The Dark Side of the Contract: Do Government Audits Reduce Corruption in the Presence of Displacement by Vendors? [§]

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ABSTRACT: Government audits have been shown to discourage corruption where audits have taken place. However, audits may also discourage public spending, either because bureaucrats can no longer capture rents to the same extent, or because those make them afraid of being labeled corrupt for procurement mistakes. If spending goes down, in turn, at least some government vendors may move elsewhere, where bureaucrats are still willing to work with them, *displacing* spending and, potentially, corruption. This paper explores random audits from an anti-corruption program in Brazil to study this new mechanism, using contract-level data to estimate what happens when bureaucrats are audited – both locally and where government vendors move to, in the aftermath of audits. We find that past audits significantly reduce local spending, and that vendors significantly displace it to other municipalities. As a result, while over-invoicing indeed falls locally, corruption is significantly displaced by vendors: the *net effect* of past audits on the probability of over-invoicing is actually *positive*, and they have *no effect* on the amount over-invoiced – reversing the conclusions from estimates that ignore contamination of the control group. What is more, we show that displacement is consequential: implementation quality and health outputs and outcomes are *hurt* by past audits. Taking advantage of randomly assigned capacity-building trainings, we document that bureaucrats' responses are consistent with both "greasing the wheels" and procurement risk.

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> "Every dollar that a corrupt official or a corrupt business person puts in their pocket is a dollar stolen from a pregnant woman who needs health care. (…) In the developing world, corruption is public enemy number 1".
>
> – Jim Kim, former president of the World Bank[1]

> "Even in the case of petty bribery or extortion, it is relevant to ask, what is the alternative?"
>
> – Colin Leys (1965, p. 220)

1 Introduction

Several studies document that corruption can be successfully deterred by a variety of monitoring strategies, from government audits to campaigns that disclose information to citizens.[2] Such studies, however, provide an incomplete picture of the effects of those strategies. The reason is that corruption is just one out of many decisions over which bureaucrats can exert influence, including spending – linked to the quantity and quality of public goods and services. Understanding what audits do to public spending is essential: if incentives are distorted such that spending is reduced, at least some government vendors may *move elsewhere*, where bureaucrats are still willing to work with them, *displacing* spending and, potentially, corruption. In other words, such monitoring strategies may generate *contamination of the control group*, leading one to *over-estimate* their effects on corruption if displacement is not taken into account.

Here is why past audits are expected to induce at least partial displacement by vendors. Corruption often emerges from a bargaining process that starts with a vendor helping a bureaucrat bear the costs of preparing documentation for a competitive tender, through which goods or services will be procured. In doing so, the requirements of those goods or services can be can distorted such as to favor that vendor in the competitive process, and/or public resources can be misallocated through over-invoicing once the goods or services have been rendered, as a *quid-pro-quo* for the vendor's help earlier on. When higher perceived audit risk leads a bureaucrat to no longer request help from a vendor, the only way for that vendor to *still equate marginal costs and revenues* is to help some other bureaucrat elsewhere – one who is still willing to

[1] http://mobile.reuters.com/article/idUSBRE9BI11P20131219?irpc=932

[2] e.g.: Avis, Ferraz and Finan (2018) for the effects of government audits on corruption within federal transfers to Brazilian municipalities; Olken (2007) for the effects of government audits on road projects in Indonesia; Reinikka and Svensson (2005) for the effects of a newspaper campaign on government transfers to schools in Uganda; and DiTella and Schargrodski (2003) for the effects of a crackdown on corruption in Argentinean hospitals' purchases.

rely on his help. For that reason, audits are expected to at least partially displace corruption to other municipalities where vendors are active.[3]

This paper reexamines the effects of a random-audits program in Brazil, studying its effects on public spending, and its net effects on corruption once displacement by vendors is taken into account. The Brazilian anti-corruption program randomly draws municipalities to be audited with respect to their use of federal funds since 2003, in a joint venture between the Office of the Comptroller General and the national lottery. Auditors analyze municipalities' accounts and documentation, and physically inspect public works and service delivery to assess whether voluntary earmarked federal transfers are effectively spent according to their guidelines. Following Avis, Ferraz and Finan (2018), we take advantage of the randomness of past audits taking place at a municipality or its neighbors, as those have been shown to make the perceived probability of being audited by the program in the future more salient.

We draw on a novel dataset with contract-level data within the State of São Paulo, compiled by the State Court of Accounts for the 2011-2015 period. Such dataset has four key advantages over audits data: (1) it allows us to accurately observe *public spending*, beyond planned budgeted amounts; (2) it allows us to measure corruption *objectively*, from over-invoicing (i.e.: differences between invoices entered in the system and the amount budgeted for those goods and services, instead of auditors' perceptions from audit reports), in what comes to both its *extensive and intensive margins*; (3) it allows us to measure corruption for *all* municipalities, not just for those that were audited by the random-audits program; and (4) it allows us to observe outcomes at the contract level, such that we can explicitly analyze the effects of the program on *bureaucrats and vendors*.

By contrasting different configurations of past audits across different contracts (municipality-vendor pairs), we can estimate the effects of audits both locally and elsewhere. For some pairs, neither bureaucrats nor vendors have experienced past audits – this is our control group. For some pairs, only bureaucrats have experienced past audits; this happens whenever vendors have moved in *only after the audit*, and did not have active contracts in *any* municipality randomly drawn to be audited in the past. We can use those to estimate the *local effects* of audits *within new contracts*. For other pairs, the opposite is true: only vendors have experienced past audits; this happens whenever vendors are active across multiple municipalities and (at least) one of those – but not the municipality itself – has been drawn to be audited in the past. We can use those to estimate the *displacement effects* of audits. Last, for some pairs, *both* bureaucrats and vendors have experienced past audits; either together, in existing contracts, or independently, if vendors moved in

[3] If the costs of supporting bureaucrats in a new relationship are higher, then spillovers would not completely offset the decrease in corruption induced by audits. If, however, there are static inefficiencies that prevent vendors from dynamically maximizing rents given their established relationships, then audits could even induce *higher* corruption overall.

only after the local audit but had active contracts in municipalities previously drawn to be audited by the program. We can use those to estimate the *local effects* of audits *within existing contracts*.[4]

We find that past audits significantly decrease local spending, which falls by as much as 29% in contracts for which the bureaucrat (but not the vendor) experiences past audits. In turn, spending *increases* substantially elsewhere, in contracts for which the vendor (but not the bureaucrat) experiences past audits, by as much as 23%. Not much happens with spending within existing contracts. These results are robust to using alternative specifications for shocks to the salience of audit risk. Drawing on variation in the distance to audits in the previous year generates exactly the same patterns. Consistently with the salience mechanism, that specification yields even larger effect sizes.

As a robustness exercise, we also rely on unique data from the anti-corruption program's audit reports for all Brazilian municipalities, at the investigation level, within Health transfers for the 2003-2007 period. Even though audit reports do not provide contract-level data that would allow us to explore the same empirical strategy, we can analyze the effects of audits within transfers of different "procurement intensities"; after all, the result that the effects of past audits on spending are concentrated in new contracts suggests audit risk is particularly key at the procurement stage. Although audit reports do not convey information on budget execution, we can use the amounts reported by auditors as a proxy for public spending within transfers audited by the program. We find that the effects of audits on spending are concentrated precisely on programs with *high procurement intensity*. Spending falls by as much as 35% more within procurement-intensive programs as a result of audits within 75km in the previous year – consistent with the effects documented using data from the São Paulo's State Court of Accounts. In contrast, recent nearby audits do not systematically affect spending within programs with a low share of actions related to procurement.

Do government audits also displace corruption? Using the same empirical strategy, we find that whenever the bureaucrat (but not the vendor) experiences past audits, over-invoicing decreases significantly, in line with Avis, Ferraz and Finan (2018). However, whenever the vendor (but not the bureaucrat) experiences a recent audit nearby, over-invoicing in contracts elsewhere significantly *increases*. Effect sizes are large: the probability of over-invoicing decreases by at least 29% in the former case, and increases by at least 47% in the latter.

We can estimate the net effect of the program on corruption by computing the linear combination of those partial effects, weighted by the share of contracts of each municipality-vendor configuration. Ignoring displacement effects, one would have concluded that the probability of over-invoicing within federal and

[4] Since being independently drawn to be audit represents a very small share of contracts in our dataset.

State transfers decreases roughly by 6% after past audits. In contrast, accounting for spillovers suggests that past audits actually *increase* such probability, by 0.65%. When it comes to the intensive margin, ignoring displacement effects would have led one to conclude that past audits decrease the amount over-invoiced by *almost 90%* within programs funded by municipality's own resources. Accounting for spillovers, in turn, suggests past audits decrease that amount by less than 22% – and their effects are *no longer statistically significant*. Are those conclusions specific to over-invoicing? We find that the effects of audits on corruption using data from audit reports, which include many other irregularities – from falsified signatures to off-the-record payments, besides over-invoicing –, are driven by investigations within programs with high procurement intensity, precisely those for which spending decreases the most in the aftermath of recent nearby audits. By the same token, audits do not significantly decrease corruption within programs with low procurement intensity, for which they also do not significantly decrease spending. Last, effect sizes are strikingly similar across datasets, even though audit reports capture a much broader definition of corruption.

Given the richness of our data, we can say more about the nature of displacement by vendors. Where do vendors move to in the aftermath of an audit? We explore heterogeneous treatment effects of past audits by characteristics of vendors' networks in 2010 – the first year for which we have data on government contracts – to study that question. While vendors are no more likely to move to municipalities where they were active at baseline, budget execution in those contracts matters: vendors are significantly *more likely* to move to municipalities where they had either *smaller contracts* or contracts facing *lower budget execution* at baseline. That is the case even though they are able to *over-invoice lower amounts* within those contracts. They are also *more likely* to move to municipalities *saturated with vendors* at baseline – where there may be more opportunities to help moving things forward, but where higher supply-side competition enables them to *embezzle to a much lesser extent*. This pattern is consistent with our model, in which corruption emerges from vendors helping bureaucrats handle costly procurement. Static inefficiencies, such as fixed costs of building relationships in new municipalities, can explain why vendors were not present in those other locations in the first place: even though vendors pocket a share of the budget in those new contracts, they actually end up with a worse bottom line. In fact, Lagaras, Ponticelli and Tsoutsoura (2017) document that vendors' growth rate and survival probability are actually hurt in the short-run when audits take place in municipalities where vendors are active. Consistent with those findings, we find that embezzlement in vendors' marginal contract following a recent nearby audit is *significantly lower* than the average amount in contracts characterized by over-invoicing.

Is displacement by vendors welfare-neutral – i.e.: does it just move resources around –, or is the disruption it brings about consequential? We study this question in two ways. First, we look at implementation quality, exploring detailed accounts of mismanagement problems in audit reports (from infrastructure problems, to incomplete documentation, to performance problems indicated by citizens' complaints). We find that past audits increase performance problems – longer queues and waiting times, and citizens' complaints about the quality of health services or health facilities. Second, we look at whether audits make health indicators worse. We contrast outputs and outcomes, such as hospital beds and immunization coverage – which are tied to procurement-intensive programs –, on the one hand, to outcomes such as the population share covered by family doctors and medical consultations per thousand inhabitants – which are tied to programs that are not procurement-intensive –, on the other. We find that the occurrence of audits within 75km in the previous year has a negative effect on all procurement-intensive health outputs and outcomes, particularly for immunization coverage and hospital beds per 1,000 inhabitants. What is more, audits within 75km in previous years *increase* municipal mortality rates – significantly so for the effect of audits in the previous year on preventable deaths, and marginally so for the combined effects of audits in the previous two years on child mortality.

Last, why do bureaucrats distort quantity and quality as a response to higher perceived audit risk? On the one hand, they may do so because corruption "greases the wheels" of public service delivery (Banerjee, Hanna and Mullainathan, 2012): procurement generates embezzlement opportunities. When there is less scope for embezzling public funds, incentives for bearing the costs of procurement fade out. On the other hand, they may do so because of procurement risk: the probability of being framed and punished as corrupt for procedural mistakes discourages procurement. We take advantage of randomly assigned capacity-building trainings that took place as part of the program "Strengthening Public Management" (*Fortalecimento da Gestão Pública*, or FGP, also ran by the Office of the Comptroller General) between 2006 and 2014, to study that question. We find that invoicing and billing mistakes *decrease without decreasing spending* in new contracts involving bureaucrats who experienced capacity-building trainings, consistent with lower procurement risk in the model. What is more, vendors are systematically *less able to increase spending* in new contracts with municipalities previously assigned to trainings, suggesting that bureaucrats' responses are at least partially consistent with fear of being labeled corrupt for procurement mistakes. Having said that, "greasing the wheels" seems to also be at play: while past audits decrease over-invoicing in new contracts, they *significantly increase it* in existing contracts – disproportionately among those randomly drawn to receive capacity-building training. Even locally, corruption seems to be displaced: towards contracts identified by trained bureaucrats as involving lower audit risk.

While previous research points out that audits can backfire due to displacement across different types of contracts (Gerardino, Litschig and Pomeranz, 2017) or different bureaucratic margins (Shin, 2008; Yang, 2008; Rasul and Rogger, 2015), this paper provides first-hand evidence of a new mechanism: geographic displacement by vendors responding to lower spending by local bureaucrats, who react to past audits motivated by both "greasing the wheels" and procurement risk. We show that exposing corruption may not only just drive it elsewhere, but also be detrimental to public service delivery. While the Brazilian anti-corruption program represents a major improvement in monitoring and transparency, the focus of the justice system on administrative and criminal penalties to bureaucrats, rather than on criminal penalties to vendors, and that of public opinion on corruption, instead of on the public service provision, seem to have thrown the baby out with the bathwater.

Results suggest that punishing vendors for corrupting bureaucrats, differentiating between active and passive waste (Bandiera, Pratt and Valetti, 2009), expanding the scope of desirable outcomes beyond formal procedures, and supporting local procurement staff in complying with complex guidelines might be important steps towards balancing incentives between procuring, on the one hand, and making proper use of public funds, on the other.

2 Relation to the literature

Several papers have taken advantage of the Brazilian anti-corruption program to study important issues in Public Economics and Political Economy. Ferraz and Finan (2008) exploit variation induced by the corruption program to study whether voters punish corrupt politicians. By comparing municipalities equally corrupt – according to audit reports – that had reports publicized by the media right before or right after elections (only because of the randomness of the program's lotteries), they can estimate the effect of information on reelection rates. Ferraz and Finan (2011) rely on the same program to answer a different question: can electoral institutions reduce corruption? Through a regression discontinuity design, they document that second-term mayors are corrupt to a greater extent than first-term mayors. Ferraz, Finan and Moreira (2012) analyze the cross-sectional correlation between the incidence of corruption measured by the program's audit reports and educational outcomes, finding a negative association between missing government funds and students' achievement.

Avis, Ferraz and Finan (2018) estimate the effect of past audits on corruption. We extend their work in two ways. First, we document the effects of audits on public spending, and estimate their effects on corruption accounting for spillovers driven by vendors' behavior. Second, we study the nature of displacement by vendors, analyzing heterogeneous effects of audits according to vendors' baseline network characteristics, and studying the mechanisms that lead bureaucrats to decrease spending in the aftermath of audits.

Our findings are consistent with other papers that show that external monitoring can backfire. Yang (2008) documents that enforcement backfires in the case of customs reform in Philippines: narrowly focusing on a specific method of avoiding import duties, enforcement generated displacement; the author cannot reject the hypothesis of zero change in total duty avoidance as a result. Gerardino, Litschig and

Pomeranz (2017) study a procurement audits program in Chile, documenting that audits discourage the use of complex administrative procedures with more rules for auditors to check. What we document is, however, a *new mechanism*: we do not focus on whether audits displace corruption locally across different types of contracts (even though we do show this to take place among trained bureaucrats), nor whether they displace corruption locally across different bureaucratic margins; instead, we study whether audits *displace corruption across space*, an effect driven by vendors responding to both "greasing the wheels" and procurement risk.

Our findings are also consistent with Meckel (2017)'s results for how private vendors respond to changes in incentives arising from higher enforcement of public monitoring. In her setting, vendors are less willing to cater to Government programs once an electronic payment system is introduced, reducing the scope for skimming off the top. In our setting, higher *perceived* enforcement decreases vendors participation locally, driving corruption elsewhere, where bureaucrats are still willing to rely on their help for setting up costly procurement. In both settings, local supply is affected, with negative welfare consequences for consumers (higher prices, in the former, and worse outputs and outcomes, in the latter).

Lagaras, Ponticelli and Tsoutsoura (2017) estimate the effects *on firms* of being exposed as corrupt by the Brazilian random-audits program. Our results help to rationalize their findings: negative short-term impacts of audits on firms exposed to be corrupt are consistent with lower spending in local contracts, and by relationship-building costs of setting up shop elsewhere. In turn, Colonneli and Prem (2017) investigate the long-term effects of being audited for firms. In contrast to short-run effects, public sector vendors that experience audits *grow faster* five years later. All in all, those papers are consistent with our findings about audits driving vendors towards increased sales and corruption elsewhere, but with *significantly lower* embezzlement in vendors' marginal contracts (at least in the short-run). Together, the evidence supports the argument of static inefficiencies: fixed costs of entering new markets prevent vendors from being corrupt in other settings, until local crackdowns push them to pay those fixed costs – with short-run losses converted into long-run gains for those vendors that survive in the long-run.

Our results for the mechanisms driving bureaucrats' responses link to a large literature. There are at least two theoretical reasons for why exposing corruption might be detrimental to efficiency within a bureaucratic politics' setting. First, under limited liability, the optimal principal-agent contract must leave rents to the agent (Bolton and Dewatripont, 2004). If such rents are reduced, then incentives must become less powerful (so that the participation constraint is still satisfied). This is what Banerjee, Hanna and Mullainathan (2012) label as *greasing the wheels*. Second, because it is hard to separate corruption from discretion (Bandiera, Pratt and Valleti, 2009; Huntington, 1968; Leys, 1965; Leff, 1964), discouraging the former often discourages the latter. This is what we label as *procurement risk*. This paper provides first-hand evidence for these mechanisms. We show that higher perceived audit risk induces plummeting public spending, and our findings suggest that bureaucrats' responses are consistent with both "greasing the wheels" and fear of being labeled corrupt for procurement mistakes.

Last, our attempt at understanding whether displacement effects are consequential by looking at whether audits affect local health indicators maps into a growing literature investigating the causal effects of corruption. While corruption is conjectured to have high social costs, from static resource misallocation to inefficient investment in factors of production for which returns are seized (Rose-Ackerman, 1997), there is limited evidence of its effects (Banerjee, Hanna and Mullainathan, 2013; Pande, 2007).[5] On the one hand, corruption has been documented to pose a major obstacle to the decentralization of public service provision in developing countries, with embezzlement levels sometimes higher than the amount that actually reaches targeted individuals (Olken, 2006), and sometimes even reversing the progressivity of public expenditures (Reinikka and Svensson, 2004). On the other hand, whether it actually induces inefficient outcomes or rather represents transfers to bureaucrats, which "grease the wheels" of public service delivery, remains largely an open question.[6] Since corruption captures resources from public goods' provision, as Jim Kim puts it in the opening quote, monitoring is expected to increase resources towards public services. However, rent capture is only one dimension of bureaucratic performance.[7] Exposing corruption may reduce incentives for bureaucrats to exert effort in other dimensions, such as providing the optimal quantity and quality of public goods (Leff, 1964; Leys, 1965; Huntington, 1968; Banerjee, Hanna and Mullainathan, 2012). If that is the case, monitoring could actually hurt public service delivery, as the opening quote by Colin Leys hints at.

Since it is hard to obtain experimental variation in corruption, researchers have resorted to two alternative approaches. First, mechanism experiments (Ludwig, Kling and Mullainathan, 2012) that identify the effects of corruption in narrow settings in which it can be experimentally varied (e.g., Zamboni and Litschig, 2018; Bertrand et al., 2007). The advantage of this approach is control: the variation is randomly assigned and tightly linked to the mechanism of interest. Its disadvantage is external validity: it is unclear to what extent results in these narrow settings would carry over to the effects of corruption in society at large. Second, natural experiments that exploit policy changes (e.g., Reinikka and Svensson, 2005; DiTella and Schargrodsky, 2003). The advantage of this approach is external validity: this is exactly the variation that one is interested in when thinking about the effects of corruption. Its disadvantages are manifold. In particular, anti-corruption policies usually affect everyone, such that counterfactual analysis must rely on

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[5] Ferraz, Finan and Moreira (2012), Méon and Weil (2010), and Méon and Sekkat (2005) present cross-sectional evidence on the relationship between corruption and efficiency.
[6] For a theoretical perspective, see Shleifer and Vishny (1993) and Banerjee, Hanna and Mullainathan (2012).
[7] See Finan, Pande and Olken (2016).

strong assumptions. Furthermore, because corruption is observed only after the program kicks-off, it is often inferred from other variables (such as prices) that include variation unrelated to the mechanism of interest.

This paper exploits variation that is both internally and externally valid. By leveraging on a national anti-corruption program, we take advantage of a shock that affects corruption in society at large. Since the program is based on random audits, we can estimate effects of audits on local public service delivery. That allows us to extend the findings of Olken (2007), Bobonis, Fuertes and Schwabe (2016) and Avis, Ferraz and Finan (2018), which document the effects of monitoring on corruption, but not on public service delivery.

Interestingly, displacement by vendors may be a key mediator of the effects of monitoring strategies on public service delivery. Our findings for local negative effects are at odds with those of Reinikka and Svensson (2005), which document sizable positive effects of a newspaper campaign in Uganda that disclosed official figures about the central government's transfers to local primary schools. A key potential reason for this difference is that Reinikka and Svensson (2005) consider a single transfer that is delivered directly to schools, hence, not subject to displacement by vendors in the presence of lower monitoring costs. In turn, we consider funds managed by local bureaucrats to procure goods and services, subject to a complex set of procurement guidelines. Different from just channeling resources to end users, bureaucrats have to manage complex relationships with vendors, and must decide across several margins. This is the typical budget implementation process in developing countries, and the effects that we find are consistent with those of bureaucratic rigidity (Bertrand et al., 2016, which finds substantial negative effects of distorted incentives in terms of GDP growth) and with those of monitoring bureaucratic performance (Rasul and Rogger, 2015; Shin, 2008). Along those lines, our findings are also consistent with Best, Hjort and Szakonyi (2017)'s result that bureaucratic effectiveness may be key to the welfare effect of other State programs.[8]

3 The Brazilian anti-corruption program

The Brazilian anti-corruption program (*Programa de Sorteios Públicos*) is a joint venture between the Office of the Comptroller General (*Controladoria Geral da União*, CGU henceforth) and the national lottery. It is based on periodic televised random draws that select municipalities to be audited by CGU officials. Auditors analyze municipalities' accounts and documentation, and physically inspect public works and service delivery, to assess whether earmarked federal transfers are effectively spent according

[8] They find a policy of bid preferences for Russian firms to have opposite impacts depending on the quality of the procurement staff.

to their guidelines. CGU's auditors also interact with local councils and civil society organizations to empower local citizens in playing an effective role in monitoring the use of tax revenues.

The program was announced in January 2003. Four municipalities were audited as a pilot in February; this was followed by the announcement, in March, of the municipalities selected in the first draw to be audited in April. There was no decree or media announcement of the program prior to 2003.[9] While other overseeing institutions were already in place prior to the introduction of the Brazilian anti-corruption program, its announcement represented not only a substantial increase in the probability of municipalities being audited systematically, but also on the intensity of electoral damages of being exposed as corrupt (Seabra, 2018). Summary audit reports are systematically broadcasted on the internet, newspapers, television, and radio, and there is evidence that voters punish corrupt mayors exposed by the program (Ferraz and Finan, 2008).[10]

Beyond electoral punishment, a number of administrative penalties were applied based on CGU audit reports, and even Federal Police operations were triggered by evidence put forward by the anti-corruption program. To date, conviction for involvement in procurement irregularities exposed by the program is one of the very few reasons for which one can lose tenure at the public sector. Furthermore, public servants convicted for corruption have to repay the embezzled amount out of their own pockets or go to prison.

To date, the anti-corruption program is still the most important corruption-deterrence mechanism in Brazil, and the fact that several political attempts have been made to terminate it, to decrease the number of audited municipalities, or to increase the time span between draws is testimony to its first-order effects on the Brazilian bureaucratic politics' status quo.

4 A simple model

This section discusses a model of bureaucratic decisions. The goal is to illustrate the relevant trade-offs faced by local procurement staff in their interaction with vendors, and to provide predictions for the effects of a higher perceived audit probability on different margins of bureaucrats' and vendors' action space.

