Audit threats and year-end spending by government agencies: experimental evidence from Chile

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Abstract

Purpose – The paper aims to estimate the capacity of supreme audit institutions' (SAIs) audits to deter potentially wasteful year-end procurement spending. It also studies heterogeneous responses to SAIs' audits depending on whether agencies' top managers are appointed through a competitive procedure or not.

Design/methodology/approach – A letter signed by the head of Chile's SAI was sent to a hundred randomly chosen agencies two weeks before the end of the fiscal year, with instructions on year-end spending accounting and an audit threat. In addition, a hundred agencies that did not receive the letter were used as a control group.

Findings – Agencies that received the letter reduced year-end aggregate procurement spending by 33% relative to controls. Purchases of office supplies, safety equipment, personal care products and paper products experienced the most considerable reductions. The decrease in year-end spending was smaller for agencies with at least one top manager appointed through a competitive procedure.

Research limitations/implications – A SAI's audit threat significantly reduced year-end procurement spending. Larger reductions in agencies headed by political appointees and across categories of goods that

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Statements and declarations: Data availability: the data sets analyzed in the current study are not publicly available because they were shared to the authors by public agencies that own them. Data are, however, available from the authors upon reasonable request and with permission of the public agencies that own the data.

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have been flagged as likely to be purchased to exhaust the budget suggest the avoided expenditures would have been wasteful. Further research is needed to determine if the net social value of year-end procurement spending deterred by SAIs audits is negative as suggested.

Social implications – This paper has implications for the institutional support of SAIs audits and civil service.

Originality/value – This paper provides novel experimental evidence of SAIs' audits' deterrence power on public agencies' year-end procurement spending.

Keywords Efficiency, Governance, Public finance, Regulatory compliance, Randomized control trials, Social value procurement

Paper type Research paper

1. Introduction

A fundamental pillar of the sustainable development goals (SDGs) is to improve the governance of the public sector. Supreme audits institutions (SAIs) are "the linchpin of a country's integrity system" (Dye and Stapenhurst, 1998), at the forefront of promoting good governance. Improving the capacity of these institutions is a critical challenge to achieving the SDGs by 2030.

External audits of public agencies by SAIs can promote good governance and the efficient use of resources in at least three ways. First, audits can enhance internal control by identifying deficiencies in general practices. Second, they can detect the misuse of resources allowing for their recovery. Third, the mere possibility of audits can deter bureaucrats from engaging in misconduct, corruption and inefficient spending. The results of audits can be used directly to determine the quantitative relevance of the first two channels. However, quantifying the importance of the third channel requires evidence on how public agencies behave under different perceptions of the audit probability.

We provide this evidence through an experiment in which audit threats were randomly assigned to government agencies by the audit authority. This method has proven a powerful tool to identify the behavioral effects of variations in the perceived probability of being audited by individuals and organizations (Olken, 2007; Kleven *et al.*, 2011; Zamboni and Litschig, 2018). We use this experiment to quantify, to the best of our knowledge for the first time, how year-end spending by government agencies responds to audit threats.

Expiring budgets and the belief that unspent money will be discounted from future budgets create incentives within public agencies to exhaust the budget, even if expenditures are nonessential or wasteful. This incentive is believed to be exceptionally strong at the end of the fiscal year, leading to a year-end spike in expenditures (Douglas and Franklin, 2006; IMF, 2009; Liebman and Mahoney, 2017). We leverage this well-known phenomenon by running an experiment in which public agencies were randomly selected to receive a letter from the head of Chile's SAI two weeks before the end of the 2012 fiscal year, that is, right before the expiration of the budget. The letter contained general instructions on how to implement year-end spending accounting rules as well as an audit threat.

We find that government agencies respond to the audit threat. Treated agencies reduced their procurement spending in the final two weeks of the fiscal year by 33% compared with controls. Office supplies, safety equipment, personal care products and paper products were the four categories with the largest decrease, all of which arguably qualify as nonessential expenditures that is likely to be wasteful (Liebman and Mahoney, 2017) [1]. Furthermore, year-end procurement spending decreased less for agencies where at least one high level manager was appointed by Chile's civil service system than for those where all high level managers are political appointees: 21% vs 42%. This suggests that agencies with executives that are insulated from political pressure are less prone to engage in wasteful spending.

The remainder of the paper is organized as follows. Section 2 lays down our conceptual framework, placing our paper in the theoretical and empirical literature studying oversight over bureaucracies and year-end spending. We present a simple model that formalizes our hypotheses in Section 3. Section 4 describes Chile's SAI mandate, the field experiment and the data. Section 5 presents the empirical results. Section 6 concludes.

2. Relation with the literature

2.1 Bureaucracies, audits and year-end spending

Our paper relates to an extensive literature that studies the behavior of bureaucracies, drawing from economics, political science and public administration. This literature has emphasized that bureaucrats have objectives that may differ from those of society. For example, in his seminal contribution, William Niskanen hypothesized that bureaucrats seek to maximize their influence by pushing for larger budgets (Niskanen, 1968, 1971). The conflict between bureaucrats' and societies' goals can be modeled as a principal (the legislature) that delegates a bundle of tasks to an agent (a bureaucrat) (Gailmard and Patty, 2012), and where bureaucrats have better information on the production function of the goods and services provided by their agencies.

In this framework, audits are a tool that can shape agencies' behavior. Bendor *et al.* (1985) follow Niskanen's tradition and assume that "bureaus are tempted to misrepresent costs and benefits to obtain larger budgets," which contrasts with the legislature's goal of providing a budget that funds expenditures up to the point where marginal benefit equals marginal cost. However, they depart from Niskanen in noting that Congress can use audits to close the informational gap. The mere possibility of audits corrects bureaus' behavior, reducing the gap between actual and reported costs as they anticipate they will be forced to pay back the misappropriated funds (and some additional costs which may be monetary or nonmonetary) if audited.

Expiring budgets are likely to exacerbate the tension between agencies' goals and the public interest. Agencies usually reach the end of the fiscal year with a large share of their budget unexpended, either because of precautionary savings (Liebman and Mahoney, 2017) or procrastination (Frakes and Wasserman, 2020). This leads to year-end spikes in expenditures that may be partially wasteful (Liebman and Mahoney, 2017).

The empirical literature on the impact of SAIs on public agencies includes many correlational studies that, although informative, do not establish the causal effect of audits or the channels through which SAIs influence agencies' behavior [2]. To the best of our knowledge, only one study has used a letter experiment to estimate the deterrence power of an SAI. Zamboni and Litschig (2018) worked along Brazil's SAI in 2009 to randomly select a group of municipalities that faced a higher audit probability. Selected municipalities received a letter that informed them that they were part of a group of 120 municipalities, out of which 30 would be audited. Receiving this letter increased the audit probability from the baseline (about 2%) to 25%. The authors find a large reduction in irregularities in procurement in dimensions that are likely correlated with corruption (e.g. using a noncompetitive procedure when an auction is mandated).

