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

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September 1, 2022

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Abstract

Research on the effect of anti-corruption interventions suggests that increased monitoring reduces inefficiencies in public spending and improves the delivery of goods and services. Where does this incentive come from? Current explanations highlight the role of both bottom-up (e.g. police crackdowns, legal investigation) and top-down mechanisms(e.g. electoral sanctions, reduced tax collection). However, recent work on the electoral consequences of corruption simultaneously casts doubt on the bottom-up mechanism and provides indirect evidence for an alternative route: Voters rarely have the chance to punish corruption because politicians adjust their behavior in office in reaction to increased monitoring. This paper uses data from an anti-corruption program that randomly selects municipalities for auditing in Brazil to test this implication directly. I find that audits lead to a reduction in public spending and an increase in the concentration of spending across budget cateogories in a pattern that reveals incumbents' attempt to preserve their reelection chances. The effect is more pronounced among mayors eligible for reelection and when audits happen close to an election year. Moreover, I show that the reduction in spending occurs primarily in highly visible budget categories.

*I thank Jake Bowers, Avital, Livny, Gisela Sin, Matt Winters, and workshop participants at Tulane CIPR for their helpful feedback.

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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 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

[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.

among politicians who exhibit positive economic outcomes (Fernández-Vázquez, Barberá, and 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 paper 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 when 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 paper 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 paper 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 effectiveness 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 paper 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.

2 Monitoring, Spending, and Electoral Accountability

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

[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).

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 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 nearby municipalities. Focusing on the same program, Brollo (2011) shows that municipalities 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

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

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, 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. 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 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)][4]

Closer to the topic of this paper, 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.

3 Electoral Incentives to Reduce Spending in Reaction to Increased Monitoring

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 paper, the new information may also make the issue of

[4]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.

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 literature 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 in advance, 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.

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 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.

4 Research Design

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 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

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

[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.

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).

4.2 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

[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).

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.[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 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).

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 paper correspond to mayors' reaction to the news that their administration will be monitored. Section C of the appendix show that results point in the same direction when using the extent of corruption uncovered by the audits as an

[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: 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.

explanatory variable, which suggests that mayors react to both increased monitoring and revealed corruption in a pattern that reveals incentives to preserve their reelection chances. The results using the extent of corruption as the explanatory variable also show the findings in this paper can coexist with other explanations for top-down and bottom-up sanctioning (Brollo 2011; Timmons and Garfias 2015).

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 A1 in the appendix shows the list of budget categories.

Second, I measure how concentrated public spending is across budget categories by calculat-

[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: 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 χ^2 test shows no evidende against the null hypothesis of overall balance ($\chi^2 = 14.37$, $df = 13$, $p = 0.35$).

ing the effective number of budget categories. More formally, the effective number of budget categories in a given municipality-year is

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

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 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 paper 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 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 B of the appendix shows the underlying numerical results.