Consider a risk-neutral bureaucrat (B) deciding whether to use public funds transferred by the federal government to procure or not. Procurement involves an effort cost (c_B), and entails a benefit (G) which can be thought of as personal utility or managerial rewards for providing public services. If she does not procure, there is no further action. If she does procure, she can decide whether to be corrupt or not. Corruption entails sharing the surplus (π) of the contractual relationship with a risk-neutral vendor (V), with the share of the

[9] See Appendix B for more details about the program.
[10] Pande (2011) documents that information experiments lead voters to punish corrupt politicians more broadly.

surplus allocated to the bureaucrat (α) determined by an exogenous process, such as Nash bargaining. If she is not corrupt, then N identical vendors compete for the surplus (the expected surplus for each bidder is $\frac{\pi}{N}$), and the bureaucrat gets no share of it.

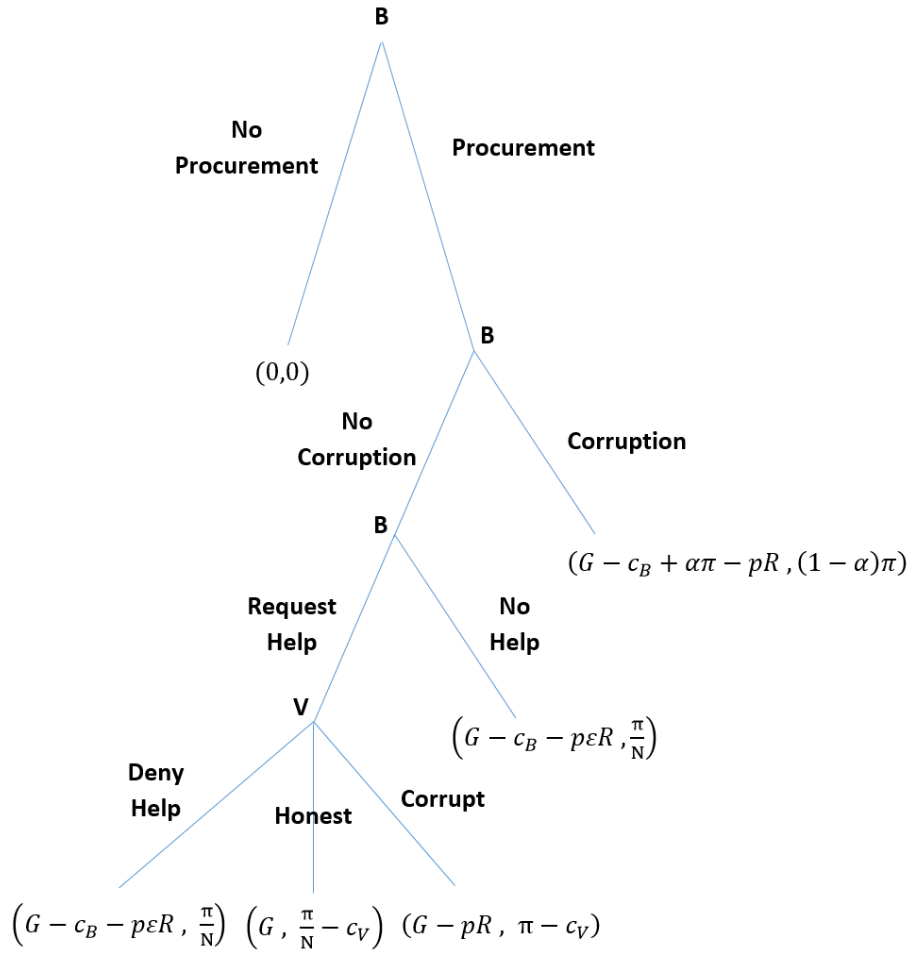
The probability of being audited and being found corrupt is p. If exposed as corrupt, the bureaucrat faces punishment R, which can be thought of as an administrative or reputational penalty. If the bureaucrat does not procure, being audited implies no punishment. If she procures and is corrupt, the expected punishment is pR. Even if she is not corrupt, however, it is still possible that she faces a penalty, as procurement mistakes (which happen with probability ε) are also coded as corruption by auditors – even when bureaucrats take no share of the surplus. In the model, vendors are not punished for corruption.[11]

In face of effort costs and procurement risk, the bureaucrat can randomly draw one of the vendors to help her design the procurement process. Since vendors are more sophisticated, we assume they incur accidental procurement mistakes with probability zero.[12] If the bureaucrat requests help, the vendor can always deny it, in which case the expected outcome is the same as that when the bureaucrat does not ask for help. Conversely, if he helps, it is the vendor, rather than the bureaucrat, who faces effort costs (c_V). In that case, the vendor can be either honest, in which case he still competes with other N-1 vendors for the surplus, or corrupt, in which case he distorts the procurement process such that he is sure to win and capture the whole contractual surplus.

[11] The program focuses on bureaucrats rather than vendors (Seabra, 2018). Having said that, this assumption could be relaxed without changing results to the extent that punishment for vendors does not increase their marginal costs so much that it to completely offsets incentives to support additional bureaucrats elsewhere in face of a higher perceived audit probability.

[12] Again, this assumption could be relaxed without affecting results as long as such probability is not so high that it offsets incentives to support additional bureaucrats elsewhere in face of a higher perceived audit probability.

Figure – Bureaucrats' and Vendors' payoffs from procurement and corruption decisions



We assume that $c_B < R < G < R + c_B$, i.e.: on the absence of reputational penalties or procurement costs, procurement would be socially optimal. Also, let c_V be the unit cost for the vendor for helping in one additional contract, and let vendor's total cost across all contracts be given by $M^2/2$, where M is the number of contracts v helps with. Last, let $\pi \sim F(\,)$, continuous and twice differentiable.

This simple model illustrates the relevant trade-offs (all proofs are deferred to Appendix D). Upon procuring, B is corrupt if $\alpha\pi \geq \max\{c_B\,,\ (1-\varepsilon)pR\}$, that is, if her share of the contractual surplus is high enough, and if the expected punishment in excess of that coming from the possibility of procurement mistakes is not too high. **An increase in the perceived probability of being exposed as corrupt, p, decreases the likelihood that bureaucrats engage in corruption.**

In what comes to vendors, honestly helping the bureaucrat is a dominated strategy in this game. If B is not corrupt, she requests V's help if $c_B \geq (1-\varepsilon)pR$, that is, if the costs of procurement for the bureaucrat (accounting for the probability of procurement mistakes) are so high that they compensate for the certainty of being exposed as corrupt in case V helps and B is randomly drawn to be audited.

V helps and is corrupt if $\frac{(N-1)}{N}\pi \geq M$, that is, if the expected net benefit of supporting bureaucrats in an additional procurement process is greater than his payoff under the fair odds of bidding in an honest tender. We assume that vendors choose M to maximize profits for a given p, such that $M^* = \frac{(N-1)}{N}\pi$. Hence, when perceived audit risk increases, vendors are unwilling to help the local bureaucrat in additional contracts, given the optimality condition for M^* – one of the drivers behind bureaucrats' decision to decrease spending.

B always procures if V helps. If V does not help, B procures if $\pi \geq \frac{1}{\alpha}(pR + c_B - G)$ when B is corrupt, or if $\varepsilon \leq \frac{G-c_B}{pR}$ otherwise. In sum, B procures if the expected costs of procurement – which include punishment from being exposed as corrupt (having embezzled resources or not) – are not higher than the expected benefits of public service provision. **An increase in p decreases the likelihood that bureaucrats engage in procurement.**

Notice that lower public spending stems from two sources: on the one hand, from the lower expected payoff of procurement when B is corrupt ($G - c_B + \alpha\pi - pR$; this is what we call "greasing the wheels") and, on the other hand, from the lower expected payoff of procurement when B is not corrupt (which always involve the negative term $-p\varepsilon R$; this is what we call "procurement risk").

Given lower spending in local contracts (decreasing M below M^*), the optimality condition kicks in to induce vendors to offer help to bureaucrats elsewhere. Wherever p has remained constant, local bureaucrats will still be willing to ask for that vendor's help. Hence, **an increase in the perceived probability of being exposed as corrupt in a given locality increases the likelihood that vendors engage in procurement and corruption elsewhere.**

Last, we show that lower ε maps into a higher likelihood of procurement, since now there is a lower probability of being punished for corruption when the bureaucrat would actually prefer not to embezzle. It also decreases the likelihood that that the bureaucrat asks for vendor's help. As a corollary, analogously to the effects of a higher perceived audit probability, lower ε is expected to increase spending and corruption in other contracts elsewhere, since vendors still have to equate marginal costs and revenues. In sum, **a decrease in the probability of procurement increases the likelihood that bureaucrats engage in procurement, and increases the likelihood that vendors support procurement and engage in corruption elsewhere**.

While the model certainly abstracts from many relevant features from the complex interactions between bureaucrats and vendors, its minimal setup is enough to generate sharp predictions that can be taken to the data. This is what we do in the following sections.

5 Do government audits displace public spending?

Budget execution in Brazil is notably low, and particularly so when it comes to federal transfers to municipalities – the object of the anti-corruption program. As a striking example, budget execution was around 10% for the Growth Acceleration Program in Health (*PAC Saúde*, a federal program implemented through voluntary transfers to municipalities). Such low spending is obviously not driven by the lack of need – Brazil fares among the worst Latin American countries across several health indicators –; conversely, it has been blamed chiefly on municipalities' inability to streamline procurement procedures.[13] In this section, we test the hypothesis that audits make this problem worse locally, by decreasing spending in contracts for which bureaucrats experience past audits, while increasing spending in contracts for which vendors experience past audits elsewhere, in line with the predictions of the model.

This section first introduces our unique datasets in subsection 5.1. Subsection 5.2 presents our empirical strategy to estimate whether budget execution responds to past audits among bureaucrats and vendors, according to model's predictions, followed by results in subsection 5.3. Subsection 5.4 presents robustness checks. In subsection 5.5, we discuss how to interpret our results in face of minimum spending mandates in Brazil, and tackle the issue of what happens to idle resources – a question with direct implications for welfare analysis.

5.1 Data

We draw on a novel dataset with contract-level data within the State of São Paulo, compiled by the State Court of Accounts (TCE-SP) for the 2011-2015 period. The dataset aggregates the quarterly inputs from 644 municipalities in the State (all except the State capital, audited by its own Court of Accounts), which are required by TCE-SP, for all contracts that can be audited by the Court: all expenses funded by municipality's own resources, by transfers from the State Government, or by matching municipal funds to federal transfers. It provides, for each contract, information on the municipality and the vendor, and, for each pair contract-year, the amount that was planned, and later (partially) executed, breaking down budget execution into the invoicing and payment stages. In 2003, TCE-SP launched a huge project with the goal of collecting detailed data about expenditures of local governments in São Paulo State. TCE-SP piloted the

[13] See http://www.contasabertas.com.br/website/arquivos/8134.

system in 2008 and 2009, and officially launched it in 2010 – when data-entry became mandatory. For this reason, we restricting attention to spending within contracts for the 2011-2015 period, such that we do not have to rely on pre-2010 data for computing our indicator variables of past audits, since those depend on complete information for vendors' networks.

Whenever a municipality holds multiple contracts with a vendor at any particular year, we sum the amounts across all contracts at each budget execution stage. This ensures a conservative measure of over-invoicing: it prevents mislabeling reporting mistakes on contractual add-ons as over-invoicing.[14] The downside of this decision is that it leads us to under-estimate the incidence of over-invoicing in the data, since it yields a weighted average of invoicing across (potentially) multiple contracts with each vendor, some of which could have invoiced (well) below planned amounts.

Last, we correct planned amounts for downward revisions (which are perfectly legal) to avoid over-estimating underspending when planned contracts have been cancelled or scaled down. We do not, however, correct for upward revisions in most specifications, since – as we show – those respond strategically to perceived audits risk: bureaucrats try to top-up planned amounts ex-post, after they have invoiced above planned amounts. This practice is illegal since the competitive tender specified the originally planned amount, and modifying the terms of reference could have resulted in a different outcome for the procurement process. To be clear, upward revisions are different from contractual add-ons (which we account for by summing amounts over municipality-vendor-year triples); the latter is a legal procedure to increase budgeted amounts, up to a small percentage of the amount established by the terms of reference.

Doing so, we end up with a dataset with 3,082,012 municipality-vendor-year observations. It also allows us to identify the program category linked to each contract: Health (25.4% of observations), Education (23.2%), Public Administration, i.e.: administrative spending (17%), or Other.

Such dataset has four key advantages over audits data. First, it allows us to accurately observe *public spending*, beyond planned budgeted amounts. Second, it allows us to measure corruption *objectively*, from over-invoicing – differences between invoices entered in the system and the amount budgeted for those goods and services, rather than auditors' impressions from audit reports –, in what comes to both its *extensive and intensive margins*. Third, because reporting is mandatory (and we observe perfect compliance), it allows us to measure it for *all* municipalities, not just for those that were audited. While over-invoicing in the system can be automatically recognized by State auditors, it is still the case that about

[14] For instance, if a municipality signs a contractual add-on with a vendor, it might accurately report its planned spending but accidentally bill the add-on's invoices against the pre-existing contract – which would make it look like over-invoicing in the system had we not aggregated such contracts. The data does not allow us to distinguish when multiple contracts with vendors are add-ons or not.

4.75% of contracts in our data display over-invoicing. Fourth, it allows us to observe outcomes at the contract level, allowing us to explicitly analyze the effects of the program on *bureaucrats and vendors*.

CGU can only audit voluntary transfers from the federal government to municipalities. In the TCE-SP dataset, we can observe municipal matching funds for such contracts. Beyond federal transfers, there is reason to believe that past audits would affect bureaucrats' behavior in contracts funded by other transfers, or by municipality's tax revenues. The reason is that, since many federal police operations have been triggered by the random-audits program (Avis, Ferraz and Finan, 2018), it could still be rational for bureaucrats and vendors to react to higher perceived audit risk even in contracts that are not, in principle, eligible to be audited by the program. Alternatively, CGU audits may increase the salience of TCE-SP audits, by the same token that nearby audits seem to increase the perceived probability of being audited by the program (Avis, Ferraz and Finan, 2018).

The TCE-SP dataset has three main drawbacks. First, comprising contracts only from the State of São Paulo; second, not featuring the specific object of CGU's random-audits program (only municipalities' matching funds); and, third, not allowing us to detect the many other ways corruption can take place in those contracts, particularly through means that cannot be recognized by just analyzing municipalities' entries in the system – such as off-the-record invoicing, or payments to ghost firms. To deal with all those concerns, we complement the data from the State Court of Accounts with a unique dataset assembled by Fundação Getulio Vargas' Center of Politics and Economics of the Public Sector (CEPESP-FGV/SP), based on audit reports for constitutionally mandated transfers across all Brazilian municipalities.

This dataset covers audits between 2003 and 2007; more specifically, we have data for draws 2 to 24 (draw 1 was a pilot). Each audit report was independently coded by two research assistants, who did not know what the data would be used for; disagreements were solved by assigning the report to a third research assistant. In our dataset, transfers are dated according to the auditor's record of the year the action investigated accrues to. For example, consider the audit report for Quissamã, selected in the 8th draw (March 2004). While the audit took place in 2004, auditors have looked into transfers' life cycle back to 2001. As an example, auditors note that resources transferred in December 2001, which should have been kept in a separate bank account, were rather kept in municipality's health secretariat general account. We categorize this irregularity under *documentation or accounting problems*, coded in our dataset as evidence of mismanagement.[15]

Our dataset includes 32 types of irregularities, ranging from *documentation problems* to *off-the-record invoice*. One third of these categories are coded as evidence of corruption, and the remainder 21 as evidence

[15] See Table A1 in Appendix A for the complete classification list, and Appendix C for more examples of pre- and post-program investigations described in audit reports.

of mismanagement (besides compliance, when auditors find no irregularity). We follow Ferraz and Finan (2008, 2011) in defining corruption as procurement problems, over-invoicing and off-the-record invoicing.[16] The complete classification list is included in Appendix A.

In this dataset, we focus on Health programs for two main reasons.[17] First, even though CGU auditors investigate transfers linked to several ministries, including Education, Transports, and Environment, it is only for audit reports within Health transfers (up to July 2007) that the year of each instance investigated by auditors was coded in this dataset. Second, Health has a wealth of administrative data on outputs and outcomes upon which we can draw to study the effects of the program on downstream outcomes, beyond bureaucratic decisions. Those outputs and outcomes are precisely assigned to specific programs as part of the Ministry of Health's Monitoring & Evaluation framework, what gives us precision predictions about which should be affected the most by higher perceived audit risk.

This dataset has 11,419 observations. Of those, 10,538 are linked to Health transfers for which we have coded procurement intensity, distributed across 9 health transfers and 731 municipalities, between 1997 to 2007.[18] Procurement intensity is coded as follows. First, drawing upon Health Ministry's list of actions under each transfer (*Funcional Programática*), we coded each action as procurement-related or not, based on whether its official description involves words such as "inputs", "modernization", or "acquisition".[19] Second, for each transfer, we computed the percentage of actions coded as procurement-related. In our regressions, we use an indicator variable for high procurement intensity, equal to 1 if the transfer's procurement intensity is 50% or higher, and 0 otherwise.[20] In our sample, 44.8% of investigations are linked to procurement-intensive programs.

The dataset based on CGU's audit reports has two main limitations. First, while we can also observe bureaucrat's behavior at the contract-level, there is no information on vendors that would allow analyzing the extent of spillovers. Second, there is no information on spending within those contracts. Contract-level budget execution is not recorded in audit reports. Even worse, municipal health spending is recorded by the Brazilian Dataset on Municipal Budgets (FINBRA) only for the total budget, not separately for voluntary and constitutionally mandated transfers (only the former are the object the random-audits program). To this

[16] Whenever auditors point out evidence of corruption, we restrict attention to the incumbent's extensive margin decision of whether or not to be corrupt, rather than to the intensive margin decision of how much to embezzle. The reason is that the exact embezzled amount is rarely included in audit reports. We have not tried to replicate Ferraz and Finan (2008)'s effort to proxy for this amount from the description of "missing items" in the event of corruption.
[17] We show that, for the TCE-SP dataset, Health is no different from other program categories in what comes to the effects of recent nearby audits on over-invoicing or public spending.
[18] Those are the 9 most prevalent Health programs in CEPESP's dataset, representing 92.3% of the total number of coded investigations, see Table A2 in Appendix A.
[19] See Table A3 in Appendix A for the complete list.
[20] Results are robust to the choice of the cutoff, see the Supplementary Appendix.

point, to analyze potential displacement effects of audits on spending, Avis, Ferraz and Finan (2018) rely on data from the Brazilian Institute of Applied Economic Research (IPEA), which reflect planned budget figures, rather than budget execution.

To circumvent this problem, we proxy for contract-level spending in the CGU dataset using *audited amounts* reported in audit reports. Because auditors leave CGU's headquarters with pre-set service orders, they do not have discretion over what to audit. Service orders target *critical events* within a transfer's life-cycle: proper financial management and documentation of inflows and outflows of the transfer account, all procurement processes (if any) funded by the transfer, all wage payments (if any) funded by the transfer, the conditions of storage of any goods purchased with those funds, and the quality of the services ultimately funded. Because auditors typically audit the entire paper trail linked to the particular object investigated under a service order, that means the audited amount recorded in the audit reports reflects the *cumulative outflows*. It does not necessarily reflect the amount payed – it could be the amount budgeted for a procurement process that has not taken place, or the amount invoiced for services that have not been payed. But it should reasonably approximated it, as without budget execution the transfer would not generate new critical events in its life-cycle, and hence would not increase the audited amounts recorded in audit reports. In turn, in the TCE dataset, spending is given by the amount payed, precisely recorded for each contract and year in the State Court of Accounts' dataset.

Since the weaknesses of each dataset are not shared by the other, by combining them in all analyses that follow we hope to provide convincing evidence that we can accurately capture the effects of interest. In fact, our estimates for the effects of the program on both corruption and spending are consistent both qualitatively and quantitatively across datasets.

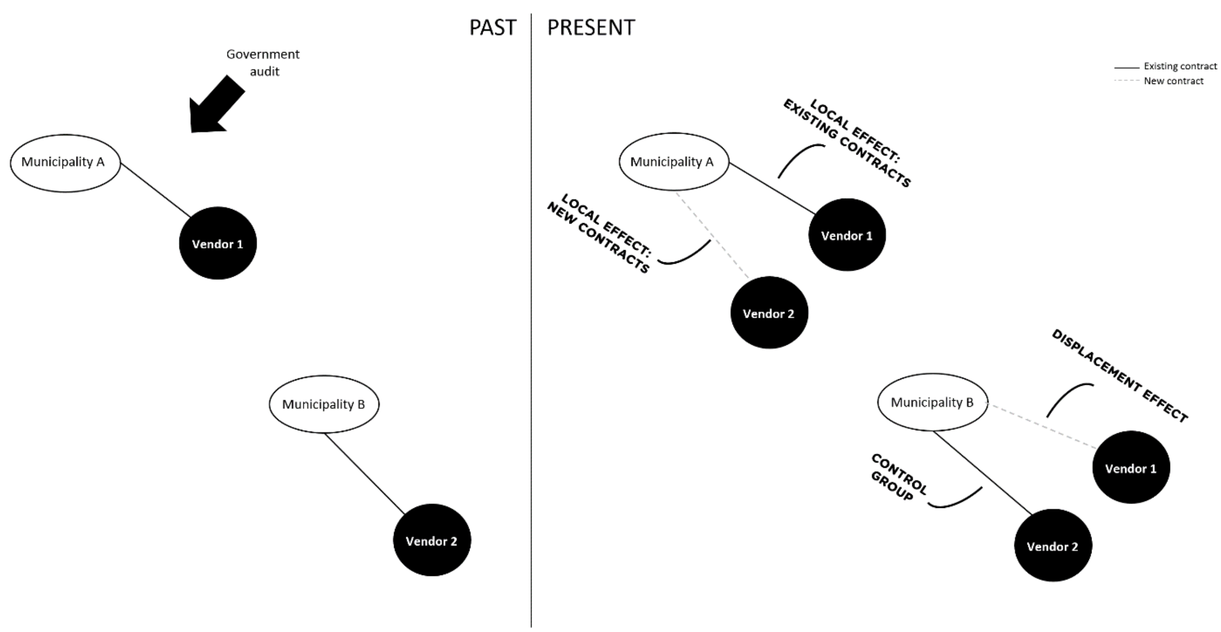
Last, we also have socio-demographic data at the municipality level from the Brazilian Institute of Geography and Statistics' 2000 and 2010 censuses; mayor characteristics and administrative attributes from the 2000 and 2009 Municipal Information Datasets (*Base de Informações Municipais*, BIM); political variables, such as turnout and political alignment with the state governor, for each political term from the Superior Electoral Tribunal (*Tribunal Superior Eleitoral*, TSE); and public finance data from the National Treasury and from the Information System for Public Health Budgets (*Sistema de Informações sobre Orçamentos Públicos em Saúde*, SIOPS).

5.2 Empirical Strategy

Following Avis, Ferraz and Finan (2018), we take advantage of past audits as a source of exogenous variation in the perceived probability of being audited in the future. To illustrate our empirical strategy for identifying the local and displacement effects of the program, consider a simple example – illustrated by the Figure – in which there are only two municipalities (A and B), two vendors (1 and 2), and two time periods (past and present). For simplicity, assume that, in the past, A only had a contract with vendor 1, and B, only with vendor 2. In the present, both A and B have active contracts with 1 and 2. Last, assume only A experienced audits in the past, at a time when it had an active contract with vendor 1.

By contrasting different configurations of past audits across different contracts (municipality-vendor pairs), we can estimate the effects of audits both locally and elsewhere. For some pairs, neither bureaucrats nor vendors have experienced past audits – this is our control group (Municipality B and Vendor 2, in the Figure). For some pairs, only bureaucrats have experienced past audits; this happens whenever vendors have moved in *only after the audit*, and did not have active contracts in *any* municipality randomly drawn to be audited in the past (Municipality A and Vendor 2, in the Figure). We can use those to estimate the *local effects* of audits *within new contracts*. For other pairs, the opposite is true: only vendors have experienced past audits; this happens whenever vendors are active across multiple municipalities and (at least) one of those – but not the municipality itself – has been drawn to be audited in the past (Municipality B and Vendor 1, in the Figure). We can use those to estimate the *displacement effects* of audits. Last, for some pairs, *both* bureaucrats and vendors have experienced past audits; either together, in existing contracts, or independently, if vendors moved in only after the local audit but had active contracts in municipalities previously randomly drawn to be audited by the program (Municipality A and Vendor 1, in the Figure). We can use those to estimate the *local effects* of audits *within existing contracts*.