More broadly, letter experiments have been widely used to study external control's impact on individuals' and firms' behavior. Olken (2007) randomly selected Indonesian villages for preannounced audits of road construction projects, dramatically increasing the perceived probability of an audit (from 4% to 100%). The study finds an 8% reduction in missing funds in treated villages compared with control villages. Similarly, the field experiment in Kleven *et al.* (2011) randomly sent tax enforcement letters to individual taxpayers in Denmark, finding a significant effect for self-reported income. In Latin America, Pomeranz (2015) randomly selected Chilean firms to receive a letter containing audit threats, finding an effect on VAT revenues that fades out over 15 months. Also,

Lagunes (2017) sent letters to randomly assigned districts in Perú informing them that a civil society organization was monitoring specific public works. Delays in completing the works were similar for treated and control districts, yet the cost of public works in treated districts was substantially lower.

Two additional studies have also exploited Brazil's SAI's randomly assigned audits to estimate the impact of SAI's audits on municipalities' behavior. Ferraz and Finan (2008) compare municipalities audited before and after a mayoral election. The study shows that the disclosure of audit results had a significant impact on electoral behavior: for each corruption event revealed to voters, incumbent mayors whose municipalities were audited before the election were 20% less likely to be reelected than those mayors whose municipalities were audited after the election. More recently, Avis *et al.* (2018) analyzed the same audit random assignment program in Brazil and found that 8% fewer irregularities were detected in municipalities that were previously audited compared with municipalities that were audited for the first time. The authors conjecture that the result is driven by the deterrence effect of nonelectoral consequences following audits, although electoral concerns may also play a role.

Our paper is the first paper providing experimental evidence on the deterrence power of an SAI outside Brazil. In contrast with the papers for Brazil, in our case agency heads are not elected officials so that the electoral consequences of audit results' disclosures are unlikely to play a role. And we consider audits on year-end spending, a time with strong incentives for wasteful spending.

2.2 Career civil service and audits

Despite the considerable attention paid to audits, they are not the only tool at the disposal of the legislature to better align bureaucrats' interests with their own. The legislature has the authority to design bureaucracies to facilitate the selection of executives whose preferences match those of the legislature (Gailmard and Patty, 2007). How bureaucrats are selected has drawn significant attention from scholars and practitioners, in particular, the relative value of political and career civil service appointments; i.e. patronage versus merit (Simon *et al.*, 1991). According to one view, political appointees best serve government administrations because of their higher responsiveness to elected officials (Heclo, 1975; Maranto, 1998; Moe, 1990). Others believe, however, that prioritizing professionalism and competency enhances bureaucratic performance (Cohen, 1998; Lewis, 2007).

Empirical results support the view that the introduction of professional civil service enhances the performance of the public sector. Gilmour and Lewis (2006), Gallo and Lewis (2012) and Lee and Whitford (2013) provide evidence that public programs and agencies perform better when career civil servants administer them. Lewis (2007) finds that programs also receive better performance reviews. Moreover, Rauch (1995) shows that the introduction of a civil service in US cities during the first two decades of the 20th century had a positive effect on the share of municipal expenditure allocated to investment in infrastructure, which led to higher growth. Similarly, using cross-country data, Cingolani *et al.* (2015) find a positive correlation between bureaucratic autonomy and socioeconomic outcomes, while Evans and Rauch (1999) and Rauch and Evans (2000) find that meritocratic recruitment is strongly associated with economic growth and private investment. Our paper contributes to this literature by studying how the recruitment process of bureaucrats influences the effectiveness of audits.

3. A simple model of audits, bureaucrats preferences and year-end spending

We present a simple model to formalize the two hypotheses we test in our experiment. First, an exogenous increase in the perceived probability of being audited decreases year-end

spending and leads to less wasteful spending. Second, the above effect is larger closer to zero for agencies headed by career civil servants (as opposed to political appointees).

We build on the classical contributions of Becker and Stigler (1974) and Bendor *et al.* (1985) and assume bureaucrats' behavior is affected by their perceived probability of being audited and punished if caught at fault. As in Bowling *et al.* (2004), we consider bureaucrats that differ in their objectives, going from those whose goals are perfectly aligned with the legislature (social welfare maximizers) to those whose only goal is to maximize their agency's budget.

The objective function of a type-α bureaucrat is a weighted average of the social value of expenditures and the private benefits she receives from administrating a larger agency, with $\alpha > 0$ being the weight on the latter. The bureaucrat type is exogenous, possible values of $\alpha = 0$ (altruism) to $\alpha = 1$ (budget maximization).

Social welfare from year-end spending made by a given public agency is given by:

$$W(g) = \log(g) - \lambda g,$$

where g denotes expenditure and $\lambda > 0$ the shadow cost of public funds. At the end of the fiscal year, an agency's remaining budget is given by B. The available budget exceeds the socially optimal level of expenditures $(1/\lambda)$ because monies were accumulated throughout the year. We do not take a stand regarding why this is the case, and the results do not depend on whether the accumulation of this surplus is due to precautionary savings (Liebman and Mahoney, 2017) or procrastination (Frakes and Wasserman, 2020).

The SAI audits year-end spending with a given probability and audits uncover all wasteful spending defined as those that exceed the social optimum [3]. If wasteful expenditures are discovered, the bureaucrat is forced to pay back the wasted monies [4]. We assume that the probability of an audit is common knowledge and equal to p, and that the bureaucrat considers the expected loss from audits when considering the private benefits she receives from expenditures. Given this setting, the bureaucrat will not choose a level of expenditure below the social optimum and hence solves:

$$\max_{\frac{1}{\lambda} \leq g \leq B} \alpha\left(g - p\left[g - \frac{1}{\lambda}\right]\right) + (1-\alpha)W(g).$$

From the first-order condition we have that:

$$g^* = \frac{1}{\lambda - \frac{\alpha}{1-\alpha}(1-p)},$$

where we assume $\lambda > \frac{\alpha}{1-\alpha}(1-p)$ and $g^* \leq B$ to have an interior solution. The following propositions follow:

P1. Perceived probability of audits and wasteful spending

The bureaucrat's optimal spending, g^*, is increasing in α and decreasing in p. Also, denoting the elasticity of g with respect to p by $\varepsilon_{g,p}$, we have:

$$\varepsilon_{g,p} = -\frac{\alpha p}{1-\alpha} g^*.$$

Finally, since $g^* > 1/\lambda$:

$$\frac{\mathrm{d}W\left(g^{*}(p)\right)}{\mathrm{d}p} = -\alpha\left[g^{*} - \frac{1}{\lambda}\right] < 0.$$

An increase in the perceived probability of audits or a decrease in the bureaucrat's budget-maximizing motive lead to a decrease in wasteful spending and improve social welfare.

P2. Bureaucrats' preferences and sensitivity to p.

The elasticity of g with respect to p, $\varepsilon_{g,p}$, is decreasing in α:

$$\frac{\partial \varepsilon_{g,p}}{\partial \alpha} = -\frac{pg^{*}}{(1-\alpha)^{2}}\left[1 + \frac{\alpha(1-p)g^{*}}{1-\alpha}\right] < 0.$$

That is, for a given increase in the perceived probability of audits, the proportional fall in wasteful year-end spending is larger when the bureaucrat's budget-maximizing motive is stronger.

Figure 1 depicts the central implications of both propositions. It plots α on the x-axis and $\varepsilon_{g,p}$ on the y-axis, for $\lambda = 1$ and $p = 0.05$ [5]. There are two important takeaways from this figure. First, $\varepsilon_{g,p}$ is always negative. Second, it is larger (in absolute value) the larger is α. The hypotheses we test in the following sections follow from these predictions:

H1. An exogenous increase in the perceived probability of being audited decreases year-end spending and leads to less wasteful spending.

