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Compliance shocks under low bureaucratic capacity

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There is a general consensus on the negative effects of corruption on state capacity and welfare, but there is little understanding of how anti-corruption initiatives affect other areas of policy making. This paper frames anti-corruption policies as a delegation problem and examines the effects of enforcement shocks on bureaucratic action. As politicians increase the oversight of the bureaucracy to ensure that their actions are more compliant, bureaucrats react to a higher risk of punishment and choose safer actions. This model shows, however, that under low policy capacity an increase in oversight may in fact distance the bureaucratic action from the political preferences if punishment is highly asymmetrical. I test this hypothesis using data from the Brazilian random audit programme and detailed records of municipal spending during 2006-2012. I show that being audited substantially increases the chances that bureaucrats will face legal sanctions for the policies they attempt to deliver, but there are no corresponding sanctions for under-delivery. As a result, I find that a random audit increases the gap between the budget planned by politicians at the beginning of the year and the resources actually spent by bureaucrats. This is a sizable effect where bureaucrats spend 1.5-2% less of the average municipal budget and 10-15% less of the discretionary expenses. This effect is disconnected from the political cycle and the reported corruption risk. The results suggest important insights for the design of anti-corruption policies: if the size and threat of punishment is large enough, not delivering policies may be a safer strategy than risking non-compliance. Hence, combining an increase in the risk of punishment with higher incentives for policy delivery could mitigate the potential side effects of audit-based anti-corruption programmes.

1 Introduction

There is a general consensus on the negative effects of corruption on the welfare and quality of public services. While reducing corruption is always desirable, it is not clear what are the best tools and policies to accomplish that. A common type of anti-corruption strategy are monitoring and auditing programmes aimed at revealing cases of mis-use of public funds and identifying corrupt bureaucrats and politicians. These programmes are designed to curb corruption by exposing agents and facilitating future sanctions. Yet, even if this strategy may be is an effective tool to reduce corruption, it may generates substantial side effects on the overall capacity of the state to deliver public services.

In this research, I frame anti-corruption monitoring schemes into a broader political and bureaucratic delegation problem. Politicians need to decide the appropriate level of delegation granted to bureaucrats. On the one hand, reducing delegation and discretionary power of the bureaucracy allows politicians to ensure that policies will be implemented closer to their preferences. On the other hand, delegating more responsibilities to bureaucrats allows politicians to make better use of the bureaucrats' policy expertise. In a context of low state capacity, however, bureaucratic outcomes are not completely under control so, even if high-level bureaucrats attempt a compliant policy, such action may be compromised along the way and the final policy may be non-compliant.

In this paper, I examine the role of enforcing a delegation space in a context of low state capacity. A distinctive feature of anti-corruption policies is the asymmetry of the delegation enforcement, in which there is greater concern with the resources spent as opposed to the money kept in the public administration. That is, bureaucrats may be sanctioned for running a corrupt tender to allow a specific company to win a public contract but there would hardly be any sanctions (in the scope of the anti-corruption programme) for not spending that money at all. This imbalance generates relevant consequences to how bureaucrats react to anti-corruption programmes and, more specifically, tends to increase the gap between the policies preferred by politicians and the actual policies delivered by bureaucrats.

I explore the empirical case of Brazilian municipalities. Using administrative data from over 4,400 municipalities during 7 years, I test the effect of random audits in the gap between the politicians' planned budget at the beginning of the year and the budget actually executed by bureaucrats at the end of the year. I find that these audits increase such gap in 1.5%-2% (or 10%-15% of the discretionary budget) for a period of three years, which largely coincides with a growth in anti-corruption sanctions unleashed by these audit reports. This effect, however, is connected to neither the political cycle nor the amount of problems found in these reports, which suggest a true bureaucratic nature.

These results point towards a relevant policy implication of this type of anti-corruption programmes: by increasing punishment against bureaucrats who fail to deliver idoneous policy, the bureaucracy may immediately react by delivering less policies. This may be particularly true in a context where there are poor built-in incentives within the public administration, so

that controlling downstream corruption is made harder and under-delivery is more feasible. While the long term effects on the welfare are not clear (and potentially ambiguous), it is likely that a better designed anti-corruption programme should have more symmetrical incentives for non-compliance or some alternative design where policy outcomes are taken into account.

This research connects to three main literatures. First, the empirical set-up is similar to other researchers analysing the effect of the random audit programme in Brazilian municipalities (Ferraz and Finan, 2008, 2011; Jeffrey F. Timmons, 2015; Mondo, 2016; Lichand et al., aper). Yet, I use the audits as a random shock on the probability of punishment by combining the audits with a new dataset on corruption sanctions between 2010-2016 from the MPF (Federal Prosecutors Office). I find complementary results to those of Lichand et al. (aper), namely that the risk of punishment affects public spending in the municipalities during following years.

Second, it connects to research on political-bureaucratic delegation (Huber and Shipan, 2006; Epstein and O'halloran, 1994; Gailmard and Patty, 2012). In this paper, I focus on the enforcement of the delegation space as opposed to exploring the optimum delegation bounds. In this respect, I propose a few expansions to the delegation model under low state capacity developed by Huber and McCarty (2004) and examine the consequences of enforcement shocks under symmetric and asymmetric punishment.

Finally, this paper also connects to a third body of literature concerned with bureaucratic discretionary and public procurement (Coppier et al., 2006; Piga, 2011; Bandiera et al., 2009; Gerardino et al., 2017). By trying to create better bureaucratic control in the past few years, compliance policies targeting government spending used a combination of increased transparency and regulation to reduce the discretion power of public officials and increase policy oversight (Piga, 2011). While these strategies may be somewhat effective to reduce corruption, there are substantial costs associated with limiting the decision space of non-corrupt bureaucrats (Coppier et al., 2006; Piga, 2011).

The rest of the article is organised as follows. First I provide an overview and background on corruption control in general and the context of the random audits in Brazilian municipalities. Later, I develop a model of delegation under low state capacity and asymmetric punishment. The following section, provides the empirical strategy for testing the model predictions. Finally, I test alternative explanations to the empirical results and identify policy implications of the main results.

2 Background and context

2.1 Corruption control

Corruption tends to be persistent and only and handful of countries have managed to move from a high-corruption dynamic to a low-corruption one (Klašnja et al., 2016; Lui, 1986). Political competition is often shown to have positive effect on deterring corruption since, even in imperfect democracies, the perspective of elections affects how politicians design policies and

manage their budgets (Alt et al., 2011; Besley and Case, 1995; Ashworth, 2012). Thus, accordingly, evidence suggest that electoral incentives can deter politicians from engaging in future corruption (Ferraz and Finan, 2008, 2011; Klašnja, 2015; Winters and Weitz-Shapiro, 2016; McCann and Domınguez, 1998).

There are also evidence showing that the level of corruption is reactive to the capacity of the bureaucracy and incentives given to public officials. Cross national research has shown that meritocratic recruitment and a clear career ladder progression in less developed countries has a strong relationship with lower corruption risk (Rauch and Evans, 2000). Similar studies have argued that lower political influence over the state's bureaucracy are associated with lower degrees of corruption (Charron et al., 2017; Dahlström et al., 2011). Furthermore, specific components of better bureaucracy have also a positive effect at reducing corruption. This is the case of public officials salaries (Di Tella and Schargrodsky, 2003), or increased oversight and accountability through audits and better monitoring procedures (Ferraz and Finan, 2011; Olken, 2007). Taken together, there seems to be sufficient evidences that some degree of better government bureaucracy can reduce overall corruption.

In this connection, audits are an ubiquitous anti-corruption tool that seem to feed the bureaucratic channels of corruption control. By auditing the formal compliance of resource allocation, monitoring agencies increase transparency of the policy process which both exposes corrupt bureaucrats and politicians, and gathers evidence to support follow up sanctions. While there are evidence that these types of audits function as a deterrent of future corruption (Avis et al., 2016), there is little research on potential untended consequences of this mechanism.

I frame this issue as a delegation problem. Politicians will generally grant some degree of discretionarity to bureaucrats which, on the one hand, allows them to make use of their expertise to deliver policies but, on the other hands, leaves room for corruption (Aidt, 2003). In order to tighten the grip on corrupt practices, politicians can increase the oversight and punishment for some types of non-compliance. In a context of low policy capacity, however, high level bureaucrats only have a limited control over the whole policy process (Huber and McCarty, 2004), and cannot ensure that lower level bureaucrats will always be compliant. A potential risk of this setting is that high level bureaucrats, while trying to reduce the level of non-compliance, may opt for avoiding risker policies as opposed to strengthening the control over lower level bureaucrats. That is, in order to address the concerns of an auditor, bureaucrats may sacrifice the choice of policies they deem to be better for the ones that can more easily pass a compliance test. This may not always be desirable, specially if the compliance targets are not particularly good.

Emerging research on this field has revealed similar patterns: seeking compliance in anti-corruption audits may have a negative effect on the quality of bureaucratic decision (Lichand et al., aper; Gerardino et al., 2017). The general structure of this argument is that an increase in the risk of punishment for non-compliance generates two counter reactions: first, it may reduce non-compliance on the same set of policies; second, it may reduces the propensity of bureaucrats delivering the policies that are at higher risk of non-compliance but are otherwise

desirable. While the first effect is the intended consequence of the formal audits, the second may not be.

Research on public procurement highlights a similar mechanic. By increasing oversight of formal requirements, public officials incentives tend to move further away from the actual outcome of the policies and closer to the compliance framework (Kelman, 1990; Lambert-Mogiliansky and Sonin, 2006; Piore, 2011; Piga, 2011). Though the net effect of this processes is not obvious, it is likely that the worst the compliance scheme is designed the less aligned it will be to the actual policy objectives. For instance, Bandiera et al. (2009) suggests that the inefficiency cost in public procurement in Italy not only offsets the cost of corruption, but also is partially explained by the efforts to avoid corruption.

2.2 Corruption control in Brazil - Random audits

Municipalities in Brazil are the main units responsible for public service delivery in education (pre-secondary), health (basic care) and urban/sub-urban transport. Nevertheless, they seldom generate sufficient tax income to support the required services and often rely on cash transfers from the federal government. Starting in 2003 the federal government started a random audit programme conducted by the General Controllers Office (*Controladoria Geral da União*, *CGU*) to verify the execution of the transferred resources in municipalities with less than 500000 inhabitants (the threshold may slightly change by round). These audits are targeted at identifying corruption, mishandle of public resources and formal inadequacies. Each of these audits are published as a report for each municipality.