Figure – Municipality-vendor configurations and identification of local and displacement effects



In the TCE-SP dataset, 28.9% of observations did not experience past audits. At the other extreme, in 24.9% of the cases both bureaucrats and vendors experienced past audits. In 44.7% only vendors had experienced them, and in 1.4%, only bureaucrats – a natural asymmetry given that vendors able to participate in public procurement in Brazil are typically active across several municipalities.

We estimate the following equation:

$$\ln(Spending_{mvjt}) = \alpha + \theta_j + \theta_t + \beta_1 PastAudits_{mt} + \beta_2 PastAudits_{vt} + \beta_3 (PastAudits_{mt} \text{ x } PastAudits_{vt}) + \sum \gamma_k X_{mt}^k + \epsilon_{mvjt} \quad (1)$$

, where $\ln(Spending_{mvjt})$ is the amount payed for the contract between municipality m and vendor t within program category j at year t (in natural logarithms); θ_j stands for program category fixed-effects (Education, Health, Transportation, Administration, and Other); θ_t stands for year fixed-effects; $PastAudits_{mt}$ equals 1 if municipality m has been audited by the program at any year before t, and 0 otherwise; $PastAudits_{vt}$ equals 1 if *any municipality* with whom v had an active contract before t was audited by the program at the time, and 0 otherwise; and X_{mt} stands for a vector of municipal-level controls. In equation (1), based on our simple model from Section 4, we expect $\beta_1 \leq 0$ and $\beta_2 \geq 0$, while $\beta_3 \geq 0$, since opposing forces are at play.

Since we only have data for vendors' networks from 2010 onwards, however, those variables are potentially measured with error: it is likely that more vendors experienced past audits than our data allows us to observe.[21] To deal with this issue, we turn to audits in the previous year – which we observe for all contracts from 2011 onwards –, as their effect on the salience of audit risk triggered by past audits should be even stronger.

Leveraging on Avis, Ferraz and Finan (2018)'s results showing that past audits in neighboring municipalities also reduce corruption, we explore variation in recent nearby audits: those taking place within 75km of municipality's centroids in the previous year (in the case of vendors, of *any* municipality with which it had an active contract at that time). This distance should approximately correspond to a one-hour drive from municipality's centroid, a timeframe typically considered as 'close vicinity' in São Paulo State. In the Supplementary Appendix, we show that results are robust to replacing this indicator variable with a continuous measure of minimum distance to audits in the previous year. Last, this specification

[21] There is also potential measurement error from vendors experiencing audits elsewhere, as we only observe contracts within the State of São Paulo. To minimize that bias, we code the indicator of past audits as missing – rather than 0 – in the year vendors appear for the first time in our dataset, leaving us with 2,755,370 observations.

allows us to include municipality fixed-effects – exploring within-municipality variation in distance to nearby audits in the previous year –, minimizing concerns with omitted municipal-level characteristics (that do not vary over time) in the specification with past audits.

Within contracts from 2011 to 2015 in the TCE-SP dataset, 31.7% of observations did not experience audits within 75km in the previous year. At the other extreme, in 40.8% of the cases both bureaucrats and vendors experienced recent nearby audits. In 20.4% only vendors had experienced them, and in 7.1%, only bureaucrats. The nice dispersion across cells – with a higher share of contracts in which bureaucrats but not vendors experience past audits – yields statistical power to detect even small effect sizes of audits on the variables of interest. In the CGU dataset, about 60% of investigations take place in municipalities that experienced audits within 75km in the previous year.

We estimate the following equation:

$$\ln(Spending_{mvjt}) = \alpha + \theta_m + \theta_j + \theta_t + \beta_1 NearbyAudits_{m,t-1} + \beta_2 NearbyAudits_{v,t-1} + \beta_3\left(NearbyAudits_{m,t-1} \text{ x } NearbyAudits_{v,t-1}\right) + \epsilon_{mvjt} \quad (2)$$

, where $\ln(Spending_{mvjt})$ is the amount payed for the contract between municipality m and vendor t within program category j at year t (in natural logarithms); θ_j stands for program category fixed-effects; θ_m and θ_t stand for municipality and year fixed-effects, respectively; $NearbyAudits_{m,t-1} = 1$ at year t if there was an audit within 75km of municipality m's centroid in the previous year (including, possibly, m itself facing an audit), and 0 otherwise; and $NearbyAudits_{v,t-1} = 1$ at year t if there was an audit within 75km of the centroid of *any municipality* with whom v had an active contract at the time, and 0 otherwise. Once again, we expect $\beta_1 \leq 0$ and $\beta_2 \geq 0$, while $\beta_3 \geq 0$.

Last, in what comes to data from audit reports, we interact the effect of audits within 75km in the previous year with the indicator of procurement-intensity computed for each transfer. The reason is that the predictions of our model apply to procurement contracts; in particular, it might not be the case that employees are as mobile as vendors when reacting to changes in perceived audit risk. If that is the case, then contracts involving little or no procurement would provide an interesting counterfactual for the effects of changes in perceived audit risk on bureaucrats' and vendors' behavior. We estimate the following equation:

$$\ln(Spending_{mit}) = \alpha + \theta_m + \theta_t + \beta\ NearbyAudits_{m,t-1}\ x\ ProcIntensive_{mit} + \delta\ NearbyAudits_{m,t-1} + \gamma ProcIntensive_{mit} + \epsilon_{mit} \quad (3)$$

, where $\ln(Spending_{mvjt})$ is the amount payed for the contract between municipality m and vendor t within program category j at year t (in natural logarithms); θ_m and θ_t stand for municipality and year fixed-effects, respectively; $NearbyAudits_{m,t-1} = 1$ at year t if there was an audit within 75km of municipality m's centroid in the previous year (including, possibly, m itself facing an audit), and 0 otherwise; and $ProcIntensive_{mit}$ equals 1 for procurement-intensive programs, and 0 otherwise.

There are two forces expected to drive $\beta \leq 0$. First, bureaucrats in municipalities experiencing nearby audits are expected to be procure to a lesser extent ($\beta_1 \leq 0$ using the notation of equations 1 and 2). Second, vendors are expected to displace spending to municipalities in the control group ($\beta_2 \geq 0$).

In all cases, we estimate OLS regressions, clustering standard errors at the municipality and at the vendor levels.

5.3 Results

We start by describing our datasets. Tables F.1 and F.2 displays balance tests for past audits and recent nearby audits in the TCE dataset, respectively, comprising 644 municipalities in São Paulo for the 2011-15 period. The analysis is undertaken at the contract level, assigning to each observation pre-determined municipal characteristics.

In what comes to variables for which we can observe variation over time, for municipalities of contracts in the control group, 19.5% of mayors were serving second term – i.e., most face higher-powered electoral incentives at the time spending and corruption decisions take place. On the other hand, those have typically been elected by high margins (28.9%) in high-turnout election (85%). Within contracts in the control group, the lions' share of mayors are male (87.7%) and with high-school education or higher (88.9%). There are some statistically significant differences between treatment and control contracts (Table F.1), without conditioning on anything else: municipalities audited in the past have a higher likelihood of being male, and of having current incumbents elected by higher electoral margins, although in lower turnout elections. Also, in those municipalities, current incumbents have a lower likelihood of being from the same political party as the State governor.

[Table F.1]

Table F.2, which tests for differences in covariates across contracts for which bureaucrats have experienced audits within 75km in the past year or not, documents that controlling for municipality and year fixed effects makes any of the afore-mentioned differences statistically insignificant.

[Table F.2]

We replicate the same analysis for the CGU dataset, comprising municipalities from all Brazil for the 2003-2007 period (Table F.3). As discussed, on average, municipalities that experience audits within 75km in the previous year are 1km closer to São Paulo, the State capital. Proximity to the administrative center positively correlates with per capita income (over 60% higher) and population size (nearly 2% higher). While those differences are statistically significant, including municipality fixed effects should account for any confounding factors arising from differences in fixed characteristics.

[Table F.3]

Next, the high figures for participation of federal and State transfers in municipalities' revenue (81.5% for municipalities of contracts in the control group) highlight how decentralization in Brazil works: while municipalities have been delegated large responsibilities – within Health, all primary care, including handling emergencies –, they very seldom have own tax revenues to fund the spending required to sustain such activities. Being so dependent on transfers from other governmental entities (and mainly the federal government, which funds 95.9% of transfers for municipalities of contracts in the control group), this is ultimately the reason for why the Brazilian anti-corruption program became such a central element of this engine, hoping to limit local bureaucrats' moral hazard in handling those transfers.

For contracts in the control group, municipalities have on average only 3.5% of its formal employment concentrated in the public sector, a very low figure in comparison to most other Brazilian States, which reflects the fact that São Paulo is the country's economic powerhouse. Even in that case, only 60% of contracts in the control group are from municipalities equipped with radio stations, and the figure is even smaller for internet access (19.3%); both point to the conclusion that distance should still be a relevant factor at play for information diffusion.

Since there are some statistically significant differences in variables for which we observe variation over time even when controlling for municipality and year fixed-effects – in particular, municipalities of contracts in the control group tend to have a higher share of mayors serving second term, elected by a slightly lower electoral margin, and significantly less likely to be affiliated to the Workers' Party (PT) –, we control for those variables across all our regressions.

Table 1 displays the results of our analysis for the effects of past audits on public spending. Columns (1) and (2) display the effects within contracts funded by federal and State transfers, while columns (3) and

(4) do so for those contracts funded by municipalities' own resources. Columns (2) and (4) restrict attention to the sub-sample of contracts characterized by over-invoicing (about 2% of the total). All columns include all municipal-level controls, program category and year fixed-effects, and cluster standard errors at the municipality and at the vendor levels.

[Table 1]

Results are as follows. When only bureaucrats experience past audits, spending in contracts funded by transfers (Column 1) falls by 12.6%, an effect statistically significant at the 5% level. The effect of past audits on spending is even larger for contracts funded by municipality's own resources (Column 3): estimated at 43.4%, significant at the 1% level. In contrast, in contracts for which only vendors experience past audits, spending substantially *increases*: 23.0%, for contracts funded by transfers, and 8.6% for those funded by municipality's own resources, significantly at the 1% and 5% levels, respectively. The asymmetric responses of each contracting party line up with the predictions of our model. In contracts for which both bureaucrats and vendors experience past audits, the effect of past audits on bureaucrats is at least partially reversed, also in line with the model's predictions. The net effect of past audits is still to *decrease* spending within contracts funded by transfers (by 9.1% , significant at the 5% level), but not within contracts funded by own resources (a small increase, not statistically significant) – an important difference across funding sources in what comes to the effects of higher perceived audit risk on spending *within existing contracts*.

Columns 2 and 4 showcase an interesting phenomenon, by restricting attention to contracts that display over-invoicing. Such contracts are characterized by substantially larger sums, about 5-fold the amount payed within the average contract. First, in contracts that display over-invoicing for which only bureaucrats experience past audits, spending *falls to an even greater extent*: effects sizes are 47.4%, for contracts funded by transfers, and 89.2% for those funded by own resources, both statistically significant at the 1% level. Not much happens within existing contracts, regardless of the funding source. In contrast, spending within vendors' new contracts that display over-invoicing in the aftermath of audits is *systematically lower* (10.8% within contracts funded by transfers, significant at the 10% level, and 5.7% but insignificant for those funded by own resources). Such pattern would be equivalent to entry costs for corruption in new municipalities, and helps rationalize why vendors were not active in those locations in the first place. Importantly, in face of these results, when we analyze whether corruption is displaced by audits alongside spending we document their effects on its extensive and intensive margins separately, as the patterns for spending suggest that those might move in different directions.

Next, Table 2 considers the effects of recent nearby audits on public spending. As mentioned, under this strategy, measurement error in past audits should be much less of a problem, and higher variation in treatment status over time enables us to explore within-municipality variation, absorbing municipality fixed-effects. Columns (1), (2) and (3) display the effects within contracts funded by federal and State transfers, while columns (4), (5) and (6) do so for those contracts funded by municipalities' own resources. Columns (2) and (5) control for municipal-level characteristics. Columns (3) and (6) restrict attention to the sub-sample of contracts characterized by over-invoicing. All columns include program category, municipality and year fixed-effects, and cluster standard errors at the municipality and at the vendor levels.

[Table 2]

Results are as follows. When only bureaucrats experience audits within 75km in the previous year, spending in contracts funded by transfers (Column 1) falls by 18.7%, an effect statistically significant at the 1% level. The effect of recent nearby audits on spending is even larger for contracts funded by municipality's own resources (Column 4): estimated at 37.6%, also significant at the 1% level. In contrast, when only vendors experience audits within 75km in the previous year, spending substantially increases: 44.6%, for contracts funded by transfers, and 34.4% for those funded by municipality's own resources, both significantly at the 1% level. When both bureaucrats *and* vendors experience audits within 75km in the previous year, the net effect is a *decrease* in spending of 18.6% and 11.6%, respectively for programs funded by transfers and own resources. Point estimates barely affected by adding controls (Columns 2 and 5). Restricting attention to contracts with over-invoicing (Columns 3 and 6) yields very similar results to Table 1: larger effect sizes than average for the effects of audits on spending when they affect bureaucrats only (55.9% for contracts funded by transfers, and 78.6% for those funded by own resources), but no significant effect when they only affect vendors. Within existing contracts funded by transfers characterized by over-invoicing, past audits actually increase spending (significantly at the 5% level), consistent with the mechanism that incentives for procurement depend on embezzlement opportunities. Effects are qualitatively identical to those in Table 1, but effect sizes are larger, consistent a salience mechanism for perceived audit risk in the aftermath of audits.

Next, we turn to the CGU dataset to investigate whether recent nearby audits decrease spending (proxied by audited amounts recorded in audit reports) to a greater extent within contracts funded by procurement-intensive programs. In Table 3, column (1) considers actions that are object of auditors' investigations occurring only between 2003 and 2007, after the program had been announced; columns (2) and (3), those occurring for the whole period for which we have data (1997-2007); the latter includes municipal-level controls. All columns include municipality and year fixed-effects, and cluster standard errors at the municipality level.

[Table 3]

Results are as follows. Procurement-intensive programs tend to be associated with significantly lower spending, since the lions' share of spending funded by the transfers audited by CGU tends to be concentrated on wage payments. Having said that, audits within 75km in the previous year only have statistically significant effects (at the 1% level) on spending exactly within procurement-intensive programs. Audits decrease spending by about 35% within those programs (stable across different specifications), of the same order of magnitude as that documented for the effect of recent nearby audits on bureaucrats using the TCE dataset, within procurement contracts for the State of São Paulo. Columns (2) and (3) show that the effect of recent nearby audits on spending takes place above and beyond that of the announcement of the program (or that of any other differential trends with onset after 2003). In Column 2, recent nearby audits increase spending significantly (at the 5% level) in transfers with low procurement intensity (mostly targeted at wage payments). Even in those cases, the net effect of audits on spending within procurement-intensive programs – by adding the coefficients – is still very negative and statistically significant.

All in all, our findings for the effects of the audits on public spending are consistent with the simple model introduced in Section 4. Higher perceived audit risk displaces public spending to other municipalities where vendors move to in the aftermath of audits.

5.4 Robustness checks

This subsection addresses potential threats to the causal identification of the parameters of interest in the previous sub-section. In the main text, we focus on one dimension for ease of visualization: the sensitivity of our results to the choice of radius that determines proximity to audits in the previous year. We illustrate graphically that results are robust to using a continuous measure – namely, the minimum distance of bureaucrats/vendors to audits in the previous year.

We estimate the following equation:

$$\ln(Spending_{mvjt}) = \alpha + \theta_m + \theta_j + \theta_t + \beta_1 MinimumDistance_{m,t-1} + \beta_2 MinimumDistance_{v,t-1} + \beta_3\left(MinimumDistance_{m,t-1} \text{ x } MinimumDistance_{v,t-1}\right) + \epsilon_{mvt} \quad (2')$$

, where $\ln(Spending_{mvjt})$ is the amount payed for the contract between municipality m and vendor t within program category j at year t (in natural logarithms); θ_j stands for program category fixed-effects; θ_m and θ_t stand for municipality and year fixed-effects, respectively; $MinimumDistance_{m,t-1}$ is the minimum distance of municipality m's centroid to audits in the previous year ($= 0$ if m faces an audit itself), and 0 otherwise; and $MinimumDistance_{v,t-1}$ is the minimum distance of the centroid of *any municipality* where v had an active contract at the time to audits in t-1. All distance variables are expressed in natural logarithms.[22]

Figure 3 displays semi-parametric regressions – separately for each coefficient $\hat{\beta}_1, \hat{\beta}_2$ and $\hat{\beta}_3$ – in which we residualize the variable of interest and the dependent variable with respect to all other variables in equation (1'), before plotting a binned scatterplot and a linear fit showcasing the marginal effect of variation in minimum distance to audits for each contracting party on budget execution.

[Figure 3 – Panels A, B and C]

In each case, one can see a tight linear relationship between variation in minimum distance (in natural logarithms) and that in spending, which exactly replicates the patterns in case for the 75km-radius indicator of proximity to recent audits. Panel A displays a positive relationship between minimum distance from bureaucrats to recent audits and spending, Panel B displays a negative relationship, while Panel C displays a negative relationship – suggesting bureaucrats' reaction to recent nearby audits is slightly less negative when vendors also experience audits (in pre-existing contracts between both parties, or elsewhere).

In the Supplementary Appendix, we display regression tables to document that results are robust to using this minimum distance to audits in the previous year instead of the binary indicator of audits within 75km in t-1, to using the number of neighbor municipalities audited as an alternative definition of audit threats, and to using a continuous measure of transfers' procurement-intensity instead of the binary indicator of procurement-intensity.

5.5 What happens with budget leftovers?

Last subsection shows that audits have a very large negative effect on Health spending. Where does that money go? Answering this question matters for two reasons. First, this result seems to be inconsistent with

[22] We add a small constant (0.001) to the minimum distance variable before taking logs to avoid generating missing values in the case of own audits.

mandated minimum budget shares: Brazilian municipalities have to spend at least 15% of their tax revenues on Health. If spending drops by as much as 35%, how do municipalities comply with that mandate? Second, if municipalities really do not spend those funds, their destination has implications for welfare. For instance, if idle funds are re-budgeted by the federal or State governments and reallocated to municipalities able to spend them, then the analysis of the effects of the program on downstream outcomes must take such reallocation into account.

In what comes to the first issue, most municipalities actually spent way more than 15% in Health, making such constraint non-binding for the most part. What is more, minimum spending mandates are verified based on planned budget, not on budget execution. For that reason, there is no inconsistency between our results and those legal mandates. In what comes to the second issue, since our findings apply to both municipal matching funds and to contracts funded by own budget – none of which involve reallocation –, for those contracts funds are guaranteed to just sit idle in municipalities' accounts. In fact, the negative effect of audits on spending that we estimate is consistent with the increase in budget leftovers among Brazilian municipalities since the introduction of the anti-corruption program in 2003.[23]

6 Do government audits displace corruption?

In this section, we turn to the effects of audits on corruption. Subsection 6.1 discusses our empirical strategy for identifying the causal effect of audits on bureaucrats' and vendors' behavior, and for estimating the net effects of the program on corruption accounting for spillovers. Subsection 6.2 presents the results, followed by robustness checks in subsection 6.3. Subsection 6.4 analyzes heterogeneity in displacement effects by characteristics of vendors' networks at baseline.

6.1 Empirical strategy

We estimate the effects of past audits on the prevalence of corruption using the following equations:

$$\begin{aligned}\text{Corruption}_{mvjt} = {} & \alpha + \theta_j + \theta_t + \beta_1 PastAudits_{mt} + \beta_2 PastAudits_{vt} + \\ & \beta_3 (PastAudits_{mt} \text{ x } PastAudits_{vt}) + \sum \gamma_k X_{mt}^k + \epsilon_{mvjt}\end{aligned} \tag{4}$$

$$\begin{aligned}\text{Corruption}_{mvjt} = {} & \alpha + \theta_m + \theta_j + \theta_t + \beta_1 NearbyAudits_{m,t-1} + \beta_2 NearbyAudits_{v,t-1} + \\ & \beta_3 \left(NearbyAudits_{m,t-1} \text{ x } NearbyAudits_{v,t-1}\right) + \epsilon_{mvjt}\end{aligned} \tag{5}$$

[23] See https://civitarese.wordpress.com/2016/07.

$$\text{Corruption}_{mit} = \alpha + \theta_m + \theta_t + \beta\ NearbyAudits_{m,t-1}\ x\ ProcIntensive_{mit} + \delta\ NearbyAudits_{m,t-1} + \gamma ProcIntensive_{mit} + \epsilon_{mit} \quad (6)$$

, where $Corruption_{mvjt}$ is an indicator variable = 1 if the contract between municipality m and vendor v within program category j displays over-invoicing at year t, and 0 otherwise, in the TCE-SP dataset; and $Corruption_{mit}$ is an indicator variable = 1 if investigation i in municipality m displays evidence of corruption at year t, and 0 otherwise, in the CGU dataset. As before, θ_j stands for program category fixed-effects (Education, Health, Transportation, Administration, and Other); θ_t stands for year fixed-effects; $PastAudits_{mt}$ equals 1 if municipality m has been audited by the program at any year before t, and 0 otherwise; $PastAudits_{vt}$ equals 1 if *any municipality* with whom v had an active contract before t was audited by the program at the time, and 0 otherwise; and X_{mt} stands for a vector of municipal-level controls; $NearbyAudits_{m,t-1} = 1$ at year t if there was an audit within 75km of municipality m's centroid in the previous year (including, possibly, m itself facing an audit), and 0 otherwise; $NearbyAudits_{v,t-1} = 1$ at year t if there was an audit within 75km of the centroid of *any municipality* with whom v had an active contract at the time, and 0 otherwise; nd $ProcIntensive_{mit}$ equals 1 for procurement-intensive programs, and 0 otherwise.

In equations (4) and (5), based on our model from Section 4, we expect $\beta_1 \leq 0$ and $\beta_2 \geq 0$, and $\beta_3 \geq 0$. In equation (6), we expect $\beta \leq 0$ since bureaucrats in municipalities experiencing nearby audits are expected to be less corrupt ($\beta_1 \leq 0$, using the notation of equations 4 and 5) and since vendors are expected to displace corruption ($\beta_2 \geq 0$). We estimate OLS regressions, clustering standard errors at the municipality and at the vendor levels.

Last, we compute the net effect of audits on over-invoicing's extensive margin as follows:

$$\hat{\beta}_{net} = \frac{s_1\hat{\beta}_1 + s_2\hat{\beta}_2 + s_3(\hat{\beta}_1 + \hat{\beta}_3)}{s_1 + s_2 + s_3} \quad (7)$$

, where $\hat{\beta}_1$, $\hat{\beta}_2$ and $\hat{\beta}_3$ are estimates from equation (4); s_1 is the share of contracts for which only the bureaucrat experienced past audits; s_2 is the share of contracts for which only the vendor experienced past audits; and s_3 is the share of contracts for which both the bureaucrat and the vendor experienced past audits. Our interest lies in comparing $\hat{\beta}$, the estimated effect of audits without accounting for spillovers, with $\hat{\beta}_{net}$, its estimated effect when spillovers are taken into account.

Last, to estimate the net effect of audits on over-invoicing's intensive margin, we modify equation (5) replacing the dependent variable by the amount over-invoiced at each municipality-vendor-year triple (i.e.: equal to the amount invoiced subtracted of the planned amount (net of downward revisions), if that difference is positive; and 0 otherwise). Computing $\hat{\beta}_{net}$ using those estimates yields the effect of audits on the intensive margin of over-invoicing when spillovers are taken into account.

6.2 Results

We start by describing corruption prevalence in our datasets. In what comes to the prevalence of over-invoicing, 4.74% of the contracts each year are characterized by invoicing above planned (net of downward revisions) spending in the TCE-SP dataset. The amount over-invoiced averages R$ 27.215 (about USD 7,000), almost 300% of the planned amounts in those contracts. Figures 1 and 2 showcase the average prevalence of over-invoicing (its extensive and intensive margins, respectively) between 2010 and 2015, across São Paulo State's municipalities (other than the State Capital).

