMSE | 0.30 | 0.31 | 0.31 | 0.31 |
| N Clusters | 4 | 4 | 4 | 4 |

$^*p < 0.05$

**Table B.7: Effect of nearby corruption on winning reelection among non-audited municipalities in interaction with party switching**

| Contiguity | Infractions | SE | p-value | Audited | SE | p-value | Interaction | SE | p-value |
|---|---|---|---|---|---|---|---|---|---|
| 1 | 0.078 | 0.036 | 0.031 | 0.179 | 0.390 | 0.647 | -0.037 | 0.036 | 0.031 |
| 2 | 0.064 | 0.004 | 0.000 | 0.252 | 0.077 | 0.001 | -0.047 | 0.004 | 0.000 |
| 3 | 0.040 | 0.010 | 0.000 | 0.039 | 0.141 | 0.785 | -0.006 | 0.010 | 0.000 |
| 4 | 0.049 | 0.009 | 0.000 | 0.094 | 0.136 | 0.489 | -0.011 | 0.009 | 0.000 |
| 5 | 0.048 | 0.007 | 0.000 | 0.015 | 0.154 | 0.920 | -0.002 | 0.007 | 0.000 |
| 6 | 0.048 | 0.007 | 0.000 | 0.157 | 0.125 | 0.210 | -0.012 | 0.007 | 0.000 |
| 7 | 0.045 | 0.006 | 0.000 | 0.196 | 0.123 | 0.109 | -0.012 | 0.006 | 0.000 |
| 8 | 0.042 | 0.005 | 0.000 | 0.327 | 0.078 | 0.000 | -0.017 | 0.005 | 0.000 |
| 9 | 0.038 | 0.005 | 0.000 | 0.273 | 0.115 | 0.018 | -0.013 | 0.005 | 0.000 |
| 10 | 0.036 | 0.004 | 0.000 | 0.351 | 0.173 | 0.043 | -0.014 | 0.004 | 0.000 |