The audit reports are focused on the federal cash transfers towards either infrastructure improvements (hospitals, schools, road pavement), contracting new staff (teachers, hospital staff, consultants) or purchasing equipments (school supplies, hospital equipment) and generally associated with public service provision. These reports have been coded and analysed in research over the past years (Ferraz and Finan, 2008, 2011; Jeffrey F. Timmons, 2015; Mondo, 2016; Lichand et al., aper). This scholarship has shown that exposing corrupt politicians affects their reelection chances, that the possibility of reelection affects the propensity of corruption and that curbing corruption may have a negative impact on health outcomes, among others.

CGU, nonetheless, has no legal power to sanction the municipalities. Once the reports are completed, they are sent to the Federal Prosecution Office (*Ministério Público Federal*, *MPF*) which can take legal action against the identified cases of corruption. In this context, the reports themselves are not a mechanism of enforcement but rather a tool to increase transparency and facilitate future sanctions [1].

The MPF, in turn, is independent to follow other sources for complaint, such as the ones coming from police investigations. Yet, the CGU investigations seem to be an important source of information to the MPF and, in 2014, it even launched an investigation against the former

[1] More details on this process: http://www.cgu.gov.br/sobre/perguntas-frequentes/auditoria-e-fiscalizacao/avaliacao-de-programas-de-governo

against the declining number of audits per year[2]. The MPF typically classifies the cases of corruption in public administration as Administrative Misconduct, which include procurement fraud, embezzlement, illegal invoicing and miss-use of public funds. After a case is brought up by a public prosecutor, the complaints are made public, but an actual ruling may take years to be completed. Not all cases, however, are brought to court and some are settled with mediations and result in a Conduct Adjustment Agreements (TAC) which stipulates fines and obligations to avoid future charges.

2.3 Spending cycle

Municipal budget in Brazil is known to be politically contentious and may impact electoral results just as much as it is affected by the political alignment of incumbents (Brollo and Nannicini, 2012; Sakurai and Menezes-Filho, 2008, 2011). To partially contain the inherent tendency for local politicians to overspend, there are strict regulations on the debt size of municipalities that are monitored by the federal government.

Public expenses cycle in Brazil has three main milestones. First, the budget is proposed by mayors and approved by the municipal council at the beginning of every fiscal year, which also coincides with the calendar year. As the budget is set, it is published as an Annual Budgetary Law (*Lei Orçamentária Anual* , *LOA*) for the coming year. Second, as policies are being developed, the corresponding budget starts being committed to be spent. This is typically the case of public procurement, where the budget commitment happens as tender documents are being prepared. Third, as the suppliers successfully deliver their contractual agreements and payroll is due, expenses are liquidated and paid. If, however, expenses were committed but not yet paid within the same fiscal year, which could either be a delay in payment itself of in the project execution (if suppliers do not deliver products, expenses will remain committed but not paid), the unpaid commitments move on to the next year's budget as a liability.

Municipalities are required to report their annual financial records at the end of each year, thought until 2013 the reported expenses were only the committed budget as opposed to the actual payments. At the same time, if municipalities wish to receive federal grants, beyond the mandatory transfers, they are required to submit bi-monthly spending reports including their original budget and any updates. The federal government main leverage to ensure fiscal responsibility on the municipalities is threatening to cut down the annual transfers.

3 Data

3.1 Audit data

The complete audit reports are available at the CGU website. For the audit rounds 20 to 40, between 2006-2015, CGU has tabulated information on the audit date, value of the monitored

[2]https://veja.abril.com.br/politica/ministerio-publico-federal-investiga-queda-nas-fiscalizacoes-de-municipios/

programmes and types of problems identified (formal mistakes, offenses and severe offenses). Table 1 presents of summary of these records. There were 1265 municipalities audited in that period and the average municipality has 52 identified offenses, 9.5 serious offenses and 4.5 formal mistakes. The average amount of audited transfers was about R$170 million (US$ 55 million), in 2016 values. I also calculate the relative incidence of offenses per audit, Flaw rate, as the number of non-formal problems identified per audit divided by the log of the audited value. Since the higher is the audit scope, the more likely it is that offenses will be found, I build this measurement to compare audit outcomes across municipalities.

Based on CGU internal audit manual, formal mistakes are defined as cases of legal non-compliance that have no substantive effects the programme delivery. Serious offenses, in turn, are substantial problems that affect policy delivery and are associated with actions that compromise the state's finance, such as overpaying for services, not keeping track invoices or embezzlement, for instance. Simple offenses, in turn, are issues that only generate harm to the public budget indirectly, by compromising the operational capacity of the programmes.

3.2 Financial records

A second sources of data are the financial records reported by the municipalities to the Brazilian Treasury department. This database, FINBRA, is updated every year and is the main source of information on municipal level financial records in Brazil. Municipalities are required to report their annual expenses at the end of each year but additional reporting is a condition for applying to additional federal funding.

During the period 2006-2012, only the committed expenses are reported at the end of the year. I use data on the unpaid commitments, which are also reported, to calculate the actual payments made in each year. This information is matched with the planned budget at the beginning of the year extracted from bi-monthly reports submitted by 4440 unique municipalities during that period, and an average of 3922 per year[3]. Out of the 5600 municipalities, I examine a sample of 3200-3800 municipalities per year, depending on the availability of data.

Table 2) provides a summary of the municipal finance. The average municipality has around 26 thousand inhabitants, an annual budget of R$41 million (US$ 17 million) and about 90% of that planned budget is effectively executed. The committed spending is mainly focused on health, education and administrative expenses making up to %69 of the budget. Payroll, pensions and financial obligations add up to %44 of the budget, where mayors and bureaucrats have little control over those costs. Investments, on the other hand, are more subject to the discretion of the local government and add up to 17 % of the annual committed budget.

[3]Prof. Ricardo Silva Carvalho was extremely helpful by describing the public finance regime in Brazil and sharing the organized data on the municipal planned budget

3.3 MPF investigation

The third source of data in this research are all the legal actions taken against public officials in Brazil by the National Prosecutors office (MPF). This database is publicly available and contains information on the type of crime and legal action taken against public officials. It contains information on the municipality where the events occurred. It is a complete dataset of all investigations from 2010 until 2016 but it contains unsystematic information on anti-corruption sanctions prior to 2010.

Table 3 summarises the amount of cases per year. From 2010 onward there are more than 2000 cases per year, where extra judicial cases are the most common. Each year, more than a 1000 municipalities are investigated and many of them have multiple cases in the same year.

3.4 Electoral data

I combine the previous dataset with electoral records from the municipalities provided by the Brazilian electoral court (*Tribunal Superior Eleitoral*, *TSE*) [4]. In Brazil, starting in 2004, mayors are allowed to run once for re-election. During the period of analysis, there is only one election, in 2008. Mayors that are already on their second term during the period 2004-2008 are not allowed to run for a second time. However, mayors who are in their first term can run for reelection in the 2008 election but will not be allowed to be reelected in 2012.

In the period 2005-2008, about 23% of the mayors in municipalities with less than 500000 inhabitants were reelected and would not be allowed to run for a third term. During the period 2009-2012, about 29% of the mayors in the same municipalities were on their second term.

4 A model of delegation under low state capacity

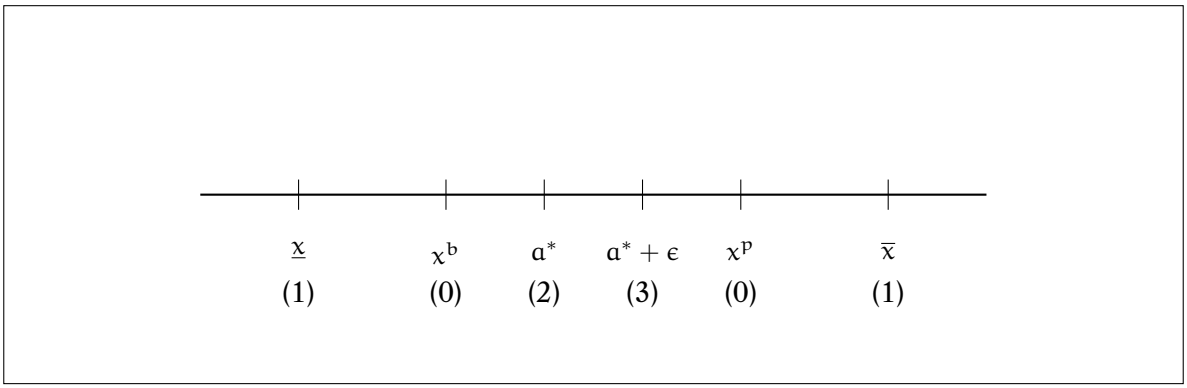
Models of political delegation are often a single dimensional representation of political and bureaucratic preferences (Huber and Shipan, 2006; Epstein and O'halloran, 1994; Gailmard and Patty, 2012). These models follow a sequence of events in which: 1. A politician with policy preference x_p chooses a delegation space for bureaucratic action $[\underline{x}, \overline{x}]$ 2. A bureaucrat with policy preference x_b takes action a, given their own preference and the delegation space 3. The policy is realised at $a + \epsilon$, where ϵ is an uncertainty term. Bureaucrats seek to deliver policies as close to their preferences as possible, while politicians use the delegation space to limit the scope of action of bureaucrats.

Politicians delegate so that they can befit from the policy expertise of bureaucrats. In the model, ϵ is a random term that is unknown to the politicians but known by bureaucrats. As bureaucrats have less uncertainty over policymaking, there is some degree of delegation that

[4] http://www.tse.jus.br/eleitor-e-eleicoes/estatisticas/repositorio-de-dados-eleitorais-1/repositorio-de-dados-eleitorais

allows politicians to benefit from that knowledge while ensuring that the final policy is somewhat aligned with their preferences. If x^p close enough to x^b, politicians may benefit more from delegating and grant more discretionary power to bureaucrats. This is generally known as the "ally principle"(Gailmard and Patty, 2012). Alternatively, if the preference gap is large enough, the risk of having a final policy too far off from the politician's preference may more then offset the benefits of reducing the policy uncertainty. In this case, politicians are likely to assign a narrower delegation space.

