

Is Corruption Good For Your Health?[§]

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ABSTRACT: While corruption crackdowns have been shown to effectively reduce missing government expenditures, their effects on the quality of public services have not been credibly documented. This matters because, if corruption generates incentives for bureaucrats to deliver those services, then deterring it might actually hurt downstream outcomes. This paper exploits variation from an anti-corruption program in Brazil, designed by the federal government to enforce guidelines on earmarked transfers to municipalities, to study this question. Combining random audits with a differences-in-differences strategy, we find that the anti-corruption program greatly reduced occurrences of overinvoicing and off-the-record payments, and of procurement manipulation within health transfers. However, health indicators, such as hospital beds and immunization coverage, became worse as a result. Evidence from audited amounts suggests that lower corruption came at a high cost: after the program, public spending fell by so much that corruption per dollar spent actually increased. These findings are consistent with those responsible for procurement dramatically reducing purchases after the program, either because they no longer can capture rents, or because they are afraid of being punished for procurement mistakes.

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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, president of the World Bank¹

“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

There is increasing understanding of what tools can most effectively fight corruption; in particular, external monitoring (in the form of crackdowns, audits or information disclosure) has been shown to significantly reduce missing government expenditures.² 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, corruption may also *generate incentives* for bureaucrats to provide these services (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 suggests.

To study this question, one must look beyond corruption, analyzing the effects of monitoring on downstream outcomes. This paper exploits variation from an anti-corruption program designed to enforce guidelines on earmarked transfers to local governments in Brazil, estimating its impacts both on the incidence of corruption within health transfers and on municipalities’ health indicators.

In Brazil, as in most developing countries, many local public goods are provided by subnational governments. Within health, for instance, a local bureaucracy employed by each municipal government is responsible for a wide array of activities, from contracting hospital reforms to purchasing vaccines to paying public servants’ wages. Managing millions of dollars every year, these bureaucrats have several ways to embezzle public funds: e.g.: by overbilling the government for a hospital reform, by settling off-the-record transactions with vaccine vendors, or by having ghost employees in the public sector payroll. Since most Brazilian municipalities rely almost exclusively on federal redistribution in order to provide local public

¹ <http://mobile.reuters.com/article/idUSBRE9BI11P20131219?irpc=932>

² E.g.: DiTella and Schargrodski (2003) for the effects of a crackdown on corruption in hospital purchases; Olken (2007) for the effects of government audits on road projects; and Reinikka and Svensson (2004) for the effects of a newspaper campaign on government transfers to schools.

goods, bureaucrats' moral hazard problem is particularly acute: citizens have weaker incentives to monitor spending funded by taxes that were not locally raised.³

To address this problem, in 2003, the federal government created a monitoring technology: the Brazilian anti-corruption program. The program randomly draws municipalities to be audited with respect to their use of federal funds, in a joint venture with the national lottery. Auditors analyze municipalities' accounts and documentation, and physically inspect public works and service delivery, to assess whether earmarked federal transfers are effectively spent according to their guidelines.

Taking advantage of the randomness of audits, we can estimate the *causal* effects of the program not only on the incidence of corruption, but also on the quality of public services. The key constraint to doing so is data: estimating the effects of the program on corruption requires observing how the latter varies over time within municipalities. We take advantage of the fact that auditors follow transfers' paper trail back, for at least 3 years prior to the time of the audit. Drawing upon a unique dataset for the incidence of corruption across earmarked health transfers between 1997 and 2007, we observe all decisions within transfers that were subsequently audited – even before the audit actually took place. The main difference from this dataset to that used by Ferraz and Finan (2008) is that irregularities are dated according to the year *when they happened*, instead of according to the year when they were investigated by auditors.

We combine this dataset with a behavioral model for local procurement staff. We conjecture that the latter should respond to recent audits that happen nearby, since those should make the probability of being audited by the program in the future more salient. With our data, we can test the hypotheses that recent nearby audits are as good as randomly assigned, and that they affect corruption prevalence and public service delivery.

While recent nearby audits might induce variation in corruption, this is only part of the story: most of the effects of the program might have occurred when it was announced, even before audits took place. To investigate the overall effect of the program, we resort to a differences-in-differences strategy. The first dimension of comparison contrasts municipalities before and after local procurement staff learned they could be audited. The Brazilian anti-corruption program is particularly suitable for this strategy: since it was introduced exactly at the midpoint of the 2001-04 term, we can assess its effects holding decision-makers fixed. The second dimension of comparison contrasts sets of transfers with different opportunities for embezzlement. The hypothesis is that the program should have had differential effects across transfers with different scope for corruption (while the differential incidence of corruption across these transfers should have remained constant otherwise). Since corruption is fundamentally linked to procurement

³ This same argument is linked to what the public finance literature calls the “flypaper effect”; see Schwartz (2005).

problems, we compare sets of transfers with different “procurement intensities”. We code this variable as the percentage of transfer’s *actions* involving procurement-related words (such as “construction”, “expansion” or “acquisition”), following the Health Ministry’s official description of all actions under each transfer.⁴

We find that the anti-corruption program substantially reduced corruption within health transfers, decreasing occurrences of overinvoicing and off-the-record payments, and of procurement irregularities such as participation of ghost firms or tailoring terms of references to specific vendors. Our results suggest that the program decreased corruption by over half its baseline prevalence within the 2001-04 term. Moreover, the effect does not dissipate during the subsequent term, and is unaffected by controlling for proximity to elections.