**Table B.8: Reproducing results from Figure 2 and Table B1 using logistic regression.**

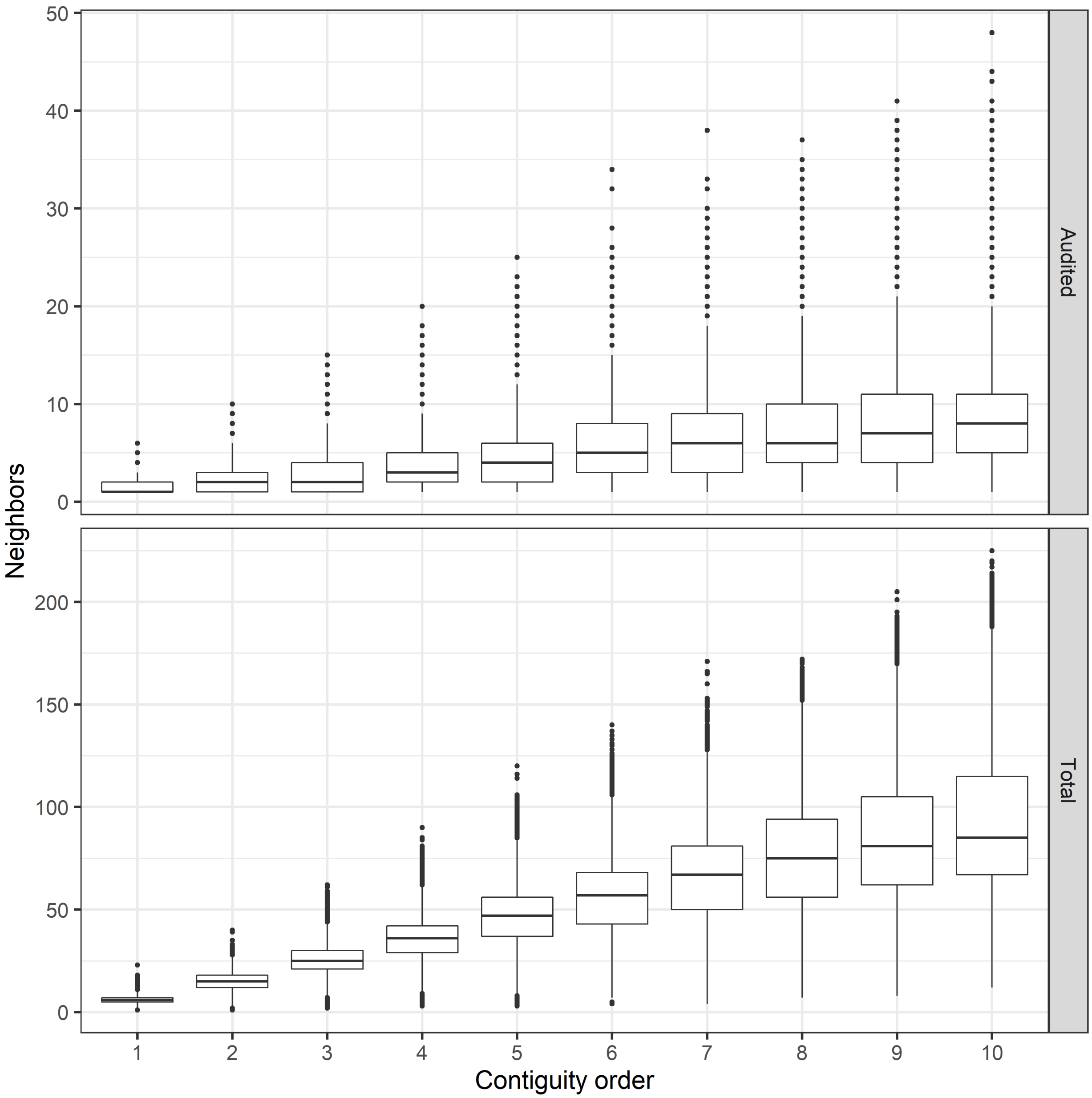


**Figure B.2: Distribution of audited and total number of neighbors by contiguity order.**

| | 2004 | 2008 | 2012 | 2016 |
|---|---|---|---|---|
| Intercept | 0.09* | 0.16* | 0.05* | 0.11* |
| | (0.02) | (0.02) | (0.01) | (0.01) |
| Infractions | 0.01* | 0.01* | 0.00* | 0.01* |
| | (0.01) | (0.01) | (0.00) | (0.00) |
| Audited | 0.04 | 0.03 | 0.03 | 0.07 |
| | (0.07) | (0.06) | (0.03) | (0.07) |
| Interaction | −0.01 | −0.01 | −0.00 | −0.01 |
| | (0.02) | (0.01) | (0.01) | (0.02) |
| $R^2$ | 0.00 | 0.00 | 0.00 | 0.00 |
| Adj. $R^2$ | 0.00 | 0.00 | 0.00 | 0.00 |
| Num. obs. | 2346 | 2877 | 3674 | 3128 |
| RMSE | 0.33 | 0.41 | 0.26 | 0.35 |

$^*p < 0.05$

**Table B.9: Effect of nearby corruption at the second cumulative contiguity order on party switching by election year.**

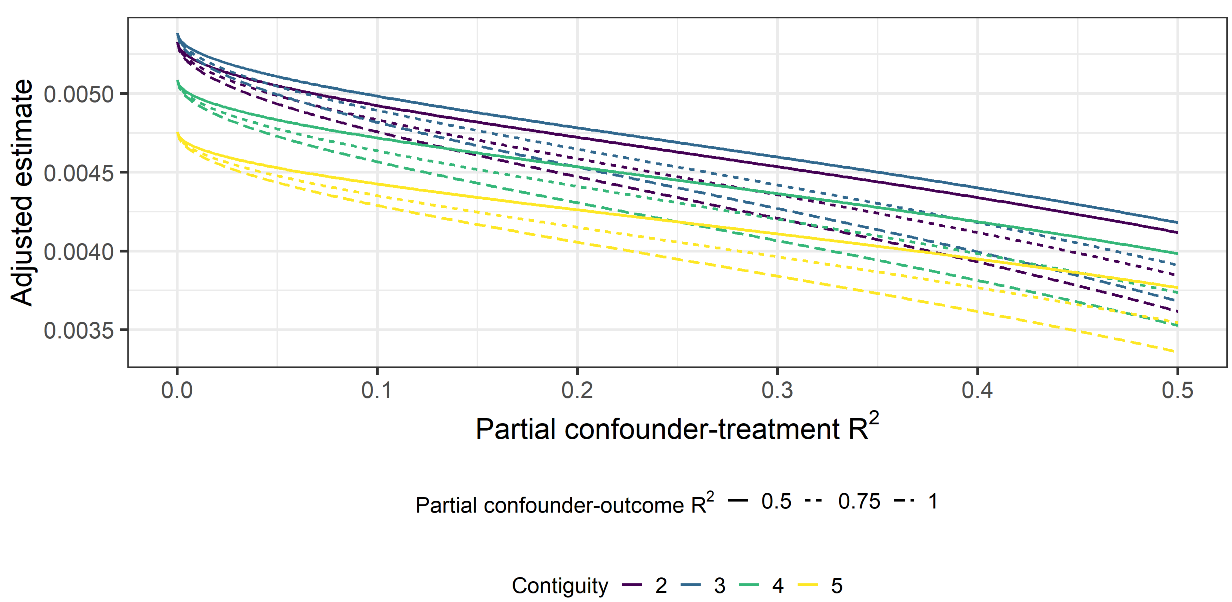


**Figure B.3: Sensitivity analysis for the effect of nearby corruption on party switching among non-aduited municipalities across optimal upper bounds suggested by cross-validation**

# Appendix C

# Supplementary Information for Chapter 3

## C.1 Extended Simulation Results

Figures C.1-C.4 show simulations results with $\tau = 0.26$. Figures C.5-C.8 do so for $\tau = 0.63$ The interpretation of the simulation results is the same as in the main text.

## C.2 Extended Frequentist Lasso Results

Figure C.9 shows the corresponding lasso coefficients at the corresponding value of $\lambda$, which informs model selection for both the lasso and lasso + OLS. Figure C.10 shows the coefficients along $\lambda$ for the adaptive lasso. The blue area indicates the range between the value of $\lambda$ that minimizes root mean squared error (RMSE) and $\lambda$ with RMSE within one standard deviation from it. As the coefficients converge towards zero, the interpretation is to exclude them from the estimation step. See the main text for further details.