Figure 1: Delegation problem

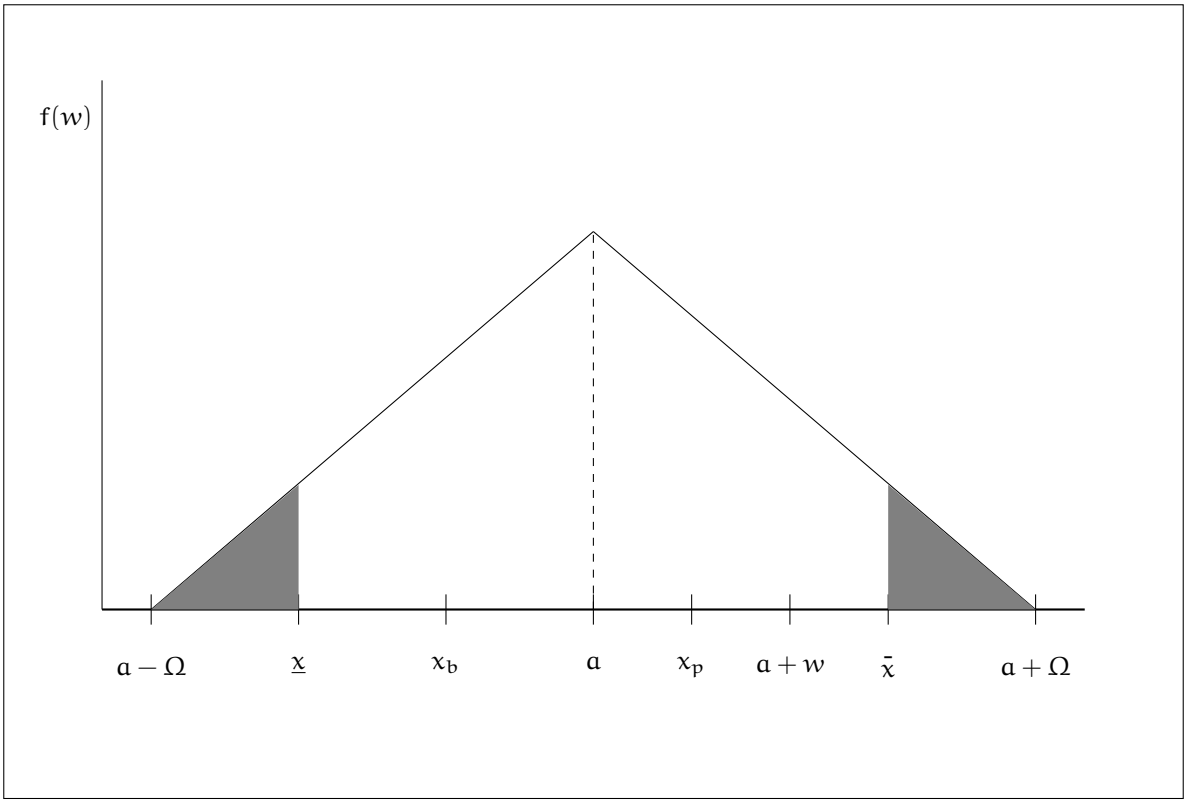


Huber and McCarty (2004) add another layer of uncertainty to this model and explore the delegation problem under low policy capacity. While bureaucrats may face less uncertainty than politicians with respect to some aspects of policymaking, they do not have full control of the whole policy delivery process. This is modeled as an additional uncertainty term that neither the politician nor the bureaucrat know beforehand. In this case, bureaucrats set a which is then affected by a random variable w with distribution $f(\omega)$. Bureaucrats and politicians are aware of the distribution of w, but it's value is only revealed after a is chosen.

In Figure 2, $f(w)$ is the probability density function defined in the $[-\Omega, \Omega]$ interval. Bureaucrats choose a policy a, but the actual implemented policy is $a + \omega$. The interval between $[\underline{x}, \overline{x}]$ defines the compliance space. The areas shaded in grey, in turn, are the cases of non-compliance which may occur with probability $P(a + w < \underline{x}|a)$ and $P(a + w > \overline{x}|a)$. Different from the first problem of delegation, bureaucrats only have partial control over the risk of non-compliance by choosing a, but not ω.

While most research on delegation is focused on analysing the properties of the $[\underline{x}, \overline{x}]$ space, in this paper I will analyse the effect of enforcement of a given space on the policy choices of bureaucrats. The general intuition is that more enforcement of a delegation space should lead to values $a + \omega$ that are more likely to be compliant. However, I show that if punishment is not symmetrical, that is, non-compliance in one of the shaded areas in figure 2 is a worst offense than the other, more enforcement may in fact distance a from x^p. In order to explore

Figure 2: Delegation problem under low policy capacity



further this intuition, I will build a version of a delegation model and examine the impacts of enforcement shocks on the bureaucratic choices.

4.1 Model set up

I start with the basic model set up described above and incorporate the design proposed by Huber and McCarty (2004)[5]. The realisation of ω is described by a symmetric and finite probability density function $f(\omega)$: .

$$f(\omega) = \frac{\Omega - |\omega|}{\Omega^2}, \quad \omega \in [-\Omega, \Omega] \tag{1}$$

Where Ω is a parameter of policy capacity and $\sigma^2_\omega = \Omega^2/6$. A larger Ω is associated with a more spread out distribution of w. The politician sets the delegation space $[\underline{x}, \overline{x}]$ and can enforce that space by punishing bureaucrats based on the actual policy outcome. That is, bureaucrats

[5] For simplicity, I exclude the uncertainty politicians face when making policy decisions. This simplification does not affect the main results since I am not analysing the politicians' choice but, instead, only the bureaucrats problem given a set of bounds.

face punishment if $a + \omega$ is not in the delegation space, regardless if the intended action a is compliant. That is:

$$\mathbb{E}(C|a,\omega) = \begin{cases} \gamma\xi, & \text{if } a+\omega \notin [\underline{x},\overline{x}] \\ 0, & \text{if } a+\omega \in [\underline{x},\overline{x}] \end{cases} \tag{2}$$

Where γ is the probability of being punishment, ξ is the size of punishment and $\mathbb{E}(C)$ is the conditional expectation of punishment. Bureaucrats have a quadratic utility function which consist of how close the policy outcome is from their preferred policy and the expectation of punishment they face. Assuming that bureaucrats do not have extreme preference so that $x^b \in [\underline{x},\overline{x}]$, the expected utility can be described by the following integrals:

$$\begin{aligned} \mathbb{E}U_b = &-\int_{a-\Omega}^{\underline{x}} [(a-\omega-x^b)^2 + \gamma\xi] f(\omega) d\omega \\ &-\int_{\underline{x}}^{\overline{x}} [(a-\omega-x^b)^2] f(\omega) d\omega \\ &-\int_{\overline{x}}^{a+\Omega} [(a-\omega-x^b)^2 + \gamma\xi] f(\omega) d\omega \end{aligned} \tag{3}$$

The three integrals above correspond to the three areas in figure 2. The first term is the probability of non-compliance with respect to $\underline{x}$ and its associate level of utility, the second term is the case of full compliance and the third term is the case of non-compliance with respect to $\overline{x}$.

Assessing $\mathbb{E}U_b$ and analysing the bureaucrat's choice of a^* that maximizes the expected utility functions, we are left with the optimal choice of actions the bureaucrat has for a given level of delegation and threat of punishment [6] :

$$a^* = \frac{2\Omega^2 x^b + \gamma\xi(\underline{x}+\overline{x})}{2(\Omega^2+\gamma\xi)} \tag{4}$$

For a sufficiently low risk of punishment, the optimum action approaches the preferred policy location x_b. And, as the risk and size of punishment grows, the delegation space plays a larger role in determining the actual policy decision. A marginal increase in γ has a straight forward impact on a^*:

$$\frac{da^*}{d\gamma} = \frac{\xi\Omega^2}{2(\Omega^2+\gamma\xi)^2}(\underline{x}+\overline{x}-2x^b) \tag{5}$$

So, for $x^b < (\underline{x}+\overline{x})/2$, $\frac{da^*}{d\gamma} > 0$. And, for $x^b > (\underline{x}+\overline{x})/2$, $\frac{da^*}{d\gamma} < 0$. That is, a marginal increase in γ pushes a^* to the centre of the delegation space and the magnitude of this effect is given by the level of policy capacity Ω, the current risk of punishment γ and the size of punishment ξ.

[6] The full derivative is $\frac{d\mathbb{E}_b}{da} = \frac{2\Omega(x^b - 2a(\Omega^2+\gamma\xi)+\gamma\xi(\underline{x}+\overline{x})}{\Omega^2}$

4.2 Asymmetric punishment

The size of punishment, however, does not have to be the same for all types of non-compliance. In many settings, politicians may be more concerned with some types of infraction than others. In the case of budget allocation, for instance, politicians may be more concerned with over-spending than they are with underspending. In that case, the model allows two different values of ξ for the two types of non-compliance. So, instead of equation 3, it follows:

$$\mathbb{E}(C|a,\omega) = \begin{cases} \gamma\xi_1, & \text{if } a+\omega < \overline{x} \\ \gamma\xi_2, & \text{if } a+\omega > \underline{x} \\ 0, & \text{otherwise} \end{cases} \tag{6}$$

Assessing the conditional expectation of the utility function of bureaucrats and finding for optimum action, a^*, the resulting expression is:

$$a^* = \frac{2x_b\Omega^2 + \gamma[\xi_1(\underline{x}+\Omega) + \xi_2(\overline{x}-\Omega)]}{2\Omega^2 + \gamma(\xi_1+\xi_2)} \tag{7}$$

Similar to equation 4, as γ converges to 0, the bureaucratic action is closer to the preferred location x_b. By the same token, if the magnitude of punishment is also low enough, bureaucrats are less concerned with compliance and more concerned with the own preferences. The marginal variation on γ, nonetheless, has a different effect on the the value of a^* with respect to the distance to the centre of the delegation space. Assuming that $\xi_1 = 0$ and $\xi_2 > 0$, so that punishment is completely one-sided, increasing γ may in fact generate more extreme policy outcomes than otherwise.

$$\frac{da^*}{d\gamma} = -\frac{2\Omega^2\xi_2}{(2\Omega^2+\gamma\xi_2)}(x^b + \Omega - \overline{x}) \tag{8}$$

In this case, for every $x^b + \Omega > \overline{x}$, $\frac{da^*}{d\gamma} < 0$. So, as long the bureaucrats preferred policy is still at risk of non-compliance and at faces punishment, every increase in the probability of punishment will push the action a to the left of the policy space. This makes intuitive sense: an increase the risk of punishment given that the size of punishment is largely asymmetrical, may drive the choices of policy towards the opposite end of the policy spectrum.