Combining the differences-in-differences strategy with the randomness of recent nearby audits has two advantages. First, even though the identification assumption for the differences-in-differences estimator cannot be tested, to the extent that the effects of recent nearby audits across transfers of different procurement intensities agrees with the differences-in-differences estimate, we can credibly rule out alternative explanations for the latter. Second, by combining the two sources of variation, we can separate the effect of the announcement of the program from that of the actual audits. On the one hand, the differences-in-differences estimate captures the total effect of the anti-corruption program on corruption prevalence and public service delivery. On the other hand, once we control for the differential effect of recent nearby audits across transfers with different procurement intensities, the change in the differences-in-differences coefficient across specifications allows pinning down the effect of the program’s announcement *separately* from that of audits themselves. We find that most of the effect seems to be driven by the program’s announcement: while differential corruption across transfers is also significantly reduced by recent nearby audits, responses to actual audits are much less dramatic than those to the announcement of the program.

While few papers document the effects of monitoring on the incidence of corruption, even fewer analyze its effects on the outcomes that corruption is supposed to detrimentally affect. Having shown that the program significantly reduced corruption within health transfers, we move on to investigate if it improved health indicators. We resort to the Health Ministry’s Monitoring & Evaluation framework, which specifies which outputs and outcomes are used to track the implementation quality of each earmarked federal transfer to municipalities.⁵ In order to exploit the same empirical strategy, we include in our analysis

⁴ See Appendix A.

⁵ For the transfers 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; see Appendix A.

outputs and outcomes that are exclusively linked to transfers with high or low procurement intensity. While we cannot include outcomes such as infant mortality (which is affected by *all* health transfers), we can contrast outputs and outcomes such as hospital beds and immunization coverage – which are tied to procurement-intensive transfers –, 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 transfers that are not procurement-intensive –, on the other.

Surprisingly, we find that the anti-corruption program makes health indicators significantly worse. The program reduced per capita hospital beds, immunization coverage, and the share of households with access to piped water, connected to the sewage network or with septic tanks. Effects are sizable, equivalent to the average effect of losing between half and all support from federal transfers towards municipality' budget (in a cross-sectional comparison over the baseline period, amongst low-procurement-intensity transfers). Contrary to the hypothesis that such negative effects might result from temporary adjustments to the new incentives set out by the program, effects become larger and more precisely estimated when we include the subsequent political term in the analysis.

To delve into the mechanisms behind those effects, we assess the impact of the anti-corruption program on public spending. Using audited amounts as proxy for spending in each set of transfers, we find that lower corruption can be entirely attributed to lower public spending. The differences-in-differences estimate suggests that public spending plummeted by at least 50% after the program was introduced, consistent with the magnitude of the effect of the program on health indicators' summary measure. Lastly, the effect of the program on public spending is so dramatic that corruption *per dollar spent* actually increased after the program. These findings are consistent with those responsible for procurement reducing purchases after the program, either because they no longer can capture rents to the same extent, or because they are now afraid of being punished for accidental procurement mistakes.

Our findings are at odds with those of Reinikka and Svensson (2004), 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. One possible explanation for this difference is that while Reinikka and Svensson (2004) consider a single transfer that is delivered directly to schools, this paper considers the universe of earmarked federal transfers to municipalities. Before benefiting citizens, such funds must be used by local bureaucrats to procure goods and services, subject to a complex set of procurement guidelines. This is the typical budget implementation process in developing countries, and the effects we find are consistent with the effects of monitoring bureaucratic performance documented elsewhere (Rasul and Rogger, 2015).

Overall, our results provide first-hand evidence that cracking down corruption may be detrimental to the outcomes that we ultimately care about. While the Brazilian anti-corruption program represents 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 adversely affected public spending and downstream outcomes. Expanding the scope of desirable outcomes beyond formal procedures, differentiating between active and passive waste (Bandiera, Pratt and Valetti, 2009), and supporting local procurement staff in complying with complex guidelines might be important steps towards balancing incentives between procuring and making proper use of public funds.

The remainder of this paper is organized as follows. Section 2 briefly introduces the Brazilian anti-corruption program. Section 3 describes the empirical strategy, and presents the results for the effects of the program on corruption. Section 4 considers the effects of the program on health indicators. Section 5 discusses the mechanisms behind those effects, analyzing the impact of the program on public spending and on finer-grain measures of implementation quality. Section 6 concludes the paper.