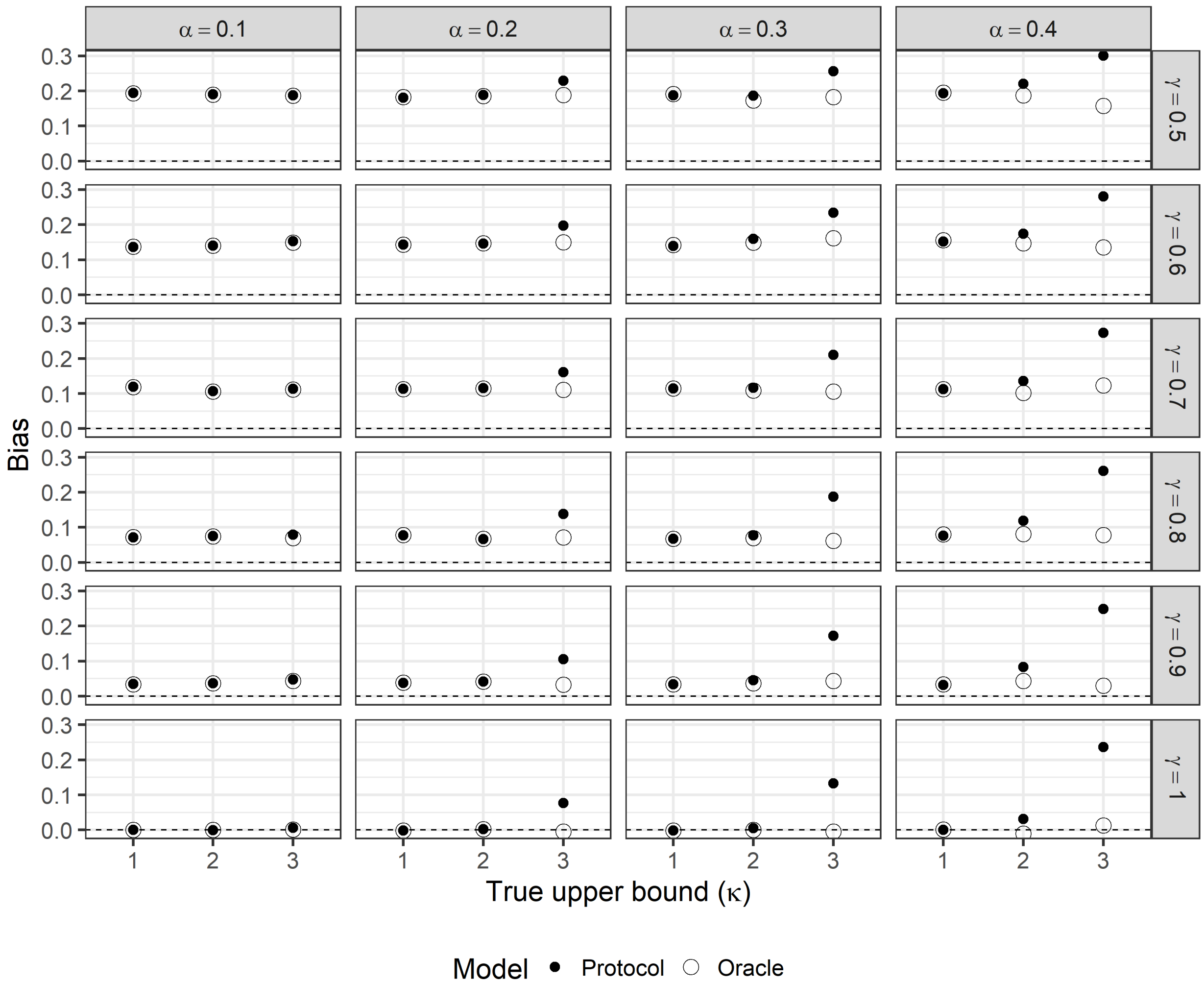


**Figure C.1: Bias for simulations with $\tau = 0.26$**

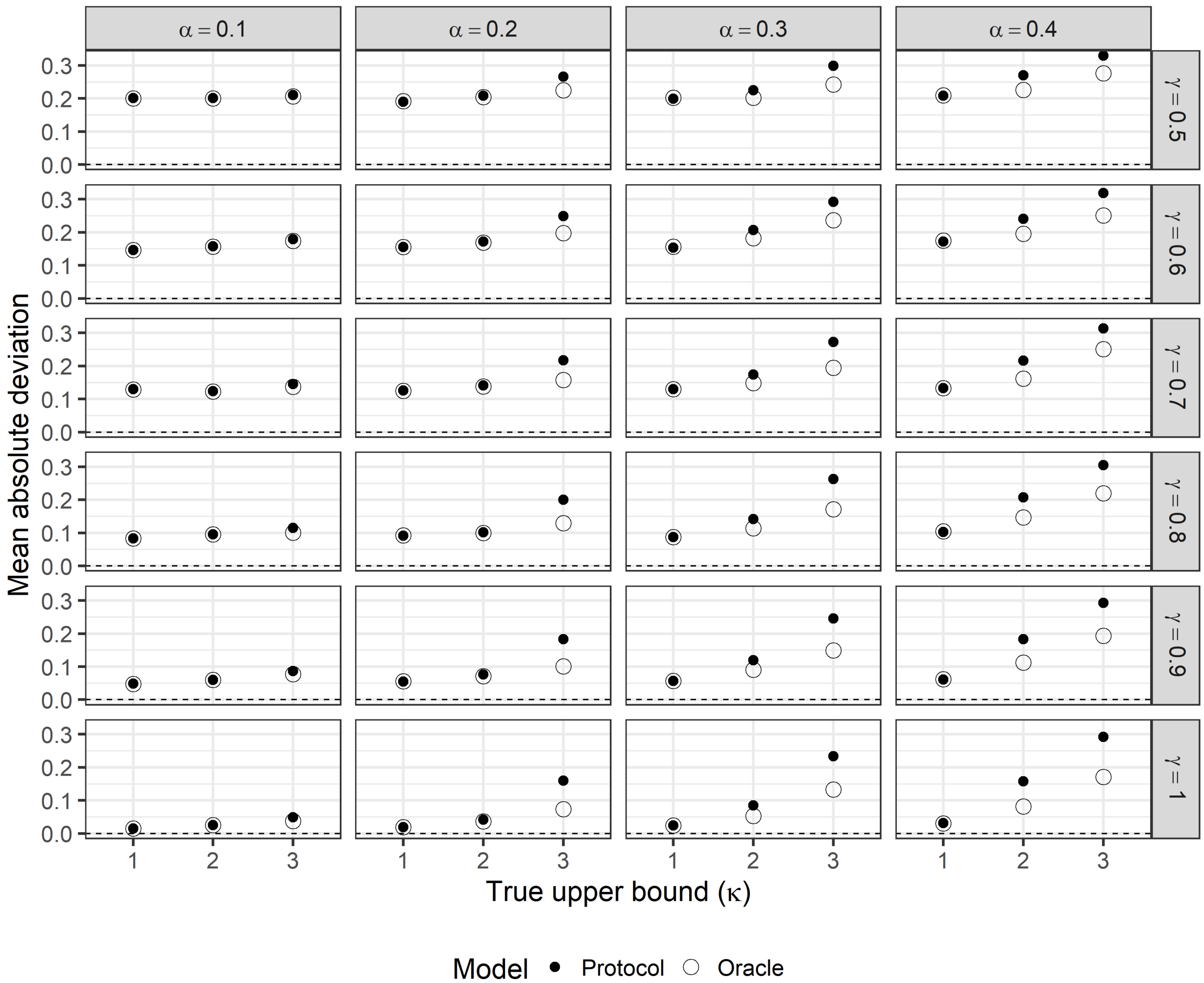


**Figure C.2: Mean absolute deviation for simulations with $\tau = 0.26$**

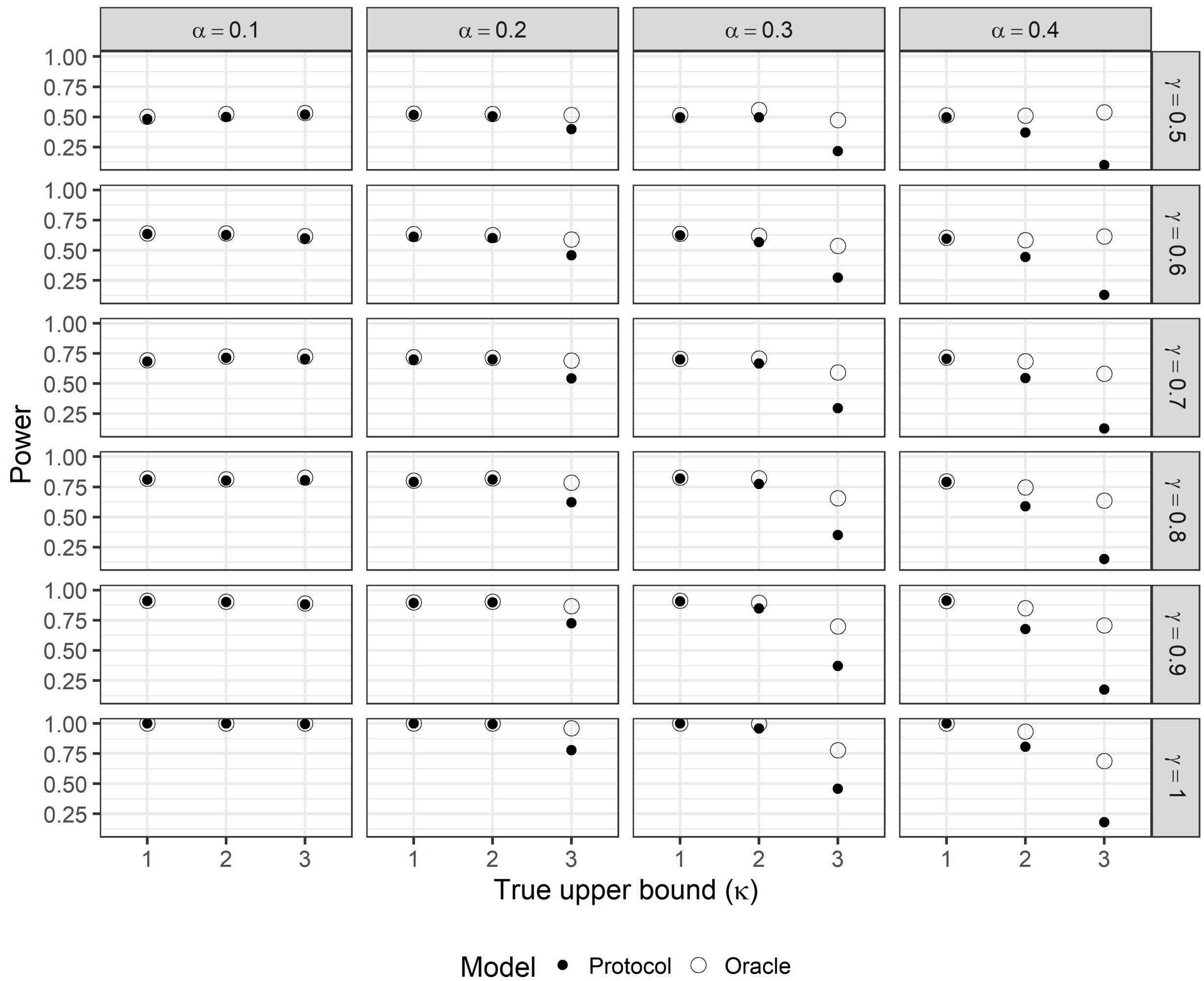