Figures 3 and 4 illustrate this central argument: if punishment is asymmetrical, bureaucrats respond to an increase in the probability of being punished by further diverging from the center of the decision space. If, however, punishment is symmetrical, bureaucrats respond to the same increase by moving closer to the center of the decision space.

Figure 3: Effect of probability of punishment under asymmetric punishment

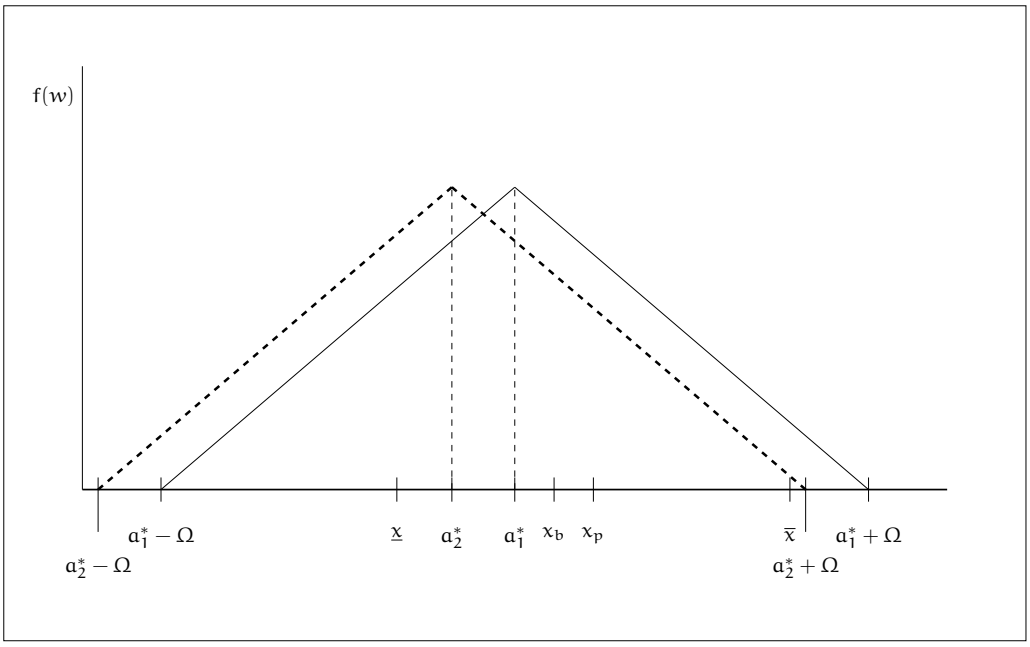
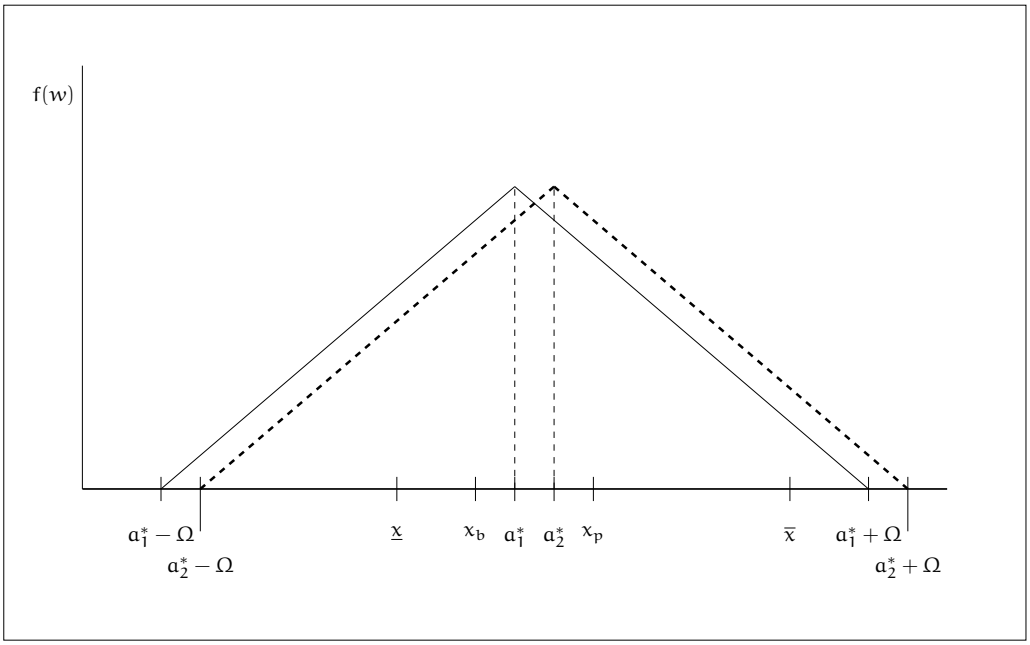


Figure 4: Effect of probability of punishment under symmetric punishment



4.3 A general case

Politicians are interested in controlling $g(.) = \mathbb{E}(|a^* - \omega - x^p|) = |a^* - x^p|$ and attempt to use an increase in the probability of punishment to reduce such gap. I start assuming that a politician's preference is at the center of the delegation space $[x^p - i, x^p + i]$ and analyse the effect of a

marginal increase of γ on $|a^* - x^p|$. In the case where $\xi_1 = \xi_2 = \xi$:

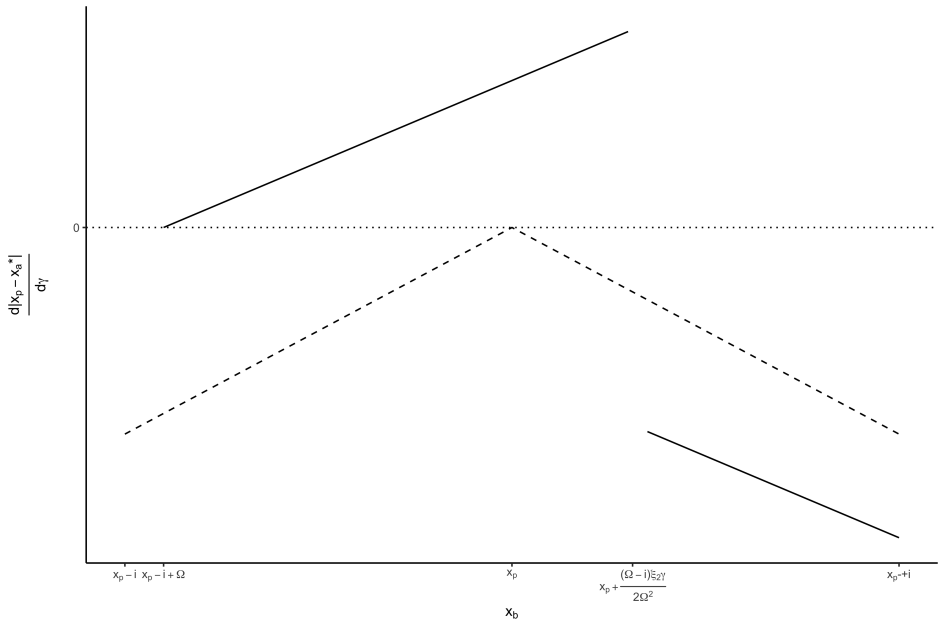
$$\frac{dg(.)}{d\gamma} = -\frac{\xi\Omega^2|x^b - x^p|}{(\Omega^2 + \gamma\epsilon)^2} \tag{9}$$

For all values of x_b, $\frac{dg(.)}{d\gamma}$ is negative, which reflects the general intuition that increasing the threat of punishment for any positive value of ξ should approximate the bureaucratic action to the politician's preference. Nevertheless, if punishment is asymmetric, a marginal increase in γ has diverging effects on the value of $g(.)$.

$$\frac{dg(.)}{d\gamma} = \begin{cases} \dfrac{2\Omega^2\xi_2(x^p - x^b + i - \Omega)}{(2\Omega^2 + \gamma\xi_2)^2}, & \text{if } 2\Omega^2(x^b - x^p) + (i - \Omega)\gamma\xi_2 > 0 \\ -\dfrac{2\Omega^2\xi_2(x^p - x^b + i - \Omega)}{(2\Omega^2 + \gamma\xi_2)^2}, & \text{otherwise} \end{cases} \tag{10}$$

This translates to two types of behaviour. Assuming that $x^b + \Omega > x^p + i$, so that the preferred bureaucratic choices risks of non-compliance, equation 10 is positive for values of $x^b < x^p + (\Omega - i)\gamma\xi_2/2\Omega^2$. This corresponds to an increasing gap between the politician's prefered policy and bureaucratic action for every marginal increase in the probability of punishment. On the other hand, for $x^b + \Omega > x^p + i$ and $x^b > x^p + (\Omega - i)\gamma\xi_2/2\Omega^2$, a compliance shock will marginally decrease the gap between preferences and action. Figure 5 illustrates this argument comparing the functions of the two cases discussed above, where the full line is the asymmetric punishment and the doted line the symmetric case.

Figure 5: Marginal impact of probability of punishment on $|x^p - a^*|$, in both symmetric and asymmetric punishment cases



Nevertheless, as ξ increases, the space in which a compliance shock reduces the gap $|x^p - a^*|$ is tighter. So, for a large enough ξ, the space $[x^p + (\Omega - i)\gamma\xi_2/2\Omega^2, x^p + i]$ diminishes as $(\Omega - i)\gamma\xi_2/2\Omega^2 \to i$. That is, the space in which a compliance shock could have a negative effect on $g()$ may not exist if the magnitude of punishment is too high and, in that case, all extra monitoring effort will overshoot the political action by at least mirroring the gap to in the other side of the delegation space.