H2. Agencies headed by career civil servants decrease their year-end spending less when exposed to an exogenous increase in their perceived probability of being audited.

The first hypothesis follows directly from the model. The second one relies on the assumption that the process that selects career civil servants leads to a pool of bureaucrats that place a lower weight on budget maximization on average, as compared to political appointees. This assumption is reasonable, as career civil servants are better shielded from political pressures to exhaust the budget at the end of the fiscal year.

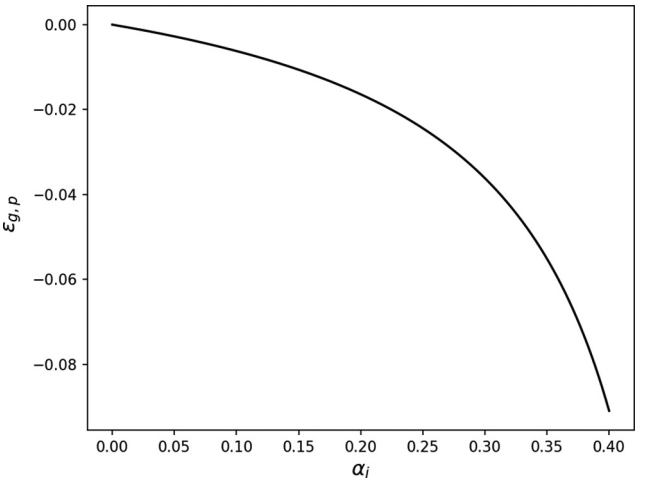


Figure 1.
Elasticity of year-end expenditure with respect to the perceived probability of audits for different values of α

In what follows, we test both hypotheses in a setting where we experimentally modified Chilean public agencies' perceived probability of being audited by the highest audit authority in the country.

4. Background and experimental design

4.1 Institutional background

4.1.1 Chile's SAI. Many features make Chile's Comptroller General (CCG) a good candidate for finding a significant power of deterrence of external audits. First, it is a very active SAI. It performs not only ex post audits but also ex ante controls by reviewing the legal basis of public agencies' administrative acts before they are executed. In 2014, the CCG performed 22,997 ex ante controls, published 16,941 legal opinions (which constitute administrative jurisprudence), initiated 314 disciplinary processes against public officers, audited 942 central and local government agencies (investing near 800 thousand oversight hours), performed 4,476 targeted investigations, and ordered the restitution of US$13.5m that were identified as misused funds (CGR, 2014). Second, the CCG's independence is guaranteed by the Constitution. Its head is proposed by the President and needs the approval of a 3/5 majority in Congress. She holds the position for eight years, and her term can span up to three administrations [6]. She is also almost impossible to remove and cannot be reelected. Also, and in contrast with other heads of public agencies in Chile, the head of the CCG faces almost no constraints to fire workers. Finally, the CCG can impose sanctions directly on public officers.

In addition, the law provides total discretion to the CCG on the agencies and programs it can select for an audit. The law also empowers the CCG's audit teams while doing their investigations. Once an agency is selected for an audit, it receives a communication from the CCG and is placed under the administrative authority of the CCG delegates. It must provide any data, reports, documents or background information deemed necessary for investigation. In addition, all public officers are required to testify if needed. Not complying with these obligations carries an immediate suspension and other sanctions.

The institutional features described above alleviate concerns about the capture of auditors by public agencies, a severe threat to audits' effectiveness in weaker institutional contexts (Lino *et al.*, 2022) [7]. In addition, they suggest that we may expect substantial behavioral effects of variations in the perceived probability of being audited, as public officers have reasons to fear the possibility of being caught at fault and sanctioned by the CCG.

At the same time, the CCG is constitutionally restricted to verifying agencies' compliance with formal procedures. That is, Chile's CCG faces limitations to directly investigate the efficiency and effectiveness of public agencies (Pollitt *et al.*, 1999; Lonsdate *et al.*, 2011). Nevertheless, the effectiveness of compliance audits seems to critically hinge on how other agents respond to the audit results (Blume and Voigt, 2011). By synthetically presenting the actions of an agency (bookkeeping practices, expenditures, etc.), a compliance audit can expose bad practices to stakeholders and trigger corrective actions [8].

4.1.2 Chile's civil service system. About half of the public agencies in Chile are subject to the Senior Public Management System (*Sistema de Alta Dirección Pública* or SADP), that is, the branch of Chile's civil service in charge of helping select high-level public executives. Depending on the hierarchical level of the position, the SADP's board or a selection committee prepares a short list of three or five applicants, from which the authority empowered to make the final appointment (Minister, Head of Service, etc.) must choose the appointee [9].

The SADP was created in 2003. The SADP board is an autonomous body. Its members are proposed by the President and need the approval of 4/7 of the Senate. Their appointments last six years and are nominated by pairs alternately every three years. Like the CCG's head, they are almost impossible to remove.

The incorporation of public services and job positions into SADP was defined by law and was implemented gradually. In 2004, it covered 668 posts. In 2012, 936 public executives in the Central Government were appointed through this system, and 1,307 in 2021.

4.2 Experimental design

The experimental sample consisted of 200 Central Government agencies, out of a universe of 223 agencies in 2012. We excluded two agencies that existed in 2012 but not in 2011 because we use data from 2011 to 2012 in our analyses. The remaining 21 agencies were left out because they are not under the CCG's jurisdiction.

We grouped the agencies according to the ministry they belong to and assigned randomly half of the agencies within each ministry to the treatment and the other half to control. The leftover agencies from ministries with an odd number of agencies were grouped and assigned randomly to treatment and control [10].

We dropped two treated agencies from the analyses because there was no administrative data available for them, leading to a sample of 98 treated and 100 control agencies. Appendix 3 shows how treated and control agencies are distributed across ministries [11].

Treated agencies received two letters and were invited to a workshop that took place after they received the first letter but before (or at the same time) they received the second letter. Controls received no letter and were not invited to the workshop.

The first letter was sent both through regular and electronic mail on December 12, 2012. The letter was signed by the head of the SAI (the Comptroller General), and invited the chief financial officer to participate in a half-day informative workshop on Public Sector Accounting Standards and year-end accounting practices. The workshop sessions were held between December 18th and 20th. Most of the workshop dealt with standard accounting practices. Toward the end of the workshop, participants were informed that a separate report on expenditures that involved "floating debt" would be required that year, with floating debt defined as expenditures accrued in the current budget but paid for in the following fiscal year (more on this concept shortly). Participants were also introduced to a spreadsheet designed specifically to report these expenditures. Over 90% of treated agencies attended the session they were invited to.

The second letter contained the audit threat and was sent on December 19th (see Appendix 1 for a translation into English of this letter). It was also signed by the Comptroller General and sent through regular and electronic mail. The letter asked agencies to complete the spreadsheet with all the purchases declared as involving floating debt, and noted that the reported information could be verified in future audits.

