Mayors Reduce Spending in Response to Increased Monitoring to Minimize Electoral Backlash: Evidence from Anti-Corruption Audits in Brazil[*]

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August 24, 2023

Abstract

Research on the effect of anti-corruption interventions suggests that increased monitoring at the local level reduces inefficiencies in public spending and improves the delivery of goods and services. In expectation, this occurs through both top-down sanctions from the central government and bottom-up accountability from citizens. However, recent evidence casts doubt on the second mechanism, implying than only top-down enforcement works. This paper argues in favor of bottom-up accountability by suggesting that incumbents undertake preemptive action to ward off potential electoral sanctions from their constituents. This happens because incumbent politicians' informational advantage lets them anticipate the consequences of increased monitoring before voters learn about it. Using data from an anti-corruption program that randomly selects municipalities for auditing in Brazil, I show that being selected for auditing leads to a reduction in public spending in a pattern that reveals incumbents' attempt to preserve their reelection chances. This is effect is more pronounced among mayors eligible for reelection and when audits happen close to an election year.

Word count: 7584

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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 of 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; Pande 2011 for reviews).

Recent work on anti-corruption monitoring casts doubt on the bottom-up accountability mechanisms. While research based on surveys and survey experiments reveals voters' distaste for corruption, these anti-corruption norms rarely translate into fewer votes against corrupt incumbents (Boas, Hidalgo, and Melo 2018; Incerti 2020). The literature identifies several explanations for this gap between attitudes and behavior. For example, citizens may forgive corruption among incumbents that perform satisfactorily in other dimensions (e.g. Fernández-Vázquez, Barberá, and Rivero 2016; Konstantinidis and Xezonakis 2013; Pereira

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[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.

and Melo 2015), there may not be credible alternatives to replace corrupt politicians (Boas, Hidalgo, and Melo 2018; Pavão 2018), or uncovering corruption may push voters away from participating in elections (Bauhr and Grimes 2013; Chong et al. 2015). Either way, the implication is that if increasing monitoring works, it is because of top-down enforcement instead of bottom-up accountability.

This paper argues in favor of bottom-up accountability by highlighting politicians' informational advantage. Incumbents often learn about the existence and potential outcomes of increased monitoring before citizens, which gives time to respond strategically and counter the potential negative consequences (Fisman and Golden 2017). Moreover, I argue that they do so in a pattern that reveals incentives to anticipate bottom-up accountability. Using data from an anti-corruption program in Brazil that randomly selects municipalities to audit their use of federal funds, I find that being selected for auditing leads mayors to decrease overall public spending[2]. This effect is more pronounced when mayors are eligible for reelection and when audits happen close to or during an election-year. I also find no evidence that this reduction does not tend to come at the expense of specific budget categories. Taken together, these findings suggest that incumbent mayors adapt their behavior in office in reaction to increased monitoring in an attempt to minimize being presented as bad types in front of their constituencies, which serves the purpose of preserving incumbents' reelection chances.

2 Monitoring, accountability, and spending

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

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[2]Following research on political budget cycles, the original hypothesis was that being selected for auditing decreases spending, yet the findings still align with a theory that favors bottom-up accountability

inefficiencies that raise the cost of government activity and harm the provision of public goods and services (Olken and Pande 2012).[3] 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 paper, 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.[4]

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

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

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

updates politicians' belief in 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 audited municipalities and their neighbors. Focusing on the same program, Brollo (2011) shows that municipalities where corruption was uncovered experience a reduction in transfers from the federal government as indirect punishment.

Second, increased monitoring can assist citizens in holding providers and politicians accountable by incorporating them 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.

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, a replication finds similar effect on health outcomes but no evidence of citizen involvement [cite raffler WP]. Moreover, Olken (2007) finds no effect of this type of encouragement on 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 in Brazil, subsequent work shows that this effect disappears after the 2004 local election (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, Adida, 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 in 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. This has led scholars to try to understand the circumstances under which voters fail or choose not to sanction corruption. Explanations include voters forgiving corruption among politicians from their preferred political party (Anduiza, Gallego, and Muñoz 2013; Eggers 2014) or when they provide good economic outcomes (Fernández-Vázquez, Barberá, and Rivero 2016; Konstantinidis and Xezonakis 2013; Muñoz, Anduiza, and Gallego 2016; Pereira and Melo 2015). Another possibility is voters ignoring corruption information when the source is not credible (Botero et al. 2015; Weitz-Shapiro and Winters 2017; Winters and Weitz-Shapiro 2018). Finally, in the absence of clean alternatives, voters may choose to disregard corruption information in favor of aspects of politicians' performance in office (Boas, Hidalgo, and Melo 2018; Pavão 2018) or simply opt out from participating in elections altogether (Bauhr and Grimes 2013;

Chong et al. 2015).