**Figure C.3: Power for simulations with $\tau = 0.26$**

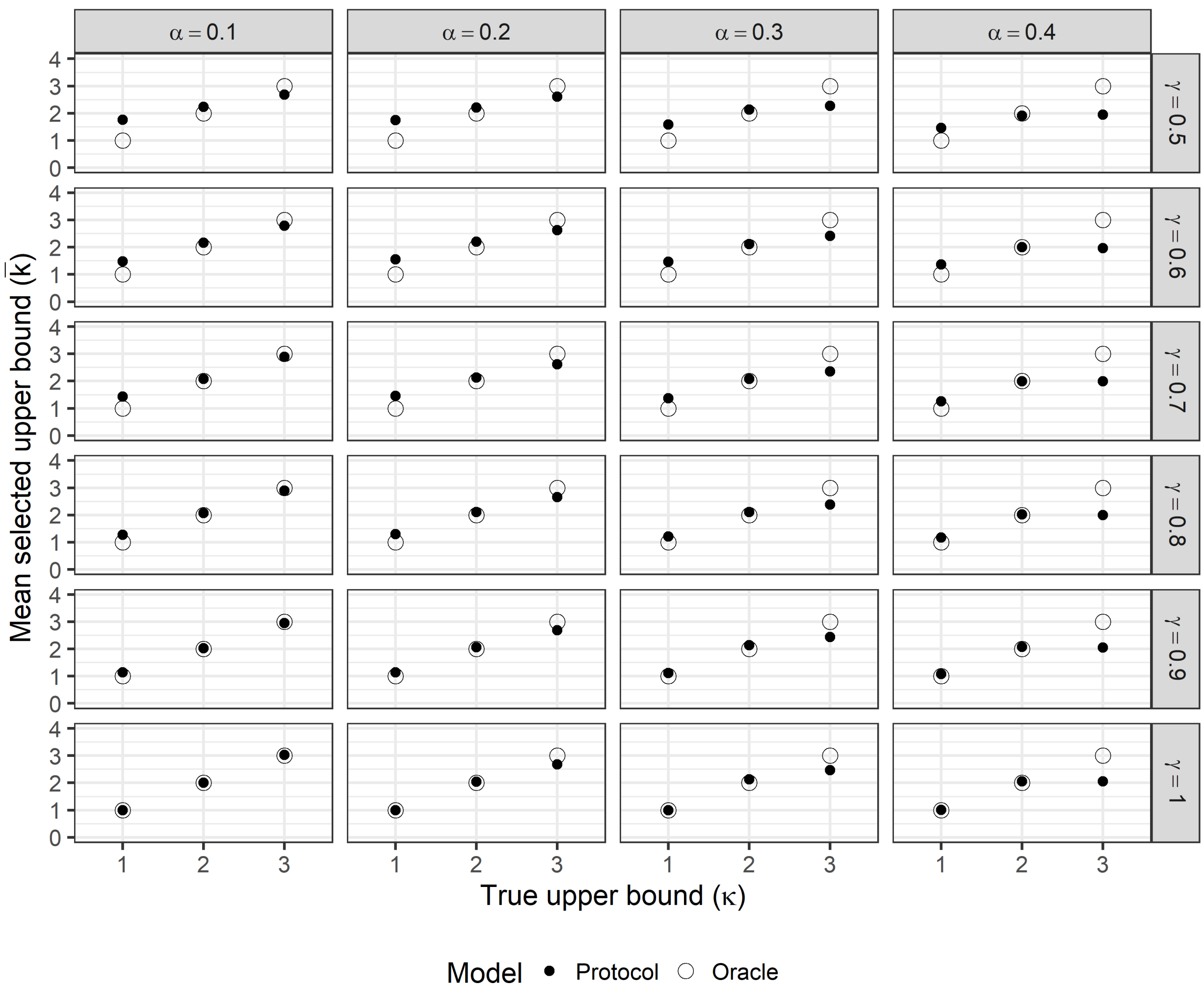


**Figure C.4: Mean selected upper bound for simulations with $\tau = 0.26$**

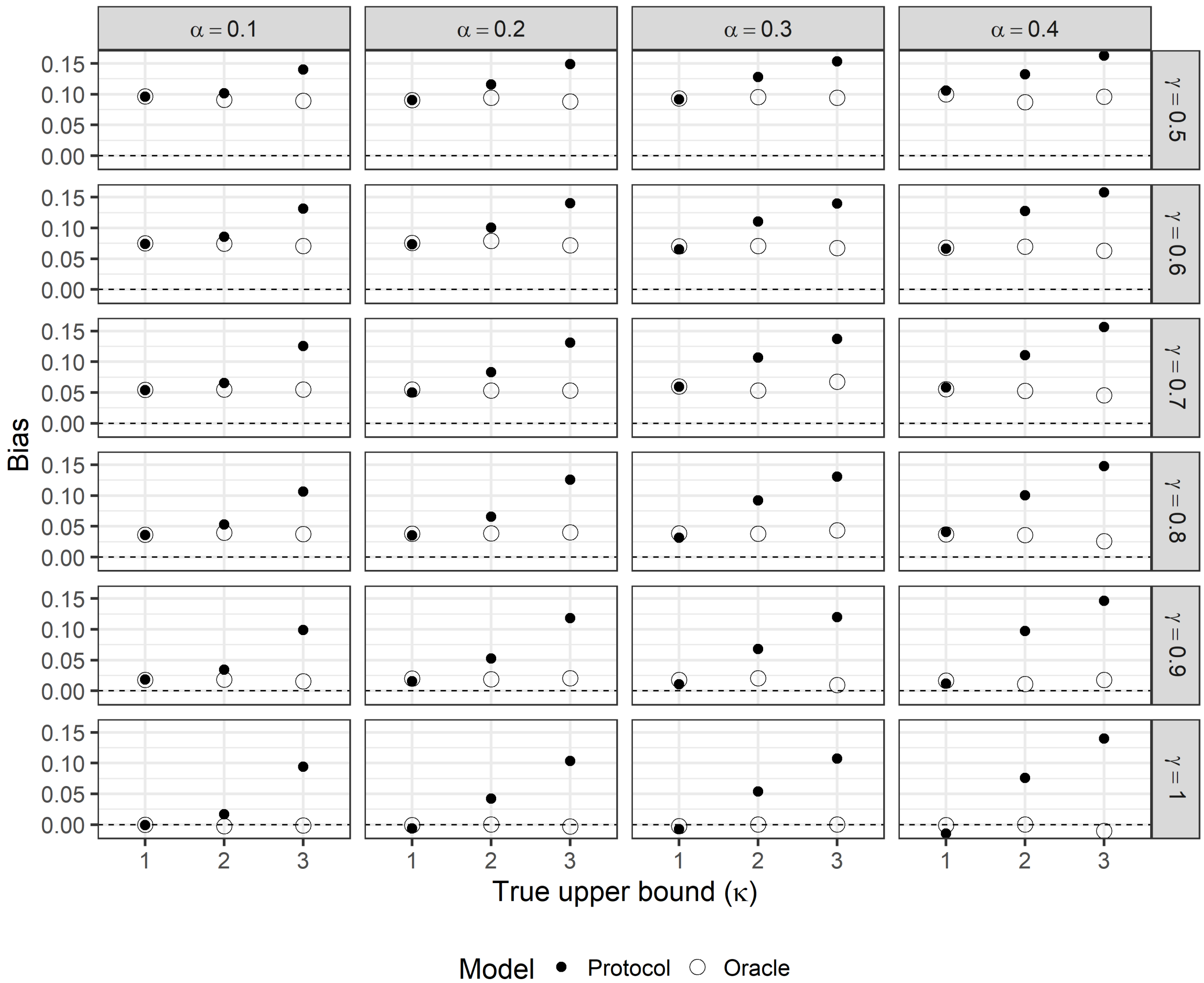


**Figure C.5: Bias for simulations with $\tau = 0.63$**

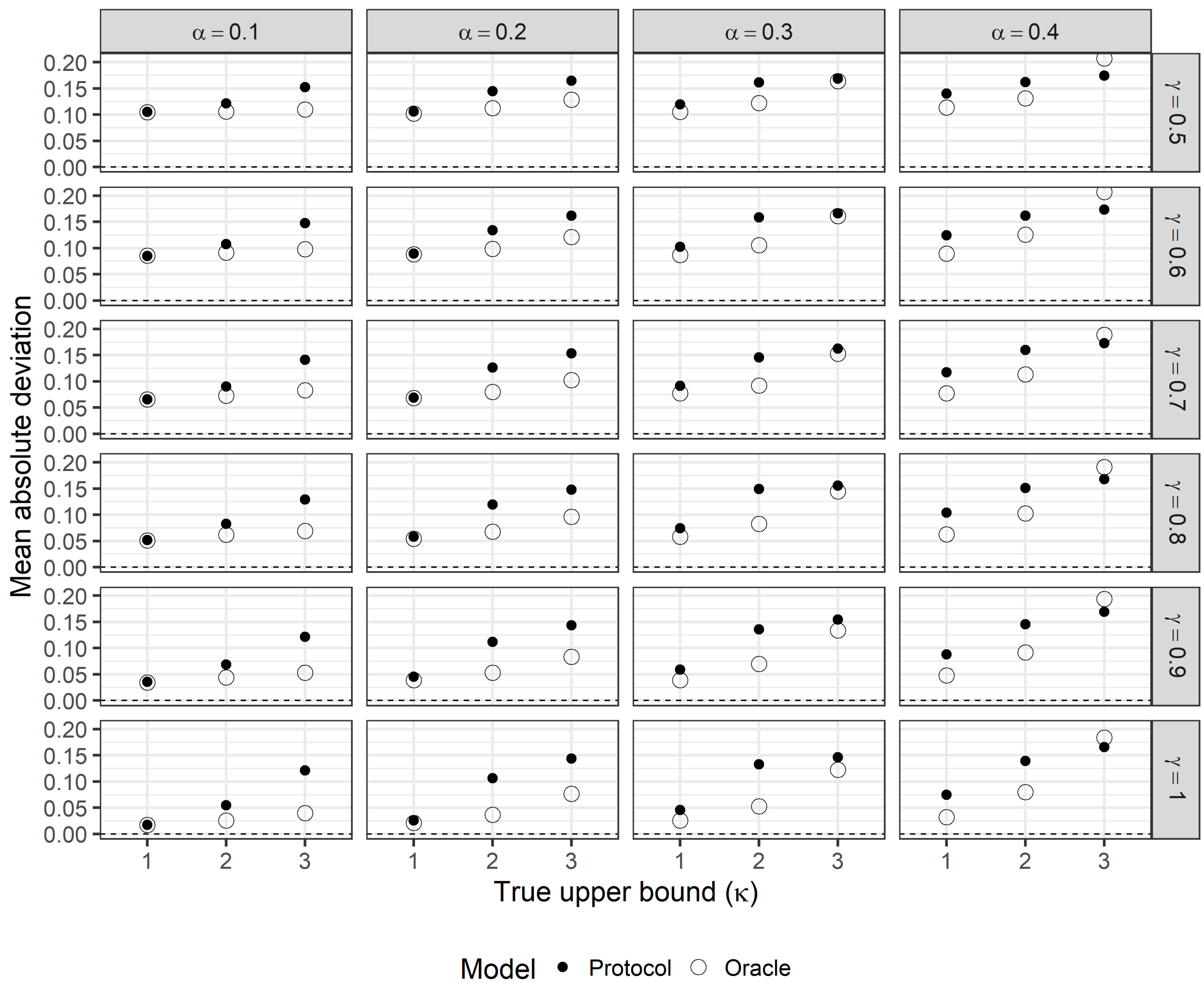