We designed the experiment in close consultation with the CCG. The experimental design included the workshop because proceeding in this way would be perceived by agencies as closer to how the CCG relates with agencies than if agencies had received a letter with an audit threat out of the blue. At the same time we were careful to design the workshops so they would not affect the treatment of the experiment: the topics covered related to floating debt were limited to reviewing how to fill the spreadsheet and fundamental rules governing floating debt. A survey conducted in early 2013 indicates that the workshop did not contribute, on average, to the agencies' knowledge on how to treat floating debt: financial officers from treated agencies had the same knowledge about accruing expenditures, both in general and when floating debt is

involved, as those from control agencies. The results of this survey are presented in Appendix 2.

4.3 Floating debt, year-end spending and the model's predictions

The floating debt is naturally tied to year-end spending because it represents expenses made within a fiscal year, using current budget resources, but paid for in the following fiscal year. These expenditures are usually made toward the end of the fiscal year and are said to be "floated" to the following fiscal year. The following accounting identity establishes the relationship between paid expenditures, floating debt and unspent budget:

$$Budget = Paid\ Expenditures + Floating\ Debt + Unspent\ Budget. \quad (1)$$

If, toward the end of the fiscal year, the agency realizes it has spent less than its desired level of expenditure and would like to reduce the Unspent Budget item on the right hand side of equation (1), it can accrue additional expenditures this year and pay for them either this year (increasing the Paid Expenditures item) or in the following year (increasing the Floating Debt item).

Seen through the lens of the model in Section 3, year-end spending by an agency of type $\alpha > 0$ will include items that decrease social welfare. The CCG conjectured that these items were also likely to involve the sort of irregular behavior CCG audits are designed to deter. The reaction of an agency to an increase in the probability of audits of year-end expenditures associated with floating debt, the parameter p in our experiment, could be of two kinds. First, the agency could decide to anticipate some payments and make them before the fiscal year ends, thereby increasing Paid Expenditures and decreasing Floating Debt in equation (1). Alternatively, the agency could refrain from making some purchases, thereby increasing Unspent Budget and decreasing Paid Expenditures and Floating Debt in equation (1). Agencies had means to pay immediately at essentially no cost [12], so that finding a decrease in year-end expenditures would suggest the CCG's conjecture was correct and agencies refrained from making expenditures they did not want the CCG to audit.

4.4 Data

Procurement data come from *ChileCompra*, the government agency that administers the electronic platform through which public agencies purchase goods and services. The sample size drops slightly from the agencies that were originally assigned to the treated and control groups, with five agencies in the treatment group and ten in the control group that are excluded because they made no purchases through *ChileCompra* in December 2011 and/or December 2012. For data on budget accrual, we rely on administrative data provided by CCG. Finally, data on 2012 SADP coverage was provided by Chile's civil service. We keep the same sample throughout all the analyses for consistency, and show in Appendix 4 that the results are robust to using the full sample in the cases where only administrative data is used [13].

Table 1 presents descriptive statistics for key variables across the three data sources. The statistics are shown for 2011 and 2012, the years used in the empirical analyses. Panel a shows procurement data. We observe over 200,000 purchases in December of each year, distributed over more than 6,000 goods and services. On average, agencies in our sample purchased more than US$3m in December, the average purchase was for US$3,000. Panel b shows administrative data on budget accrual. The average budget of an agency is approximately US$350m. Most of the budget is paid within the fiscal year, leaving a small share that is either unpaid or unexecuted. Interestingly, while unpaid budget only slightly increased between 2011 and 2012, the unexecuted budget almost doubled from $8m to $15m on average across the sample. The stock of floating debt accumulated at the beginning of the

Variable	2011	2012
Panel a: December procurement		
No. of different goods or services purchased	7,020	6,497
Avg. no. of purchases per agency with positive expenditure	1,123	1,127
Avg. bought per agency with positive expenditure (million US$)	3.5	3.3
Avg. of Purchase (US$)	3,160	2,931
Panel b: Budget accrual		
Total budget (million US$)	339	356
Paid accrued expenses (million US$)	325	331
Unpaid accrued expenses (million US$)	7	8
Unexecuted budget (million US$)	8	15
Cumulative floating debt (% budget)	2.7	3.0
Budget executed on December (% budget)	17.7	20.0
Panel c: Civil service		
Enrolled in SADP (% of agencies)	–	53.6

Table 1.
Data description

year (cumulative floating debt) amounts to approximately 3% of the budget on average, and the budget executed during December is close to 19% of the budget, more than twice the value it would have if expenditures had been distributed evenly throughout the year (100/12 = 8.3%). Panel c shows that slightly more the half of the agencies in our sample was enrolled in SADP by 2012.

5. Results

5.1 Balance

Table 2 presents the balance between treated and control groups' for the variables displayed in Table 1, showing that the randomization successfully built comparable groups with differences that fall within what is expected from random variations. Panel a shows that

Variable	Control	Treated	*p*-value
Panel a: December 2011 procurement			
No. of different goods or services	179	250	0.11
No. of purchases	718	1.514	0.06
Total value purchases (million US$)	2.6	4.5	0.05
Average value of purchase (US$)	8,124	8,620	0.87
Panel b: Budget accrual			
Budget (2011, million US$)	217	459	0.18
Paid accrued expenses (million US$)	203	442	0.17
Unpaid accrued expenses (floating debt, million US$)	6	7	0.94
Unexecuted budget (million US$)	7	10	0.68
Cumulative floating debt (2012, % budget)	3.2	2.2	0.19
Budget executed on December (2011, % budget)	17.7	20.0	0.06
Panel c: Civil service			
Enrolled in SADP (2012, % of agencies)	56.7	50.4	0.41
Number of agencies	90	93	

Note: For each variable, the table presents the average across the control and treated groups and the *p*-value of the difference of the average between both groups

Table 2.
Balance between the treated and the control group

treated and control groups present no statistically significant differences at the 5% level on the average number of different goods and services purchased on December 2011, the total number of purchases, or the average value of their purchases. The total value of purchases is statistically significant at this level, although only marginally. In panel b, we find no differences that are statistically significant at the 5% level for the total 2011 budget and its components. The same can be said of accumulated floating debt at the beginning of 2012 and the fraction of the budget executed in December 2011. These variables are important, as agencies that accumulate a large stock of floating debt or spend a large fraction of their budgets in December may call the attention of the CCG, in which case the perceived probability of being audited would be larger. Panel c shows that the percentage of agencies enrolled in SADP in 2012 was similar for treated and control agencies [14].

5.2 Treatment effect on percentage of floating debt

Table 3 shows the effects of the experiment on floating debt as a percentage of the total budget. The intervention specifically targeted floating debt and, as expected, reduced this accounting practice. Furthermore, the effect was large: while treated agencies decreased their floating debt from 3.61% of the budget in year 2011 to 3.21% in 2012, agencies in the control group increased their floating debt from 2.80% in 2011 to 3.83% in 2012. Overall, the difference-in-differences estimator shows that the intervention reduced floating debt by 1.43 percentage points, a difference that is statistically significant at the 1% level. This difference is also economically significant as it implies that treated agencies reduced their floating debt by 40%, as a percentage of their baseline level.

5.3 Treatment effect on year-end spending

As argued above, agencies have two ways of reducing floating debt: paying immediately for what they used to float, in which case year-end spending is not affected; or cancelling expenditures, in which case year-end spending is expected to fall. Here, we use procurement data to estimate the effect of the experiment on year-end spending. We also identify the types of goods and services that were most affected by the treatment.