These explanations share the goal of trying to identify the circumstances under which information does not lead to accountability. However, an alternative explanation for 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).

If this applies in the context of increased anti-corruption monitoring, the implication is that bottom-up accountability still exists, but politicians anticipate electoral backlash before it happens. Research on the electoral consequences of investigating and uncovering corruption 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 in reaction to uncovered or alleged corruption, but they do not show whether those who are directly investigated for corruption also engage in similar anticipatory behavior.

2.3 Electoral incentives to change spending in reaction to increased monitoring

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 performance accordingly. In the case of audits seeking to uncover corruption, as it occurs with the Brazilian audit program discussed in this paper, 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). This means that 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 to counter 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). Either way, this literature suggests 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 should an incumbent change spending in reaction to increased monitoring? The literature on political budget cycles would suggest that mayors would increase or concentrate spending to please their constituencies. This is supported by formal theoretical accounts that characterize incumbents' activity as a tradeoff between public spending and rent seeking. In this context, an exogenous shock such as increased monitoring reduces the space for rent seeking by improving voters' ability to verify whether the proposed spending platform is met (Persson and Tabellini 2000, chap. 4).

Empirically, evidence from survey experiments and observational studies also suggests this by showing how 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 politician 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).[5] Most relevant to this paper. Pereira and Melo (2015) use data from the state of Pernambuco in Brazil to show that the negative effect of uncovered corruption on the probability of incumbent reelection disappears among mayors with higher public spending.

Taken together, these findings suggest that mayors can try to counter the potentially negative electoral consequences of increased monitoring through increased spending.[6] This was the original expectation of this paper, but the findings in the Results section point out in the opposite direction: mayors with reelection incentives tend to decrease spending in reaction

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

[6]This assumes that constituencies generally prefer targeted spending, which does not need to be true for every constituency. For example, Alt, Mesquita, and Rose (2011) show that governors in the United States tend to lower spending to preserve their reelection chances. See chapter 2 in Persson and Tabellini (2000) for an extensive theoretical discussion on how the distribution of voter preferences affects spending.

to increased monitoring.

Why would mayors decrease spending to preserve their reelection chances? Formal theories of accountability in elections highlight how performance information helps voters hold politicians accountable in two ways. First, voters can retrospectively sanction poor performance with their vote. Second, voters can prospectively use this information to select good types (Barro 1973; Fearon 1999; Ferejohn 1986). Returning to the spending-rent seeking tradeoff model from a prospective selection perspective, voters may interpret variation over time in spending as a noisy signal of incumbent type, which does not help incumbents in preserving their reelection chances (see Persson and Tabellini 2000, chap. 4 for a detailed discussion).

For the purposes of this paper, this implies that decreasing spending in reaction to corruption may also serve as an indication of incumbents attempting to preserve their reelection chances. This happens because there is a temporal gap between being selected for monitoring and the release of results to the public, which gives incumbents time to minimize the chances of uncovering irregularities from monitoring by reducing overall government activity. Previous research on the Brazilian anti-corruption audits hints at this possibility, since audited mayors that are eligible for reelection misappropriate fewer resources than those who are term-limited (Ferraz and Finan 2011).

2.4   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 paper. 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.

Finally, a reduction in overall spending may emerge as an artifice of attempting to target spending to specific audiences, as reallocating resources from some budget areas to others does not need to involve a one to one conversion in monetary amounts. I address this possibility by estimating the effect of increased monitoring across budget areas.

3   Research Design

3.1   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).[7] According to the Global Corruption 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.[8] Across 13 years, the program conducted 40 lotteries, translating into 2,187 audits across 1,918 municipalities.[9]

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

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[7]See https://www.transparency.org/en/countries/brazil.

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

[9]The program continued after 2015, but was reformulated to include random and non-random audits, as well as audits to specific 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.

irregularities found by the auditors and the associated monetary value.[10] 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, 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).

3.2   Explanatory variables: Audit selection and timing

Sincce 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 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 36th 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 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.[11] That is, for each

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[10]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).