[Figure 1]

[Figure 2]

In the CGU dataset, which captures not only over-invoicing but 10 other irregularities classified as corruption (see Appendix A), 12.8% of investigations are coded as evidence of corruption between 2003 and 2007. That share is somewhat higher within procurement-intensive programs (13.6%). Given the retrospective nature of the audits – auditors follow the transfers' paper trail, typically at least 3 years prior to the time of the investigation – we observe the prevalence corruption since 1997. Interestingly, 31.4% of investigations were coded as evidence of corruption before the program was announced (37.1% within procurement intensive programs), a much higher figure, consistent with the evidence that its announcement may have substantially deterred corruption (Lichand, Lopes and Medeiros, 2019). In this paper, we restrict attention to the causal effects of audits themselves, which randomly assigned.

We start with the question of what audits do to the extensive margin of corruption – the probability of over-invoicing – in the TCE-SP dataset. Table 4 displays the results, documenting the predicted net effect of past audits when spillovers are taken into account or not. Columns (1) to (4) display the effects within contracts funded by federal and State transfers, while columns (5) and (8) focus on contracts funded by municipalities' own resources. Odd-numbered columns present naïve estimates, ignoring spillover effects driven by vendors' behavior. They do so by regressing the indicator variable of over-invoicing on an indicator of whether the municipality (the bureaucrat) experienced past audits. In contrast, even-numbered columns also include indicators for whether the vendor experienced past audits, and their interaction.

Columns (1), (2), (5) and (6) define over-invoicing as the difference between the amount invoiced and the planned amount (net of downward revisions, if any). Columns (3), (5), (7) and (8) also correct planned amounts for upward revisions – which are illegal, as discussed –, just so that we can document whether those respond strategically to higher perceived audit risk. All columns include municipal-level controls, program category fixed-effects and year fixed-effects. In all columns, standard errors are clustered at the municipality and at the vendor levels.

[Table 4]

Results are as follows. Ignoring spillovers, one would conclude that past audits *decrease* the probability of over-invoicing within contracts funded by transfers (Column 1) by about 6%, and that within contracts funded by municipalities' own resources (Column 5) by about 4%, even though neither effect is precisely estimated.

Exploring different configurations of past audits across different contracts allows testing the predictions of the model for the effects of higher perceived audit risk on corruption. When only bureaucrats experience past audits, the probability of over-invoicing falls substantially by 2.5 percentage points for in contracts funded by transfers (Column 2) and 2.7 percentage points for those funded by own resources (Column 6) – huge effect sizes, over 50% of the average prevalence, both statistically significant at the 1% level. In contrast, within contracts for which only vendors experience past audits, the probability of over-invoicing *increases* by 0.3 percentage points for transfers (Column 2) and 0.5 percentage points for those funded by municipality's own resources (Column 6), the latter significant at the 5% level. Once again, the asymmetric responses of each contracting party line up with the predictions of our model. In contracts for which both bureaucrats and vendors experience past audits, not much happens – counteracting forces balance each other, and the effect of past audits is not statistically different from zero.

Using the estimates in Columns (2) and (6) to compute the predicted net effects of past audits on the extensive margin of corruption once geographical displacement by vendors is taken into account, the conclusions from Columns (1) and (5) are *qualitatively reversed*: effects are over-estimated due to contamination of the control group. Even though the probability of over-invoicing falls substantially within contracts in which only the bureaucrats experienced past audits, and increases by only about 1/10 of that effect size in contracts for which only the vendor experienced past audits, the former stand for less than 1.5% of the contracts, while the latter amount for almost 45% of the cases. For that reason, the net estimated effect of past audits on the probability of over-invoicing is actually *positive* – 0.65% for contracts funded by transfers, and about 3% for contracts funded by own resources, although none of which precisely estimated.

Columns (4) and (8) document strategic revisions in response to higher perceived audit risk. The first thing to notice on those columns is that the prevalence of over-invoicing when upward revisions are netted out of planned amounts becomes less than half – only about 2% of the contracts. Next, effect sizes for the effects of past audits on bureaucrat's new and existing contracts are *less than 1/3* what they are when upward revisions are not accounted for across contracts funded by both transfers and own resources (Columns 2 and 6), in sharp contrast to vendors' new contracts, for which their effect is *basically unchanged* (a 0.3 percentage point increase, statistically significant at the 1% level in both cases). Together, the evidence suggests bureaucrats use upward revisions strategically to try to cover the traces of over-invoicing in new and existing contracts in face of higher perceived audit risk. Interestingly, because strategic reporting does not affect vendors' new contracts, if we compute the predicted net effect of audits using this version of over-invoicing we find *even more striking reversals* when spillovers are accounted for: from a 3.4% decrease in the probability of over-invoicing within contracts funded by transfers to a 7% increase (significant at the 10% level), and from a 1.5% increase, statistically insignificant, within contracts funded by own resources to a 9.5% increase (significant at the 1% level).

Table 5 turns to the intensive margin of over-invoicing. Columns (1) and (2) display the effects within contracts funded by federal and State transfers, while columns (3) and (4) focus on contracts funded by municipalities' own resources. Odd-numbered columns present naïve estimates, ignoring spillover effects driven by vendors' behavior. They do so by regressing the indicator variable of over-invoicing on an indicator of whether the municipality (the bureaucrat) experienced past audits. In contrast, even-numbered columns also include indicators for whether the vendor experienced past audits, and their interaction. All columns include municipal-level controls, program category fixed-effects and year fixed-effects. In all columns, standard errors are clustered at the municipality and at the vendor levels.

[Table 5]

Naïve estimates would lead one to conclude that past audits decrease the amount over-invoiced very substantially – by roughly 50% within contracts funded by transfers, and almost 90% (statistically significant at the 10% level) within those funded by municipality's own resources. As it is the case for the effects of audits on the extensive margin, however, effects are over-estimated due to contamination of the control group. The amount embezzled only falls statistically significantly in bureaucrats new contracts in the aftermath of audits. Even though effect sizes are very large (Columns 2 and 4), the share of contracts for which only bureaucrats experience past audits is very small. As a result, although still negative, predicted net effects are much smaller: roughly 28% and 21%, respectively, and the latter is no longer statistically significant.

Are those conclusions specific to over-invoicing? To answer that question, we turn to the CGU dataset to investigate whether recent nearby audits decrease corruption (measured from audit reports) to a greater extent within contracts funded by procurement-intensive programs, consistently with their effects on spending. Column (1) considers actions that are object of auditors' investigations occurring only between 2003 and 2007, after the program had been announced; and columns (2) and (3), those occurring for the whole period for which we have data (1997-2007). All columns include municipality and year fixed-effects, and cluster standard errors at the municipality level.

[Table 6]

Results are as follows. Audits within 75km in the previous year only have statistically significant effects (at the 5% level) on corruption within contracts funded by procurement-intensive programs. The effect is of the order of 20-30% of average corruption prevalence (depending on the sample we use), of the same order of magnitude as that documented using the TCE dataset for over-invoicing within procurement contracts for the State of São Paulo. Unsurprisingly, procurement-intensive programs tend to be associated with much higher likelihood of corruption – given the nature of most irregularities linked to corruption, mostly linked to procurement problems. Columns (2) and (3) show that the effect of recent nearby audits on corruption takes place above and beyond that of the announcement of the program (or that of any other differential trends with onset after 2003).

We conclude that accounting for geographical displacement by vendors significantly changes the interpretation of the effects of past audits on corruption. Effects on the intensive margin become much more modest, and not distinguishable from zero, and the likelihood of over-invoicing might even increase overall, despite falling locally in the aftermath of audits.

6.3 Robustness checks

This subsection addresses potential threats to the causal identification of the parameters of interest in the previous sub-section. In the main text, we focus on one dimension for ease of visualization: the sensitivity of our results to the choice of radius that determines proximity to audits in the previous year. We illustrate graphically that results are robust to using a continuous measure – namely, the minimum distance of bureaucrats/vendors to audits in the previous year.

We estimate the following equation:

$$\text{Corruption}_{mvjt} = \alpha + \theta_m + \theta_j + \theta_t + \beta_1 MinimumDistance_{m,t-1} + \beta_2 MinimumDistance_{v,t-1} + \beta_3\left(MinimumDistance_{m,t-1} \text{ x } MinimumDistance_{v,t-1}\right) + \epsilon_{mvt} \qquad (5')$$

, where $Corruption_{mvjt}$ is an indicator variable = 1 if the contract between municipality m and vendor v displays over-invoicing at year t, and 0 otherwise; θ_j stands for program category fixed-effects; θ_m and θ_t stand for municipality and year fixed-effects, respectively; $MinimumDistance_{m,t-1}$ is the minimum distance of municipality m's centroid to audits in the previous year ($= 0$ if m faces an audit itself), and 0 otherwise; and $MinimumDistance_{v,t-1}$ is the minimum distance of the centroid of *any municipality* where v had an active contract at the time to audits in t-1. All distance variables are expressed in natural logarithms.[24]

Figure 4 displays semi-parametric regressions – separately for each coefficient $\hat{\beta}_1, \hat{\beta}_2$ and $\hat{\beta}_3$ – in which we residualize the variable of interest and the dependent variable with respect to all other variables in equation (1'), before plotting a binned scatterplot and a linear fit showcasing the marginal effect of variation in minimum distance to audits for each contracting party on over-invoicing.

[Figure 4 – Panels A, B and C]

In each case, one can see a tight linear relationship between variation in minimum distance (in natural logarithms) and that in over-invoicing, which exactly replicates the patterns in case for the 75km-radius indicator of proximity to recent audits. Panel A displays a positive relationship between minimum distance from bureaucrats to recent audits and over-invoicing, Panel B displays a negative relationship, while Panel C displays a negative relationship – suggesting bureaucrats' reaction to recent nearby audits is partially reversed when vendors also experience them (in pre-existing contracts between both parties, or elsewhere).

In the Supplementary Appendix, we display regression tables to document that results are robust to using this minimum distance to audits in the previous year instead of the binary indicator of audits within 75km in t-1, to using the number of neighbor municipalities audited as an alternative definition of audit threats, and to using a continuous measure of transfers' procurement-intensity instead of the binary indicator of procurement-intensity.

[24] We add a small constant (0.001) to the minimum distance variable before taking logs to avoid generating missing values in the case of own audits.

6.4 Where do vendors move to? Heterogeneity by vendors' network characteristics

Taking advantage of our unique contract-level dataset, we can say more about the structure of spillovers driven by vendors. This subsection tests whether characteristics of vendors' networks at baseline matter for displacement effects.

We estimate heterogeneous treatment effects of past audits on vendors' new contracts, according to three variables: (1) the number of program categories across which each municipality-vendor pair had active contracts in 2010, a proxy for the *scale* of their relationship at baseline[25]; (2) the total amount payed across all contracts in 2010 for each municipality-vendor pair, a proxy for the *intensity* of their relationship at baseline; and (3) the number of contractors active in each municipality in 2010, a proxy for the *density of vendors* in each municipality at baseline. In building these variables, we restrict attention to 2010 – the first year for which have data on procurement contracts – in order to avoid endogenous responses of vendors' networks to past audits.

Our simple model in Section 4 does not provide sharp predictions for how spending and corruption within vendors' new contracts should vary with the scale of their presence, the intensity of their relationship or the density of other vendors. On the one hand, one may expect vendors to move towards "home turf" on the aftermath of audits, trying to increase their scale or the intensity of *pre-existing relationships* where perceived audit risk is less salient. On the other hand, it is reasonable to assume that, where vendors are already active, they *already optimize* the extent to which they support local bureaucrats in bearing procurement costs. By the same token, on the one hand one would expect vendors' help to be *more valuable* for municipalities with a lower density of other vendors; on the other hand, municipalities with a larger density of vendors might be precisely the ones that *most need to rely on vendors' help* to bear procurement costs.

Table 7 showcases these analyses, by interacting the indicator of past audits affecting vendors with each of those characteristics of vendors' baseline networks, one at a time. Columns (1) to (3) display heterogeneous treatment effects of past audits on spending, columns (4) and (6) do so for the extensive margin of over-invoicing, and Columns (7) to (9), for its intensive margin. We pool contracts funded by transfers or by own resources in each column. [26] All columns include all municipal-level controls, program

[25] We cannot use the number of contracts as our dataset sums over all active contracts within each program category to handle the issue of contractual add-ons.

[26] Since there are pairs municipality-vendor with multiple active contracts in a given year, whenever not all those contracts are funded by transfers or by own resources, pooling contracts from all funding sums over duplicates, decreasing the total number of observations.

category and year fixed-effects, and cluster standard errors at the municipality and at the vendor levels. We control for the indicator of whether the bureaucrat was audited in the past, and its interaction with the indicator for vendors, but omit those coefficients from the table to focus on the heterogeneity within vendors' new contracts.

[Table 7]

First, it is interesting to notice that the scale and the intensity of the relationship between vendors and bureaucrats at baseline indeed predict higher spending and corruption in subsequent contracts. Having active contracts in an additional program category at baseline is associated with higher spending in subsequent contracts between municipality-vendor pairs (6.4% higher, Column 1), and with a higher probability of over-invoicing (1 percentage point or about 16.7% higher, Column 4) within those contracts. A 10% increase in spending in contracts at baseline is associated with higher spending in subsequent contracts (6.1% higher, Column 2), and with a higher probability of over-invoicing (0.2 percentage point or about 16.7% higher, Column 4). In turn, in what comes to the density of vendors, a 10% increase in the number of contractors at baseline is associated with 8.4% lower spending (Column 3), 0.2 percentage points lower probability of over-invoicing (Column 3), and about 25% lower embezzlement (Column 9) in subsequent contracts with any particular vendor, consistent with supply-side competition increasing bureaucrats' bargaining power.

Second, we find no systematic heterogeneity in the effects of audits on vendors' new contracts by the scale of their relationships with bureaucrats at baseline. The intensity of these relationships is, however, predictive of where vendors move to. The latter are less likely to increase spending in municipalities where they had a more intense presence at baseline: spending in new contracts with municipalities where vendors were absent at baseline on the aftermath of audits increases by 60% (Column 2), and then decreases by 0.5% for each 10% increase in spending (relative to the average) in contracts between the municipality-vendor pair at baseline. Such result is consistent with the story that *vendors enter new markets* on the aftermath of audits, where they are *less able to extract rents* (at least at first). The density of vendors at baseline is also predictive of where vendors move to. The latter are more likely to increase over-invoicing in municipalities with many contracts in 2010. The effect size is, however, small: a 10% increase in the probability of over-invoicing when the number of contractors at baseline doubles, and somewhat imprecisely estimated (significant at the 10% level). Such result is consistent with the story that *municipalities with more contractors have more opportunities for vendors* to help moving procurement forward. Having said that, given the average effects of baseline density on over-invoicing, vendors are *expected to capture less* in those contracts (in which bureaucrats presumably exercise higher bargaining power). In the presence of fixed costs for entering new markets, our results help rationalize Lagaras, Ponticelli and Tsoutsoura, 2017)'s findings for short-term losses for vendors on the aftermath of audits.

7 Is displacement consequential?

Is geographical displacement by vendors welfare-neutral – i.e.: does it just move resources around –, or is the disruption it brings about consequential? We study this question in two ways. First, we look at implementation quality, exploring detailed accounts of mismanagement problems in audit reports (from infrastructure problems, to incomplete documentation, to performance problems indicated by citizens' complaints). Second, we look at whether audits make health indicators worse.

We start by discussing the empirical strategy and results for our analysis of the effects of audits on implementation quality, in subsection 7.1. Next, subsection 7.2 presents the data, empirical strategy and results for the effects of audits on health outputs and outcomes, followed by robustness checks in subsection 7.3.

7.1 Implementation Quality

This subsection looks at implementation quality, by taking advantage of the richness of the data coded from CGU's audit reports. As before, we explore heterogeneous effects of recent nearby audits across transfers of different procurement intensities. For each mismanagement category in our dataset, we estimate the following regression:

$$\begin{aligned} MM_{mit}^{k} = {} & \alpha^{k} + \theta_{m} + \theta_{t} + \beta^{k}\left(NearbyAudits_{m,t-1}\ x\ ProcIntensive_{mit}\right) + \\ & \delta^{k}\ NearbyAudits_{m,t-1} + \gamma^{k} ProcIntensive_{mit} + \epsilon_{mit} \end{aligned} \quad (8)$$

In equation (8), $MM_{mit}^{k} = 1$ if investigation i at municipality m at year t is coded as evidence of mismanagement category k, and 0 otherwise. We are interested in testing whether $\beta^{k} \geq 0$.

There are two reasons to expect mismanagement to increase with perceived audit probability. First, a mechanical effect: with lower spending, several measures of implementation quality – such as stock management, necessarily impaired by the lack of medication on the absence of timely procurement – should be adversely affected. Second, an incentive effect, of the same nature as that which generates lower spending: either greasing the wheels or procurement risk discourage effort into moving resources towards public service delivery.

Table 8 looks separately at each mismanagement category in our dataset. In column (1), *resource diversion* represents diverting resources meant to be used for the purposes of one transfer towards other transfers (within Health or not); notice that this category does not include over-invoicing or off-the-record invoice, instances of resource diversion that are coded as evidence of corruption. In column (2), *Health council problems* range from precarious facilities to infrequent meetings (below legal requirements). In column (3), *performance problems* stand for patient complaints about medical consultations, hospital admissions or unavailable medication, or for auditors' direct account of long queues or low-quality health services. In column (4), *infrastructure and stock problems* encompass precarious facilities in health units (including mobile units, such as ambulances), medication not properly kept in stock, or in- and out-flows not properly accounted for. In column (5), *human resources' problems* represent problems with late wage payments, absenteeism, or irregular composition of health teams (different from legal requirements, e.g. when nurses perform roles that should have been assigned to physicians). Last, in column (6), *documentation or accounting problems* range from mix-ups in bank accounts supposed to be kept separate to invoices with illegible information. All columns include municipality and year fixed-effects, and cluster standard errors at the municipality level.

[Table 8]

Results are that audits within 75km in the previous year significantly increase performance problems. The estimate is significant at the 10% level, and equivalent to a 21% increase in investigations featuring long queues, patients' complaints, or auditors' perceptions of low-quality service delivery. Mismanagement seems to increase diffusely across many different categories following recent nearby audits; the only negative coefficient is on health council problems, which account for only 3% of the investigations. Point estimates are particularly large for human resource problems (an 18% increase) and infrastructure and stock problems (a 10.6% increase), even though those are not precisely estimated.

The evidence is consistent with the claim that audits decrease corruption locally, but distort hurt public service delivery.

7.2 Health outputs and outcomes

Next, we turn to the question of whether, beyond impacts on perceived implementation quality, past audits make health indicators worse.

For yearly municipal-level data on health outputs and outcomes, we rely on Brazil's National Health Database (*Base de Informações de Saúde*, DATASUS) from 2004 (the first year for which there is variation in audits at $t-1$) to 2015 (the last year for which we have corruption and spending data). We assemble a dataset with the following indicators: number of hospital beds per thousand inhabitants; immunization shots per thousand inhabitants[27]; share of households with proper sewage disposal (either connected to the general network or with septic tanks, which are widespread in rural areas); share of households connected to piped water; population share assisted by the Family Health program (*Programa Saúde da Família*); medical consultations per thousand inhabitants; preventable deaths[28]; deaths by external causes; and under-1-year-old infant mortality per thousand.

We start by contrasting the effects of audits within 75km in the previous year on health outputs and outcomes linked to programs of different procurement intensities, motivated by our results for the effects of audits on corruption and spending in the CGU dataset. In order to employ this empirical strategy, however, we need outputs and outcomes that are affected by certain programs *but not others*. We deal with this issue by resorting to the Health Ministry's Monitoring & Evaluation framework, which specifies which outputs and outcomes are used to track the implementation quality of each federal transfer to municipalities. According to this framework, the outputs and outcomes we include in this first analysis are linked either to programs with high procurement-intensity *only*, or to programs with low procurement-intensity *only*. For programs with no M&E indicators, we resort to the Health Ministry's description of all actions under each health transfer to define proxies for its outputs and outcomes.[29]

The outputs and outcomes that we are able to include are as follows. For high procurement-intensity transfers: hospital beds per thousand inhabitants, immunization shots per thousand inhabitants, household share with proper sewage disposal, and household share with access to piped water – all of which are linked to programs with procurement intensity 50% or higher. For low procurement-intensity transfers: population share covered by the Family Health program, and medical consultations per thousand inhabitants – both of which are linked to programs with procurement intensity below 50%.

[27] The immunization indicator includes 28 vaccine-preventable diseases, listed under the Epidemiological and Environmental Surveillance in Health program (VIGISUS).
[28] Preventable deaths are all deaths excluding those from external causes (such as accidents or homicides). Both preventable and external deaths are coded by municipality of in-patient care, rather than by that of residence, since we are interested in the effects of the program mediated by health services.
[29] See Table A4 in Appendix A.

We estimate the effects of recent nearby audits on each health output/outcome using the following equation:

$$Y_{mjt}^{k} = \alpha^{k} + \theta_m + \theta_t + \beta^{k} \left(NearbyAudits_{m,t-1} \, x \, \mathbb{I}\{\text{j} \in ProcIntensive\} \right) + \delta^{k} \, NearbyAudits_{m,t-1} + \gamma \, \mathbb{I}\{\text{j} \in ProcIntensive\} + \epsilon_{mjt} \qquad (9)$$

In equation (9), $\mathbb{I}\{\text{j} \in ProcIntensive\}$ equals 1 for outputs and outcomes of procurement-intensive programs, and 0 otherwise; $NearbyAudits_{m,t-1} = 1$ at year t if there was an audit within 75km of municipality m's centroid in the previous year (including, possibly, m itself facing an audit), and 0 otherwise; θ_m are municipal fixed-effects; θ_t are year fixed-effects; and ϵ_{mjt} is an error term. Y_{mjt}^{k} is health output/outcome k linked to set of transfers j in municipality m at year t, where j represents either high or low procurement-intensity programs.

Since there are multiple outputs and outcomes within each set of transfers, there is no obvious way of pairing indicators across high and low procurement-intensity transfers in the analysis; what is more, estimating separate regressions for each pair would substantially inflate the probability of false positives above stated significance levels. To deal with this issue, we convert all outputs and outcomes to z-scores, and define summary measures as the average of z-scores within each set of programs. Following Kling, Liebman and Katz (2007), effect sizes are obtained by replacing Y_{mjt}^{k} in equation (8) by the summary measure of high and low procurement-intensity programs in each municipality and year.

$$Z_{mjt} = \alpha + \theta_m + \theta_t + \beta \left(NearbyAudits_{m,t-1} \, \text{x} \, \mathbb{I}\{\text{j} \in ProcIntensive\} \right) + \delta \, NearbyAudits_{m,t-1} + \gamma \, \mathbb{I}\{\text{j} \in ProcIntensive\} + \epsilon_{mjt} \qquad (9')$$

In equation (9'), Z_{mjt} is the summary measure for health outputs and outcomes linked to set of transfers j in municipality m at year t. The hypothesis we are interested on is $\beta \leq 0$.

We also consider the effects of past audits on mortality rates. We look separately at child mortality and preventable deaths, and use deaths by external causes as a placebo test – as accidents or homicides clearly should not be affected by corruption or spending. In all cases, we include two lags of the indicator variable for audits within 75km, as effects on mortality rates may take longer to materialize. We estimate the following equation.

$$Y_{mt} = \alpha + \theta_m + \theta_t + \beta_1 NearbyAudits_{m,t-1} + \beta_2 NearbyAudits_{m,t-2} + \epsilon_{mit} \qquad (10)$$

Panel A of Table 9 displays the results of our analysis for the effects of recent nearby audits on the summary measure of health outputs and outcomes. Columns (1) and (2) display the results for outputs and outcomes of programs with low procurement-intensity –medical consultations and coverage of the Family Health program, respectively –, while columns (3) to (6) do so for outputs and outcomes of programs with high procurement-intensity –immunization per 1,000 inhabitants, hospital beds per 1,000 inhabitants, % of households with adequate drinking water and % of households with adequate sanitation, respectively. Last, column (7) displays the results for the summary measure of health outputs and outcomes, stacking low and high procurement intensity summary measures for each pair municipality-year. All columns include municipality and year fixed-effects, and cluster standard errors at the municipality level.

[Table 9 – Panel A]

Results are as follows. Within low procurement-intensity programs, recent nearby audits do not affect either health output (Columns 1 and 2). In turn, within procurement-intensive transfers, immunization and hospital beds significantly *deteriorate* following audits within 75km in the previous year: immunization coverage (Column 3) falls by 1.57 percentage points, an effect statistically significant at the 5% level, while hospital beds per 1,000 inhabitants (Column 4) fall by 12.9%, significantly at the 1% level. Access to adequate water and sanitation (Column 5 and 6) are not significantly affected. Relying on our summary measure, we find that audits only significantly deteriorate outputs/outcomes *linked to procurement-intensive transfers*. The net effect of recent nearby audits within procurement-intensive health outputs/outcomes is – 0.137 standard deviations (p-value 0.0194). The magnitude of the effect is large, equivalent to 18% lower *per capita* health spending (in a cross-sectional regression in 2002, right before the introduction of the random-audits program, within the municipalities in our CGU dataset).