As the letters containing the audit threat were sent by regular and electronic mail on December 19, 2012, in the regressions that follow we compare spending during the last 12 days of December 2012 with spending during the same period in 2011 [15]. Specifically, we estimate the following equation:

$$M_{i,t} = \gamma_i + \beta_1 T_i A_t + \beta_2 A_t + u_{i,t}, \tag{2}$$

where $M_{i,t}$ stands for the logarithm of the total value of purchases made by agency i from December 20 to December 31 in year t, T_i denotes the treatment-indicator, A_t an after-treatment indicator (i.e. one for 2012 and zero for 2011), $u_{i,t}$ the error term and γ_i agency

Group/Year	2012	2011	Difference
Treated	3.21 (0.57)	3.61 (0.63)	−0.40 (0.85)
Control	3.83 (0.59)	2.80 (0.39)	1.03 (0.71)
Difference	−0.62 (0.82)	0.81 (0.75)	−1.43** (0.51)

Notes: Standard errors in parentheses. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table 3. Effect of the treatment on floating debt: difference-in-differences estimation

fixed effects. The parameter β_1 captures the treatment effect of interest, that is, the causal impact of CCG's audit threat on agencies' year-end total expenditures, which we expect to be negative. Note that agencies' fixed effects γ_i capture determinants of year-end expenditure that are constant across all agencies in 2011 (e.g. public debt), as well as systematic differences between agencies that remain constant over the years considered (e.g. state of infrastructure). The after-treatment indicator A_t captures variables that shift year-end expenditure between 2011 and 2012 equally across all agencies (e.g. changes in macroeconomic conditions between 2011 and 2012). While the randomization of the treatment secures that a simple difference in means between the treated and control groups in 2012 is an unbiased and consistent estimator of the causal effect of the treatment, adding these controls and focusing in the difference-in-differences estimator $\hat{\beta}_1$ increases the precision with which we estimate the treatment effect, a valuable improvement given the small sample size.

Results are reported in column (1) of Table 4. The treatment reduced the total value of purchases made in the last 12 days of December by 0.396 log points (i.e. by 33%). In columns (2) and (3), we restrict the estimation to agencies with at least 20 and 40 invoices, respectively. In columns (4) and (5), we present results where $M_{i,t}$ is calculated leaving out extreme values for invoices in the pooled sample: the largest and smallest 1% in column (4), the largest and smallest 5% in column (5). The estimates for the treatment effect in columns (2)–(5) are similar to those in column (1), showing the robustness of the result.

As a validity check of our research design, we also estimate placebo versions of equation (2), using the total value of purchases made during periods *before* the treatment as the dependent variable. We expect to find no impact at a period where there is no actual treatment. Table 5 shows the results. Column (1) is our benchmark and corresponds to column (1) of Table 4, i.e.

	All	At least 20 invoices	At least 40 invoices	Without extreme values 1–99%	Without extreme values 5–95%
Sample	(1)	(2)	(3)	(4)	(5)
Treatment × after	−0.396** (0.178)	−0.388** (0.179)	−0.357** (0.181)	−0.294** (0.126)	−0.316** (0.126)
After	−0.242* (0.123)	−0.242* (0.124)	−0.234* (0.128)	−0.185* (0.095)	−0.068 (0.104)
R-squared	0.141	0.137	0.127	0.156	0.101
Observations	366	362	350	366	366

Table 4. Effect of the experiment on procurement

Notes: Clustered standard errors at the agency level in parentheses. Columns (2) and (3) restrict the estimation to agencies with at least 5 and 20 invoices. Columns (4) and (5) show results when leaving out extreme observations in the pooled data. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

	Benchmark	December 1–19	December 1–12	December 13–18	January 2013
Sample	(1)	(2)	(3)	(4)	(5)
Treatment × after	−0.396**(0.178)	−0.110 (0.197)	−0.170 (0.224)	0.167 (0.229)	−0.135 (0.203)
After	−0.242* (0.123)	0.116 (0.136)	0.161 (0.149)	−0.052 (0.176)	0.236* (0.126)
R-squared	0.141	0.004	0.006	0.004	0.017
Observations	366	366	366	364	366

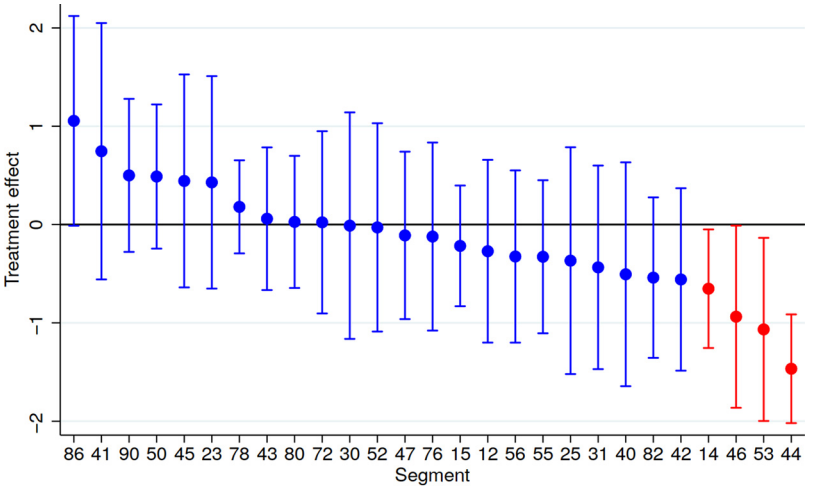
Table 5. Placebo exercises

Notes: Clustered standard errors at the agency level in parentheses. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

the treatment effect on procurement spending the last 12 days of December. Column (2) shows that there is no effect of the treatment on purchases made the first 19 days of the month, before agencies received the audit threat letter. In column (3), we use the total value of purchases made between December 1st and 12th, the date when agencies received the invitation to the workshop. There is no effect of the placebo experiment on these purchases. Column (4) shows that there is no effect on procurement spending for treated agencies between December 13th and 18th as well, ruling out that the invitation to the workshop may have had an effect on procurement spending. Finally, in column (5), we assess if the treatment had an effect on purchases made in January 2013 to rule out a purchase timing effect, finding no significant effect.

Next, we analyze the response of the purchases of specific products to the audit threat. We identify goods by the first two digits of the United Nations Standardized Product and Services Code (UNSPSC) to have enough agencies buying products in each segment [16]. There are 55 segments with at least one agency reporting purchases. From this group we analyze the 27 segments where at least 20 agencies in both the treated and control groups, made purchases. For each of these segments, we estimate equation (2) using the value of the purchases in the segment as the dependent variable. Figure 2 shows the estimated treatment effects across segments in decreasing order. We find statistically significant treatment effects at the 10% level in four of them (red bars): Segment 14, "Paper Materials and Products"; Segment 46, "Defense and Law Enforcement and Security and Safety Equipment and Supplies"; Segment 53, "Apparel and Luggage and Personal Care Products"; and Segment 44, "Office Equipment and Accessories and Supplies". For all of the above segments, the estimated treatment effect is negative [17].