[11]Actual treatment assignment happens within state-lottery strata, but since the unit of analysis is the
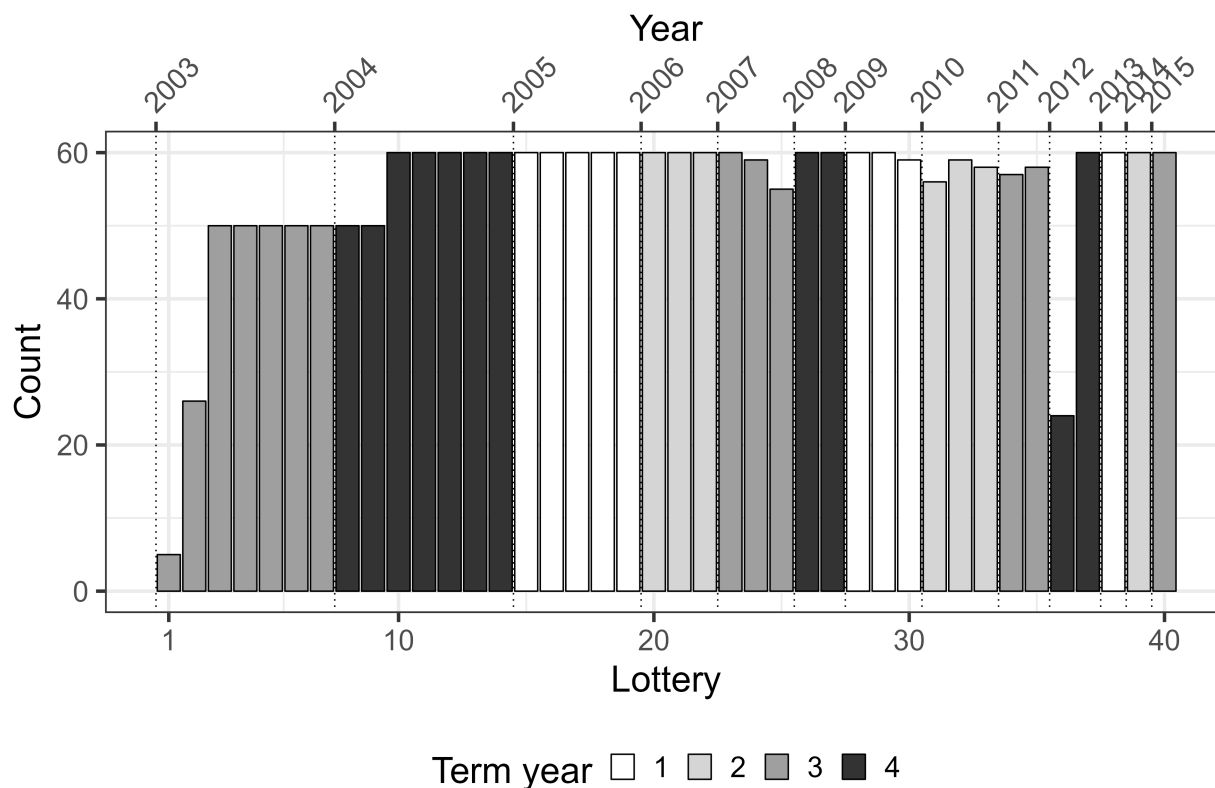
Figure 1: Distribution of audits across lotteries over time
Note: Vertical dotted lines denote the beginning of a year. Colors indicate the year within the mayoral term in which the audit happens.

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 ($p = 0.06$). This is expected since only municipalities with a population smaller than 500 thousand can be audited. However, a χ^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).

municipality-term, this is as close as I can get.

Table 1: Comparing non-audited and audited municipalities along selected covariates within state-term strata

	Non-audited	Audited	Adj. diff.	Std. diff.	p-value
Population (thousands)	30.82	23.81	-7.01	-0.04	0.06
Female population (%)	0.49	0.49	0.00	0.01	0.64
Rural population (%)	0.38	0.38	-0.01	-0.03	0.16
Human Development Index	0.69	0.69	0.00	-0.01	0.38
GDP per capita	12.54	12.73	0.19	0.01	0.60
Welfare recipients per capita	0.10	0.10	0.00	0.00	0.91
Illiteracy (%)	0.23	0.23	0.00	0.01	0.39
Post-secondary education (%)	0.03	0.03	0.00	0.00	0.84
Has local media	0.70	0.71	0.01	0.02	0.37
Mayor term limited	0.31	0.31	0.00	0.00	0.99
Previous incumbent vote (%)	0.14	0.15	0.00	0.02	0.39
PT incumbent	0.08	0.08	0.00	-0.01	0.72
PSDB incumbent	0.15	0.14	-0.01	-0.04	0.09

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

In additional analyses, I distinguish the year within the mayoral term in which the audit happened. The colors in Figure 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.[12] This means the results reported in this paper correspond to mayors' reaction to the news that their administration will be monitored. Section C of the appendix shows that results point in the same direction when using the extent of corruption uncovered by the audits as an explanatory variable, but now effects are similar regardless of a mayors' term-limit status, which suggests that increasing levels of corruption uncovered through monitoring also create incentives to preserve future reelection chances among those term-limited.