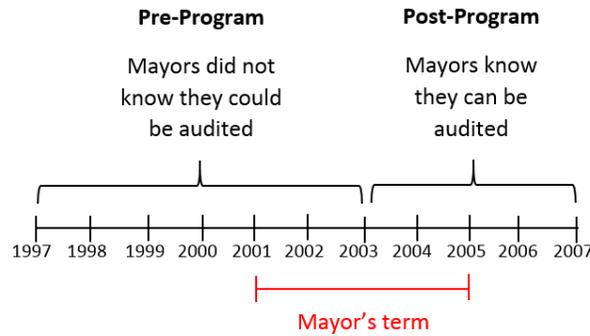
2 The Brazilian anti-corruption program

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 to their guidelines. CGU's official website points out that 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 2003.⁶ 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.

⁶ See Appendix B for more details about the program.

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).⁷



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.

Finally, no other major institutional changes took place over the same period. 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.

3 Has the anti-corruption program reduced corruption?

This section first introduces our unique dataset in subsection 3.1. Subsection 3.2 follows by discussing the empirical strategy for identifying the causal effects of recent nearby audits; results are presented in subsection 3.3. Next, subsection 3.4 discusses the empirical strategy for identifying the causal effects of the announcement of the program, followed by the results in subsection 3.5. Last, subsection 3.6 discusses robustness checks.

⁷ Pande (2011) documents that information experiments lead voters to punish corrupt politicians more broadly.

3.1 – Data

We draw upon 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 health transfers. Although 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.⁸ 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.⁹

Our sample has 11,419 investigations. 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 2007.¹⁰ 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”.¹¹ Second, for each transfer, we compute 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.¹² In our sample, 44.8% of investigations are linked to procurement-intensive transfers.

Due to the retrospective nature of audits, we have a significant number of observations (1,324 investigations) for the 2001-2002 period, although, as expected, there are more observations for the 2003-2004 period (5,748 investigations). Our dataset includes 33 types of irregularities (besides compliance, when auditors find no irregularity), ranging from *documentation problems* to *off-the-record invoice*. One

⁸ More specifically, we have data for draws 2 to 24 (draw 1 was a pilot).

⁹ 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.

¹⁰ Those are the 9 most prevalent health transfers in CEPESP's dataset, representing 92.3% of the total number of coded investigations, see Table A2 in Appendix A.

¹¹ See Table A3 in Appendix A for the complete list.

¹² Results are robust to the choice of the cutoff, see the Supplementary Appendix.

third of these irregularities are coded as evidence of corruption, and the remainder 21 as evidence of mismanagement. We follow Ferraz and Finan (2008, 2011) in defining corruption as procurement problems, over-invoicing and off-the-record invoicing.¹³ Over 2001 and 2002, the first half of the political term, 30.8% of the investigations in our dataset are coded as evidence of corruption. The complete classification list is included in Appendix A.

We also have socio-demographic data at the municipality level from the Brazilian Institute of Geography and Statistics' 2000 census; mayor characteristics and administrative attributes from the 2000 Municipal Information Dataset (*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 Budgets in Health (*Sistema de Informações sobre Orçamentos Públicos em Saúde*, SIOPS).

3.2 Recent nearby audits: Empirical strategy

We start by testing the hypothesis that local procurement staff are subject to behavioral biases (specifically, recency and accessibility biases; Kahneman, 2003). The idea is that the probability of being audited by the program becomes more salient to local procurement staff when a nearby municipality is audited. To assess this hypothesis, we estimate a non-parametric relationship between the minimum distance from municipality's centroid to audits in the previous year and the within-municipality subsequent variation in corruption prevalence. In the local polynomial smoothing, confidence intervals weight observations by the number of investigations used to generate municipality's share of investigations coded as corruption. To compute those shares, we use all 11,419 investigations between 1997 and 2007 in our dataset, regardless of whether we have coded procurement intensity for the transfer to which they are linked.

Anticipating the next subsection, we find that recent audits within 75 km are associated with negative subsequent variation in corruption. For this reason, we define an indicator variable equal to 1 if there was an audit in the previous year within 75 km of the municipality's centroid, and 0 otherwise, and stick to this parametric form for the remainder of the paper. The following equation describes the empirical strategy.

$$Corruption_{mit} = \alpha + \theta_m + \theta_t + \beta Within75km_{m,t-1} + \epsilon_{mit} \quad (1)$$

¹³ 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.

In equation (1), $Corruption_{mit}$ equals 1 if investigation i in municipality m at year t was coded as evidence of corruption, and 0 otherwise; $Within75km_{m,t-1}$ equals 1 if there was an audit within 75 km of municipality m 's centroid at year $t - 1$, and 0 otherwise; θ_m are municipality fixed-effects; θ_t are year fixed-effects; and ϵ_{mit} is an error term. All estimates are from Ordinary Least Squares (OLS) regressions, clustering standard errors at the municipality level. The prediction we are interested in is $\beta < 0$.