Table 6 presents the regression results for the four segments where we detected significant treatment effects. Columns (1)–(3) show the results for "Paper Materials and Products", "Defense and Law Enforcement and Security and Safety Equipment and Supplies" and "Apparel and Luggage and Personal Care Products," respectively. As can be seen, the decreases in year-end spending are 0.65, 0.94 and 1.07 log points, respectively, that



Notes: Vertical lines represent 90% confidence intervals. Red lines are significant at the 10% level

Figure 2.
Treatment effect across UNSPSC segments

is decreases of 48%, 60% and 66%. They are also significant at the 10% level. Column (4) shows the effects of the experiment on "Office Equipment and Accessories and Supplies," finding an even larger reduction of 1.47 log points (77%) that is significant at the 1% level. A back of the envelope calculation using these estimated effects shows that these four categories explain 16% of the total reduction in year-end expenditure attributed to the treatment, although they represent only 8% of year-end spending [18].

Appendix 5 reports placebo regressions on January purchases for the four segments analyzed above. A positive and significant estimated treatment effect would show that part of expenditure deterred by the treatment toward the end of the fiscal year is undone via more spending at the beginning of the following fiscal year. There is no evidence of such an effect. The effect is not significant for each of the four segments. Furthermore, in one case the estimated effect is negative and the positive estimates for the remaining three cases are much smaller than the effects reported in Table 4.

Overall, the results in this section suggest that year-end spending is likely to be partly wasteful as pointed out by Liebman and Mahoney (2017). Also, that an increase in the perceived probability of an audit by an SAI reduces this kind of spending.

5.4 Civil service appointees and year-end spending

In this section, we test the second hypothesis implied by the model in Section 3, that is, whether agencies headed by career civil servants – as opposed to political appointees – decrease their year-end spending less when exposed to an exogenous increase in the perceived probability of being audited. As noted before, not all agencies are covered by SADP (Table 2). We classify agencies depending on whether at least one high level manager was appointed by SADP in 2012. To allow for flexibility, we reestimate equation (2) for agencies with and without SADP appointees separately.

Table 7 reports the results. Column (1) presents the benchmark estimates. Columns (2) and (3) show the results for agencies with and without executives appointed by SADP,

Table 6.
Effect of the experiment on specific goods

UNSPSC segment	14: Paper products (1)	46: Personal care (2)	53: Security equipment (3)	44: Office supplies (4)
Treatment × after	−0.652* (0.367)	−0.937* (0.563)	−1.067* (0.566)	−1.467*** (0.337)
after	0.318 (0.277)	0.476 (0.447)	0.439 (0.395)	0.684** (0.269)
R-squared	0.026	0.051	0.057	0.125
Observations	240	112	128	280

Notes: Clustered standard errors at the agency level in parentheses. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table 7.
Effect of the experiment on SADP and non-SADP agencies

Sample	Benchmark (1)	Agencies with SADP (2)	Agencies without SADP (3)
Treatment × after	−0.396** (0.178)	−0.237 (0.225)	−0.550* (0.288)
After	−0.242** (0.123)	−0.214 (0.137)	−0.280 (0.222)
R-squared	0.141	0.093	0.195
Observations	366	196	170

Notes: Clustered standard errors at the agency level in parentheses. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

respectively. As can be seen, the treatment effect in agencies with no SADP executive doubles that of agencies with at least one SADP executive, −0.55 log points (−42%) in the former vs −0.24 log points (−21%) in the latter. The effect is statistically significant only for agencies without SADP apointees.

These results suggest that agencies with SADP executives are better managed, headed by executives empowered to cope with political pressures to exhaust the budget wastefully [19] [20]. However, it is important to interpret these results as suggestive evidence rather than a causal claim. Although we control for agency fixed effects, we cannot rule out the possibility that the results are driven by heterogeneous treatment effects across some of the dimensions over which SADP and not-SADP agencies systematically differ.

6. Conclusion

This paper assessed the deterrence role of SAIs based on a field experiment involving public agencies and the Comptroller General in Chile. The main goal of the experiment was to measure the behavioral response of public agencies to an audit threat on year-end spending. Using extensive procurement data on year-end spending, we found a 33% decrease in year-end purchases among treated agencies relative to controls. We also found that the largest reductions took place for purchases of office supplies, safety equipment, personal care products and paper products. Finally, we found that year-end spending fell more drastically in agencies without an executive appointed through Chile's civil service system than in those with at least one executive appointed through the system (42% vs 21%). We developed a simple model of an optimizing bureaucrat with an objective function that is increasing in the allocated budget to rationalize these findings.

Overall, the results suggest SAIs' audits have an important role in deterring non desirable activities in public agencies. Even though we have no direct evidence that the expenditures deterred by the experiment would have been wasteful, two pieces of evidence strongly point in that direction. First, the categories of goods that fell most have been previously flagged as items that may be purchased at the end of the fiscal year to exhaust the budget and not because of their high net social value (Douglas and Franklin, 2006; Liebman and Mahoney, 2017). Second, the fall in year-end expenditure is concentrated in agencies without an executive appointed through Chile's civil service system, which are plausibly less insulated from political pressures to exhaust the budget through wasteful spending.

The importance of SAIs as watchdogs of integrity and efficiency in the public administration makes our result an important input for the institutional design of external control over the public sector. Although Chile's SAI has unique characteristics, it shares with other SAIs a common mission and a common tool for accomplishing it: audits. Nevertheless, it is important to be cautious when extrapolating these results to other institutional contexts, because the power of deterrence of an SAI depends not only on the probability of detection, but also on the consequences faced by public officers when caught (Becker and Stigler, 1974). While Chile's SAI is restricted to verifying compliance with formal procedures, it can impose sanctions to public officers and expose inefficient practices that fall short of being illegal in their public audits reports. In countries where SAIs cannot impose direct sanctions on public officers the power of deterrence critically hinges on the response of other agents, such as Congress, the judiciary and citizens, to audit results. In addition, it is important to acknowledge that this study has not considered side effects of audits, which are relevant to obtain a comprehensive appraisal of the impact of SAI audits on society's welfare (Bonollo, 2019). Focusing on metrics that measure agencies'

performance directly, along variables that capture the well-being of public officers, constitutes a promising avenue for future research.