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[12]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

3.3 Outcome variable: Public spending

Public spending data comes from the *Insituto de Pesquisa Econômica Aplicada*.[13] The main 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 A1 in the appendix shows the list of budget categories.

Since the unit of analysis is the municipality-term and the goal of this paper is to capture incumbents' attempt to preserve their reelection chances, I focus primarily on the effect of audits on total per capita spending in the fourth year of the mayoral term, which corresponds to the election year. Figure 2 shows the distribution of this outcome, distinguishing across election years. The distributions of total spending is 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.[14]

I estimate the effect of audits on spending outcomes using OLS regression with state-term fixed-effects and clustered standard errors by municipality. Ideally, I would also account for the fact that audits are randomized by lottery waves over time, sometimes with several waves in a year. However, since the data is aggregated at the state-term level, this is as close as I can get to the ideal estimation strategy. The next section reports results using figures. Section B of the appendix shows the underlying numerical results.

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[13]Available at http://www.ipeadata.gov.br/.
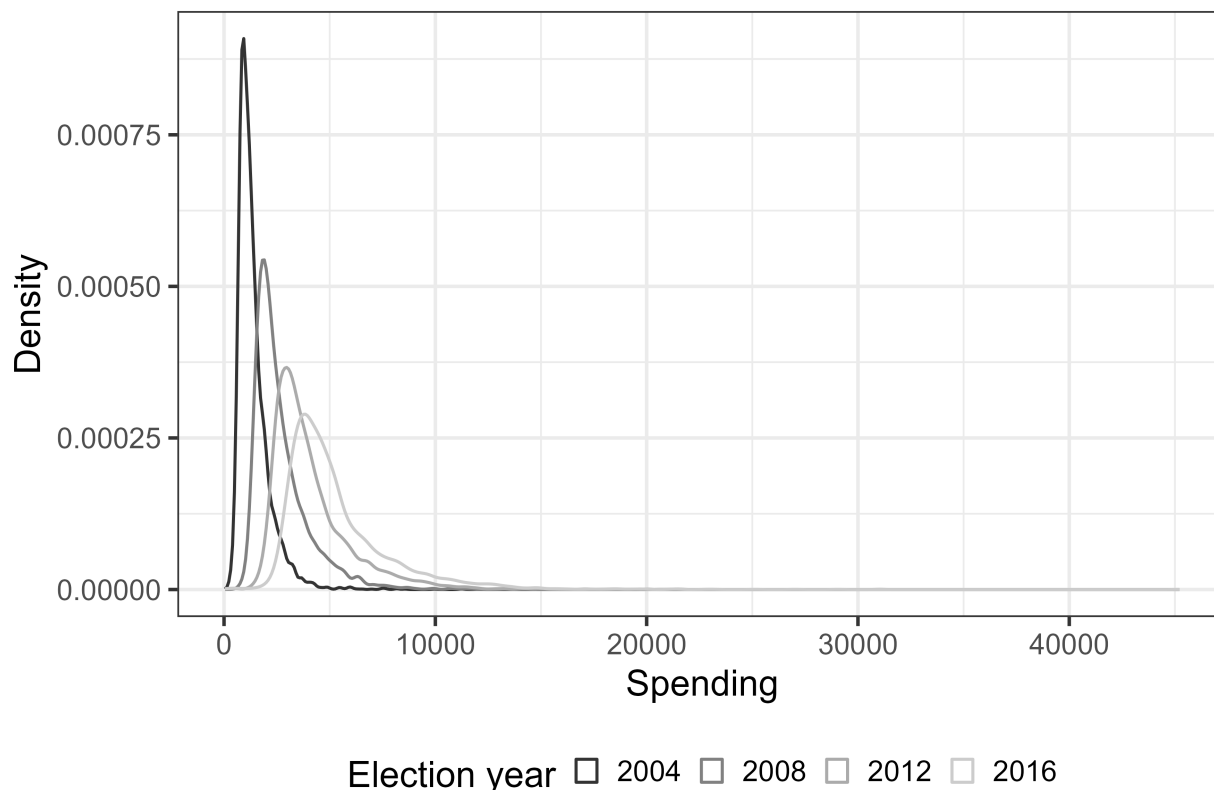[14]In this case the transformation is $\ln(y + 1)$ with y being the outcome in question.