5 Results

5.1 Main results

Figure 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%

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

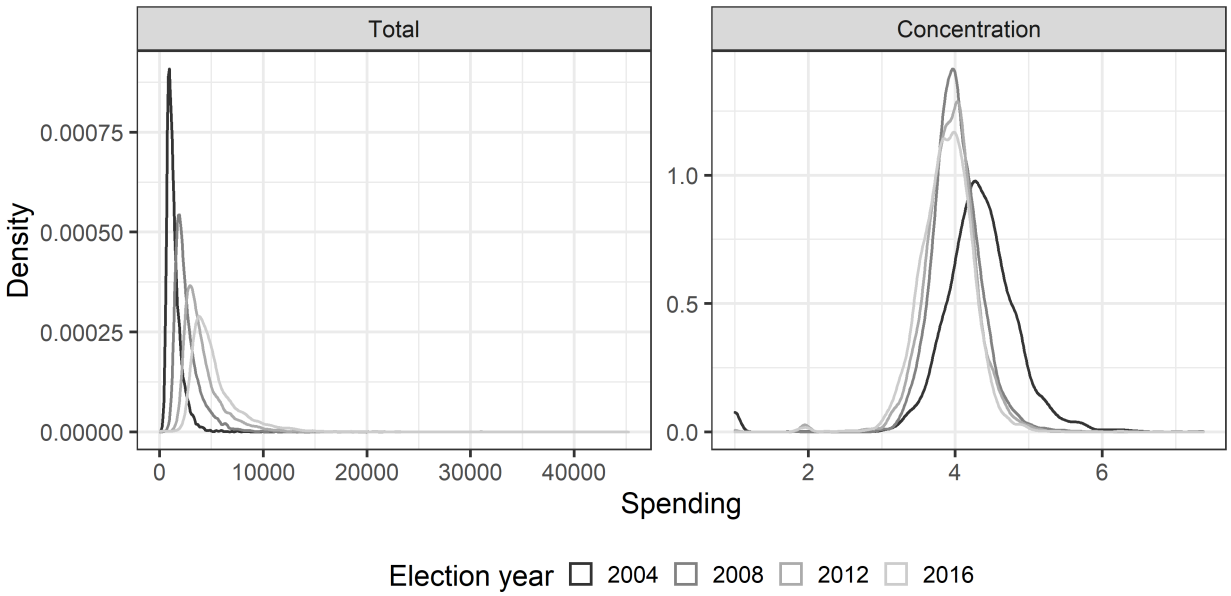

Figure 2: Distribution of total spending per capita (left) and budget concentration (right)
Note: Colors denote election years. Panels have different scales in both axes.

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.

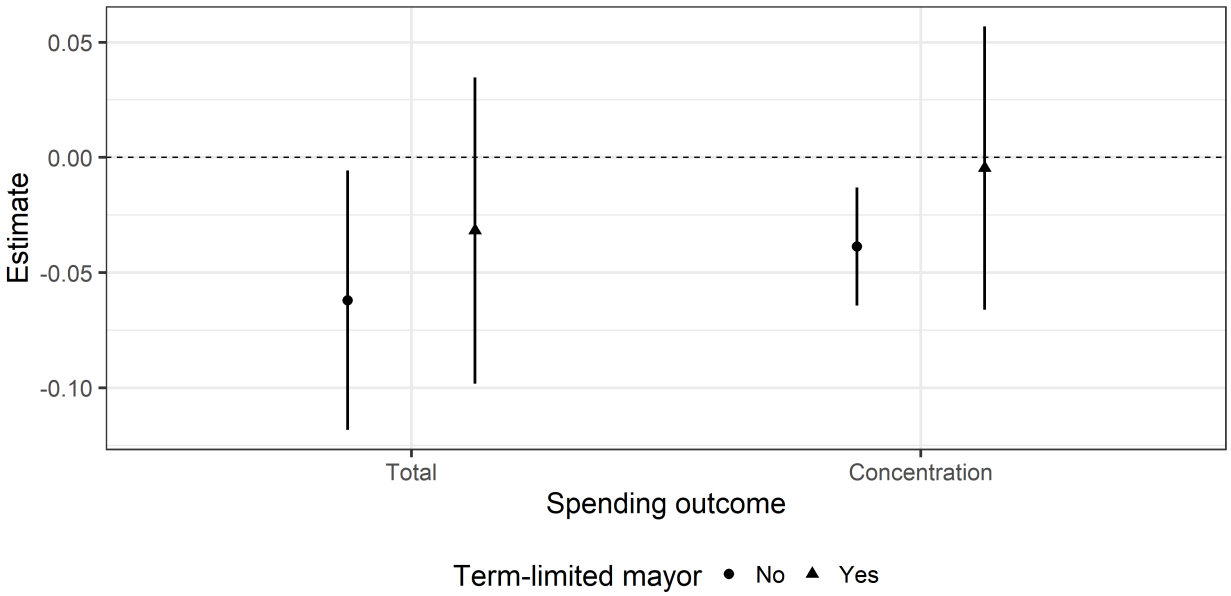

Figure 3: Effect of audit selection on election-year (logged) total spending per capita (left) and budget concentration (right) by incumbent term-limit status
Note: Outcomes are not scaled. Based on OLS regression with term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals.

5.2 Additional results

5.2.1 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 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 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 paper. 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 their constituencies to be more receptive to information about their performance in office.

Figure 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]

[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.