The punishment threshold is defined by:

$$\xi^* = \frac{2i\Omega^2}{\gamma(\Omega - i)} \tag{11}$$

Where ξ^* is the size of punishment where all values in equation 10 are always positive. Hence, as long as $\gamma < 1$ and $\Omega > i$, there is a positive value of punishment that removes the effectiveness of compliance shocks to move bureaucratic action closer to political preferences.

Hence, there are two reasons why an enforcement shock may further distance bureaucratic action from the political preference. First, an increase in the probability of punishment only pushes bureaucratic action to the opposite side of the risk which may be further from the politician's preference. Second, for a large enough punishment, the increased threat of an audit may overshoot the bureaucratic action and, instead of approaching it to the centre of the delegation spaces, it pushes it towards the other end.

4.4 Aggregated effects

While the circumstances described above may be restrictive, I suggest that it embodies an important policy problem of low capacity bureaucracies. Typical audit and oversight programmes often pose largely different punishment for bureaucrats who overspend as opposed to bureaucrats who underspend during policy deliver. This bias is particularly strong when monitoring public acquisitions and public procurement. The bureaucracy will often face much stricter consequences for purchasing goods for a higher price as opposed to purchasing lower quality goods for a lower price or, in many cases, not purchasing that at all.

I illustrate this argument with a hypothetical example. Politicians set aside a budget for buying school supplies, computers and textbooks, and will punish bureaucrats if each individual budget exceeds a limit and the budget cannot move from one to another. Bureaucrats will likely know the correct amount of each supplies to buy but, facing an increase in probability of punishment, will buy less of each supply to avoid exceeding the imposed limits. Individually, it is possible that $a_1^* + w \geq x^b$ or $a_2^* + w \geq x^b$ for each of the policies 1 and 2, but, taken together, $\mathbb{E}(a^* + w|\gamma) < \mathbb{E}(x^b)$ since ω has zero-mean.

The aggregated effects follow a similar pattern described in the previous section. Defining $X^b = \sum_{i=1}^{n} x_n^b$, $X^p = \sum_{i=1}^{n} x_n^p$ and $A^* = \sum_{i=1}^{n} a_n^*$, the effects of a shock on the probability of punishment on the overall gap between the politicians preference and the bureaucrat's action is $d|X^p - A^*|/d\gamma$. Similarly, for $X^b < X^p + (\Omega - i)\xi_2\gamma/2\Omega^2$, an increase in γ will increase the policy gap.

This problem translates into typical anti-corruption policies. In the extreme case where overspending is always a consequence of corruption but high-level bureaucrats have little control over it while being affected by the punishment, opting for no-action may be a response to enforcement shocks. Hence, as politicians assign an overall budget for policies and asymmetrically punish bureaucrats when facing costs overruns in individual projects, it is possible that the aggregated action that bureaucrats will further diverged from the politician's preference as the threat of punishment increases.

5 Research design

My first goal is to measure the effects of the random audits in the municipal spending. I use records of municipal budget proposed by mayors and approved by the municipal chamber as a measure of X^p and the actual spending records executed by bureaucrats as a measurement of A^*. During the observed period, municipalities can be subject to a random shock on the probability of punishment, γ. The main hypothesis to be tested is whether being audited affects the difference $X^p - A^*$

The basic empirical set up is a difference in difference regression with treatments happening at multiple years and where subjects can be included in the treatment group multiple times:

$$y_{it} = \alpha_i + \lambda_t + \beta_1 AB_{it} + \gamma X_{it} + X^p_{it} + \epsilon_{it} \tag{12}$$

Where i is the municipality and t is the year. AB is 1 if the municipality was audited in t, and 0 otherwise, α_i is the municipal fixed effect and λ_t is the year fixed effect. X_{it} are time varying control variables include to increase precision. The variables of interest in this model is β_1 corresponding to the Difference-in-Difference estimator of the effect of an audit on the outcome variables. The depend variable is $A^* + \omega$.

This model assumes that:

$$E(y_{0it}|\alpha_i, \lambda_t, X_{it}, AB_{it}, x^p_{it}) = E(y_{0it}|\alpha_i, \lambda_t, X_{it}, x^p_{it}) \tag{13}$$

That is, prior to an audit the conditional expectation of the outcome variables is the same for the municipalities that will be audited and the municipalities that will not be audited. This is consistent with the nature of the random audits and also with the observed variable distribution. I also explore this model excluding the individual fixed effects and test if there are any substantial difference in the baseline between treated and control groups.

I run a similar model to estimate the duration of the treatment effect. Where AT_{it} is a set of dummies for every year after the audit, starting at 1 and running until the end of the observed period:

$$y_{it} = \alpha_i + \lambda_t + \beta_1 AB_{it} + \gamma X_{it} + A^p_{it} + \epsilon_{it} \tag{14}$$

5.1 Effect on the probability of punishment

Combining data from the audits and sanctions, I examine to what degree do the audits affect the overall probability of sanctions. The complete dataset on sanctions related to corruption and administrative offenses, nevertheless, only extents from 2010-2016. Prior to that, there are specific incidences of sanctions but the sample may be bias by reporting unit. Given that there are only two years of overlap between the two datasets, I choose to analyse the sanctions independently and incorporate the audits from 2010-2016. This analysis is meant to verify if there is a substantial variation in the probability of punishment resulting following up the audits.

$$y_{it} = \alpha_i + \lambda_t + \beta_1 AB_{it} + \gamma X_{it} + \epsilon_{it} \tag{15}$$

In the equation above, y_{it} stands for both the number of sanctions imposed against agents in a municipality or a binary variable on whether sanctions are imposed at all. I run both an OLS and a Logistic model, when appropriate.

6 Results

I find substantial evidence that the audits affect public spending. In the years following an audit, municipalities end up spending 1.5-2% less of their originally planned budget, as shown in Table 4 and 5. That represents a reduction in 3-4% of the budget not committed to payroll and debt repayment and 10-15% of the capital expenses of the municipalities, a rough measurement of the actual discretionary budget in every fiscal year. The effect is stronger in the 2-3 years following the audit and fades after that. Part of such fading effect may be attributed by the reduction in sample size, and corresponding increase in standard deviation, since only the municipalities audited in 2007 are monitored for the 6 years after such audit, as seen in Table 6.

The audit shocks also substantially increase the probability and amount of future sanctions. After an audit, municipalities are 10 percentage points more likely to be sanctioned and experience 0.58 more sanctions. This effect is similar using different model specifications and seems to hold for most years of the audit programme, as seen in Table 8 and Table 9

Visually, figures 6 and 7 corroborate the statistical results. After controlling for a fixed year and individual effects, there is a clear drop in the paid expenses. The same effect is not observed in the planned budget. This effect seems to last for 2-3 years and recover after that. A similar pattern can also be observed in and Figure 8 and 9 showing the effects of an audit on the amount of MPF investigations and sanctions against officials in the municipalities.

These empirical results agree with other studies that find a sizable increase in future sanctions following up the audits (Avis et al., 2016). Nevertheless, such affect seem to reduce the actual spending of the municipalities and further distance the actual policies from the ones proposed by politicians. As an aggregated effect, this is not desirable. While spending less money

on an individual project might be a good response to lowering the level of corruption, for instance, reducing the overall budget execution in a municipality cannot be solely explained as a price effect.

Nevertheless, I test two further hypothesis to verify if the model presented in this paper is a likely explanation to the identified behaviour. I claim that given an asymmetric punishment, bureaucrats are more likely to have more extreme reactions to the threat of punishment and further distance themselves from the political preferences. If, however, this reaction is in fact a result of a lower level of corruption, the expenses gap should be proportioned to the level of corruption found in the reports. Alternatively, if the expenses gap are a direct reaction to the electoral incentives so that politicians running for re-election influence the bureaucracy to stop spending not to compromise their chances at a reelection, the gap should be larger for mayors running for reelection.

7 Alternative hypothesis and robustness checks

I test two alternative hypothesis to the bureaucratic effect suggested in this research. First, I examine if the identified effects are connected to the political cycle. The argument would be that, instead of bureaucrats further diverging their from the politician's preferred policies, the identified underspending is actually a result of a political strategy aiming at elections. By reducing spending, without reducing the proposed budget, mayors would be able to reduce the risk of corruption and reduce the risk of not being reelected.

The following model can capture the effects of an audit on mayors that were reelected or that can still run for reelection:

$$y_{it} = \alpha_i + \lambda_t + \beta_1 AB_{it} + \beta_2 AB_{it} \times EC_{it} + \gamma_1 EC_{it} + X_{it}\gamma + \epsilon_{it} \tag{16}$$

Where term EC_{ic} is 1 if a mayor is reelected (and, consequently, cannot run for office again) and 0 if the mayor can still run for office. If the budgetary effects of the audits are political, it is likely that the possibility of reelection would either intensify or curb the such effects. This model is the equivalent of a difference-in-difference-in-difference model, so:

$$\begin{aligned} E(y_{it}|EC_{it} = 1, AB_{it} = 1, X_{it}, \lambda_t, \alpha_i) &= \beta_1 + \beta_2 + \gamma_1 \\ E(y_{it}|EC_{it} = 0, AB_{it} = 1, X_{it}, \lambda_t, \alpha_i) &= \beta_1 \\ E(y_{it}|EC_{it} = 1, AB_{it} = 0, X_{it}, \lambda_t, \alpha_i) &= \gamma_1 \end{aligned} \tag{17}$$

The results of this model are reported in table 12. I find some evidence that the actual spending in municipalities with lame ducks, or mayor that cannot run for reelection, tends to be higher than in municipalities where the mayor can run for re-election. Nevertheless, the interaction term is not statistically significant in any of the model specifications.

Second, I investigate if the gap is a consequence of the actual degree of corruption found in the reports. The underlying hypothesis is that the verified difference only captures a price variation resulting from less corruption. That is, by auditing the municipalities, bureaucrats would engage in less corruption and buy goods and services for a lower price. Politicians, however, would take longer to update their budget assumptions and there would be a gap lasting a few years. While this hypothesis is weaker to the extend that less corruption would not affect the total budget, but only the unit cost, I still verify if the content of the reports are connected to the findings.