Figure 2: Distribution of total spending per capita
Note: Colors denote election years. Panels have different scales in both axes.

4 Results

4.1 Main results

Figure 3 shows the effect of audit selection on total spending per capita (logged), 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, 2.5% less than non-audited mayors with reelection incentives.

This result suggest that increased monitoring has an effect on spending amount only in municipalities with incumbents that reelection incentives. I interpret this finding as evidence
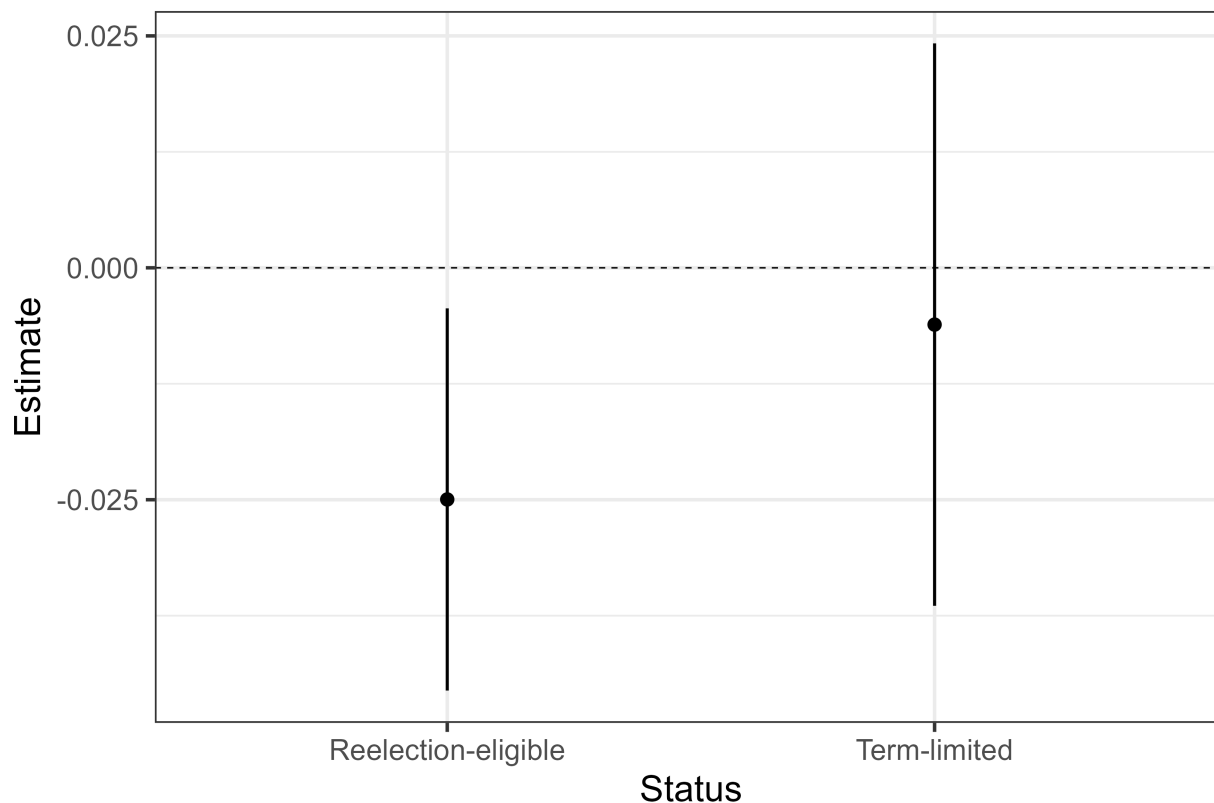
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Figure 3: Effect of audit selection on election-year (logged) total spending per capita
Note: Based on OLS regression with state-term fixed effects and clustered standard errors by municipality. Vertical lines denote 95 percent confidence intervals.

in favor of the argument that mayors investigated for potential corruption try minimize the extent of irregularities uncovered in the audits through reduced spending.

4.2 Additional results

4.2.1 Audit timing

The main argument in this paper 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 uncov-

ered 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 addressed in part by Figure 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 in favor of the bottom-up electoral accountability mechanism. 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 paper, this implies that increased monitoring should have a more pronounced effect on spending outcomes as audits happen closer to the election, since incumbents expect heightened scrutiny on their performance around then.