3.3 Recent nearby audits: Results

Figure 1 shows the non-parametric estimate of the relationship between, on the one hand, within-municipality variation in the share of investigations coded as corruption, and, on the other, the minimum distance to audits during the previous year.

[Figure 1]

Restricting attention to previous audits between 10km and 100km from municipality's centroid, the figure shows the negative effects of proximity to audits on subsequent variation in corruption within 75 km from municipality's centroid.¹⁴ Effects are statistically significant at the 5% level, up to a 60 km radius. Most importantly, consistent with the behavioral hypothesis, the effect monotonically decreases with distance, and dies out after approximately 85 km.

Moving to the parametric estimates, which rely on the indicator variable for previous audits within a 75 km radius, we first investigate whether covariates are balanced across municipalities with and without nearby audits in the previous year.

[Table 1]

On the one hand, Table 1 shows that proximity to nearby audits in the previous year is not randomly assigned. Municipalities with nearby audits in the past year are often closer to the State capital and vary in other dimensions such as mayors' age and gender distribution, turnout rates, literacy rates and reliance on federal transfers. On the other hand, all these characteristics are held fixed when we rely on variation within-municipality and within the political term. Even if we include the previous and subsequent political terms

¹⁴ We restrict attention to this range because there are few municipalities in our sample with previous audits outside of it (9.62% below 10km, and 15.6% above 100km). Including those leads to very large confidence intervals while not affecting the results shown in Figure 1. Table 2 presents results for previous audits in the municipality itself.

in the analysis, none of the variables for which we have data at different points in time differs significantly across municipalities with and without recent nearby audits. This means that, conditionally on municipality fixed-effects and year fixed-effects, audits within 75 km at the previous year are as good as randomly assigned for the purposes of our analysis.

With that result, we move on to estimate the effects of recent nearby audits on corruption in a regression framework. Table 2 presents the results. Column (1) presents the result for previous audits within 75 km controlling only for a post-program indicator variable, equal to 1 for year 2003 and above, and 0 otherwise. The reason for including this variable is that nearby audits vary only from 2004 onwards, such that not controlling for the introduction of the program would confound the effect of the program's announcement with that of the actual audits. Recent nearby audits decrease the incidence of corruption by 5.4 percentage points, an effect statistically significant at the 1% level. When we break down this indicator variable into two other indicators – own audits in the previous year, and other audits within the 75 km radius in the previous year – in column (2), we can see that the effect of previous audits on the municipality itself has a larger magnitude, as one would expect, of about 7.2 percentage points. The effect is significant at the 10% level, despite the fact that only 6.9% of municipalities were audited more than once over this period. Last, the coefficient on other audits within this radius is basically unaffected.

[Table 2]

Columns (3) and (4) introduce year fixed-effects. In doing so, recent nearby audits are no longer significant; what is more, point estimates become much smaller, about 1.2 percentage points. This is true even for own audits (1.5 p.p. point estimate, not statistically significant). The Supplementary Appendix shows that results are robust to alternative choices of the maximum distance for audits in the previous year.

These findings suggest that, conditionally on the rollout of the anti-corruption program over time, there is limited additional effect from audits themselves. For this reason, we turn to the overall effect of the program in the next subsection.

3.4 Program's overall and announcement effects: Empirical strategy

To investigate the overall effect of the program, we resort to a differences-in-differences strategy. The first dimension of comparison contrasts municipalities before and after local procurement staff learned they could be audited. The second dimension of comparison contrasts sets of transfers with high and low procurement intensities.

The idea is that each group is subject to different treatment intensities: procurement-intensive transfers, for which 37% of investigations are coded as corruption in 2001-2002, have a higher scope for reducing corruption after the program is introduced than other transfers, for which only 17% of investigations are coded as corruption over the same period. The differences-in-differences estimator documents whether procurement-intensive transfers experienced a systematically different variation in corruption prevalence following the introduction of the program.

The differences-in-differences estimate captures the causal effect of the program under the identification assumption of identical potential *reported* outcomes. Sufficient conditions for this assumption to hold are (1) parallel trends for actual potential outcomes across the two sets of transfers; and (2) parallel trends for misreporting by auditors across the two sets of transfers. The first hypothesis is the typical identification assumption for the differences-in-differences estimator. The second hypothesis means that auditors can differentially misreport corruption over time – as long as equally so for the two sets of transfers –, or differentially misreport corruption across the two sets of transfers – as long as equally so over time. If auditors misreport corruption differentially across transfers *and* over time, then misreporting would confound the effects of the program captured by the differences-in-differences strategy.