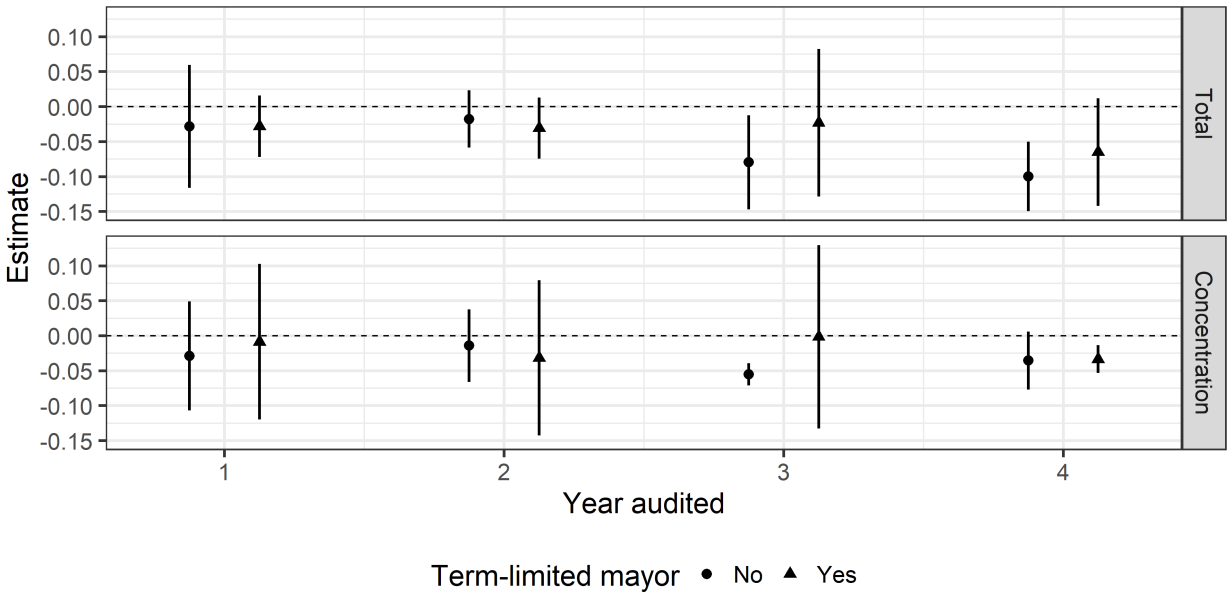

Figure 4: Effect of audit timing on election-year (logged) total spending per capita (top) and budget concentration (bottom) by incumbent term-limit status
Note: Outcomes are not scaled. Based on OLS regression with term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals.

5.2.2 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 Figures 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.[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.

5.2.3 Disaggregating budget categories

Taken together, the results in Figures 3-5 suggest that mayors reduce spending in an attempt to anticipate the electoral consequences of increased monitoring, and that this decrease comes

[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.