Figure 3 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 4 in the mayoral term. A non-zero effect in year for also appears among term-limited mayors, although the research design may not have enough power to distinguish this effect from a false positive ($p \approx 0.045$). On average, audited mayors spend 7% less on average than non-audited mayors among those eligible for reelection, whereas among term-limited mayors this difference is about 4.3%.

This pattern suggests that mayors update their spending patterns close to an election year. Since elections in Brazil usually happen in October and candidates must announce their decision to run a year in advance, by year 4 in their term mayors already know whether they will participate in elections. Moreover, I attribute the non-zero effect among term-limited mayors in year 4 to city council elections, which happen concurrently with mayoral elections.
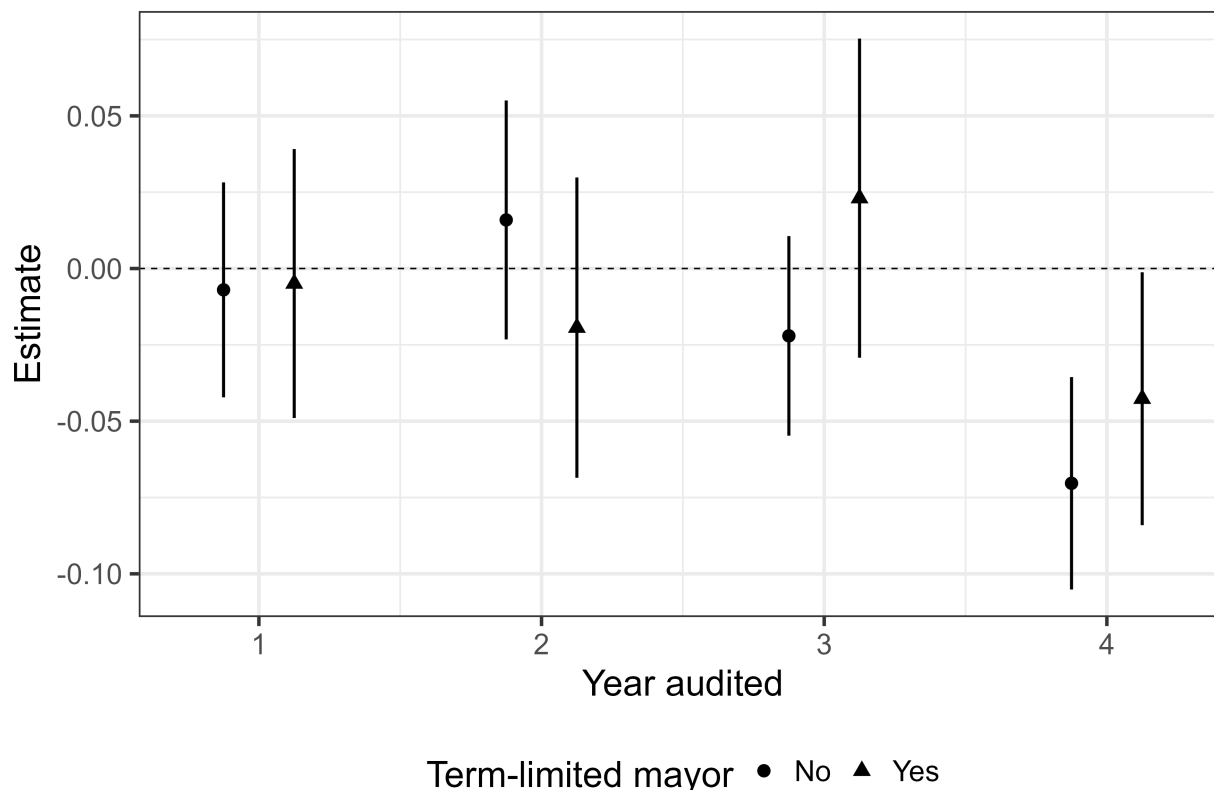
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Figure 4: Effect of audit timing on election-year (logged) total spending per capita
Note: Based on OLS regression with state-term fixed effects and clustered standard errors by municipality. Vertical lines denote 95 percent confidence intervals.

In this case, incumbents may still try to minimize irregularities uncovered by audits to protect their chances of being elected as city council members.