The empirical strategy for identifying the overall effect of the program is described by the following equation.

$$Corruption_{mit} = \alpha + \theta_m + \theta_t + \beta Post_t \times ProcIntensive_{mit} + \gamma ProcIntensive_{mit} + \epsilon_{mit} \quad (2)$$

In equation (2), $Corruption_{mit}$ equals 1 if investigation i in municipality m at year t was coded as evidence of corruption, and 0 otherwise; $Post_t = 1$ from 2003 on, and 0 otherwise; $ProcIntensive_{mit}$ equals 1 for procurement-intensive transfers, and 0 otherwise; θ_m are municipal fixed-effects; θ_t are year fixed-effects; and ϵ_{mit} is an error term. Under the assumption of identical potential reported outcomes between high and low procurement-intensity transfers, β identifies the causal effect of the anti-corruption program on corruption. All estimates are from Ordinary Least Squares (OLS) regressions, clustering standard errors at the municipality level to deal with potential serial correlation of residuals (Bertrand, Duflo and Mullainathan, 2004). The prediction we are interested in is $\beta < 0$.

We can combine the differences-in-differences strategy with recent nearby audits to separately estimate the effect of the announcement of the program and that of the actual audits. On the one hand, the differences-in-differences estimate captures the total effect of the anti-corruption program. On the other hand, once we control for the differential effect of recent nearby audits across transfers with different procurement

intensities, the change in the differences-in-differences coefficient across specifications allows pinning down the effect of the program's announcement *separately* from that of the audits themselves. The following equation describes how we modify equation (2) in order to estimate the two effects separately:

$$\begin{aligned} Corruption_{mit} = & \alpha + \theta_m + \theta_t + \beta_1 Post_t \times ProcIntensive_{mit} + \gamma ProcIntensive_{mit} + \\ & \beta_2 Within75km_{m,t-1} \times ProcIntensive_{mit} + \delta Within75km_{m,t-1} + \epsilon_{mit} \end{aligned} \quad (3)$$

Once again, all estimates are from Ordinary Least Squares (OLS) regressions, clustering standard errors at the municipality level. The predictions we are interested in are $\beta_1 < 0$ and $\beta_2 < 0$. The difference between $\hat{\beta}$ estimated from equation (2) and $\hat{\beta}_1$ estimated from equation (3) is that $\hat{\beta}_1$ parses out the effect of the actual audits, and hence stands for the effect of the announcement of the program. We compute $\hat{\beta}_1/\hat{\beta}$, the estimated share of the overall effect of the program coming from its announcement.

Finally, even though the identification assumption for the differences-in-differences estimator cannot be tested, if the differential effect of recent nearby audits across transfers of different procurement intensities ($\hat{\beta}_2$) agrees with the differences-in-differences estimate ($\hat{\beta}_1$), then we can confidently rule out alternative explanations for the latter.

3.5 Program's overall and announcement effects: Results

We start by considering the yearly average of the share of investigations coded as corruption for each set of transfers, from 2001 on, displayed in Figure 2. Not only does corruption for all transfers fall systematically after the program, but also, from 2003 onwards, the gap in corruption prevalence between high and low procurement-intensity transfers basically ceases to exist.

[Figure 2]

Furthermore, there seems to be an upward trend in corruption right before the program's announcement, consistent with the idea that the latter was not anticipated by local procurement staff. Last, even though the identification assumption for the differences-in-differences strategy cannot be tested, the two sets of transfers had basically parallel trends for the share of investigations coded as corruption before the program was introduced.

Before moving on to the regression framework, Table 3 motivates the differences-in-differences estimator by testing two hypotheses: (1) that the unconditional difference in average corruption prevalence across transfers with different procurement intensities is equal to zero, both before and after the program; and (2) that the unconditional difference in average corruption prevalence across different periods is equal to zero, both for low and high procurement-intensity transfers.

[Table 3]

Before the program, procurement-intensive transfers had a 20 p.p. higher share of investigations coded as corruption relative to other transfers (37% vs. 17%); after the program, however, this difference was reduced to zero. Corruption among low procurement-intensity transfers fell by 5 percentage points (from 17% to 12% of investigations) after the program, while that for transfers with high procurement intensity fell by 25 percentage points (from 37% to 12%). All in all, the differences-in-differences estimate based on unconditional averages suggests that the program decreased corruption by 20 percentage points, an effect over two-fold the baseline prevalence of corruption among low procurement-intensity transfers.

Since differences in unconditional averages could be confounded by the effects of other variables, in particular by compositional changes of municipal characteristics linked to investigations in each year, we take on to the regression analysis, exploiting within-municipality differences over time across the two sets of transfers. Results are presented in Table 4. Columns (1) and (2) restrict attention to the 2001-2004 term, over which we can estimate the effects of the program while holding decision-makers fixed. Column (1) controls for a post-program indicator variable, while column (2) includes year fixed-effects. In both cases, the program is estimated to have reduced corruption by at least 17.8 p.p., and the effect is statistically significant at the 1% level.