In order to test this I include the CGU coding of severe flaws in the reports with respect to the total amount of budget audited as a proxy for corruption. This information is only observed after an audit. Nevertheless, if it is assumed that without the audits the level of corruption in a municipality would remain unaltered in the period analysed, the fixed municipal effect would capture such component for the places that were not audited:

$$E(y_{0it}|CO_i, \alpha_i, \lambda_t, X_{it}) = E(y_{0it+1}|CO_i, \alpha_i, \lambda_{t+1}, X_{it+1}) \tag{18}$$

Where Co_i is the level of corruption only observed with an audit. However, after an audit takes place, the following might be true:

$$E(y_{1it} - y_{0it}|CO_{1i}, \alpha_i, \lambda_t, X_{it}, AB_{it}) = \tau + \beta_2 AB_{it} \times CO_{1it} \tag{19}$$

The results of this model are reported on table 11. I find no substantial evidence that the audit effects are connected to the audit flaws in the reports. While the flaws do have a cross-section effect on the actual spending, they do not have a statistically significant in interaction with the audits. On top of that, it seems that the level of flaws are associated with a lower actual spending, after controlling for the planned budget, which suggests that the expenses gap in fact captures some type of government capacity as opposed to only a price effect of corruption.

Finally, I review the main models and test several placebo models by changing the actual year of an audit to verify if the results found in the previous section hold. I use leads and lags on the audit date to simulate the effect on other years. In table 10 I report a set of models with artificial lags and leads. I find that this model set up does not identify audit effects where there is no audits.

8 Discussion and policy implications

These findings provide important insights into the design of anti-corruption programmes. First, under a very asymmetrical punishment regime bureaucrats may slow down policy delivery in reaction to an increase in the risk of getting caught. If bureaucrats cannot control the full process of delivering a policy to ensure that there is no corruption it might be a better strategy not to deliver that policy at all if that does not generate any substantial repercussions. From a welfare perspective, it might be that not indulging corruption is preferable in the long run than delivering

more policies in the short run. Nonetheless, if incentives were leveled differently, there might be a more efficient balance between the two objectives. Furthermore, a second insight from this result is that municipalities that are more effective at hitting the politician's target are more affected by the enforcement shocks. Hence, it is likely that such effect targets hits harder the more compliant bureaucracies.

While testing for alternative hypothesis on the channel of the identified audit effect, I don't find them to be statistically relevant. It is unlikely that the effect is directly driven political incentives or by a price effect of corruption. Yet, it is still possible that some complex interaction between corruption and political incentives may drive the size of this effect but, based on the model suggested, the effect should be connected to the degree of state capacity.

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Table 1: Summary of audit data 2006-2015

Statistic	N	Mean	St. Dev.	Min	Max
# formal mistakes	1,265	4.5	5.5	0	42
# offenses	1,265	52.0	29.6	2	249
# serious offenses	1,265	9.5	11.8	0	92
audited amounts (MM BRL, 2016 values)	1,265	170.6	468.2	0.3	9,837.2
Flaw rate	1,265	15.1	7.0	1.7	102.1

Summary data from CGU audits from 2006-2015. The issues identified are categorized as formal mistakes *(falhas formais)*, offenses *(falhas médias)* and serious offenses *(falhas graves)*. Audited amounts correspond to the value of the programmes monitored in each municipality, inflation adjusted to 2016 values

Table 2: Summary of municipal financial records

Statistic	N	Mean	St. Dev.	Min	Max
Population (2000 census)	22,053	26,237.71	50,469.17	789	498,095
Planned budget (MM BRL, 2016 values)	22,053	41.65	99.83	0.03	3,005.43
Committed budget (MM BRL, 2016 values)	22,053	40.20	93.31	1.28	1,952.04
Actual spending (MM BRL, 2016 values)	22,053	36.19	86.54	0.02	3,005.41
Paid/planned	22,053	0.90	0.10	0.04	1.80
share of health spending (committed)	22,053	0.27	0.10	0.00	8.58
share of education spedning (committed)	22,053	0.27	0.10	0.00	8.58
share of administrative spedning (committed)	22,053	0.15	0.08	0.00	4.55
share of investments (committed)	22,053	0.17	0.15	0.00	5.60
share of financial obligations (committed)	22,053	0.02	0.02	0.00	0.57
share of payroll and pensions (committed)	22,053	0.42	0.13	0.00	10.92
share government transfers	22,053	0.91	0.13	0.00	1.42

Values are calculated based on the FINBRA database for 2006-2012 and inflation adjusted to 2016 values. Only municipalities with less than 500.000 people are included, which is the threshold of the audit programme for most of the years. The effective number of observations may be slightly different from this table and future regressions since, in most model specifications, these not all variables are used as controls.

Table 3: Summary of MPF corruption cases

	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
Extra judicial cases	2	2	10	37	1,731	2,234	2,732	4,894	5,484	8,017	7,854
Judicial cases	46	58	216	2,057	1,693	1,803	1,954	3,340	2,336	1,908	1,763
# unique municipalities	40	50	153	728	998	1,274	1,553	2,148	2,225	2,755	3,437

Summary of the MPF investigations and sanctions from 2006-2016. Cases can either be judicial, such as criminal investigations, or be dealt in arbitrage courts, extra judicial. The database is meant to have complete information from 2010-2016, though there are a few reported cases prior to that. This contains cases from all local federal courts in Brazil and identified by the name of the municipality were the investigated cases happened

Table 4: Model summary: Audit effect

	(1)	(2)
	Actual spending	Actual spending
TREATMENT	-0.0190***	-0.0183*
	(0.00536)	(0.00801)
Planned budget	0.341***	0.0818***
	(0.0253)	(0.0150)
Observations	26730	26730
R^2	0.968	0.796
Time FE	Yes	Yes
Individual FE	No	Yes
Controls	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Both models have a year fixed effect. Controls are federal government transfers, total revenue per capital and share of budget assigned to investments. All monetary values are in 2016 prices and are log-transformed in the regressions. TREATMENT is 0 for years prior to an audit and 1 for all years after an audit. Reported R^2 of fixed effect model is the within R^2 . Reported standard errors are robust and by municipality.

Table 5: Model summary: 2 year lasting treatment effect

	(1)	(2)
	Actual spending	Actual spending
2y TREATMENT	-0.0198***	-0.0154**
	(0.00598)	(0.00592)
Planned budget	0.341***	0.0816***
	(0.0253)	(0.0150)
Observations	26730	26730
R^2	0.968	0.796
Time FE	Yes	Yes
Individual FE	No	Yes
Controls	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Both models have a year fixed effect. Controls are federal government transfers, total revenue per capital and share of budget assigned to investments. All monetary values are in 2016 prices and are log-transformed in the regressions. 2y TREATMENT is 0 for years prior to an audit and 1 for the following 2 years after an audit. Reported R^2 of fixed effect model is the within R^2.Reported standard errors are robust and by municipality.

Table 6: Model summary: Audit effect per year

	(1) Actual spending	(2) Actual spending
YEARS AFTER AUDIT=1	-0.0149	-0.0103
	(0.00773)	(0.00657)
YEARS AFTER AUDIT=2	-0.0252**	-0.0215*
	(0.00972)	(0.00959)
YEARS AFTER AUDIT=3	-0.0266*	-0.0255
	(0.0124)	(0.0132)
YEARS AFTER AUDIT=4	-0.0191	-0.0154
	(0.0116)	(0.0110)
YEARS AFTER AUDIT=5	-0.00732	-0.00740
	(0.0102)	(0.0111)
YEARS AFTER AUDIT=6	0.00186	0.00318
	(0.0125)	(0.0128)
Planned budget	0.341***	0.0817***
	(0.0253)	(0.0151)
Observations	26730	26730
R^2	0.968	0.796
Time FE	Yes	Yes
Individual FE	No	Yes
Controls	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Both models have a year fixed effect. Controls are federal government transfers, total revenue per capital and share of budget assigned to investments. All monetary values are in 2016 prices and are log-transformed in the regressions. Reported R^2 of fixed effect model is the within R^2. Reported standard errors are robust and by municipality.

Table 7: Model summary: Audit effect per year

	(1) Actual spending	(2) Actual spending
TREATMENT		-0.0407***
		(0.00842)
2y TREATMENT	-0.0410***	
	(0.0105)	
TREATMENT x CAPACITY		0.000404***
		(0.0000993)
2yTREATMENT x CAPACITY	0.000389**	
	(0.000132)	
CAPACITY	0.000429***	0.000420***
	(0.0000240)	(0.0000240)
Planned budget	0.335***	0.335***
	(0.0247)	(0.0248)
Observations	26713	26713
R^2	0.969	0.969
Time FE	Yes	Yes
Individual FE	No	No
Controls	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Both models have a year fixed effect. Controls are federal government transfers, total revenue per capital and share of budget assigned to investments. All monetary values are in 2016 prices and are log-transformed in the regressions. 2y TREATMENT is 0 for years prior to an audit and 1 for the following 2 years after an audit. CAPACITY is calculated by: $\Omega = \sigma\sqrt{6}$. Reported R^2 of fixed effect model is the within R^2. Reported standard errors are robust and by municipality.