4.2.2 Spending across the term

The second alternative explanation is that 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 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 5 shows the effect of audit timing on spending outcomes across the term among municipalities with reelection-eligible mayors.[15] Audits have a non-zero effect on total spending per capita only in year 4 and only when they happen in year 4 among both reelection-eligible and term-limited mayors ($p \approx 0.045$ among term-limited mayors). Point estimates in year 4 are equivalent to those presented in Figure 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 anticipate bottom-up accountability.

4.2.3 Budget concentration

A final alternative explanation is that the overall reduction in spending is an artificial byproduct of concentrating budget in some areas to please specific constituencies. This would still align with a story about bottom-up electoral accountability, but not in the terms described in this paper.

Figure 6 addresses this implication by estimating the effect of auditing on election-year spending across 20 budget categories in municipalities with reelection-eligible incumbents.[16]

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[15]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.

[16]I estimate effects on 20 out of a total of 21 budget categories because I omit the category of regional
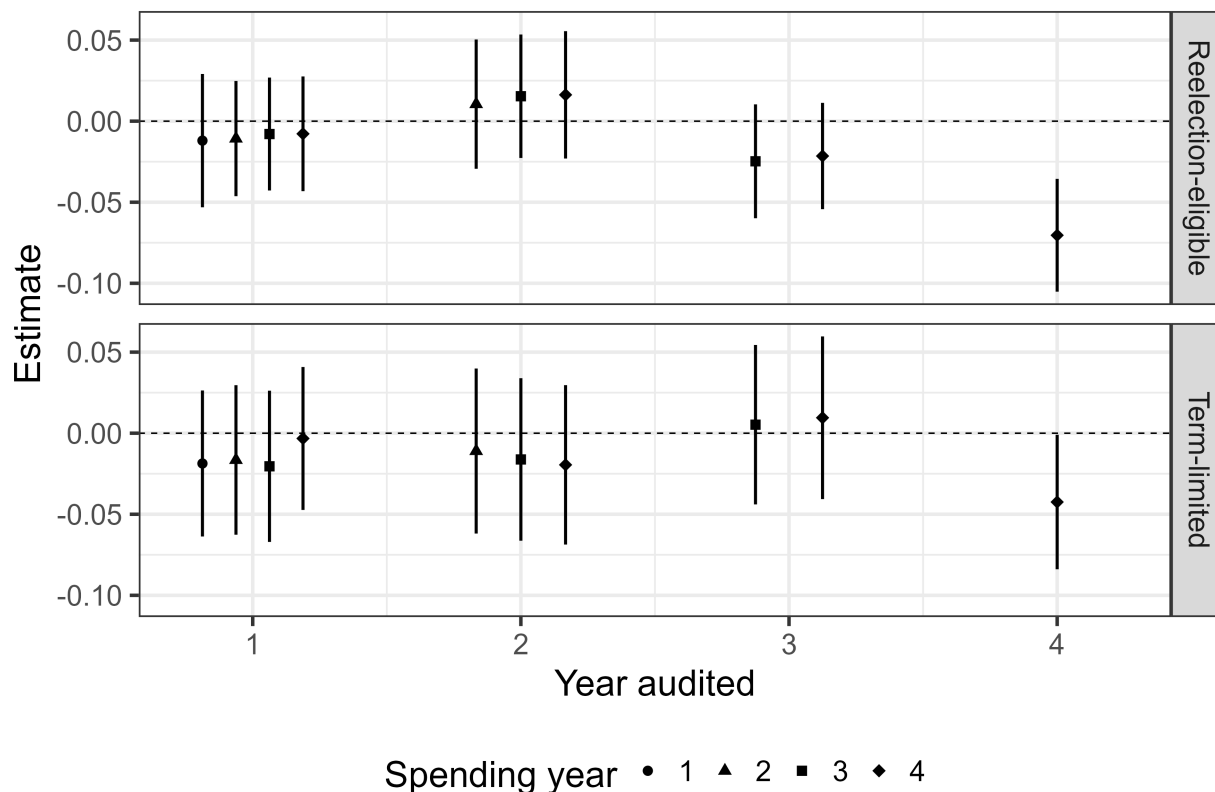
Figure 5: Effect of audit timing on (logged) total spending per capita (top) across the mayoral term by term-limit status

Note: Based on OLS regression with state-term fixed effects and clustered standard errors by municipality. Vertical lines denote 95 percent confidence intervals. 14 cases of municipalities audited twice in the same term omitted.