Next, Panel B showcases the results for the effects of nearby audits in the previous two years on mortality indicators, all normalized to z-scores. Column (1) displays results for child mortality, column (2) for preventable deaths, and column (3) showcases our placebo test, estimating the effects of audits on external deaths (accidents or homicides).

[Table 9 – Panel B]

We find that audits within 75km in the previous year *increase* preventable deaths by 0.01 standard deviations (Column 2), an effect statistically significant at the 10% level. In what comes to child mortality

(Column 1), recent nearby audits' coefficient is about the same order of magnitude (0.015 standard deviations), and the combined effect of facing nearby audits two years in a row is a marginally significant (p-value 0.106) *increase* in under-1 mortality per 1,000 births. Even though those magnitudes are quite small, mortality is an extreme indicator – the fact that audits have precisely estimated negative effects is already overwhelming. In contrast, audits do not affect external deaths (Column 3), as expected.

Together, these are striking results. Geographical displacement by vendors is consequential: it makes local implementation quality and health indicators systematically worse.

Those results also strongly reject the hypothesis that audits merely undo overspending: in sharp contrast to the patterns documented in Liebman and Mahoney (2017), it is not the case that higher spending (in their case, driven by budgets that expire by the end of the year) correlates with worse quality or worse public service delivery.

7.3 Robustness checks

This subsection addresses potential threats to the causal identification of the parameters of interest in the previous sub-section. In the main text, we focus on one dimension for ease of visualization: the sensitivity of our results to the choice of radius that determines proximity to audits in the previous year. We illustrate graphically that results are robust to using a continuous measure – namely, the minimum distance of bureaucrats/vendors to audits in the previous year.

We estimate the following equation:

$$Y_{mt} = \alpha + \theta_m + \theta_t + \beta MinimumDistance_{m,t-1} + \epsilon_{mit} \tag{10'}$$

, where Y_{mt} are preventable deaths per 1,000 inhabitants; θ_m and θ_t stand for municipality and year fixed-effects, respectively; and $MinimumDistance_{m,t-1}$ is the minimum distance of municipality m's centroid to audits in the previous year ($= 0$ if m faces an audit itself), and 0 otherwise.

Figure 5 displays a semi-parametric regression in which we residualize the variable of interest and the dependent variable with respect to the fixed effects in equation (9'), before plotting a binned scatterplot and a linear fit showcasing the effect of variation in minimum distance to audits on our mortality indicator.

[Figure 5]

One can see a negative linear relationship between variation in minimum distance (in natural logarithms) and preventable deaths, which exactly replicates the patterns in case for the 75km-radius indicator of proximity to recent audits.

In the Supplementary Appendix, we display regression tables to document that results are robust to using this minimum distance to audits in the previous year instead of the binary indicator of audits within 75km in t-1, and to using the number of neighbor municipalities audited as an alternative definition of audit threats.

8 "Greasing the wheels" or procurement risk?

We have shown that audits make outcomes worse. This effect could be explained by two very different mechanisms, as the model in Section 4 makes it clear. On the one hand, an increase in the perceived audit probability decreases the net expected benefits of being corrupt, making procurement undesirable for some contracts. In this case, corruption *greases the wheels* of public service delivery (Banerjee, Hanna and Mullainathan, 2013): bureaucrats are only willing to pay the effort costs of procurement when they expect to benefit from embezzling resources to a greater extent. On the other hand, an increase in the perceived audit probability also decreases the net expected benefits of procurement *even when the bureaucrat is not corrupt*, due to *procurement risk* arising from inaccurate punishment of procurement mistakes.

Which mechanisms are at play in the context of the Brazilian anti-corruption program? This is an important question, given their completely different implications for the design of monitoring programs to minimize such distortions. To answer that question, we start by taking advantage of the richness of the TCE dataset to decompose the effect of audits on spending by budget execution stage, contrasting planning and revision (both at the pre-procurement stage) with invoicing and payment (both at the post-procurement stage). If effects are concentrated on the post-procurement stage, we could rule out *procurement risk*.

If that is not the case, however, then we have to explore additional sources of variation: at the pre-procurement stage, the model shows that variation in p, the perceived probability of being audited, is not enough to disentangle greasing the wheels from procurement risk. To tease them apart, we explore random variation in ε, the probability of accidental procurement mistakes, taking advantage of Strengthening Public Management (*Fortalecimento da Gestão Pública*, or FGP), a capacity-building program also build on public lotteries, introduced by CGU in 2006.

Next, subsection 8.1 provides a brief description of FGP. Subsection 8.2 presents our empirical strategy for decomposing the effects of recent nearby audits on spending at different budget execution stages, and for estimating the effects of FGP on spending and corruption, followed by the results in subsection 8.3.

8.1 The capacity-building program

Introduced by CGU in 2006, Strengthening Public Management (*Fortalecimento da Gestão Pública*, or FGP) was designed to provide capacity-building trainings to Brazilian municipalities under 50,000 inhabitants.[30] Exactly as the random-audits program, FGP was based on public lotteries. The difference is that the lotteries could only draw municipalities that enrolled to be part of the program. From 2008 onwards, CGU allowed neighbor municipalities to also participate in the face-to-face trainings at the municipality drawn by FGP.

The program was meant to provide reading materials and face-to-face lectures to municipal government staff members on public management best practices, including ethics, governance, and conformity to legislation. Lopes (2011) analyzes the materials used in those trainings and concludes that their main goal was to decrease the prevalence of corruption in municipal public spending.

FGP was discontinued in 2014. It trained between 94 and 343 municipalities per year between 2007 and 2013. Many more municipalities enrolled to participate – in our sample, about 1/3 of municipalities each year were eligible to receive face-to-face trainings (including neighbors of enrolled municipalities); however, only a very small share was actually trained each year (a minimum of *none*, in 2012, and a maximum of 6.1%, in 2010). To serve the excess demand, CGU instituted other programs, from strengthening internal controls (over 300 municipalities each year, from 2007 to 2010; hence, before the period for which we have data) to online executive education courses (with nearly 5,000 participants over the years). Because those initiatives were not randomly assigned, we disregard them in our analysis.

8.2 Empirical Strategy

First, we take advantage of our contract-level dataset to analyze where effects are concentrated in the budget execution process. Lower spending can come about as a result of lower planned expenses – before procurement takes place –, or as a result of lower delivery rates (after goods or services have been contracted) and/or lower payment rates (after goods or services have been rendered) – both at the post-procurement stage. We analyze what recent nearby audits do to bureaucrats and vendors at each of those stages.

$$Y^{s}_{mvt} = \alpha + \theta_m + \theta_t + \beta^{s}_{1} MinimumDistance_{m,t-1} + \beta^{s}_{2} MinimumDistance_{v,t-1} +$$

[30] The program was first introduced as *Programa de Fortalecimento da Gestâo Municipal*, and was rebranded as FGP in 2008 (Lopes, 2011).

$$\beta_3^s(MinimumDistance_{m,t-1} \text{ x } MinimumDistance_{v,t-1}) + \epsilon_{mvt} \quad (11)$$

, where Y_{mvt}^s is spending indicator at stage s. To exactly decompose the effects of audits on spending, we write:

$$\ln(Spending_{mvt}) = \ln(Planned_{mvt}) \text{ x } \%Invoiced_{mvt} \text{ x } \%Payed_{mvt}$$

, where $\ln(Planned_{mvt})$ is planned spending (in natural logarithms), $\%Invoiced_{mvt}$ is the delivery rate (invoiced / revised), and $\%Payed_{mvt}$ is the payment rate (payed / invoiced) at year t for the contract between municipality m and vendor v. We estimate equation (11) using each of those as dependent variable Y_{mvt}^s. We are interested is comparing β_1^s and β_2^s across pre- and post-procurement budget execution stages. In particular, if the effects of audits on spending are not statistically different from zero on planned spending, then we could reject the hypothesis that effects are driven by procurement risk.

Second, we take advantage of FGP's random draws for capacity-building trainings. Assignment was random conditional on eligibility, defined by municipalities that voluntarily signed up for the program, and all their neighbors. We restrict the sample to eligible municipalities throughout these analysis. Table F.4 confirms that municipalities drawn to be trained are not systematically different from those that are not, conditioning on municipality and year fixed-effects.

[Table F.4]

We estimate the following equation:

$$Y_{mvt} = \alpha + \theta_m + \theta_t + \beta_1 Trained_m + \beta_2 Trained_v + \beta_3(Trained_m \text{ x } Trained_v) + \epsilon_{mvt} \quad (12)$$

, where Y_{mvt} is an indicator of data-entry mistakes (negative values), upward budgetary revisions (which we have shown to respond strategically to past audits; in R$), spending (in natural logarithms), or an indicator of over-invoicing, at year t for the contract between municipality m and vendor v; $Trained_m = 1$ if municipality m was assigned to capacity-building trainings at any year prior to t, and 0 otherwise; and $Trained_v = 1$ if *any* municipality where vendor v was active at the time was assigned to capacity-building trainings at any year prior to t, and 0 otherwise.

The hypotheses of interest are $\beta_1 \leq 0$ when it comes to mistakes and upward budgetary revisions – a manipulation check that the trainings actually raised capacity –; $\beta_1 \geq 0$ and $\beta_2 \geq 0$ when it comes to spending, as the net expected costs of procuring *without vendors' help* decrease with lower procurement risk; and $\beta_1 \leq 0$ and $\beta_2 \geq 0$ when it comes to over-invoicing, as the model predicts that with lower procurement risk bureaucrats should rely on vendors' help to a lesser extent.

The predictions for the effects of trainings on spending really draw those apart from the effects of higher perceived audit risk: trainings should increase spending in vendors' new contracts *without decreasing spending* in bureaucrats' new contracts. The predictions for their effects on corruption, in turn, highlight a potential dark side of trainings: they should also geographically displace corruption, this time to municipalities that haven't yet been drawn to be trained.

We also estimate the saturated version of equations (1) and (5), to estimate heterogeneous treatment effects of past audits, according to whether bureaucrats have been trained in the past:

$$\begin{aligned} Y_{mvt} = \alpha + \theta_m + \theta_t + {} & \beta_1^1 PastAudits_{mt} \text{ x } Trained_m + \beta_1^0 PastAudits_{mt} \text{ x } (1 - Trained_m) + \\ & \beta_2^1 PastAudits_{vt} \text{ x } Trained_m + \beta_2^0 PastAudits_{vt} \text{ x } (1 - Trained_m) + \\ & \beta_3^1 (PastAudits_{mt} \text{ x } PastAudits_{vt})\ x\ Trained_m + \\ & \beta_3^0 (PastAudits_{mt} \text{ x } PastAudits_{vt})\ x\ (1 - Trained_m) + \epsilon_{mvt} \end{aligned} \tag{13}$$

We are interested is testing $\beta_1^1 \geq \beta_1^0$ and $\beta_2^1 \leq \beta_1^0$ when it comes to spending, and $\beta_1^1 \leq \beta_1^0$ $\beta_2^1 \leq \beta_2^0$ when it comes to over-invoicing, since, once again, lower procurement risk decreases the cost of procuring *without vendors' help*.

8.3 Results

We start by decomposing the effects of recent nearby audits on spending in the TCE dataset, by budget execution stage. In Table 10, Column (1) documents the results for planned spending (in natural logarithms), column (2) for the delivery rate, and column (3) for the payment rate. In each column, we pool contracts funded by federal and State transfers and those funded by own revenues. All columns include all municipal-level controls, and program category and year fixed-effects. We cluster standard errors at the municipality and at the vendor levels.

[Table 10]

Results are as follows. At the pre-procurement stage, planned spending (Column 1) falls by 31.7% when only the bureaucrat experiences past audits, an effect statistically significant at the 1% level. In contrast, in contracts for which only the vendor experiences them, planned spending *increases* by 16.7%, also significant at the 1% level. There is basically no change in planned spending when both contracting parties experienced past audits.

At the post-procurement stage, past audits do not affect delivery rates (Column 2). While they do affect payment rates, the effects are quantitatively very small: when only the bureaucrat experiences past audits, invoicing decreases by 2.1 percentage points (from a baseline of 92.4%; Column 3). The effect of past audits affecting only vendors on payments (Column 3) even goes in the opposite direction of its overall effect on spending. We conclude that lower spending derives chiefly from the *pre-procurement stage*. Hence, we cannot rule out procurement risk as a mechanism behind the negative effects of audits on public spending.

Next, Table 11 turns to the effects of random assignment to trainings on data-entry mistakes (Columns 1 and 5), upward budgetary revisions (Columns 2 and 6), spending (in natural logarithms; Columns 3 and 7), and the extensive margin of over-invoicing (Columns 4 and 8). Columns (1) to (4) restrict attention to contracts funded by federal and State transfers, while columns (5) to (8), to those funded by municipalities' own resources. In each column, we restrict the sample to municipalities eligible to receive trainings (those enrolled for FGP and their numbers). All columns include municipality all municipal-level controls, and program category and year fixed-effects. We cluster standard errors at the municipality and at the vendor levels.

[Table 11]

We find that, in face of local trainings, bureaucrats incur in lower invoicing and billing mistakes (Columns 1 and 5). The effect size is large: mistakes decrease by 3-3.7 percentage points (significant at the 5% level for own resources), equivalent to an almost 20% reduction in their baseline prevalence within own resources. Trainings also reduce upward revisions very substantially within bureaucrats' new contracts (Columns 2 and 6): the effect size is so large that it suggests FGP trainings *completely eliminate* upward revisions, consistent with public management best practices. Interestingly, when any municipality where vendors were active is trained by FGP and the latter move to other eligible municipalities not yet drawn, mistakes in those new contracts go up by 2.7 percentage points (significant at the 1% level; Columns 1 and 5), a large effect consistent with strategic reporting, and in line with our findings for upward revisions in Section 5.

In line with the predictions from the model, randomly assigned trainings do not systematically decrease spending locally within transfers. However, they still strongly increase spending elsewhere, where vendors move to on the aftermath of trainings, by 38.4% (Column 3), significantly at the 1% level. That pattern is consistent with procurement risk. Having said that, past trainings end up decreasing spending within existing contracts funded by transfers (Column 3), and within new and existing contracts funded by municipalities' own resources (Column 7). This could be driven by a combination of two factors: the fact that trainings also increased perceived audit risk, and the acknowledgement of processual errors in existing contracts that can no longer be fixed after trainings, inducing bureaucrats to bring spending on those contracts to a halt. Last, also as predicted by the model, trainings decrease over-invoicing in bureaucrats' new contracts by 2.1-2.4 percentage points (significant at the 1% level; Columns 4 and 8), a sizeable effect of almost 50% of the average prevalence. Alongside the displacement in spending, trainings displace corruption significantly, which increases by 1.4-2 percentage points in vendors' new contracts (significant at the 1% level; Columns 4 and 8). Corruption still falls within existing contracts in the aftermath of trainings, but effects sizes are lower, also consistent with the model.

Next, Table 12 turns to heterogeneous effects of past audits by whether local bureaucrats were randomly assigned to trainings in the past. Columns (1) and (3) documents the effects on spending (in natural logarithms), while Columns (2) and (4) do so for the extensive margin of over-invoicing. Columns (1) and (2) restrict attention to contracts funded by federal and State transfers, while columns (3) to (4), to those funded by municipalities' own resources. In each column, we restrict the sample to eligible municipalities. All columns include all municipal-level controls, and program category and year fixed-effects. We cluster standard errors at the municipality and at the vendor levels. We are interested in testing whether the effects of audits within each contract configuration change with the status of past trainings.

[Table 12]

Results are as follows. In the aftermath of past audits, vendors are systematically *less able to increase spending* in new contracts with municipalities previously assigned to trainings. This is true across all funding sources, and effect sizes are large: within trained municipalities, past audits lead vendors to increase spending in new contracts elsewhere by less than 60% their effect size within municipalities not yet drawn to be trained (Columns 1 and 3; differences statistically significant at the 5% level). By the same token, corruption in vendors' new contracts *only increases within municipalities not yet drawn to be trained* (Columns 2 and 4; differences statistically significant at the 5% level). Both patterns are consistent with the procurement risk mechanism.

While trainings do not prevent spending from falling in bureaucrats' new or existing contracts in the aftermath of audits, they do systematically magnify the effects of past audits on corruption within those contracts. Within new contracts funded by transfers, trainings enable bureaucrats to reduce over-invoicing to a great extent (Column 2): the effect size is two-fold that within municipalities not yet drawn to be trained, and significant at the 10% level. This is also consistent with procurement risk.

In existing contracts, however, the opposite is true: past audits *increase corruption* systematically across all funding sources to a much great extent within municipalities trained by FGP (Columns 2 and 4): the effect of audits on over-invoicing in those case is at least 250% that within existing contracts in municipalities not yet drawn to be trained, and the differences are significant at the 1% level. Such pattern is consistent with trained bureaucrats displacing spending to where they perceive lower audit risk, and is consistent with the "greasing the wheels" mechanism.

We conclude that the detrimental effects of audits on public spending are driven by both "greasing the wheels" and procurement risk. Our findings provide first-hand rigorous evidence for the latter. Results are consistent with Leaver (2009)'s model, in which bureaucrats seek to minimize public attention in order to avoid criticism. In our setting, bureaucrats' 'minimal squawk' behavior stems from an incentive structure that punishes procurement mistakes as corruption.

9 Discussion and concluding remarks

We have documented a new mechanism for why anti-corruption programs are likely to backfire: geographical displacement of corruption by vendors responding to lower spending by local bureaucrats, who react to past audits motivated by both "greasing the wheels" and procurement risk. We have shown that, in the context of the Brazilian anti-corruption program, these effects are large enough to reverse the conclusions about the effects of the program on the extensive and intensive margins of corruption when such spillovers are not taken into account.

In face of the growing decentralization in developing countries, when responsibilities are moved from the central to the local governments alongside resources funded through redistribution, concerns about moral hazard typically lead the former to put monitoring mechanisms in place. Those mechanisms are designed by central government's staff members, who tend to be much higher-skilled than the local bureaucracy, due to (sometimes huge) differences in pay. Such asymmetric skills tends to render local bureaucrats incapable of handling complex procurement procedures that were centrally designed. We have shown that, while anti-corruption programs decrease corruption locally, they tend to disrupt budget execution and deteriorate public service delivery.

How to avoid the side effects of the remedy? Our findings suggest that preventing displacement by vendors might be hard. Capacity-building trainings designed to mitigate procurement risk do indeed partly prevent vendors from driving spending elsewhere in the aftermath of audits; however, trainings themselves set off huge displacement effects – vendors drive corruption to locations where bureaucrats have not yet been trained. In face of vendors' behavior, a piecemeal approach to capacity-building is likely to backfire, placing a demanding constraint on the scale of interventions powered by organizations working to support public managers. Research is needed on the optimal design of capacity-building interventions to reduce procurement risk among local procurement staff, and the extent to which those interventions can improve public service delivery locally.

Our results provide first-hand evidence that audits may hurt public service delivery. Does this mean corruption is welfare-improving in this setting? While our results should not be interpreted as corruption being good – as it can have other negative welfare consequences for society not captured by our analysis, such as deteriorating trust in Government and fellow citizens (Rose-Ackerman, 1997) –, they point out that, similar to the trade-off between the social costs of decreasing pollution and those of decreasing production, the optimal level of corruption in society might not necessarily be zero.

Reducing pollution involves costs to society: if it is achieved through a fine on firms that pollute, those are given by such direct costs to firms in addition to indirect costs to workers: lower wages and/or unemployment, due to lower output in response to higher production costs. The socially optimal level of pollution is that for which the social costs of having an additional unit of the pollutant in the environment are equal to the social costs of avoiding it. That optimal level could be zero, in principle. In practice, however, this is unlikely – for a firm not to generate any residual from production is impossible with today's technologies. Hence, unless we want a world where firms do not produce anything (and hence, with no income or consumption), we also do not want a world without pollution. Society has come to accept that idea. The concept of carbon trading by no means enforces zero pollution. To move in that direction, society had to form a consensus on *treating different types of pollution differently*. Consider a firm that purposefully throws waste in the river to avoid the costs of properly disposing of it. Society does not accept *any level* of that type of pollution. One possible reason for the differential treatment is that the latter could always be fully avoided at some costs that do not make production infeasible (say, by paying another company to collect the waste and treat it or take it to a brownfield).

Our simple theoretical model and the empirical evidence that supports it suggest the same conclusions should apply to corruption. Analogously to pollution, for embezzlement that could be eliminated at reasonable costs – politicians raising campaign contributions in exchange for favoring bidders later on, or police selling weapons to drug dealers –, such extreme logic *should not apply*. Is corruption in procurement contracts ran by local bureaucrats different? Our results suggest that it, in fact, cannot be reduced at reasonable costs – public goods provision would be significantly reduced without it. Hence, a social planner setting corruption optimally would equalize its marginal social costs – from monitoring costs to capacity-building costs to its other negative welfare effects – to its marginal social benefits – higher quantity and quality of public goods and services, and ultimately better public service delivery.

While the Brazilian anti-corruption program represented a major improvement in monitoring and transparency, the focus of administrative penalties and of public opinion on corruption, instead of on the quality of public services, all seem to have thrown the baby out with the bathwater. Some recent advances have moved Brazil in the right direction. In particular, electronic procurement has streamlined acquisition of homogenous goods and services nationwide, and some municipalities have devised centralized procurement agencies to concentrate their best human resources across different agencies. Progress in other critical dimensions, however, does not show the same promise. Recurrent corruption scandals in national politics have inflamed public opinion, with the logic of "crime and punishment" prevailing over that of implementation quality. Moreover, the Brazilian procurement framework has not undergone substantive improvements since 1993, despite being systematically criticized by its complex and restrictive guidelines, in particular in what comes to the complex procedures for procuring works and consulting services.

In this respect, Brazil is not alone. Bureaucratic management is the typical budget implementation process in developing countries, and the effects that we find are consistent with those of bureaucratic rigidity and with those of monitoring bureaucratic performance found elsewhere. Different from just channeling resources to end users, bureaucrats have to manage complex relationships with vendors, and must decide across several margins. Our results contribute to highlighting how poor quality of public management – instead of corruption, perhaps – should be considered public enemy number 1 in the developing world.

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Appendix A – Classification lists for the CGU dataset

Table A1 – List of irregularities (2003-2007 audit reports' dataset)

Panel A: Corruption

Category	Irregularity
Procurement	Irregular receipts
Procurement	Evidence for ghost firms
Procurement	Contracts not signed or falsified signatures
Procurement	Favored vendor
Procurement	Lack of publicity
Procurement	Documents set with different dates
Procurement	Other procurement problems
Procurement	Irregular class
Procurement	No realization
Resource diversion	Over-invoicing
Resource diversion	Off-the-record payments

Panel B: Mismanagement

Category	Irregularity
Resource diversion	Unconfirmed payments
Resource diversion	Diversion of resources for other goals
Resource diversion	Diversion of resources for other goals within Health
Resource diversion	Diversion of resources for other goals within Program
Resource diversion	Under-application of resources
Health council	Irregular Composition
Health council	Irregular Operation
Health council	Poor infrastructure and work conditions
Performance	Unaccomplished goals
Performance	Unfinished projects
Performance	Poorly evaluated services to health system users
Supplies and facilities	Precarious facilities
Supplies and facilities	Signs and logos not properly set
Supplies and facilities	Lack of medical supplies
Supplies and facilities	Stock control of medication
Supplies and facilities	Maintenance of medication
Human Resources	Professionals that don't fulfill work time requirements
Human Resources	Staff training
Human Resources	Staff composition
Human Resources	Public servants' payments
Documentation/Accounting	Incomplete documentation or inadequate account keeping

Table A2 – Procurement-intensity by program

Health Ministry Code	Health Program	% of action coded as procurement-related
0119	Sanitation (*Saneamento básico*)	100.00
0004	Quality and efficiency of the Unified Health System (*Qualidade e eficiência do SUS*)	54.17
0005	Pharmaceutical Assistance (*Assistência Farmacêutica*)	50.00
0013	Epidemiological and environmental surveillance in Health (*Vigilância epidemiológica e ambiental em Saúde*)	50.00
0002	Prevention and control of vector-transmitted diseases (*Prevenção e controle de doenças transmitidas por vetores*)	38.46
1214	Basic Attention in Health (*Atenção básica em Saúde*)	4.55
0001	Family Health (*Saúde da família*)	0.00
0023	Admission, emergency and hospital services (*Atendimento ambulatorial, emergencial e hospitalar*)	0.00
1335	Conditional Cash Transfer (*Transferência de renda com condicionalidades*)	0.00

Table A3 – List of procurement-related words

- “Insumos” (Inputs)
- “Implantação” (Adoption of)
- “Modernização” (Modernization of)
- “Adequação” (Tailoring of)
- “Aparelhamento” (Related to equipment/infrastructure)
- “Ampliação” (Enlargement)
- “Manutenção” (Maintenance)
- “Construção” (Construction)
- “Produção” (Production)
- “Aquisição” (Acquisition)
- “Estruturação” (Structuring)

Notes to Table A3:

1. The procurement-related words listed are used to classify each action under each health transfer as procurement-related or not, based on the description of each action in Health Ministry’s *Funcional Programática*;
2. The classification of all actions under the transfers we analyze in this paper is presented in the Supplementary Appendix.