Table 8: Model summary: Audit effect per year

	(1)	(2)	(3)	(4)
	# of Sanctions	Is sanctioned	Is sanctioned	Is sanctioned
TREATMENT	0.583***	0.109***	0.566***	0.470***
	(0.0794)	(0.0125)	(0.114)	(0.0281)
Observations	47186	47186	39611	47186
R^2	0.079	0.208		
Time FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	No
Model	OLS	OLS	Logistic	Logistic

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Model summary: Audit effect per year

	(1)	(2)	(3)	(4)
	# of Sanctions	Is sanctioned	Is sanctioned	Is sanctioned
YEARS AFTER AUDIT=1	0.456***	0.0714***	0.345**	0.539***
	(0.107)	(0.0152)	(0.125)	(0.0801)
YEARS AFTER AUDIT=2	0.852***	0.177***	0.891***	0.939***
	(0.0990)	(0.0178)	(0.132)	(0.0762)
YEARS AFTER AUDIT=3	0.458***	0.116***	0.416**	0.636***
	(0.0941)	(0.0193)	(0.139)	(0.0797)
YEARS AFTER AUDIT=4	0.602***	0.0785***	0.127	0.441***
	(0.134)	(0.0208)	(0.146)	(0.0836)
YEARS AFTER AUDIT=5	0.414***	0.0736***	0.0531	0.399***
	(0.107)	(0.0217)	(0.154)	(0.0872)
YEARS AFTER AUDIT=6	0.428***	0.0550*	-0.0744	0.319***
	(0.124)	(0.0234)	(0.165)	(0.0937)
Observations	47186	47186	39611	47186
R^2	0.079	0.209		
Time FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	No
Model	OLS	OLS	Logistic	Logistic

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10

	(1) Actual spending	(2) Actual spending	(3) Actual spending	(4) Actual spending
T+1 2y TREATMENT	-0.00151	0.00362		
	(0.00417)	(0.00346)		
Planned budget	0.341***	0.0813***	0.341***	0.0814***
	(0.0253)	(0.0150)	(0.0253)	(0.0150)
T-1 2y TREATMENT			0.00154	-0.000616
			(0.00398)	(0.00324)
Observations	26730	26730	26729	26729
R^2	0.968	0.796	0.968	0.795
Time FE	Yes	Yes	Yes	Yes
Individual FE	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All models have a fixed year effect. Controls are federal government transfers, total revenue per capital and share of budget assigned to investments. All monetary values are in 2016 prices and are log-transformed in the regressions. 2y TREATMENT are artificial treatments with either a lag (T-1) or a lead (T+1) around the years of the actual audit.

Table 11: Model summary: Interaction effect of audit and procurement flaws

	(1) Actual spending	(2) Actual spending	(3) Actual spending	(4) Actual spending	(5) Actual spending
2y TREATMENT	-0.0197**	-0.0180**	-0.0175**	-0.0146**	-0.0153*
	(0.00601)	(0.00602)	(0.00665)	(0.00560)	(0.00726)
AUDIT FLAWS		-0.0304***	-0.0131**		
		(0.00460)	(0.00483)		
2y TREATMENT x PROCUREMENT FLAWS			0.000599		0.00168
			(0.00837)		(0.00833)
Observations	26730	26730	26730	26730	26730
R^2	0.968	0.968	0.981	0.859	0.859
Time FE	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All models have a fixed year effect. Controls are federal government transfers, total revenue per capital and share of budget assigned to investments. All monetary values are in 2016 prices and are log-transformed in the regressions. 2y TREATMENT is 0 for years prior to an audit and 1 for the following 2 years after an audit. AUDIT FLAWS are the ratio of coded flaws by amount of resources audited.

Table 12: Model summary: Interaction effect of audit and reelection

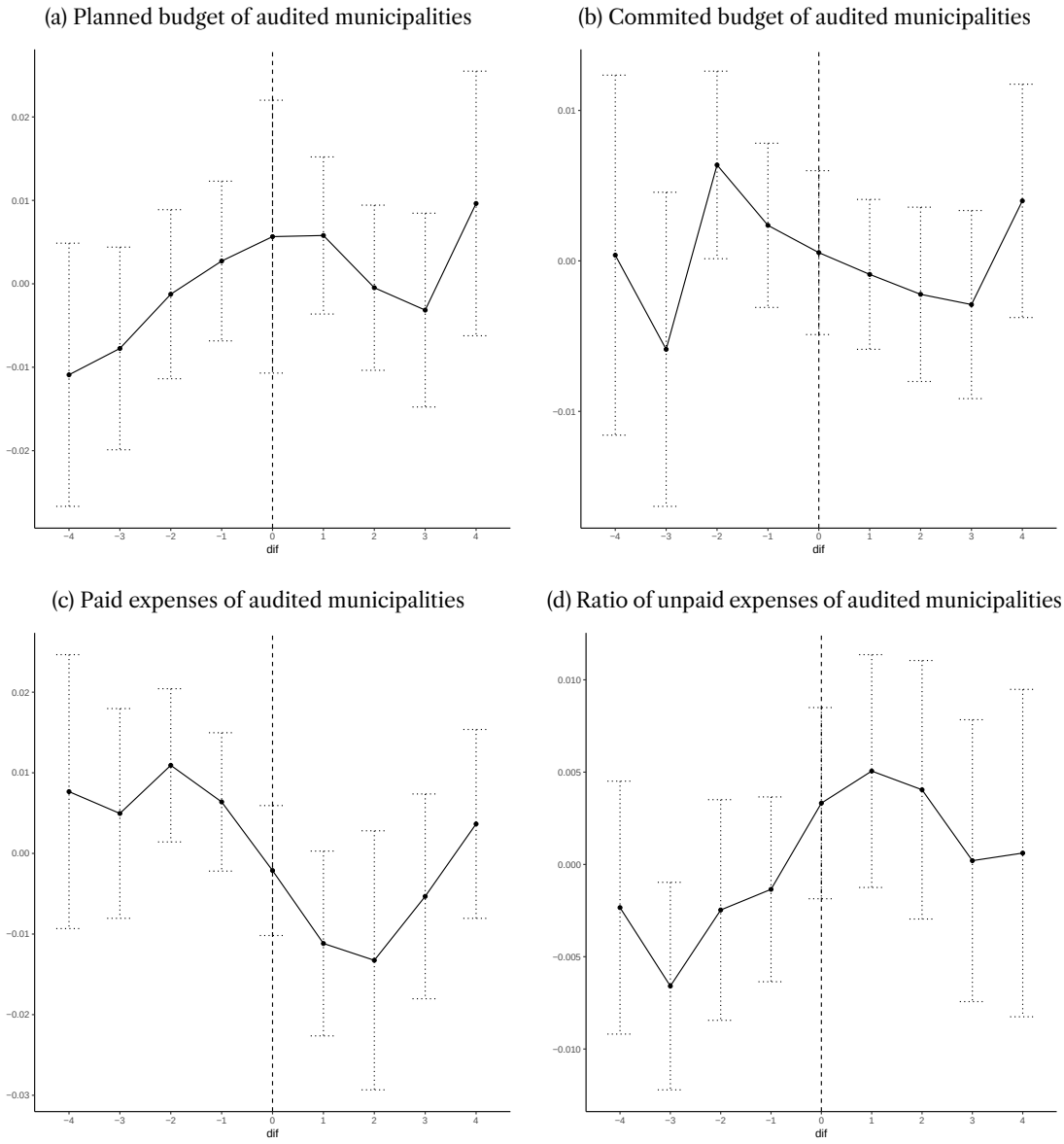
	(1) Actual spending	(2) Actual spending	(3) Actual spending	(4) Actual spending	(5) Actual spending	(6) Actual spending
2y TREATMENT	-0.0197**	-0.0201***	-0.0198**	-0.0154**	-0.0154**	-0.0150*
	(0.00601)	(0.00604)	(0.00743)	(0.00592)	(0.00594)	(0.00716)
REELECTED		0.0123***	0.0123***		0.00540	0.00553*
		(0.00238)	(0.00244)		(0.00290)	(0.00230)
2yTREATMENT x REELECTED			-0.00102			0.000547
			(0.0101)			(0.00867)
Observations	26730	26532	26532	26730	26532	26532
R^2	0.968	0.968	0.968	0.796	0.796	0.859
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	No	No	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

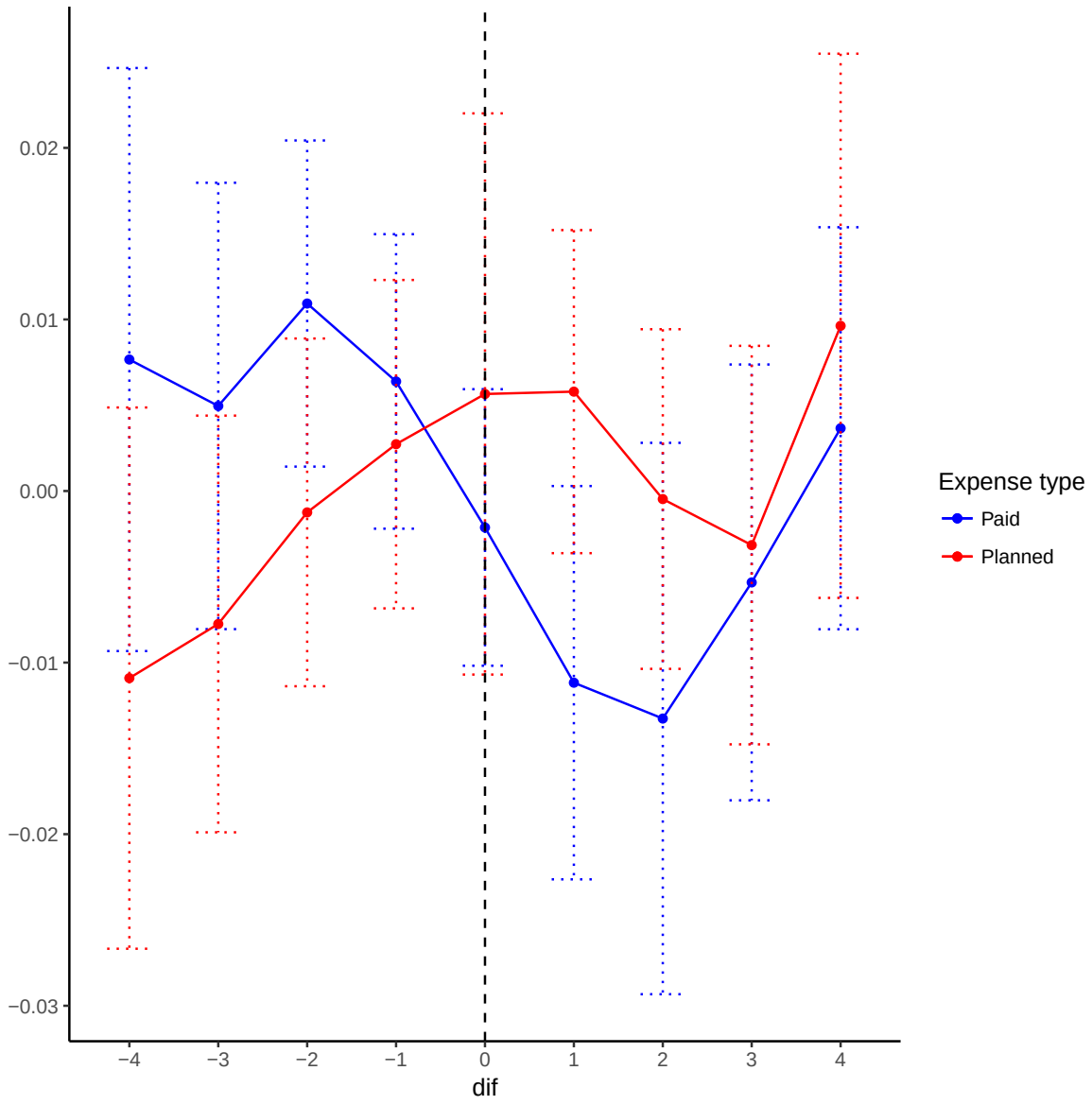
ll models have a fixed year effect. Controls are federal government transfers, total revenue per capital and share of budget assigned to investments. All monetary values are in 2016 prices and are log-transformed in the regressions. 2y TREATMENT is 0 for years prior to an audit and 1 for the following 2 years after an audit. REELECTED is 0 if the current mayor is on a second term and 0 if not. Mayors who are on a second term cannot be reelected again.