Notes

1. Karnani (2018) also finds evidence of less competition and higher prices in year-end procurement auctions in Chile.
2. Melo *et al.* (2014) find a negative correlation between a measure of the independence of state-level SAIs and the use of "creative accounting practices" by state governments in Brazil to circumvent debt ceilings. Similarly, Jin and Lin (2012) show that China's SAIs postaudit rectification effort is associated with less corruption in provincial governments. Blume and Voigt (2011) find, in a cross-country analysis, that differences in SAIs' independence and mandates are uncorrelated with fiscal indicators, government effectiveness, corruption and productivity. Saito and Mcintosh (2010) find a negative correlation between audit effort (measured by audit hours) and wasteful spending by Georgia's schools. For Latin America's SAIs, Santiso (2006) obtains a positive correlation between SAIs' external control effectiveness and budget transparency, control of corruption and the quality of civil service measures and no evidence of a direct relation with fiscal outcomes. See Bonollo (2019) for a recent review of qualitative and quantitative studies on SAI's outcomes.
3. Our conceptualization of wasteful expenditure may be understood as active waste under the binary taxonomy between active and passive waste proposed by Bandiera *et al.* (2009). As in their model, bureaucrats in our model select the optimum level of active waste, where the marginal private benefit derived from wasteful expenditure equals the marginal cost in terms of social welfare and expected sanctions if caught.
4. Alternatively, wasteful expenditure could be uncovered by the audit and made public, decreasing the bureaucrat's expected utility as third parties, such as voters and other overseeing agencies, take corrective actions. Although this interpretation fits more closely to the institutional context of this paper, we assume the bureaucrat is forced to pay back the wasted monies to simplify the exposition. Note that both alternatives affect the objective function of the bureaucrat in the same way.
5. The figure shows up to $\alpha = 0.4$ because the value of $\varepsilon_{g,p}$ diverges to $-\infty$ as α approaches its maximum value given by the restriction we impose to obtain an interior solution, $\lambda > \frac{\alpha}{1-\alpha}(1-p)$.
6. Chilean Presidents serve for four years without immediate reelection.
7. However, they may, at the same time, generate adverse side effects, such as excessive bureaucratic procedures and fear of innovation. See Bonollo (2019) for a recent review of the impacts of SAI's actions.
8. CCG's audit results can be found at www.contraloria.cl/web/cgr/informes-de-auditorias
9. The process begins with a call for applications in the written press. Next, a headhunting firm analyses the candidates' CVs, does background checks and interviews candidates. The SADP board (or the selection committee) then uses the information provided by the headhunting firm to choose the best candidates and sends a shortlist, with three or five names, depending on the position, to the authority who makes the final appointment.
10. We did not have information to stratify agencies according to other variables, such as the task performed by the agency. Yet securing a balance in the ministries they belong to should help achieve a more balanced assignment in other relevant variables that correlate with ministry and budget size, such as the nature of the task performed.
11. The full list of 198 agencies, the ministry they depend on and whether they were in the treatment or control group is available upon request.

12. Public agencies have access to Mercado Público, an online marketplace where they can easily buy products from thousands of companies at prices that are set by framework agreements run by ChileCompra, the public agency that coordinates public purchases.
13. The fraction of truncated observations is low and the treatment is not significant in a probit equation predicting truncation. This suggest that treatment effects are not biased due to sample selection.
14. Some of the average differences between treated and control agencies in Table 2, although not statistically significant, are large. This is the case because there is a large dispersion in budget size (and therefore its components), leading to a high probability of obtaining large differences between the average of two randomly chosen groups. Nevertheless, these differences should not be a concern for the analyses that follow, as all our specifications include agencies' fixed effects that control for time-invariant characteristics of agencies, such as their relative size.
15. The results that follow are robust to changing this period by a couple of days.
16. The eight-digit UNSPSC has four primary levels: segment, family, class and commodity. The first two digits correspond to the segment. For example, segment 44 corresponds to Office Equipment, Accessories and Supplies. The full list of segment numbers and names can be found at https://usa.databasesets.com/unspsc
17. Appendix 5 shows the results from a placebo exercise, which uses expenditure during the first 19 days of December instead of the last 12 days of December. We find no segment with a significant treatment effect at the 10% level in this placebo exercise.
18. We estimate, for each control agency, its counterfactual fall in year-end spending across these four segments along its counterfactual fall in total year-end spending. 16% follows from taking the average of the ratio between the sum of the estimated falls in expenditures across these four segments and the estimated fall in total expenditures.
19. Note that the results are also consistent with an alternative hypothesis where SADP empowers bureaucrats, leading them to believe they will face less severe sanctions if caught at fault. Although feasible, we believe this interpretation is unlikely given that the law does not grant SADP bureaucrats any protection against sanctions by the CCG.
20. Alternatively, seen through the lens of our model, agencies in the SADP have lower values of α which means they overspend less than agencies without executives appointed through the SADP.
21. Note that the probability of obtaining one or more rejections at the 10% level under the null across six independent tests is given by $1 - 0.9^6 = 0.47$.

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Appendix 1. National Comptroller's Letter (translation)

Information request

As part of the activities of this Comptroller Office, and according to Article 9 of the National Comptroller Office Act (No. 10.336), the concerned public body should provide to this supervising entity the information referred in the annex attached herein, at the latest on January 31, 2013.

The required information concerns the accrued and payable liability as of December 31, 2012, which shall be provided according to the form annexed. However, to expedite the sending of this information, it should be submitted electronically, under an Excel file, and sent it to this supervising entity through its institutional email (sectorpublico@contraloria.cl); the subject of the email will be

the name of the sender's entity. In addition, the latter should send a communication informing about the sending of the required information and the full amount of the accrued and payable liability by the concerned public body.

It is to be noted that any query about this matter should be directed to the following email addresses: gsaavedra@contraloria.cl or Inaveasc@contraloria.cl.

It is important to emphasize that the supporting documentation of the required information should not be submitted. It is strongly recommended to keep this supporting documentation available in case this supervising entity requires it subsequently.

Finally, the concerned public body shall adopt the necessary measures to ensure the full implementation of this instruction.

Sincerely yours,

Appendix 2. Impact of training on knowledge about year-end accrual

During 2013, we run 286 telephonic surveys to individuals that worked at the financial departments of the agencies that were part of the experiment. We were able to survey individuals from 77 and 76 control and treated agencies, respectively. Between 1 and 3 interviews were conducted per agency, with an average of 1.87.

Baseline data on education, job title and seniority were collected. There are no significant differences on these variables between treated and control agencies that responded to the survey (results available upon request). In addition, six questions covering knowledge about floating debt and how to correctly accrue expenditures were asked:

(1) Q1: What are the requirements to accrue an expenditure? For each case, answer whether it is a requirement or not:
   - Purchase order
   - Reception of good or service and its back-up document
   - Reception of good or service

(2) Q2: Which one of the following documents cannot be used by themselves to back an accrual? (mark all that apply):
   - Memorandum
   - Invoice
   - Delivery receipt
   - Purchase order
   - Receipt
   - Payment status

(3) Q3: Is it possible to accrue with resolution or agreement? (select one):
   - Always
   - Depends on what is established in the resolution or agreement
   - Only with resolution
   - Never
   - Don't know

(4) Q4: The financial obligations contracted up to December 31st each year, and that have not been paid, must be accrued as (select one):
   - Budgetary creditors
   - Not budgetary creditors

- Budgetary debtors
- Not budgetary debtors
- Other

(5) Q5: In a given year, floating debt is comprised by (select one):

- Financial obligations accrued but not paid during the current fiscal year
- Contracts and acquisitions done during the current fiscal year
- Financial obligations accrued but not paid during the past fiscal year
- Contracts and acquisitions done during the past fiscal year
- Don't know

(6) Q6: The processing of the act that approves a contract, while not being payable, must be registered as

- In an administrative registry as a commitment
- In an accountable registry, as a payment
- Don't know

Each question is coded as correct or incorrect. In the case of questions 1 and 2, that involve subquestions, we only consider the question as incorrect when the error would lead to an accrual that does not comply with the regulation. In question 1, this implies that selecting the purchase order as required (even though it is not) while also selecting that options 2 and 3 are required (which are) is not considered an error. In question 2, this implies that as long as options 1 and 4 are selected (the ones that cannot back and accrual by themselves), the question is correct regardless of whether other options were selected as well. Columns 1–6 of Table A1 show, for each question in the column, the estimated treatment effect on the probability of an individual having the answer to the corresponding question correct using a linear probability model. The effect is positive in 5 out of the 6 questions, with small differences below 10 percentage points. None of the estimates are statistically significant at the 5% level, and only 1 is at the 10% level [21]. Column 7 shows that, on average, the treatment has no impact on the number of answer an individual has correct.