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. At first glance, the figure suggests a negative effect of audit selection on spending in agriculture, sports and leisure, defense and security, and municipal operations. However, 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 gray color which estimates have FDR-adjusted p-values larger than the conventional significance cutoff of 0.05, in which case I interpret that the corresponding non-zero estimate emerged by chance.

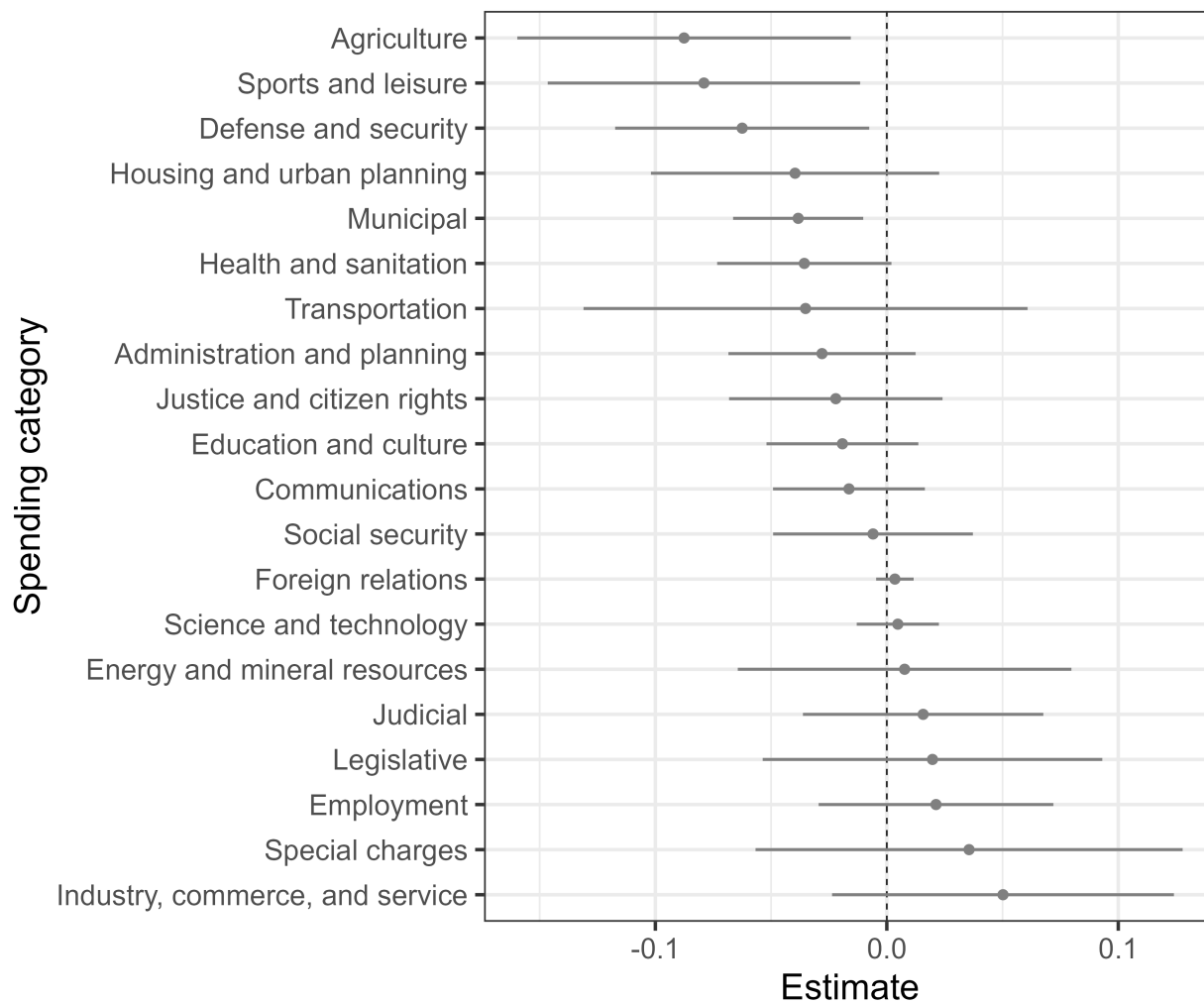development since all municipalities record zero spending in this outcome.

Figure 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 state-term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals. Gray color indicates estimates with false-discovery rate adjusted p-values larger than the conventional 0.05.

5 Conclusion

This section is in progress. For now, I want to emphasize that I am not trying to say that bottom-up accountability is more important than top-down enforcement. Instead, the main point here is that politicians internalize bottom-up accountability before it happens, but that still means that politicians care about bottom-up accountability!

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