[Table 4]

Column (3) includes the previous and subsequent political terms in the analysis, extending it to the universe of investigations for which we have coded procurement intensity. Doing so provides evidence about the extent to which the overall effect of the program previously identified is short-lived or whether it consolidates over the following term. Results are basically unchanged relative to the estimates for the 2001-04 sample. Beyond a larger sample size, including other political terms can help us rule out alternative explanations for the effects previously documented; in particular, proximity to elections, since the program

kicks off in the second-half of the term. For this reason, column (4) also includes a second-half of the term indicator, equal to 1 for 1999, 2000, 2003, 2004, and 2007, and 0 otherwise, allowing it to differentially affect procurement-intensive transfers. While proximity to elections indeed significantly reduces corruption (8.3 p.p. more among procurement-intensive transfers), the effect of the program on corruption remains at -17.6 p.p. and significant at the 1% level, remarkably close to the original estimate.

Finally, columns (5) and (6) decompose the overall effect of the program into that of the announcement and that of actual audits, the former for the 2001-04 sample, and the latter for 1997-2007. Results are as follows. First, recent nearby audits significantly decrease differential corruption prevalence across transfers of different procurement intensities, by at least 4.5 p.p. in both cases. This is reassuring, since we have documented that this source of variation is random conditional on municipality fixed-effects and year fixed-effects, and since the direction of the effects agrees with the differences-in-differences estimate. Second, about 88.5% of the overall effect of the program comes from its announcement, confirming the previous finding about the limited effects of recent nearby audits.

We present two additional analyses to provide further evidence that the effects are driven by the program. In Figure 3, we allow the differences-in-differences coefficient to vary by year, in order to flexibly estimate the timing of effects. Effects becomes negative only for the post-program, and coefficients are not statistically different from zero up until that year.

[Figure 3]

Next, we pursue a variation of the previous analysis, allowing the differences-in-differences coefficient to vary by draw. Because the local procurement staff may take some time to adjust after learning about the program (and since tampering with procurement paper trail is also evidence of corruption), one would expect to see the effects kick-in only after several draws. Consistent with that hypothesis, Figure 4 shows basically no effects until the 7th draw, followed by a clear downward slope for the coefficients, particularly after the 9th draw. The analysis suggests corruption to be quite sticky: it takes almost 2 years for the differences-in-differences coefficient to differ systematically from zero (lottery 14 was drawn on November 17, 2004).

[Figure 4]

3.6 Robustness checks

This subsection addresses alternative explanations for the effects previously documented, and assesses the sensitivity of results to the definition of some of the variables. First, it might be that the political pressure around the anti-corruption program led auditors to investigate recent evidence to a lesser extent, particularly with respect to transfers with higher scope for embezzlement. While CGU highlights that auditors have no discretion over investigations (they have to follow service orders which pre-specify all investigations; see Appendix B), selective auditing could still result from the way that service orders are issued. To test this hypothesis, the Supplementary Appendix shows that the number of investigations conducted by auditors does not vary differentially according to transfers' procurement intensity after the program.

The Supplementary Appendix also documents that results are robust to using alternative cutoffs for municipality's minimum distance to audits in the previous year, to using a lower cutoff for defining the procurement-intensive indicator variable, and to replacing the latter by a continuous measure of transfers' procurement-intensity.

4 Has the anti-corruption program improved public service delivery?

This section starts by discussing the data we use for health indicators in subsection 4.1. Next, subsection 4.2 presents the empirical strategy for identifying the causal effects of the program, including how we implement the differences-in-differences strategy and how we deal with multiple testing. Results are presented in subsection 4.3. Last, subsection 4.4 discusses robustness checks.

4.1 Data

For the annual municipal-level data for health outputs and outcomes, we rely on the National Health Database (*Base de Informações de Saúde, DATASUS*), restricting attention to the municipalities and years for which we have data on corruption. We assemble a dataset with the following indicators: number of hospital beds per thousand inhabitants; immunization shots per thousand inhabitants¹⁵; 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;

¹⁵ The immunization indicator includes 28 vaccine-preventable diseases, listed under the Epidemiological and Environmental Surveillance in Health program (VIGISUS).

share of under-1-year-olds diagnosed with malnutrition by family doctors; share of under-2-year-olds with diarrhea episodes; and under-1-year-old infant mortality per thousand.

To motivate the analysis, Table 5 assesses the cross-sectional correlation between health outputs and outcomes and corruption at the baseline period (2001-02). All cells are coefficients from Generalized Least Squares (GLS) regressions of each health indicator on the municipality's share of investigations coded as corruption and a constant term, using the number of investigations in each municipality and year as weights, and clustering standard errors at the municipality level.