**Figure C.6: Mean absolute deviation for simulations with $\tau = 0.63$**

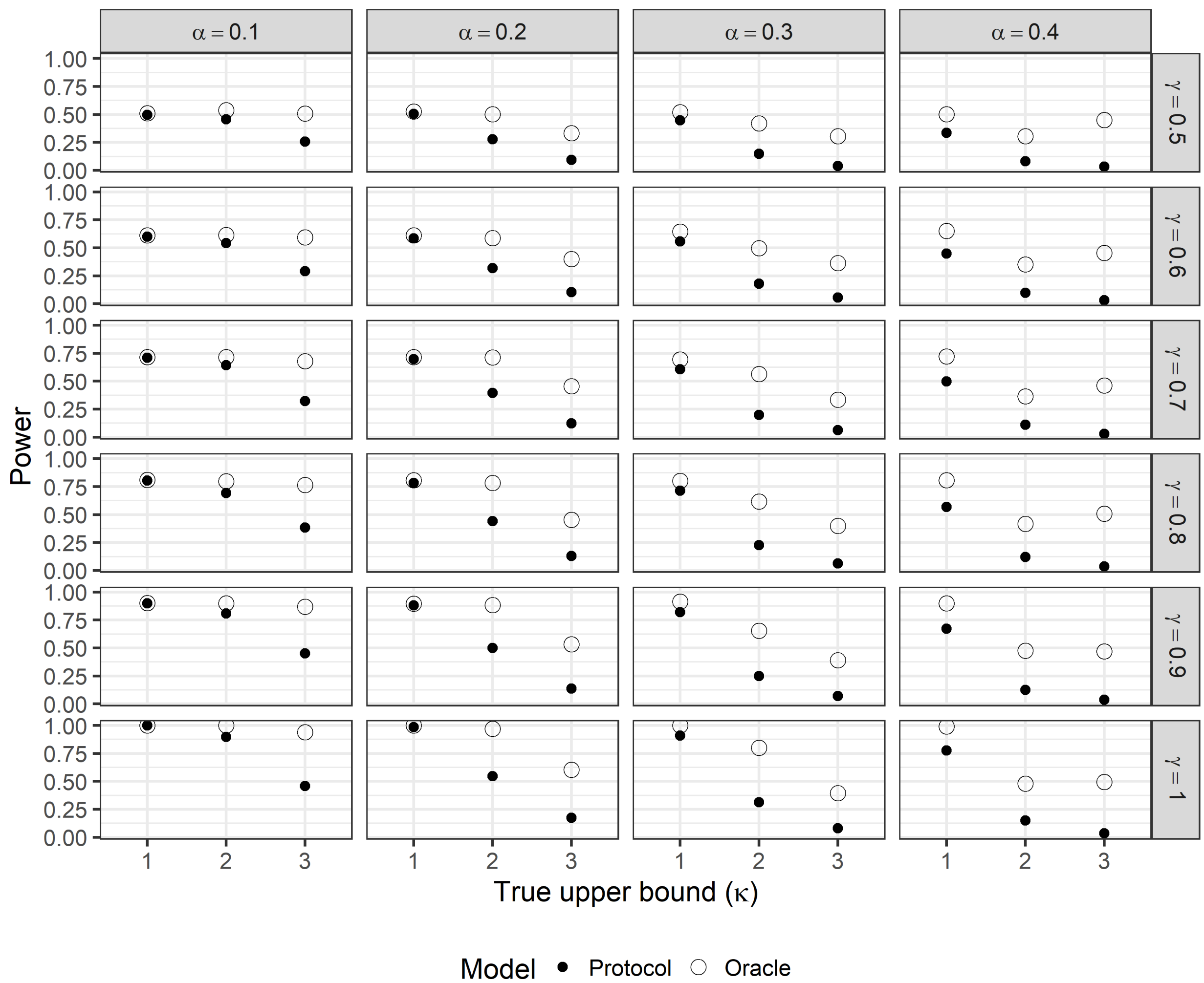


**Figure C.7: Power for simulations with $\tau = 0.63$**

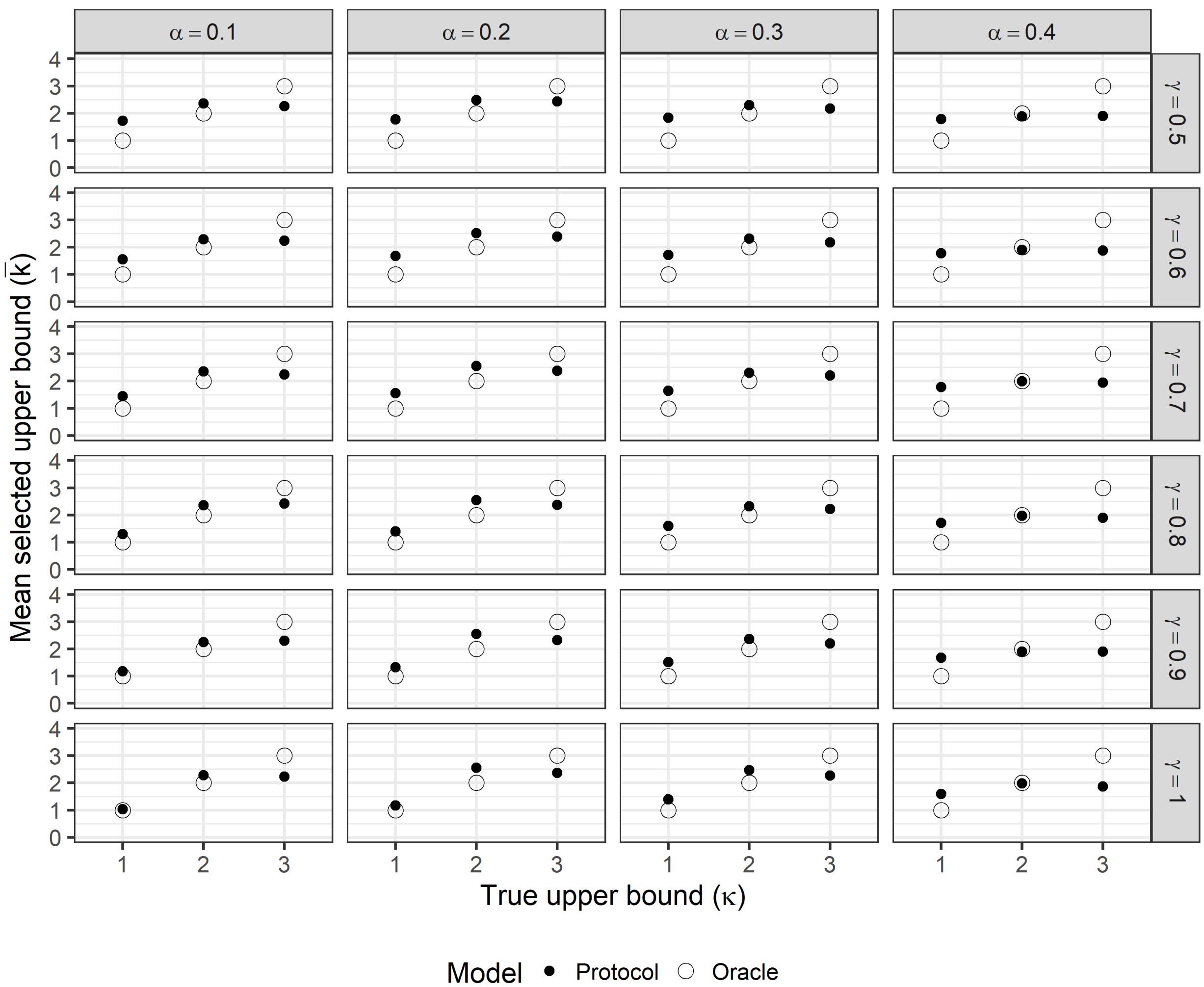


**Figure C.8: Mean selected upper bound for simulations with $\tau = 0.63$**

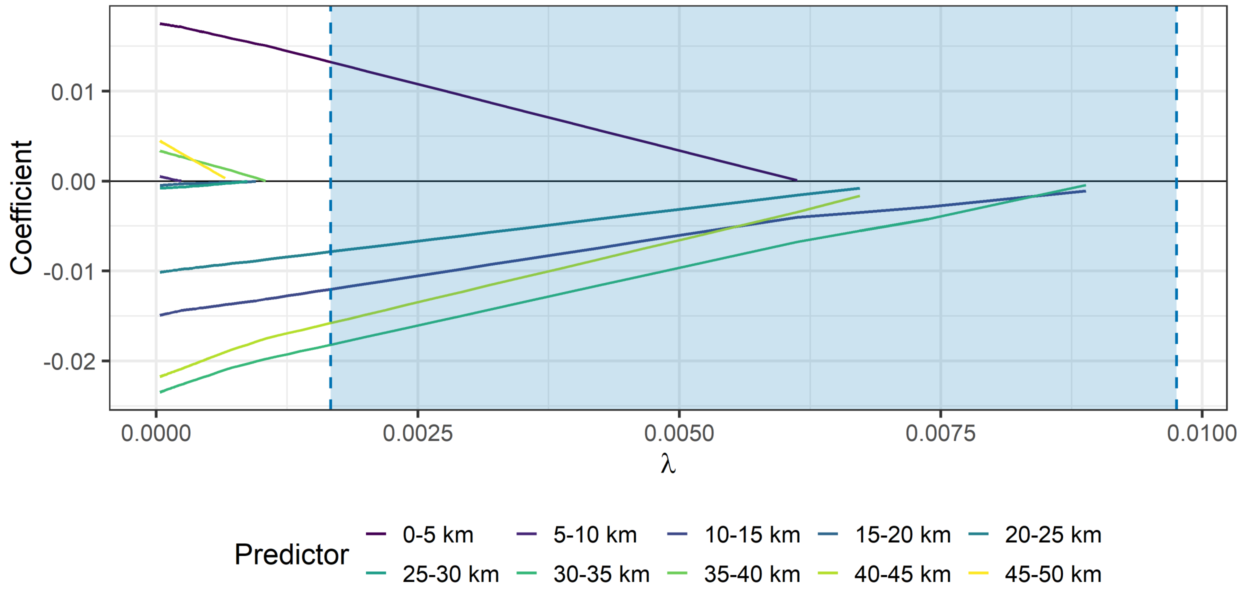