Table A4 – Health Ministry's M&E indicators by transfer

Health Ministry Code	Health Program	Outputs/ Outcomes
0119	Sanitation	(i) Household share connected to the water network; (ii) Household share connected to the sewage network.
0004	Quality and efficiency of the Unified Health System	(i) Hospital beds per thousand inhabitants [*imputed*].
0005	Pharmaceutical Assistance	(i) Distribution of specific medication [*unavailable*].
0013	Epidemiological and environmental surveillance in Health	(i) Immunization shots per thousand inhabitants [*imputed*].
0002	Prevention and control of vector-transmitted diseases	(i) Immunization against Hepatitis B [*unavailable*]; (ii) Incidence of HIV, tuberculosis, and leprosy [*unavailable*];
1214	Basic Attention in Health	(iii) Population share covered by dental health teams [*unavailable*]; (iv) Population share covered by family health teams.
0001	Family Health	(i) Population share covered by family health teams.
0023	Admission, emergency and hospital services	(i) Medical consultations per thousand inhabitants; (ii) Hospital admissions per thousand inhabitants; (iii) Transplantations per thousand inhabitants [*unavailable*].
1335	Conditional Cash Transfer	–

Notes to Table A4:

1. Source of *non-imputted* indicators: Health Ministry M&E Indicators (Caderno de Avaliação Setorial - Ministério da Saúde - Plano Plurianual 2008-2011 - Ano base 2011 - Exercício 2012);
2. In the case of the two programs for which the Health Ministry does not list M&E indicators, we impute outputs/outcomes based on the description of all actions under each transfer in Health Ministry's *Funcional Programática*. *Quality and efficiency of the Unified Health System* includes action 0004.1823 ("aparelhamento de unidades de saúde do SUS", *equipment/infrastructure for public health units*), which we proxy with hospital beds per thousand inhabitants;
3. *Epidemiological and environmental surveillance in Health* includes action 0013.3994 ("modernização do Sistema Nacional de Vigilância em Saúde – VIGISUS[31]", *modernization of the national health surveillance system*), which we proxy with immunization;
4. *Unavailable* indicators have no annual municipal-level data available in DATASUS.

[31] http://www.worldbank.org/projects/P043874/disease-surveillance-control-project-vigisus?lang=en

Appendix B – More details about the Brazilian anti-corruption program

Created in February 2001, CGU is in charge of oversight and fraud detection in every issue related to federal public funds, and it is also responsible for developing mechanisms to prevent corruption. The Brazilian anti-corruption program is a federal government's initiative to inhibit corruption across all levels of the public administration.[32] Ferraz and Finan (2008) perform a thorough analysis of the actual randomness of the program.

The anti-corruption program began with a sample of five municipalities in the first draw. The second draw included 25 municipalities. From the third to the twelfth draws, 50 municipalities were audited, and from the thirteenth on, 60 municipalities are now drawn. The distribution of draws over time and the number of municipalities drawn in each lottery are presented on the CGU website. Although states have also been audited under the program from 2004 on, the focus of the program is on municipalities. The program currently audits municipalities up to 500,000 inhabitants. Maximum population thresholds have increased over time: from 100,000 to 250,000 inhabitants at the third draw; to 300,000 inhabitants between the fourth to the eighth; and, finally, to 500,000 inhabitants for the ninth draw and after. Some draws also had a minimum population threshold of 10,000 inhabitants. The sampling procedure was designed so that the drawn samples are geographically representative, and selection probabilities currently approximate 1% for each of the 5,526 municipalities (which represent over 99% of Brazilian municipalities or about 70% of the country's population) which currently lie within the maximum population eligibility thresholds.

The program investigates earmarked transfers linked to national health and education policies (constitutionally mandated transfers), direct transfers to citizens, and politically negotiated (voluntary) transfers. Once a municipality has been randomly drawn, a service order is issued by CGU, indicating the set of transfers to be audited in each municipality. According to CGU officials interviewed by the authors, service orders encompass every transfer currently at a "critical stage in its life-cycle": (i) disbursement, (ii) procurement, or (iii) approval of previous records by municipal councils. Once service orders have been issued, a team of auditors visits the municipality to investigate irregularities.

For both education and health, all federal transfers are subject to auditing in every municipality drawn, discarding selection concerns with respect to the issuance of service orders. Auditors are then entitled to inspect the complete paper trail of the audited transfers, from the National Treasury's account to its current stage under the municipality's discretion, whether in previous years or under a previous political ruling.

[32] For a full description, see http://www.cgu.gov.br/AreaAuditoriaFiscalizacao/ExecucaoProgramasGoverno/Sorteios/index.asp.

Once the audit is complete, CGU officials describe all irregularities detected for each transfer (if any) in official reports.[33]

Irregularities documented by auditors are followed-up by the public entities responsible for implementing sanctions, including the Prosecutor's Office, the Brazilian Court of Audits ("Tribunal de Contas da União", TCE), the Federal Police, and municipal legislative houses.

[33] Mayors can challenge the conclusions of such reports; when the CGU takes such claims into consideration, a CGU team comes back to the municipality to reassess prior analysis until a final report can be issued.

Appendix C – Examples from audit reports

COMPLIANCE

When evaluating the process of purchasing, stocking, and supplying medical supplies to local health units, auditors found no evidence of irregularity related to the municipal government in Teresina de Goiás, Goiás. The only such case was due to a delay caused by the state government, which failed to transfer resources in due time. We code this finding as a case of compliance (by the municipal government, in 2007). The municipality was drawn in the 23rd round.

MISMANAGEMENT

When evaluating the medication accessibility to citizens of Londrina in 2006, auditors found out that several prescribed drugs in local health units were out of stock. Evidence was provided by interviews with patients and local employees (nurses), who reported lack of, for example, Clorana, Dipirona, Enalapril, Captopril, Cloritisona, Nifedipina, and Istamin. We code this irregularity as lack of medical supplies, an evidence of mismanagement. This irregularity occurred in 2006, in Londrina, Parana, drawn by lottery 21.

CORRUPTION

When analyzing a procurement process to purchase medical supplies in 2004, auditors found that the municipal government of Poloni had paid higher prices for medication than the one agreed upon the public-bid contract. For example, according to receipt number 115655 (Procurement number 2004/01696), the correct price of 150 mg of the medication Ranitidine was R$ 0.18 per tablet, but the municipality paid R$ 0.28 per tablet. No further documentation was presented by the municipal government, and the outbidder Empresa Soquímica Laboratórios Ltda., embezzled the resources. We code this irregularity as overinvoicing, an evidence of corruption. This irregularity occurred in 2004, in Poloni, São Paulo, drawn by lottery 17.

Appendix D – Model

Vendor's maximization problem considering all contracts:

$$\max_{M} M\frac{(N-1)}{N}\pi - \frac{\mathrm{M}^2}{2}$$

FOC:

$$\mathrm{M}^* = \frac{(N-1)}{N}\pi$$

For each contract, solving the model by backward induction:

- V helps and is honest is a dominated strategy in this game
- V helps and is corrupt if: $M \leq \frac{(N-1)}{N}\pi$
- B Requests helps if: $c_B \geq (1-\varepsilon)pR$
- B is Corrupt if: $\alpha\pi \geq \max\{c_B\ ,\ (1-\varepsilon)pR\}$
- B Procures if: $\begin{cases} \pi \geq \frac{1}{\alpha}(pR + c_B - G), if B\ is\ corrupt \\ \varepsilon \leq \frac{G-c_B}{pR}, if\ V\ does\ not\ help \\ always\ , if\ V\ helps\ (since\ G \geq R\ ) \end{cases}$

Comparative statics:

Proposition 1: An increase in perceived audit risk by bureaucrats (weakly) decreases both corruption and public spending.

PROOF:

$$\begin{aligned} Prob(Corruption) \\ &= Prob(B\ Procures|B\ Corrupt) \text{ x } Prob(B\ Corrupt) \\ &+ Prob(B\ Procures|V\ helps)\big(1 - Prob(B\ corrupt)\big)Prob(B\ Requests|V\ helps)Prob(V\ Helps) \end{aligned}$$

$$\begin{aligned} Prob(Corruption) \\ &= Prob\left(\pi \geq \frac{1}{\alpha}(pR + c_B - G)\right) \text{ x } \mathrm{Prob}\left(\pi \geq \frac{1}{\alpha}\max\{c_B\ , (1-\varepsilon)pR\}\right) \\ &+ Prob\left(\pi < \frac{1}{\alpha}(pR + c_B - G)\right) \text{ x } 1\{c_B \geq (1-\varepsilon)pR\} \text{ x } 1\left\{M \leq \frac{(N-1)}{N}\pi\right\} \end{aligned}$$

Since $\pi \sim F(\,)$,

$$Prob(Corruption) = \left[1 - F\left(\frac{1}{\alpha}\max\{c_B\,,(1-\varepsilon)pR\}\right)\right] + F\left(\frac{1}{\alpha}(pR + c_B - G)\right) \text{ x } \left[1\{c_B \geq (1-\varepsilon)pR\} \text{ x } 1\left\{M \leq \frac{(N-1)}{N}\pi\right\} - 1 + F\left(\frac{1}{\alpha}\max\{c_B\,,(1-\varepsilon)pR\}\right)\right]$$

Case 1: $c_B \geq (1-\varepsilon)pR$

$$Prob(Corruption) = \left[1 - F\left(\frac{c_B}{\alpha}\right)\right] + F\left(\frac{1}{\alpha}(pR + c_B - G)\right) \text{ x } \left[1\left\{M \leq \frac{(N-1)}{N}\pi\right\} - 1 + F\left(\frac{c_B}{\alpha}\right)\right]$$

Since $\mathrm{M}^* = \frac{(N-1)}{N}\pi$,

$$Prob(Corruption) = \left[1 - F\left(\frac{c_B}{\alpha}\right)\right] - F\left(\frac{1}{\alpha}(pR + c_B - G)\right) \text{ x } \left[1 - F\left(\frac{c_B}{\alpha}\right)\right] \qquad (A1)$$

Case 2: $c_B < (1-\varepsilon)pR$

$$Prob(Corruption) = \left[1 - F\left(\frac{1}{\alpha}(pR + c_B - G)\right)\right]\left[1 - F\left(\frac{(1-\varepsilon)pR}{\alpha}\right)\right] \qquad (A2)$$

Clearly, $\frac{\partial Prob(Corruption)}{\partial p} < 0$ for both cases.

In what comes to procurement, a higher p makes it less likely that $\pi \geq \frac{1}{\alpha}(pR + c_B - G)$ when B is corrupt, and that $\varepsilon \leq \frac{G - c_B}{pR}$ otherwise. ■

Proposition 2: An increase in perceived audit risk for some of vendor's contracts (weakly) increases both corruption and public spending in contracts elsewhere.

PROOF:

From Proposition 1, $\frac{\partial Prob(Procurement)}{\partial p} \leq 0$. Whenever the inequality holds strictly, we have $M < \mathrm{M}^*$, implying $\frac{\partial Prob(V\ helps)}{\partial p} \geq 0$.

Since when V helps B always procures, this increases the likelihood of procurement. What is more, V helping honestly is a dominated strategy in this game, corruption increases elsewhere to the same extent that procurement increases. Hence, $\frac{\partial Prob(Corruption)}{\partial p} \geq 0$ and $\frac{\partial Prob(Procurement)}{\partial p} \geq 0$. ■

Proposition 3: A decrease in the probability of procurement mistakes (weakly) increases public spending locally, and (weakly) increases corruption and public spending in vendors' contracts elsewhere.

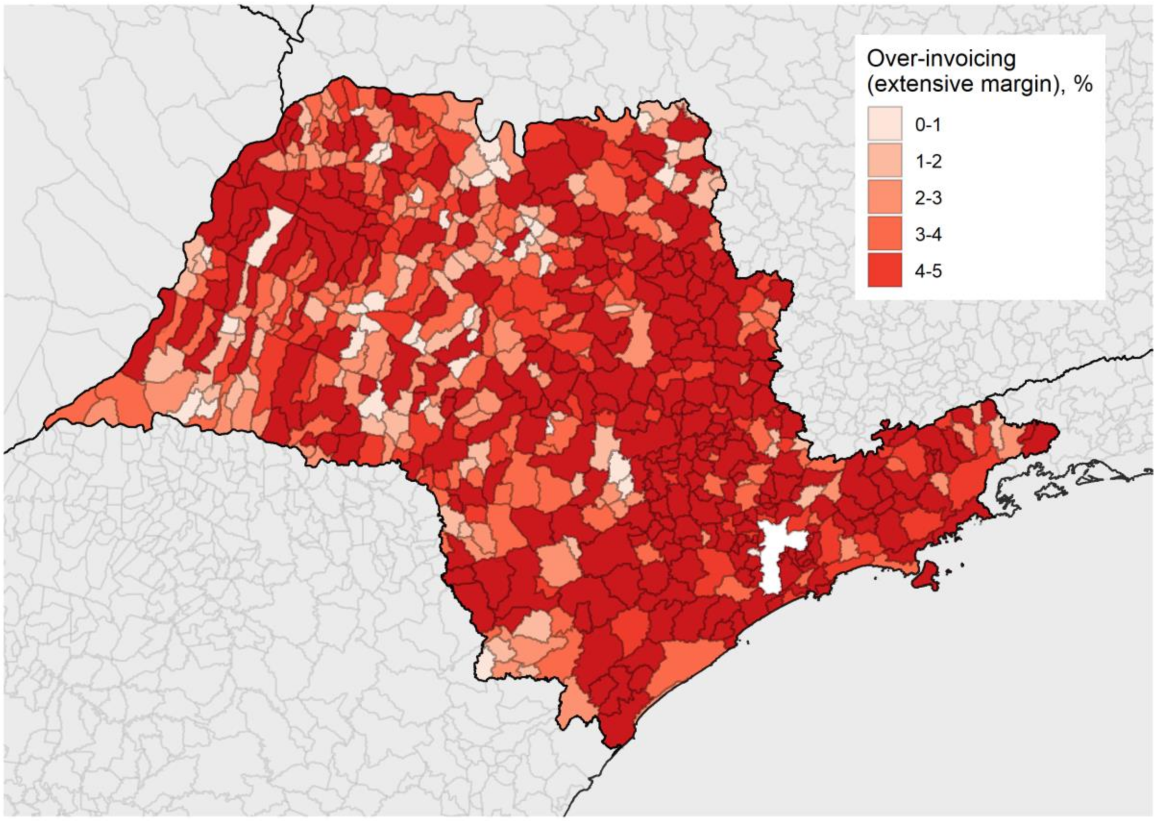
PROOF:

For the first part, a higher ε makes it less likely that $\varepsilon \leq \frac{G-c_B}{pR}$. For the second part, since $\frac{\partial Prob(B\ Requests\ help)}{\partial \varepsilon} \leq 0$, since a higher ε makes it more likely that $c_B \geq (1-\varepsilon)pR$, $\frac{\partial M}{\partial \varepsilon} \leq 0$.

Analogous to the proof of Proposition 2, in contracts elsewhere, $M < \mathrm{M}^*$ with positive probability, implying $\frac{\partial Prob(V\ helps)}{\partial p} \geq 0$. Since when V helps B always procures, this increases the likelihood of procurement. Since V helping honestly is a dominated strategy in this game, corruption increases elsewhere to the same extent that procurement increases. Hence, $\frac{\partial Prob(Corruption)}{\partial p} \geq 0$ and $\frac{\partial Prob(Procurement)}{\partial p} \geq 0$. ■

Appendix E – Figures

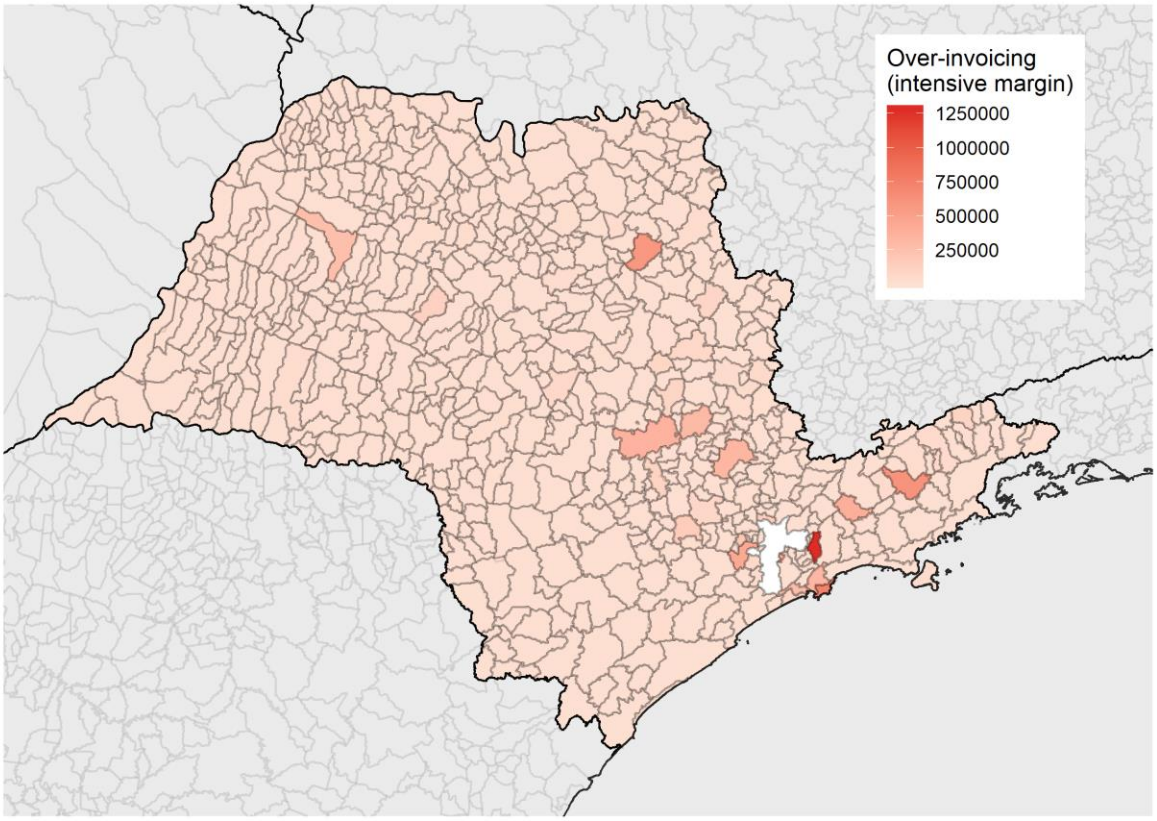
Figure 1 – Distribution of average over-invoicing (extensive margin), 2011-2015



Notes on Figure 1:

1. The map displays the share of contracts characterized by over-invoicing (invoiced amount greater than planned amount net of downward revisions) from 2011 to 2015, for each municipality in our sample;
2. Missing data only for the State capital, which is audited by its own Court of Accounts.

Figure 2 – Distribution of average over-invoicing (intensive margin), 2011-2015

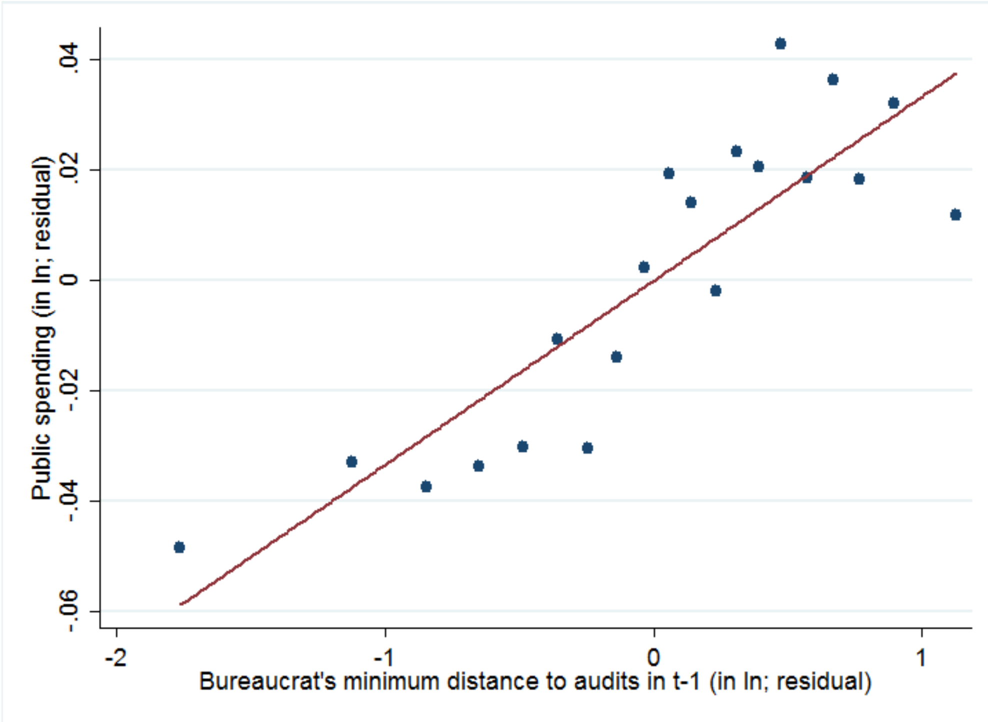


Notes on Figure 2:

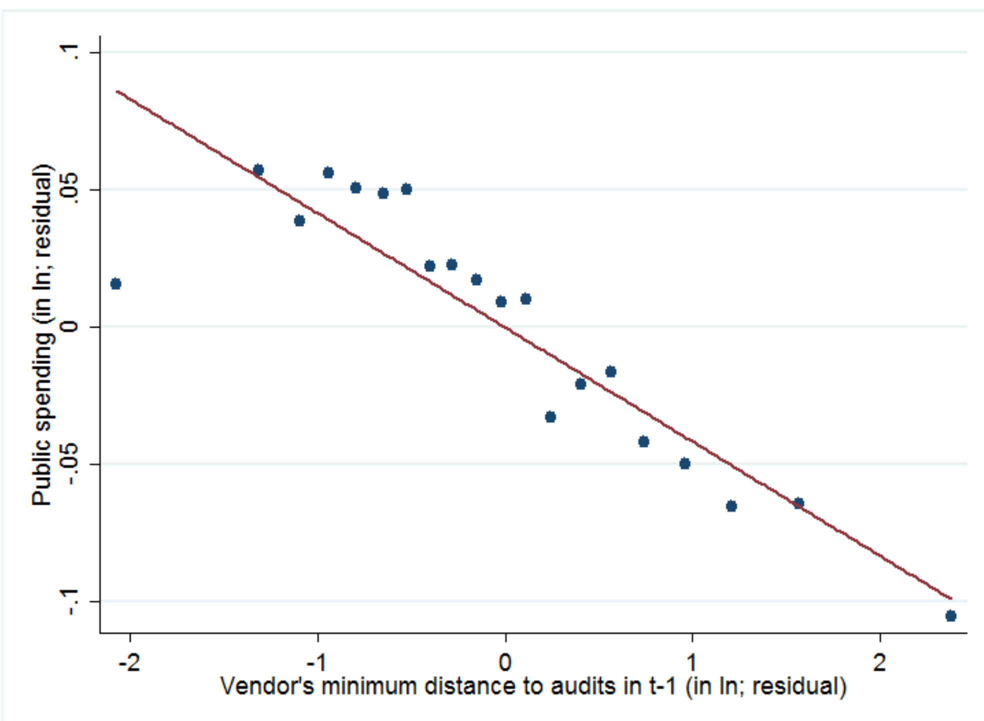
1. The map displays the average amount invoiced above planned amounts (in 2015 R$, deflated by IGP-m) within contracts characterized by over-invoicing (invoiced amount greater than planned amount net of downward revisions) from 2011 to 2015, for each municipality in our sample;
2. Missing data only for the State capital, which is audited by its own Court of Accounts.