[Table 5]

We do not find a systematic association between corruption and health indicators at baseline. Furthermore, considering point estimates from Table 5, more corrupt municipalities even have better health outcomes in some dimensions: a decrease in corruption of 20 percentage points (around the ballpark of our estimates for the overall effect of the program) is associated with 5 p.p. fewer episodes of diarrhea among under-1-year-olds, and of tuberculosis among under-2-year-olds. While this contrasts with the findings in Ferraz, Finan and Moreira (2012), which document a negative cross-sectional correlation between corruption and educational outcomes, Table 5 also shows that more corrupt municipalities at baseline also have more educated mayors and better access to public goods, such as internet coverage, which might confound the cross-sectional relationship between corruption and health outputs and outcomes.

4.2 Empirical strategy

We combine the differences-in-differences strategy with recent nearby audits to estimate the effects of the program on health outputs and outcomes. In order to exploit this empirical strategy, however, we have to overcome two challenges. First, we need outputs and outcomes that accrue *exclusively* to transfers of different procurement intensities in order to implement the differences-in-differences estimator. 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 constitutionally mandated federal transfer to municipalities. According to this framework, the outputs and outcomes we include in our analysis are linked either to transfers with high procurement-intensity only, or to transfers with low procurement-intensity only. For transfers 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.¹⁶

¹⁶ See Table A4 in Appendix A.

While we cannot include outcomes such as infant mortality, which is affected by *all* health transfers, the outputs and outcomes that we can include are the following. 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 transfers 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 transfers with procurement-intensity below 50%.

The second issue is demonstrated using the following equation:

$$Y_{mjt}^k = \alpha + \theta_m + \theta_t + \beta_k Post_t \times ProcIntensive_{mjt} + \gamma_k ProcIntensive_{mjt} + \epsilon_{mjt} \quad (4)$$

In equation (4), as before, $Post_t = 1$ from 2003 on, and 0 otherwise; $ProcIntensive_{mjt}$ equals 1 for procurement-intensive transfers, 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 transfers. 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, and 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 transfers. Following Kling, Liebman and Katz (2007), effect sizes of the overall effect of the program are obtained by replacing Y_{mjt}^k in equation (4) by the summary measure of high and low procurement-intensity transfers in each municipality and year.¹⁷

$$Z_{mjt} = \alpha + \theta_m + \theta_t + \beta Post_t \times ProcIntensive_{mjt} + \gamma ProcIntensive_{mjt} + \epsilon_{mjt} \quad (5)$$

In equation (5), Z_{mjt} is the summary measure for health outputs and outcomes linked to set of transfers j in municipality m at year t . Under the assumption of identical potential outcomes between high and low procurement-intensity transfers, β identifies the causal effect of the anti-corruption program on corruption.

¹⁷ While Kling, Liebman and Katz (2007) show that the procedure generates effect sizes with accurate confidence intervals for binary treatments, it is straightforward to see that the algebra generalizes to the differences-in-differences estimator.

All estimates are from Generalized Least Squares (GLS) regressions, clustering standard errors at the municipality level, and using the number of investigations in each municipality and year as weights. The prediction we are interested in is $\beta \leq 0$. Once again, $\beta > 0$ if cracking down corruption mainly reallocates resources towards public goods' provision, whereas $\beta < 0$ if cracking down corruption mainly reduces incentives for bureaucratic performance.

Once again, in some specifications we also include an indicator variable for audits within 75 km of the municipality's centroid in the previous year, and its interaction with the indicator variable for procurement-intensive transfers. If the coefficient for the interaction agrees with the differences-in-differences estimate, then we can confidently rule out alternative explanations for the latter.

Finally, we do not estimate Two-Stage Least Squares (2SLS) regressions using the program's announcement and recent nearby audits as instruments for identifying the effect of corruption on health outputs and outcomes. As we show in the next section, the program also affects other margins of implementation quality; for this reason, we restrict attention to reduced-form estimates.

4.3 Results

We start by considering the yearly average of the health indicators' summary measure for each set of transfers, from 2001 on, displayed in Figure 5. If the summary measure for low procurement-intensity transfers was about 0.4 standard deviations lower than that of procurement-intensive transfers over the baseline period, after 2003 the former systematically increases while the latter plateaus. By 2007, in fact, the baseline difference between summary measures of different sets of transfers was basically flipped.

[Figure 5]

While Figure 5 suggests that the effect of the program was actually the opposite of what one would expect if corruption was a tax on government expenditures, changes in unconditional averages could be confounded by compositional changes of municipal characteristics for which we have corruption data in each year, since we restrict attention to those municipalities in our analysis. Moving on to the regression framework, we exploit within-municipality differences over time across the two sets of transfers. Effect sizes are presented in Table 6. Columns (1) and (2) restrict attention to the 2001-2004 term, over which we can estimate the effects of the program while holding decision-makers fixed. Column (1) controls for a post-program indicator variable, while column (2) includes year fixed-effects. In both cases, the program

has a negative point estimate on the summary measure of health outputs and outcomes, although not statistically significant at the 10% level.