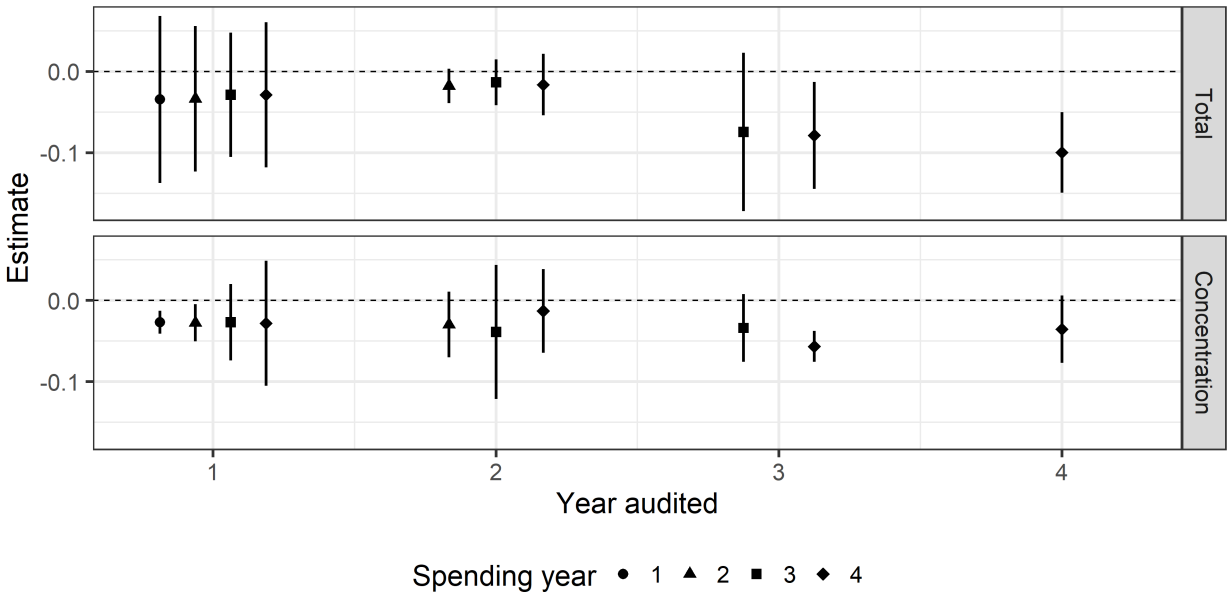

Figure 5: Effect of audit timing on (logged) total spending per capita (top) and budget concentration (bottom) across the mayoral term
Note: Outcomes are not scaled. Results restricted to municipalities with reelection-eligible mayors. Based on OLS regression with term fixed effects and clustered standard errors by term. Vertical lines denote 95 percent confidence intervals.

at the expense of some budget categories, but not others. If, as this paper 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 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

[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.

did not appear by chance.

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

6 Conclusion

This paper 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

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

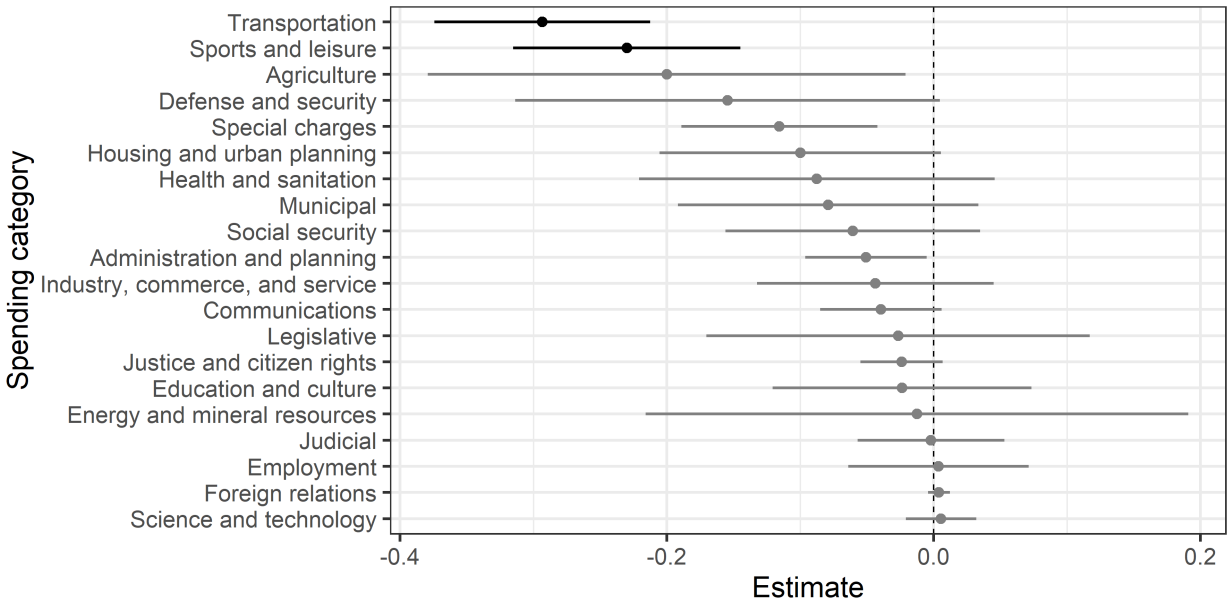

Figure 6: Effect of audit timing on election-year spending per capita across budget categories

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 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.

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 corrup-

tion 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 paper also imply that corrupt politicians have yet another strategy to get away with corruption. More generally, this paper 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 paper in combination with Section C of the appendix suggest that increased monitoring is sufficient to trigger the anticipatory behavior described in this paper, while other avenues through which increased monitoring can affect public spending depend on the level of corruption uncovered.

This paper 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 paper 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.

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