Figure A1 shows the density of correct answer, separately for individuals in treated and control agencies. While the distribution of the treated group is slightly shifted to the right, a chi-squared test for differences between discrete distributions fails to reject the null hypotheses that observations from both groups are drew from the same distribution (p-value = 40%).

Table A1.
Effect of the experiment on questions about correct year-end accrual

Dependent variable	Q1 (1)	Q2 (2)	Q3 (3)	Q4 (4)	Q5 (5)	Q6 (6)	#Correct (7)
Treatment	0.047 (0.049)	0.001 (0.058)	0.052 (0.064)	0.061 (0.065)	−0.013 (0.062)	0.072* (0.039)	0.221 (0.141)
R-squared	0.000	0.000	0.006	0.004	0.000	0.010	0.005
Observations	286	286	286	286	286	286	286

Notes: Clustered standard errors at the agency level in parentheses. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

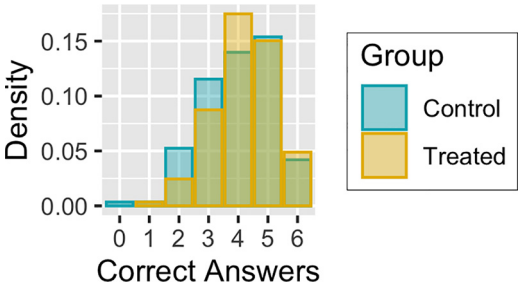


Figure A1. Distribution of correct answer among individuals from treated and control agencies

Appendix 3. Treated and controls by ministry

Ministry	Controls	Treated
Presidencia De La República	0	1
Congreso Nacional	2	2
Poder Judicial	2	1
Contraloría General De La Republica	0	1
Ministerio Del Interior Y Seguridad Pública	13	12
Ministerio De Relaciones Exteriores	3	2
Ministerio De Economia, Fomento Y Turismo	6	7
Ministerio De Hacienda	6	6
Ministerio De Educacion	4	4
Ministerio De Justicia	3	4
Ministerio De Defensa Nacional	8	8
Ministerio De Obras Publicas	3	2
Ministerio De Agricultura	3	3
Ministerio De Bienes Nacionales	1	0
Ministerio Del Trabajo Y Previsión Social	6	5
Ministerio De Salud	19	19
Ministerio De Mineria	2	1
Ministerio De Vivienda Y Urbanismo	9	8
Ministerio De Transportes Y Telecomunicaciones	1	2
Ministerio Secretaria General De Gobierno	1	2
Ministerio De Planificación	3	4
Ministerio Secretaria General De La Presidencia De La República	0	1
Ministerio Público	1	0
Ministerio De Energia	2	2
Ministerio Del Medio Ambiente	2	1
Total	100	98

Table A2. Treated and controls by ministry

Appendix 4. Results for full sample (when feasible)

This section presents the results for balance and fraction of floating debt using the full sample. Reassuringly, the results are remarkably similar to those shown in Tables 2 and 3, suggesting that the results are robust to the exclusion of a small number of observations that did not have purchases through *ChileCompra* in 2011 or 2012 and are therefore excluded from the procurement analyses.

Table A3. Balance between the treated and the control group (full sample)

Variable	Control	Treated	*p*-value diff.
Budget (million US$)	290	436	0.43
Paid accrued expenses (million US$)	277	422	0.43
Unpaid accrued expenses (floating debt, million US$)	6	6	0.89
Unexecuted budget (million US$)	7	9	0.74
Cumulative floating debt (% budget)	2.9	2.1	0.24
Budget executed on December (% budget)	18.9	20.4	0.35
Number of agencies	100	98	

Notes: For each variable, the table presents the average across the control and treated groups and the *p*-value of the difference of the average between both groups. All variables are from the 2011 fiscal year

Table A4. Effect of the treatment on floating debt: difference-in-differences estimation (full sample)

Group/Year	2012	2011	Difference
Treated	3.11 (0.53)	3.51 (0.60)	−0.40 (0.27)
Control	3.74 (0.54)	2.93 (0.42)	0.80 (0.44)
Difference	−0.62 (0.77)	0.58 (0.73)	−1.20** (0.51)

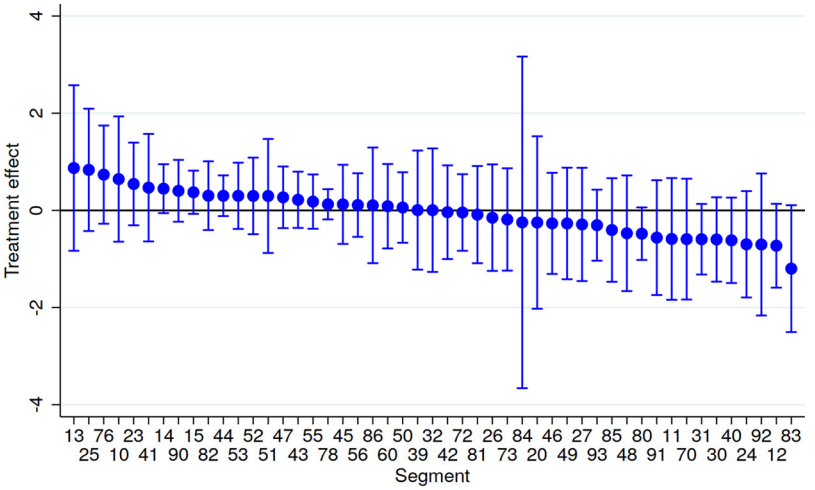
Notes: Standard errors in parentheses. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Appendix 5. Placebo checks

In this section, we provide additional placebo estimates. First, we replicate the exercise that estimates treatment effects across UNSPSC segments with at least 20 treated and 20 control agencies, but we use the first 19 days of December instead of the last 12 days. Even though the number of analyzed segments goes from 27 in the original analysis (Figure 2) to 49, Figure A2 shows that we find no segment with a significant effect in the first 19 days of December.

Next, we assess the effects of the experiment on January 2013 in the four segments where we found significant falls in year-end expenditure: Segments 14, 46, 53 and 44. In this exercise, the after treatment indicator is equal to 1 if t is January 2013 and equal to 0 if t is January 2012. The treatment indicator is the same as defined before: one for treated agencies and zero otherwise.

Table A5 shows the results. As can be seen, there are no significant effects for any of the types of purchases considered.



Notes: Vertical lines represent 90% confidence intervals. Red lines are significant at the 10% level

Figure A2. Placebo treatment effect across UNSPSC segments

UNSPSC segment	14: Paper products (1)	46: Personal care (2)	53: Security equipment (3)	44: Office supply (3)
Treatment × after	0.249 (0.285)	−0.227 (0.556)	0.193 (0.483)	0.246 (0.297)
After	0.183 (0.198)	−0.248 (0.405)	−0.566 (0.365)	−0.128 (0.251)
R-squared	0.039	0.028	0.042	0.044
Observations	278	138	178	322

Notes: Clustered standard errors at the agency level in parentheses. Stars next to point estimates indicate level of statistical significance when $p < 0.1$: *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table A5. Effect of the experiment on specific goods in January

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