Figure 3 – Minimum distance of each contracting party to audits in the previous year and spending

Panel A – Bureaucrats



Panel B – Vendors



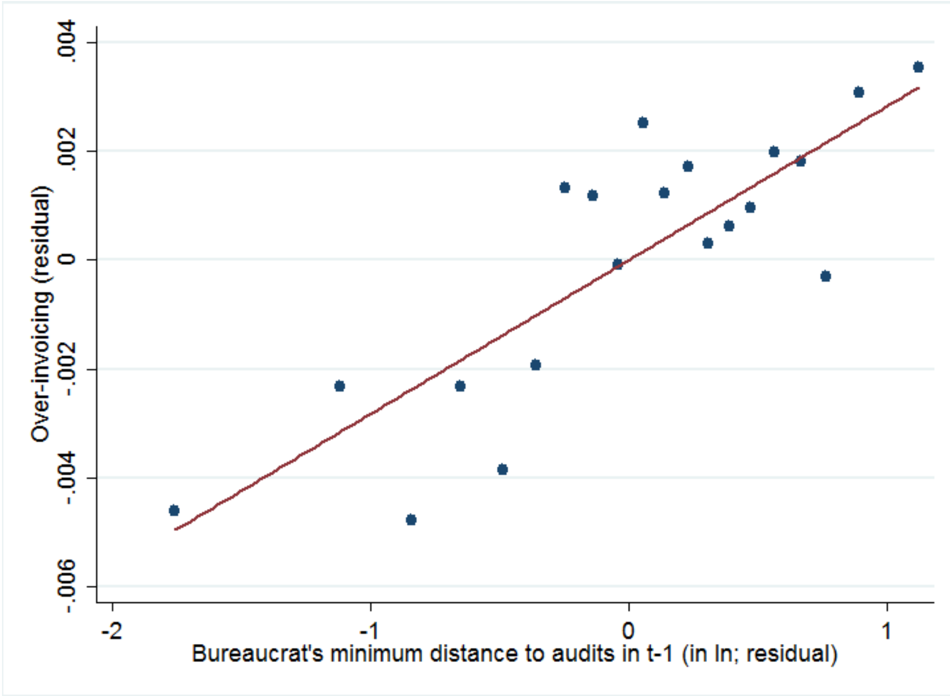
Panel C – Bureaucrats x Vendors

Notes on Figure 3:

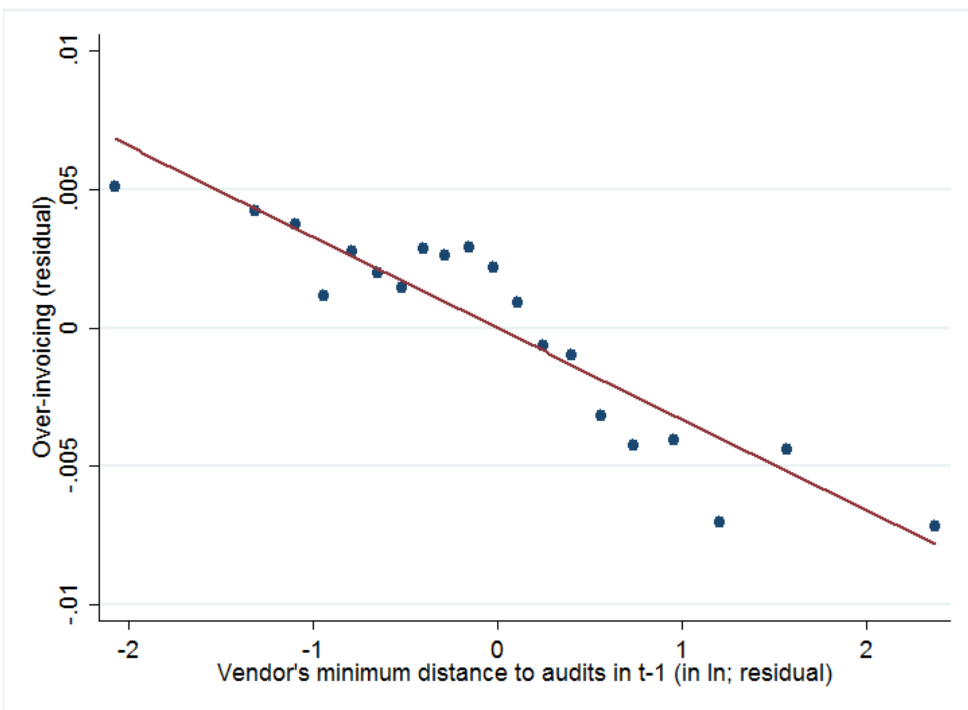
1. Binscatter plot and linear fit of the relationship between municipality's minimum distance to audits in the previous year and spending in the subsequent year (residualized for all other variables in equation (2'), including municipality and year fixed effects);
2. Data includes all municipalities between 2011 and 2015.

Figure 4 – Minimum distance of each contracting party to audits in the previous year and over-invoicing

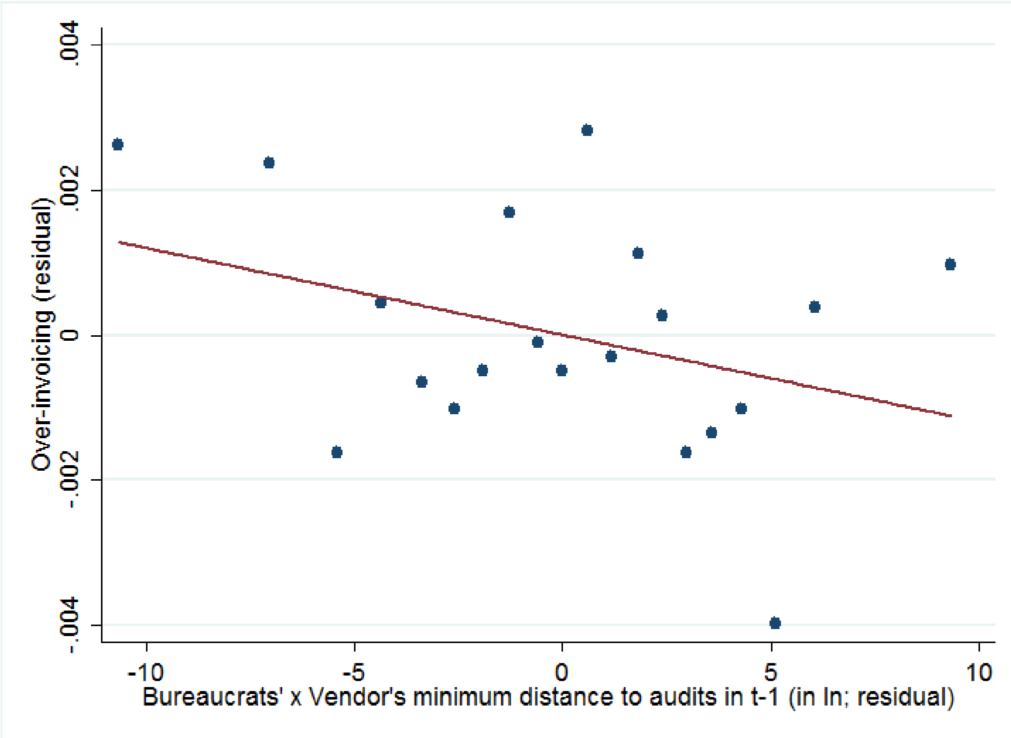
Panel A – Bureaucrats



Panel B – Vendors



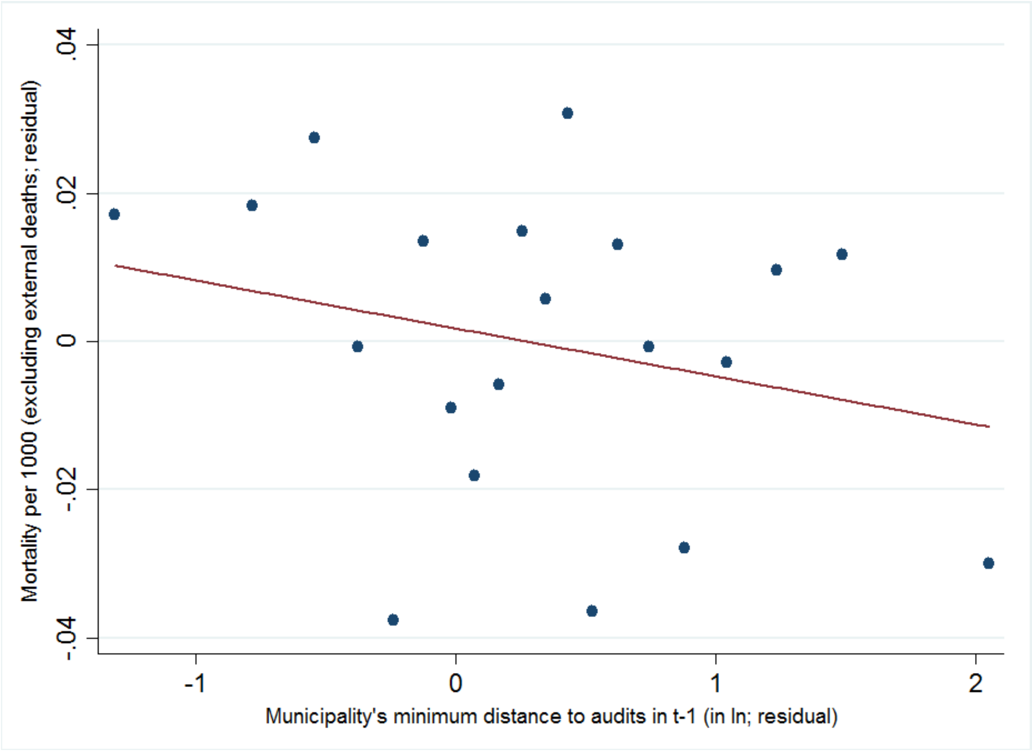
Panel C – Bureaucrats x Vendors



Notes on Figure 4:

1. Binscatter plot and linear fit of the relationship between municipality's minimum distance to audits in the previous year and the extensive margin of over-invoicing in the subsequent year (residualized for all other variables in equation (5'), including municipality and year fixed effects);
2. Data includes all municipalities between 2011 and 2015.

Figure 5 – Minimum distance of municipality to audits in the previous year and mortality



Notes on Figure 5:

1. Binscatter plot and linear fit of the relationship between municipality's minimum distance to audits in the previous year and adult mortality (excluding external causes of death) in the subsequent year (following in equation (10'), residualized for municipality and year fixed effects);
2. Data includes all municipalities between 2004 and 2015.

Appendix F – Balance tests

Table F.1 – Balance tests for past audits, 2011-2015

	Audited in the past = 1	Audited in the past = 0	Difference [1 - 0]
ln(distance to closest State capital)	6.556	6.583	-0.027**
	[0.010]	[0.007]	[0.012]
ln(per capita GDP in 2010)	2.822	2.771	0.051
	[0.028]	[0.019]	[0.034]
ln(population in 2010)	9.762	9.589	0.173***
	[0.052]	[0.035]	[0.063]
Share of public servants in formal employment (in 2009)	0.050	0.043	0.007
	[0.005]	[0.003]	[0.005]
Municipality has a radio station (in 2009)	0.400	0.291	0.109***
	[0.027]	[0.018]	[0.032]
Municipality has internet access (in 2009)	0.663	0.661	0.002
	[0.027]	[0.018]	[0.033]
Municipality has community clubs (in 2009)	0.099	0.053	0.046***
	[0.015]	[0.010]	[0.017]
Literacy rate (2000)	0.008	0.009	-0.001*
	[0.001]	[0.000]	[0.001]
Share of municipal revenues from transfers (in 2010)	0.855	0.863	-0.008
	[0.006]	[0.004]	[0.007]
Share of municipal transfer from SUS (in 2010)	0.000	0.000	-0.000*
	[0.000]	[0.000]	[0.000]
Share of Health transfer from federal government (in 2010)	0.050	0.046	0.004*
	[0.002]	[0.001]	[0.002]
ln(total revenue in 2010, R$)	17.379	17.234	0.145***
	[0.043]	[0.029]	[0.052]
ln(tax revenue in 2010, R$)	14.915	14.781	0.134
	[0.069]	[0.046]	[0.083]
Health council meets regularly (in 2009)	0.981	0.983	-0.002
	[0.008]	[0.005]	[0.009]

Table F.1 (continued) – Balance tests for past audits, 2011-2015

	Audited in the past = 1	Audited in the past = 0	Difference
Mayor serving second term	0.180	0.195	-0.015
	[0.023]	[0.015]	[0.027]
Turnout rate	83.632	84.984	-1.352***
	[0.251]	[0.166]	[0.301]
Electoral margin	37.743	28.879	8.864***
	[1.330]	[0.892]	[1.601]
Mayor from Workers' Party (PT)	0.106	0.105	0.001
	[0.018]	[0.012]	[0.021]
Mayor from the same party as Governor	0.203	0.268	-0.065**
	[0.025]	[0.017]	[0.030]
Mayor's age	48.997	49.381	-0.383
	[0.600]	[0.396]	[0.718]
Male mayor	0.921	0.877	0.045**
	[0.018]	[0.012]	[0.022]
Mayor elementary school drop-out	0.047	0.063	-0.016
	[0.014]	[0.009]	[0.016]
Mayor high-school drop-out	0.018	0.013	0.004
	[0.007]	[0.005]	[0.008]
Mayor high-school graduate	0.881	0.889	-0.007
	[0.018]	[0.012]	[0.022]

Notes on Table F.1:

1. Columns (1) and (2) present the weighted averages of each covariate for municipalities with and without past audits, respectively, with the number of contracts in each municipality-year as weights. Column (3) presents the unconditional difference between the averages of the two groups for each covariate;
2. Robust standard errors in brackets;
3. *** p<0.01, ** p<0.05, * p<0.1.

Table F.2 – Balance tests for audits within 75km in the previous year, 2011-2015

	Audits within 75 km in t-1 = 1	Audits within 75 km in t-1 = 0	Difference [1 - 0]	Difference [1 - 0] (mun & year FE)
ln(distance to closest State capital)	6.563	6.610	-0.047***	-
	[0.004]	[0.004]	[0.006]	
ln(per capita GDP in 2010)	2.929	2.872	0.057***	-
	[0.015]	[0.014]	[0.020]	
ln(population in 2010)	10.198	10.040	0.158***	-
	[0.039]	[0.038]	[0.054]	
Share of public servants in formal	0.041	0.041	-0.001	-
employment (in 2009)	[0.002]	[0.002]	[0.002]	
Municipality has a radio station	0.420	0.400	0.020	-
(in 2009)	[0.013]	[0.012]	[0.018]	
Municipality has internet access	0.691	0.688	0.003	-
(in 2009)	[0.012]	[0.012]	[0.017]	
Municipality has community clubs	0.230	0.208	0.022	-
(in 2009)	[0.011]	[0.010]	[0.015]	
Literacy rate (2000)	0.008	0.008	-0.000	-
	[0.000]	[0.000]	[0.000]	
Share of municipal revenues from	0.793	0.813	-0.020***	-
transfers (in 2010)	[0.004]	[0.004]	[0.006]	
Share of municipal transfer from SUS	0.00	0.000	-0.000*	-
(in 2010)	[0.000]	[0.000]	[0.000]	
Share of Health transfer from federal	0.057	0.060	-0.003**	-
government (in 2010)	[0.001]	[0.001]	[0.001]	
ln(total revenue in 2010, R$)	17.872	17.719	0.153***	-
	[0.037]	[0.036]	[0.051]	
ln(tax revenue in 2010, R$)	15.593	15.393	0.200***	-
	[0.053]	[0.051]	[0.073]	
Health council meets regularly	0.990	0.982	0.008*	-
(in 2009)	[0.003]	[0.003]	[0.004]	

Table F.2 (continued) – Balance tests for audits within 75km in the previous year, 2011-2015

	Audits within 75 km in t-1 = 1	Audits within 75 km in t-1 = 0	Difference [1 - 0]	Difference [1 - 0] (mun. & year FE)
Mayor serving second term	0.221	0.197	0.024	-0.004
	[0.011]	[0.010]	[0.015]	[0.014]
Turnout rate	85.335	84.123	1.212***	0.017
	[0.107]	[0.103]	[0.149]	[0.034]
Electoral margin	29.746	30.026	-0.280	-0.364
	[0.608]	[0.581]	[0.841]	[0.608]
Mayor from Workers' Party (PT)	0.118	0.125	-0.007	-0.006
	[0.008]	[0.008]	[0.012]	[0.008]
Mayor from the same party as	0.289	0.284	0.005	-0.000
Governor	[0.012]	[0.011]	[0.016]	[0.012]
Mayor's age	49.642	49.929	-0.287	-0.113
	[0.258]	[0.248]	[0.358]	[0.249]
Male mayor	0.902	0.900	0.001	-0.007
	[0.008]	[0.007]	[0.011]	[0.007]
Mayor elementary school drop-out	0.038	0.042	-0.005	-0.002
	[0.005]	[0.005]	[0.007]	[0.005]
Mayor high-school drop-out	0.011	0.012	-0.001	0.002
	[0.003]	[0.003]	[0.004]	[0.003]
Mayor high-school graduate	0.902	0.900	0.003	-0.002
	[0.008]	[0.007]	[0.011]	[0.008]

Notes on Table F.2:

1. Columns (1) and (2) present the weighted averages of each covariate for municipalities with and without audits within 75 km in the previous year, respectively, with the number of contracts in each municipality-year as weights. Column (3) presents the unconditional difference between the averages of the two groups for each covariate, and column (4) presents the within municipality difference between groups also controlling for year fixed-effects;
2. Robust standard errors in brackets;
3. *** p<0.01, ** p<0.05, * p<0.1.

Table F.3 – Balance tests for audits within 75km in the previous year, 2003-2007

	Audits within 75 km in t-1 = 1	Audits within 75 km in t-1 = 0	Difference [1 - 0]	Difference [1 - 0] (mun. & year FE)
ln(distance to closest State capital)	2'190	2'336	-0.068***	-
	[0.018]	[0.021]	[0.008]	
ln(per capita GDP in 2000)	0.535	0.556	0.047**	-
	[0.016]	[0.018]	[0.024]	
ln(population in 2000)	4'193	4'246	0.138*	-
	[0.022]	[0.025]	[0.071]	
Share of public servants in formal employment (in 2001)	0.042	0.035	-0.000	-
	[0.001]	[0.001]	[0.000]	
Municipality has a radio station (in 2001)	0.529	0.602	-0.009	-
	[0.025]	[0.028]	[0.007]	
Municipality has internet access (in 2001)	0.182	0.193	-0.001	-
	[0.019]	[0.022]	[0.001]	
Municipality has community clubs (in 2001)	0.629	0.676	0.003	-
	[0.024]	[0.027]	[0.003]	
Literacy rate (2000)	75'012	76'711	0.092	-
	[0.672]	[0.757]	[0.066]	
Share of municipal revenues from transfers (in 2000)	0.821	0.815	0.176*	-
	[0.006]	[0.007]	[0.102]	
Share of municipal transfer from SUS (in 2000)	0.097	0.092	0.006	-
	[0.003]	[0.004]	[0.019]	
Share of Health transfer from federal government (in 2000)	0.969	0.959	1.240***	-
	[0.005]	[0.005]	[0.196]	
ln(total revenue in 2000, R$)	15'708	15'827	-0.617	-
	[0.047]	[0.054]	[1.087]	
ln(tax revenue in 2000, R$)	11'539	11'942	-0.003	-
	[0.097]	[0.111]	[0.015]	
Health council meets regularly (in 2000)	0.887	0.829	0.024	-
	[0.018]	[0.020]	[0.021]	

Table F.3 (continued) – Balance tests for audits within 75km in the previous year, 2003-2007

	Audits within 75 km in t-1 = 1	Audits within 75 km in t-1 = 0	Difference [1 - 0]	Difference [1 - 0] (mun. & year FE)
Mayor serving second term	0.232	0.297	-0.251	0.010
	[0.019]	[0.016]	[0.458]	[0.025]
Turnout rate	0.868	0.852	0.007	0.866***
	[0.003]	[0.002]	[0.014]	[0.116]
Electoral margin	0.150	0.148	-0.006	-1.034
	[0.006]	[0.005]	[0.010]	[1.119]
Mayor from Workers' Party (PT)	0.063	0.045	-0.002	0.004
	[0.009]	[0.008]	[0.005]	[0.014]
Mayor from the same party as Governor	0.207	0.240	0.008	0.014
	[0.018]	[0.015]	[0.014]	[0.021]
Mayor's age	49'244	48'527	0.010	-0.268
	[0.408]	[0.342]	[0.025]	[0.453]
Male mayor	0.907	0.943	0.866***	-0.003
	[0.011]	[0.009]	[0.116]	[0.013]
Mayor elementary school drop-out	0.219	0.280	-1.034	-0.003
	[0.018]	[0.015]	[1.119]	[0.010]
Mayor high-school drop-out	0.148	0.138	0.004	0.004
	[0.015]	[0.012]	[0.014]	[0.006]
Mayor high-school graduate	0.219	0.280	0.014	-0.004
	[0.018]	[0.015]	[0.021]	[0.014]

Notes on Table F3:

1. Columns (1) and (2) present the weighted averages of each covariate for municipalities with and without audits within 75 km in the previous year, respectively, with the number of investigations at each municipality-year used as weights. Column (3) presents the unconditional difference between the averages of the two groups for each covariate, and column (4) presents the within municipality difference between groups also controlling for year fixed-effects;
2. Robust standard errors in brackets;
3. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table F.4 – Balance tests for trainings in the previous year (within eligible), 2011-2015

	FGP in the past = 1	FGP in the past = 0	Difference [1 - 0]
ln(distance to closest State capital)	6.576	6.574	0.001
	[0.014]	[0.007]	[0.015]
ln(per capita GDP in 2010)	2.845	2.774	0.071
	[0.039]	[0.020]	[0.044]
ln(population in 2010)	9.660	9.634	0.026
	[0.072]	[0.036]	[0.080]
Share of public servants in formal employment (in 2009)	0.069	0.040	0.029***
	[0.006]	[0.003]	[0.007]
Municipality has a radio station (in 2009)	0.451	0.289	0.162***
	[0.037]	[0.018]	[0.041]
Municipality has internet access (in 2009)	0.676	0.661	0.014
	[0.038]	[0.019]	[0.042]
Municipality has community clubs (in 2009)	0.026	0.076	-0.050**
	[0.020]	[0.010]	[0.022]
Literacy rate (2000)	0.009	0.008	0.000
	[0.001]	[0.000]	[0.001]
Share of municipal revenues from transfers (in 2010)	0.864	0.860	0.004
	[0.008]	[0.004]	[0.009]
Share of municipal transfer from SUS (in 2010)	0.000	0.000	-0.000
	[0.000]	[0.000]	[0.000]
Share of Health transfer from federal government (in 2010)	0.051	0.046	0.006*
	[0.003]	[0.001]	[0.003]
ln(total revenue in 2010, R$)	17.284	17.275	0.009
	[0.060]	[0.030]	[0.067]
ln(tax revenue in 2010, R$)	14.761	14.835	-0.074
	[0.095]	[0.048]	[0.106]
Health council meets regularly (in 2009)	1.000	0.978	0.022*
	[0.010]	[0.005]	[0.012]

Table F.4 (continued) – Balance tests for trainings in the previous year (within eligible), 2011-2015

	FGP in the past = 1	FGP in the past = 0	Difference [1 - 0]
Mayor serving second term	0.253	0.183	0.070**
	[0.032]	[0.016]	[0.035]
Turnout rate	84.699	84.722	-0.023
	[0.350]	[0.175]	[0.392]
Electoral margin	31.999	31.483	0.516
	[1.893]	[0.930]	[2.109]
Mayor from Workers' Party (PT)	0.033	0.125	-0.092***
	[0.024]	[0.012]	[0.027]
Mayor from the same party as Governor	0.236	0.261	-0.025
	[0.035]	[0.017]	[0.039]
Mayor's age	47.187	49.892	-2.705***
	[0.818]	[0.409]	[0.915]
Male mayor	0.879	0.894	-0.015
	[0.025]	[0.012]	[0.028]
Mayor elementary school drop-out	0.066	0.058	0.008
	[0.019]	[0.009]	[0.021]
Mayor high-school drop-out	0.000	0.019	-0.019*
	[0.010]	[0.005]	[0.011]
Mayor high-school graduate	0.877	0.888	-0.011
	[0.025]	[0.013]	[0.028]