Figure 6: Effect of an audit on the municipal spending controlled for time and individual fixed effects



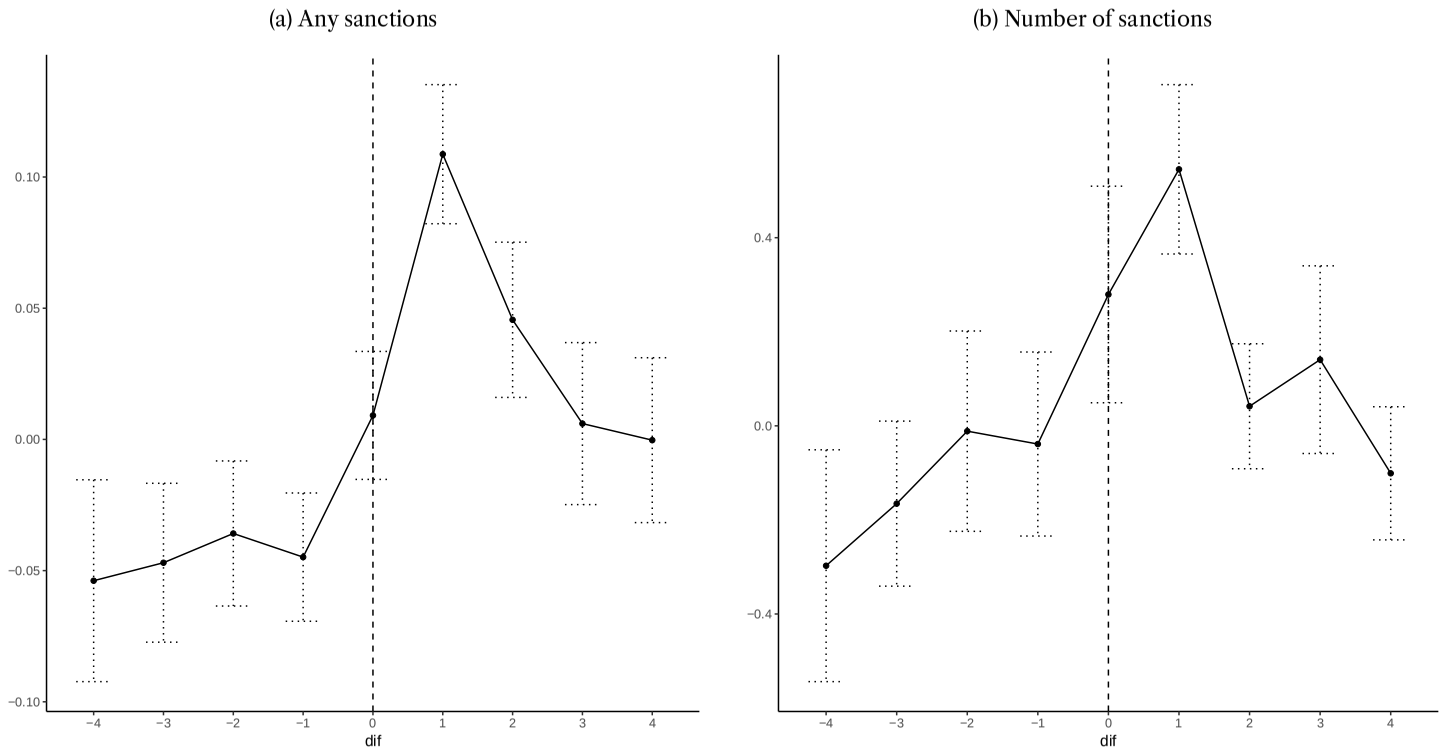
he plot shows the normalized expenses of the municipalities after controlling for individual and time fixed effects. The horizontal axis shows the years prior and after an audit, and the vertical axis the corresponding variation in budget type

Figure 7: Normalized graph of expenses by time of audit with confidence intervals of 95%



The plot shows the normalized paid and planned budget of the municipalities after controlling for individual and time fixed effects. The horizontal axis shows the years prior and after an audit, and the vertical axis the corresponding variation in the paid and planned budgets.

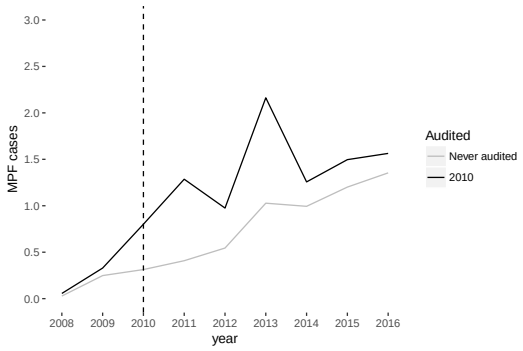
Figure 8: Effect of an audit on future MPF sanctions - scaled to the year of the audit and controlled for individual and year fixed effects



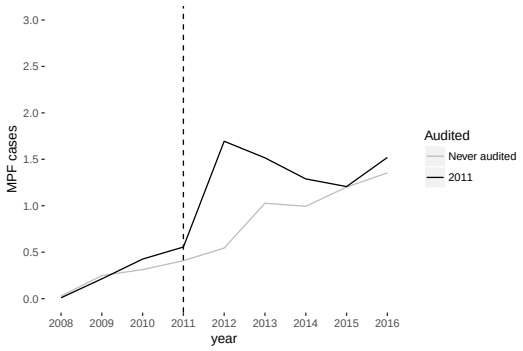
The images above plot the sanctions and investigations from the MPF after controlling for an individual and year fixed effects. In the horizontal axis, the plot shows the number of years prior or after an election. In the vertical axis it is either the variation on probability an MPF case in a municipality that year (a), or the absolute number of cases per year(b).

Figure 9: Effect of an audit on the total amount of administrative charges filed in a municipality

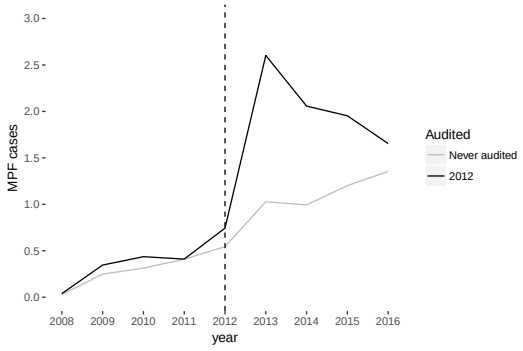
(a) Total MPF cases by year - 2010 vs. Never audited



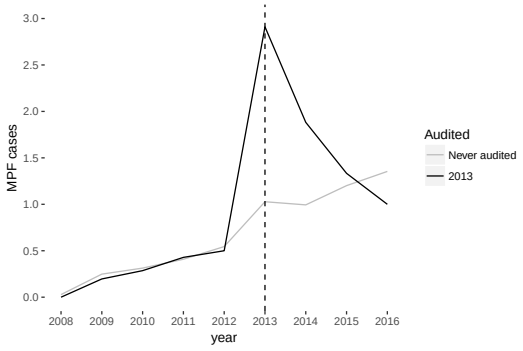
(b) Total MPF cases by year - 2011 vs. Never audited



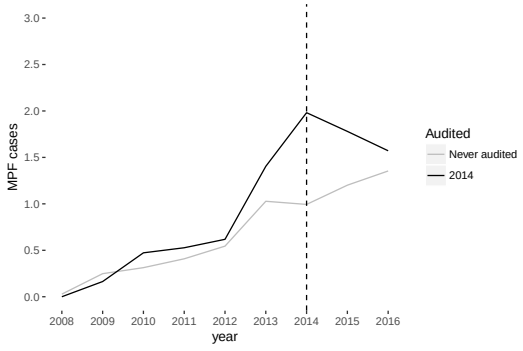
(c) Total MPF cases by year - 2012 vs. Never audited



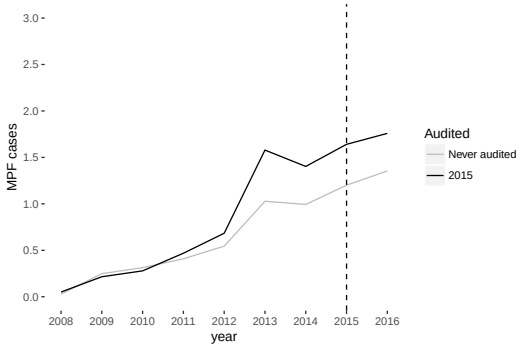
(d) Total MPF cases by year - 2013 vs. Never audited



(e) Total MPF cases by year - 2014 vs. Never audited



(f) Total MPF cases by year - 2015 vs. Never audited



The images above plot the number of sanctions and investigations from the MPF by year, comparing the municipalities that were never audited but could have been audited, against those that were audited in the specific years. There is a increase in the number of MPF cases following up an audit. There is some expected variation on years of effect since the audits and MPF cases are coded on a yearly basis but do not follow a fixed yearly calendar.