**Figure C.9: Lasso fit along $\lambda$**

*Note:* Colors denote the different predictors. The blue area denotes the values of $\lambda$ with RMSE within one standard deviation of the minimum, which is the leftmost vertical dashed line. As coefficients converge to zero, the interpretation is to exclude them from estimation.

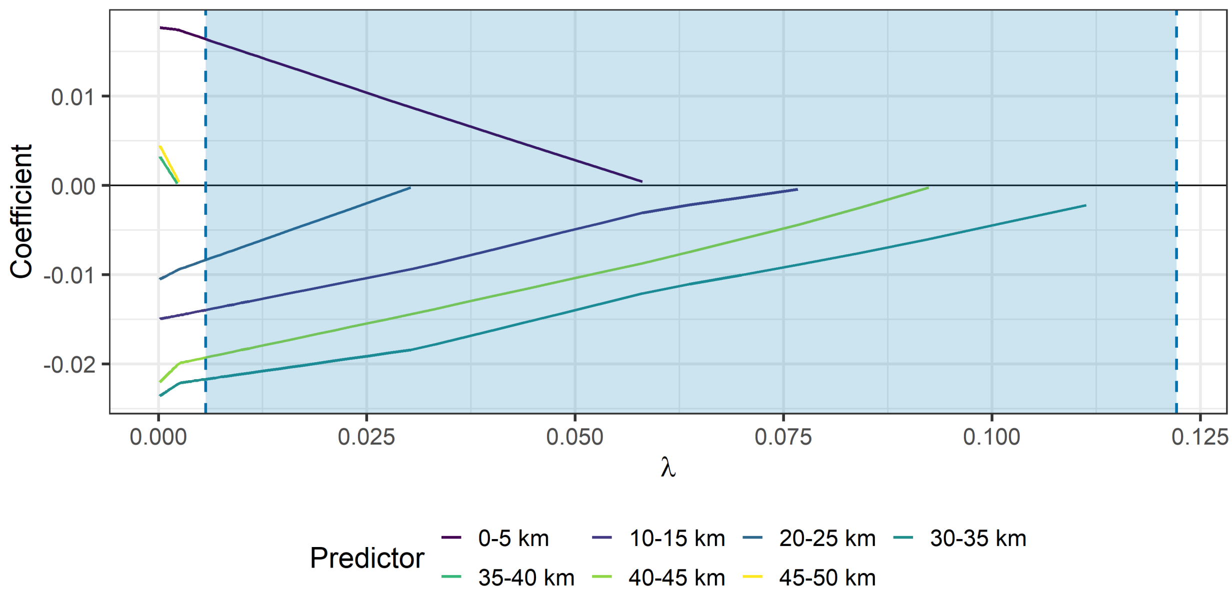


**Figure C.10: Adaptive lasso fit along $\lambda$**

*Note:* Colors denote the different predictors. The blue area denotes the values of $\lambda$ with RMSE within one standard deviation of the minimum, which is the leftmost vertical dashed line. As coefficients converge to zero, the interpretation is to exclude them from estimation. Predictors not displayed have a coefficient of zero throughout the values of $\lambda$.

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