Notes on Table F.4:

1. Columns (1) and (2) present the weighted averages of each covariate for municipalities with and without past audits, respectively, with the number of contracts in each municipality-year as weights. Column (3) presents the unconditional difference between the averages of the two groups for each covariate;
2. Robust standard errors in brackets;
3. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix G – Tables

Table 1 – Effects of past audits on public spending (contract-level data within São Paulo State)

	log(Payed) [2011-2015]			
	Transfers		Own resources	
	All contracts	Contracts with over-invoicing	All contracts	Contracts with over-invoicing
	(1)	(2)	(3)	(4)
Bureaucrat audited in the past	-0.126**	-0.474***	-0.434***	-0.892***
	[0.055]	[0.178]	[0.039]	[0.125]
Vendor audited in the past	0.230***	-0.108*	0.086**	-0.057
	[0.036]	[0.064]	[0.042]	[0.099]
Bureaucrat audited in the past	0.035	0.483***	0.462***	0.914***
x Vendor audited in the past	[0.043]	[0.178]	[0.035]	[0.132]
Program category fixed-effects	Yes	Yes	Yes	Yes
Municipality fixed-effects	No	No	No	No
Controls	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes
Dependent Variable Mean	8.296	10.06	7.990	9.805
Observations	611,558	34,348	1,790,034	96,452
R-squared	0.095	0.153	0.099	0.101

Notes on Table 1:

1. All columns are Ordinary Least Squares (OLS) regressions with dependent variable equal to the amount payed for the contract between the bureaucrat and the vendor at that given year (in natural logarithms);
2. The past audits' indicator for bureaucrats equals 1 if the municipality was audited at any prior year, and 0 otherwise. The past audits' indicator for vendors equals 1 if *any* municipality where vendors had active contracts in the past was audited at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F);
4. Robust standard errors in brackets, clustered at the municipality and year levels;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 2 – Effects of audits within 75km in the previous year on public spending
(contract-level data within São Paulo State)

	log(Payed) [2011-2015]					
	Transfers			Own resources		
	All contracts	All contracts	Contracts with over-invoicing	All contracts	All contracts	Contracts with over-invoicing
	(1)	(2)	(3)	(4)	(5)	(6)
Audits in t-1 within 75 km of Bureaucrat	-0.187***	-0.192***	-0.559***	-0.376***	-0.377***	-0.786***
	[0.024]	[0.024]	[0.085]	[0.022]	[0.022]	[0.071]
Audits in t-1 within 75 km of Vendor	0.446***	0.446***	-0.062	0.344***	0.347***	0.156
	[0.039]	[0.039]	[0.077]	[0.054]	[0.054]	[0.113]
Audits in t-1 within 75 km of Bureaucrat	-0.001	0.002	0.678***	0.260***	0.262***	0.789***
x Audits in t-1 within 75 km of Vendor	[0.034]	[0.035]	[0.097]	[0.041]	[0.041]	[0.098]
Program category fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	8.246	8.262	10.02	7.927	7.942	9.768
Observations	710,646	674,429	36,283	2,114,580	2,003,921	101,208
R-squared	0.131	0.132	0.149	0.123	0.124	0.139

Notes on Table 2:

1. All columns are Ordinary Least Squares (OLS) regressions with dependent variable equal to the amount payed for the contract between the bureaucrat and the vendor at that given year (in natural logarithms);
2. The recent nearby audits' indicator for bureaucrats equals 1 if there were audits within 75km of municipality's centroid in the previous year, and 0 otherwise. The recent nearby audits' indicator for vendors equals 1 if there were audits in the previous year within 75km of the centroid of *any* municipality where vendors had active contracts at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F);
4. Robust standard errors in brackets, clustered at the municipality and year levels;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 3 – Effects of audits within 75km in the previous year on public spending (audit reports)

	log(audited amount)		
	[2003-2007]	[1997-2007]	
	(1)	(2)	(3)
Audits in t -1 within 75 km	0.058	0.152**	0.119
	[0.080]	[0.072]	[0.073]
Audits in t -1 within 75 km	-0.353***	-0.353***	-0.354***
x Procurement-intensive	[0.071]	[0.069]	[0.069]
Procurement-intensive	-1.011***	-0.436***	-0.434***
	[0.047]	[0.098]	[0.100]
Post-2003		-0.564***	-0.564***
x Procurement-intensive		[0.106]	[0.109]
Controls	No	No	Yes
Municipality fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Dependent Variable Mean	10.82	10.92	10.90
Observations	9,080	10,527	10,398
R-squared	0.402	0.413	0.403

Notes on Table 3:

1. All columns are Ordinary Least Squares (OLS) regressions with dependent variable equal to the audited amount within that investigation (in natural logarithms);
2. The recent nearby audits' indicator for bureaucrats equals 1 if there were audits within 75km of municipality's centroid in the previous year, and 0 otherwise. The recent nearby audits' indicator for vendors equals 1 if there were audits in the previous year within 75km of the centroid of *any* municipality where vendors had active contracts at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.2 (See Appendix F);
4. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A. Procurement-intensive equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
5. Robust standard errors in brackets, clustered at the municipality and year levels;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 – Net effects of past audits on over-invoicing: extensive margin
(contract-level data within São Paulo State)

	Share of contracts	Over-invoicing [2011-2015]							
		Transfers				Own resources			
		Without upward revisions		With upward revisions		Without upward revisions		With upward revisions	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bureaucrat audited in the past	1.42%	-0.003	-0.025***	-0.001	-0.007***	-0.002	-0.027***	0.001	-0.006***
		[0.003]	[0.003]	[0.001]	[0.002]	[0.002]	[0.002]	[0.001]	[0.001]
Vendor audited in the past	44.75%		0.003		0.003***		0.005**		0.003***
			[0.002]		[0.001]		[0.003]		[0.001]
Bureaucrat audited in the past x Vendor audited in the past	24.90%		0.022***		0.006***		0.024***		0.006***
			[0.003]		[0.002]		[0.002]		[0.001]
Predicted net effect of audits (%)		-6.01%	0.65%	-3.43%	7.07%	-4.22%	3.07%	1.52%	9.56%
p-value [Predicted net effect = 0]		0.314	0.791	0.556	0.075	0.352	0.264	0.522	0.000
Program category fixed-effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed-effects		No	No	No	No	No	No	No	No
Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean		0.0499	0.0523	0.0189	0.0198	0.0474	0.0508	0.0173	0.0185
Observations		738,751	666,704	738,758	666,709	2,177,337	1,938,831	2,177,343	1,938,837
R-squared		0.004	0.004	0.001	0.001	0.005	0.006	0.002	0.002

Notes on Table 4:

1. All columns are Ordinary Least Squares (OLS) regressions with dependent variable equal to 1 if the contract invoiced an amount greater than its planned budget (net of downward revisions) at that year, and 0 otherwise;
2. The past audits' indicator for bureaucrats equals 1 if the municipality was audited at any prior year, and 0 otherwise. The past audits' indicator for vendors equals 1 if *any* municipality where vendors had active contracts in the past was audited at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F);
4. Robust standard errors in brackets, clustered at the municipality and year levels;
5. Predicted net effect = $\frac{\sum s_k \widehat{\beta}_k}{\sum s_k}$, where s_k is the share of contracts of configuration k, and $\hat{\beta}_k$ is the effect estimated within contracts of that configuration;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 – Net effects of past audits on over-invoicing: intensive margin
(contract-level data within São Paulo State)

	Share of contracts	Amount embezzled [2011-2015]			
		Transfers		Own resources	
		Accounting for spillovers?		Accounting for spillovers?	
		No	Yes	No	Yes
		(1)	(2)	(3)	(4)
Bureaucrat audited in the past	1.4%	-774.968*	-1217.473**	-908.665**	-1303.552**
		[433.444]	[495.292]	[448.013]	[527.928]
Vendor audited in the past	44.7%		-278.848		99.224
			[585.648]		[498.088]
Bureaucrat audited in the past x Vendor audited in the past	24.9%		458.206		266.663
			[628.741]		[597.319]
Predicted net effect of audits (%)		-51.29%	-28.12%	-87.97%	-21.26%
p-value [Predicted net effect = 0]		0.135	0.187	0.072	0.243
Program category fixed-effects		Yes	Yes	Yes	Yes
Municipality fixed-effects		No	No	No	No
Controls		Yes	Yes	Yes	Yes
Year fixed-effects		Yes	Yes	Yes	Yes
Dependent Variable Mean		1511	1657	1384	1537
Observations		738,751	666,704	2,177,337	1,938,831
R-squared		0.001	0.001	0.001	0.001

Notes on Table 5:

1. All columns are Ordinary Least Squares (OLS) regressions with dependent variable equal to the amount invoiced subtracted from the amount planned (net of downward revisions), when such difference is positive, and 0 otherwise;
2. The past audits' indicator for bureaucrats equals 1 if the municipality was audited at any prior year, and 0 otherwise. The past audits' indicator for vendors equals 1 if *any* municipality where vendors had active contracts in the past was audited at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F);
4. Robust standard errors in brackets, clustered at the municipality and year levels;
5. Predicted net effect $= \frac{\sum s_k \hat{\beta}_k}{\sum s_k}$, where s_k is the share of contracts of configuration k, and $\hat{\beta}_k$ is the effect estimated within contracts of that configuration;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 6 – Effects of audits within 75km in the previous year on corruption (audit reports)

	Share of investigations coded as corruption		
	[2003-2007]	[1997-2007]	
	(1)	(2)	(3)
Audits in t -1 within 75 km	0.004	0.005	0.004
	[0.022]	[0.022]	[0.022]
Audits in t -1 within 75 km	-0.037**	-0.032**	-0.032**
x Procurement-intensive	[0.016]	[0.016]	[0.016]
Procurement-intensive	0.024*	0.186***	0.189***
	[0.013]	[0.031]	[0.032]
Post-2003		-0.167***	-0.169***
x Procurement-intensive		[0.032]	[0.033]
Controls	No	No	Yes
Municipality fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Dependent Variable Mean	0.127	0.153	0.151
Observations	9,080	10,527	10,398
R-squared	0.177	0.194	0.196

<u>Notes on Table 6:</u>

1. All columns are Ordinary Least Squares (OLS) regressions with dependent variable equal to 1 if the investigation was coded as evidence of corruption, and 0 otherwise. See Appendix A for the classification list of irregularities coded as corruption;
2. The recent nearby audits' indicator for bureaucrats equals 1 if there were audits within 75km of municipality's centroid in the previous year, and 0 otherwise. The recent nearby audits' indicator for vendors equals 1 if there were audits in the previous year within 75km of the centroid of *any* municipality where vendors had active contracts at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.2 (See Appendix F);
4. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A. Procurement-intensive equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
5. Robust standard errors in brackets, clustered at the municipality and year levels;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 7 – Heterogeneous treatment effects of audits by vendors' baseline network characteristics

	[2011-2015]								
	log(Payed)			Over-invoicing: extensive margin			Over-invoicing: intensive margin		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vendor audited in the past	0.166***	0.605***	3.003***	0.009***	0.005	0.060***	1,439.931	10,506.068	19,469.651***
	[0.056]	[0.101]	[0.294]	[0.003]	[0.010]	[0.023]	[1,342.563]	[9,231.491]	[7,419.915]
Vendor audited in the past	0.002			0.001			-180.918		
x Program cats. with contracts for municipality-vendor (2010)	[0.028]			[0.002]			[375.640]		
Program cats. with contracts for municipality-vendor (2010)	0.064***			0.010***			602.382		
	[0.014]			[0.001]			[376.632]		
Vendor audited in the past		-0.049***			0.000			-1,040.893	
x Spending within municipality-vendor contracts (in ln; 2010)		[0.011]			[0.001]			[1,044.162]	
Spending within municipality-vendor contracts (in ln; 2010)		0.611***			0.019***			3,054.147***	
		[0.007]			[0.001]			[1,104.665]	
Vendor audited in the past			0.084			0.004*			-233.525
x Number of municipal contractors (in ln; 2010)			[0.057]			[0.002]			[755.667]
Number of municipal contractors (in ln; 2010)			-0.842***			-0.019***			-4,293.820**
			[0.098]			[0.005]			[2,039.074]
Program category fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed-effects	No	No	No	No	No	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	8.20	8.20	8.07	0.06	0.06	0.05	2417.00	2397.00	1579.00
Observations	1,363,953	1,292,960	2,374,010	1,449,285	1,369,172	2,575,751	1,449,285	1,369,172	2,575,751
R-squared	0.103	0.391	0.099	0.012	0.035	0.005	0.001	0.002	0.001

Notes on Table 7:

1. All columns are Ordinary Least Squares (OLS) regressions. Cols. (1) to (3) have dependent variable equal to the amount payed for the contract between the bureaucrat and the vendor at that given year (in natural logarithms); Cols. (4) to (6) have dependent variable equal to 1 if the contract invoiced an amount greater than its planned budget (net of downward revisions) at that year, and 0 otherwise; and Cols. (7) to (9) have dependent variable equal to the amount invoiced subtracted from the amount planned (net of downward revisions), when such difference is positive, and 0 otherwise;
2. The past audits' indicator for vendors equals 1 if *any* municipality where vendors had active contracts in the past was audited at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F), the past audits' indicator for bureaucrats, and its interaction with the past audits' indicator for vendors (not shown for ease of display);

4. Robust standard errors in brackets, clustered at the municipality and year levels;
5. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8 – Effects of audits within 75km in the previous year on implementation quality (audit reports)

	Mismanagement categories [2003-2007]					
	(1)	(2)	(3)	(4)	(5)	(6)
	Resource diversion	Health council problems	Performance problems	Infrastr. and stock problems	Human resources problems	Documentation problems
Audits in t -1 within 75 km	-0.002	0.008	0.002	-0.004	-0.006	-0.017
	[0.019]	[0.009]	[0.017]	[0.019]	[0.012]	[0.020]
Audits in t -1 within 75 km	0.004	-0.007	0.034*	0.019	0.015	0.002
x Procurement-intensive	[0.017]	[0.007]	[0.020]	[0.022]	[0.013]	[0.018]
Procurement-intensive	-0.069***	-0.050***	0.034**	0.221***	-0.008	-0.109***
	[0.010]	[0.005]	[0.014]	[0.014]	[0.008]	[0.013]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.148	0.0323	0.162	0.180	0.0833	0.187
Observations	9,080	9,080	9,080	9,080	9,080	9,080
R-squared	0.151	0.113	0.123	0.187	0.124	0.134

Notes on Table 8:

1. Columns (1) to (6) are Ordinary Least Squares (OLS) regressions with dependent variable equal to 1 if the investigation is coded as the mismanagement category which labels the column, and 0 otherwise. See Appendix A for the definition of all mismanagement categories;
2. The recent nearby audits' indicator for bureaucrats equals 1 if there were audits within 75km of municipality's centroid in the previous year, and 0 otherwise. The recent nearby audits' indicator for vendors equals 1 if there were audits in the previous year within 75km of the centroid of *any* municipality where vendors had active contracts at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.2 (See Appendix F);
4. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A. Procurement-intensive equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
5. Robust standard errors in brackets, clustered at the municipality and year levels;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 9 – Effects of audits within 75km in the previous year on Health outputs and outcomes

Panel A: Transfer-specific outputs and outcomes (GLS, 2003-2007)

	Low procurement-intensity		High procurement-intensity				Summary Measure
	Medical Consultations	Family Health coverage	Immunization per 1,000	Hospital Beds per 1,000	Adequate Water	Adequate Sanitation	Z-score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Audits in t -1 within 75 km	0.008	0.003					0.061
	[0.456]	[0.017]					[0.073]
Audits in t -1 within 75 km			-1.566**	-0.260***	-0.001	-0.002	-0.198***
x Procurement Intensive			[0.793]	[0.074]	[0.003]	[0.005]	[0.064]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	1.247	0.696	79.33	2.022	0.236	0.613	0.00984
Observations	953	1,026	1,105	302	1,019	1,019	2,084
R-squared	0.551	0.920	0.800	0.998	0.996	0.984	0.501

Panel B: Mortality (z-scores)

	Child mortality	Deaths (excl. external causes)	Placebo: Deaths by external causes
	(1)	(2)	(3)
Audits in t -1 within 75 km	0.015	0.011*	0.001
	[0.009]	[0.006]	[0.008]
Audits in t -2 within 75 km	0.009	-0.002	0.007
	[0.010]	[0.006]	[0.009]
p-value [Effect of consecutive audits = 0]	0.106	0.326	0.534
Municipality fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Dependent Variable Mean	0.000	0.000	0.000
Observations	66,495	66,751	66,751
R-squared	0.165	0.698	0.327

Notes on Table 9:

1. In Panels A and B, dependent variables are child mortality per 1000 born, % under 1- year-old children with malnutrition, % under 2- year-old children with diarrhea, immunization shots per 1,000 inhabitants, hospital beds per 1000 inhabitants and consultations per capita;
2. Panel A shows results for the effect of audits on outcomes and outputs from 2003-2007 within the municipalities audited by the random audits program in that period. Panel B shows results for the effect of audits on outcomes and outputs for the dataset from 2011-2015 within the State of São Paulo.
3. In Panel A, columns (1) to (6) are Generalized Least Squares (GLS) regressions, with the number of investigations in each municipality and year used as weights. In Panel B, columns (1) to (6) are Ordinary Least Squares (OLS) regressions;
4. Robust standard errors in brackets;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 10 – Effects of audits within 75km in the previous year on budget execution stages (contract-level data within São Paulo State)

	[2011-2015]		
	log(Planned)	Delivery rate	Payment rate
	(1)	(2)	(3)
Bureaucrat audited in the past	-0.317***	0.010	-0.021***
	[0.040]	[0.017]	[0.006]
Vendor audited in the past	0.167***	0.000	-0.012***
	[0.036]	[0.004]	[0.002]
Bureaucrat audited in the past	0.304***	-0.005	0.026***
x Vendor audited in the past	[0.032]	[0.015]	[0.003]
Program category fixed-effects	Yes	Yes	Yes
Municipality fixed-effects	No	No	No
Controls	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Dependent Variable Mean	8.175	0.979	0.924
Observations	2,605,542	2,524,003	2,498,414
R-squared	0.099	0.000	0.017

Notes on Table 10:

1. All columns are Ordinary Least Squares (OLS) regressions. Col. (1) has dependent variable equal to the amount payed for the contract between the bureaucrat and the vendor at that given year (in natural logarithms); Col. (2) has dependent variable equal to the ratio of invoiced to planned amount (net of downward revisions); and Col. (3) has dependent variable equal to the ratio of payed to invoiced amounts;
2. The past audits' indicator for bureaucrats equals 1 if the municipality was audited at any prior year, and 0 otherwise. The past audits' indicator for vendors equals 1 if *any* municipality where vendors had active contracts in the past was audited at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F);
4. Robust standard errors in brackets, clustered at the municipality and year levels;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 11 – Effects of randomly assigned trainings on public spending and corruption (among eligible, contract-level data within São Paulo State)

	Transfers				Own resources			
	Mistakes	Upward revisions	log(Payed)	Over-invoicing	Mistakes	Upward revisions	log(Payed)	Over-invoicing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bureaucrat trained in the past	-0.030	-218.365**	-0.127	-0.021***	-0.037**	-198.072**	-0.257***	-0.024***
	[0.020]	[93.387]	[0.087]	[0.005]	[0.015]	[78.950]	[0.043]	[0.004]
Vendor from municipality where bureaucrat was trained in the past	0.027***	-45.986	0.384***	0.014***	0.027***	39.972	0.354***	0.020***
	[0.009]	[73.697]	[0.060]	[0.004]	[0.008]	[57.993]	[0.063]	[0.004]
Bureaucrat trained in the past	0.011	70.042	-0.236***	0.011**	0.045***	17.134	0.065	0.013***
x Vendor from municipality where bureaucrat was trained in the past	[0.012]	[71.737]	[0.062]	[0.005]	[0.008]	[57.839]	[0.050]	[0.004]
Program category fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed-effects	No	No	No	No	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dependent Variable Mean	0.215	156.7	7.983	0.0487	0.203	163.4	7.677	0.0453
Observations	180,527	180,527	167,581	180,527	554,895	554,895	519,219	554,895
R-squared	0.022	0.001	0.042	0.006	0.030	0.000	0.036	0.007

Notes on Table 11:

1. All columns are Ordinary Least Squares (OLS) regressions. Cols. (1) and (4) have dependent variable equal 1 if invoiced or payed amounts are negative, if the revision rate is greater than 100%, or if payment is above the invoiced amount, and 0 otherwise; Cols. (2) and (5) have dependent variable equal to the amount payed for the contract between the bureaucrat and the vendor at that given year (in natural logarithms); and Cols. (3) to (6) have dependent variable equal to 1 if the contract invoiced an amount greater than its planned budget (net of revisions) at that year, and 0 otherwise;
2. The past training indicator for bureaucrats equals 1 if the municipality received capacity-building training from FGP at any prior year, and 0 otherwise. The past training indicator for vendors equals 1 if *any* municipality where vendors had active contracts in the past received capacity-building training from FGP at the time, and 0 otherwise;
3. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F);
4. Robust standard errors in brackets, clustered at the municipality and year levels;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 12 – Heterogeneous effects of audits by FGP capacity-building training status (among eligible, contract-level data within São Paulo State)

		Transfers		Own resources	
		log(Payed)	Over-invoicing	log(Payed)	Over-invoicing
		(1)	(2)	(3)	(4)
Bureaucrat audited in the past x Bureaucrat *not* trained in the past	$[\beta_1^0]$	0.044	-0.015**	-0.357***	-0.024***
		[0.083]	[0.006]	[0.054]	[0.003]
Bureaucrat audited in the past x Bureaucrat trained in the past	$[\beta_1^1]$	-0.236	-0.032***	-0.427***	-0.031***
		[0.178]	[0.010]	[0.084]	[0.007]
Vendor audited in the past x Bureaucrat *not* trained in the past	$[\beta_2^0]$	0.316***	0.010***	0.190***	0.011***
		[0.056]	[0.003]	[0.049]	[0.003]
Vendor audited in the past x Bureaucrat trained in the past	$[\beta_2^1]$	0.179***	-0.004	0.113**	0.000
		[0.069]	[0.006]	[0.056]	[0.005]
Bureaucrat audited in the past x Vendor audited in the past x Bureaucrat *not* trained in the past	$[\beta_3^0]$	-0.047	0.007	0.313***	0.017***
		[0.073]	[0.005]	[0.055]	[0.004]
Bureaucrat audited in the past x Vendor audited in the past x Bureaucrat trained in the past	$[\beta_3^1]$	-0.024	0.043***	0.344***	0.042***
		[0.122]	[0.007]	[0.086]	[0.008]
p-value $[\beta_1^0 - \beta_1^1 = 0]$		0.12	0.10	0.42	0.20
p-value $[\beta_2^0 - \beta_2^1 = 0]$		0.05	0.03	0.06	0.06
p-value $[\beta_3^0 - \beta_3^1 = 0]$		0.87	0.00	0.73	0.01
Program category fixed-effects		Yes	Yes	Yes	Yes
Municipality fixed-effects		No	No	No	No
Controls		Yes	Yes	Yes	Yes
Year fixed-effects		Yes	Yes	Yes	Yes
Dependent Variable Mean		8.007	0.0510	7.720	0.0485
Observations		151,267	162,147	464,006	493,286
R-squared		0.040	0.006	0.031	0.006

Notes on Table 12:

1. All columns are Ordinary Least Squares (OLS) regressions. Cols. (1) and (3) have dependent variable equal to the amount payed for the contract between the bureaucrat and the vendor at that given year (in natural logarithms); and Cols. (2) to (4) have dependent variable equal to 1 if the contract invoiced an amount greater than its planned budget (net of revisions) at that year, and 0 otherwise;
2. The past audits' indicator for bureaucrats equals 1 if the municipality was audited at any prior year, and 0 otherwise. The past audits' indicator for vendors equals 1 if *any* municipality where vendors had active contracts in the past was audited at the time, and 0 otherwise;
3. The past training indicator for bureaucrats equals 1 if the municipality received capacity-building training from FGP at any prior year, and 0 otherwise. The past training indicator for vendors equals

1 if *any* municipality where vendors had active contracts in the past received capacity-building training from FGP at the time, and 0 otherwise;

4. Controls include all municipal-level characteristics listed in Table F.1 (See Appendix F);
5. Robust standard errors in brackets, clustered at the municipality and year levels;
6. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.