[Table 6]

Column (3) includes the previous and subsequent political terms in the analysis, encompassing the universe of investigations for which we have coded procurement intensity. Effect sizes increase substantially (by more than 100%) relative to the estimates for the 2001-04 sample, and become statistically significant at the 1% level. The magnitude of the effects in columns (1) to (3) is equivalent to losing between two-thirds and all the support of federal transfers towards the municipality's health budget (in a cross-sectional comparison at the 2001-02 period, within low procurement-intensity transfers).

Column (4) includes a second-half of the term indicator, allowing it to differentially affect procurement-intensive transfers. While proximity to elections also significantly deteriorates health outputs and outcomes relative to low procurement-intensity transfers (an effect size of similar magnitude to that of the program), the effect of the program on corruption remains significant at the 1% level and remarkably close to column (3)'s estimate.

Finally, columns (5) and (6) include the indicator of recent nearby audits and its interaction with procurement-intensity. We find that recent nearby audits have a negative and significant (at the 10% level) effect on health outputs and outcomes when we consider the full sample, between 1997 and 2007, but not when we restrict attention to 2001-04. Taken together with the similar pattern we documented for the effect of the announcement of the program, this is a striking result. It suggests that health indicators become systematically worse not because of a short-term disruptive effect of the anti-corruption program, followed by improvements once corruption has been brought down. Instead, negative effects become larger and more precise once we include the subsequent term in the analysis.

Because the direction of the effects of recent nearby audits agrees with the differences-in-differences estimate, we can credibly rule out that results are driven by alternative explanations; in particular, by differential pre-existing trends in health indicators linked to different sets of transfers (as Figure 5 might suggest). Last, about 71.4% of the overall effect of the program on health outputs and outcomes comes from its announcement, an order of magnitude consistent with the effects of the program on corruption documented in the previous section.

4.4 Robustness checks

While we have no leeway with respect to the health outputs and outcomes we can use to assess the effects of the program under the differences-in-differences strategy, we could look at the effect of recent nearby audits on any outcome, regardless of whether it is associated with high or low procurement-intensity transfers only. The Supplementary Appendix presents results for the effects of nearby audits on outcomes such as infant mortality and the share of young children affected by episodes of diarrhea or tuberculosis. Since effects on those variables might take longer than one year to manifest, we also experiment with an alternative definition of previous audits, considering any previous audit within this range since the program was launched.

We find no significant effects of either variable on these health indicators. While this might be seen as confirmation that the program has no positive effects on health outputs and outcomes, it is important to remember that nearby audits had limited marginal effects on corruption beyond the announcement of the program.

Finally, we replicate the analyses in Table 6 without weighting observations by the number of investigations used to compute the share of investigations coded as corruption in each municipality and year. While our preferred specification is the one which weights municipalities by the same criteria we use for computing municipal-level measures of corruption, it is not unreasonable to give all municipalities equal weight since reduced-form estimates do not rely on corruption directly. Results are similar and, if anything, stronger for the differences-in-differences estimate, which become about 50% larger and statistically significant at the 5% or 1% level in all columns. However, the effect of recent nearby audits is not as robust. While those still have a negative effect on health indicators for the 1997-2007 sample, this effect size is no longer significant.

5 Mechanisms

The previous results are evidence that, in this setting, cracking down corruption hurts public service delivery. To gain a deeper understanding of why that is the case, subsection 5.1 analyzes how public spending evolves for transfers of different procurement-intensities, both before and after the introduction of the program. If corruption indeed creates incentives for bureaucratic performance (either because bureaucrats capture rents, or because complying with procurement guidelines is costly), then spending should fall as a result of lower opportunities for embezzlement – trickling down to worse health indicators. Since we do not have data on actual spending and have to rely on audited amounts as a proxy, subsection 5.2 provides additional evidence that the effects we document are not merely an artifice of misreporting,

by looking at the quality of policy implementation. Auditors record a range of implementation problems in audit reports, and we explore the richness of our dataset to test whether implementation quality falls alongside the documented fall in public spending. Subsection 5.3 discusses robustness checks. In subsection 5.4, we discuss how our findings relate to the previous results from the corruption literature.

5.1 Spending forensics

This subsection tests the hypothesis that the Brazilian anti-corruption program has reduced public spending. Since the effect sizes estimated in the previous section were equivalent to cutting down federal transfers towards municipalities' health budget by at least 63%, this is the benchmark for the estimates we expect to find if lower spending is driving these results. Consistent with this hypothesis, budget execution of *voluntary* federal transfers to municipalities is notably low. For the federal government's Growth Acceleration Program (PAC) in health, budget execution has been around 10%, and municipalities' inability to streamline procurement has been credited as the most important reason for such low execution rates.¹⁸

Before we can get to the empirical strategy, however, we have to deal with a challenge about spending data. Municipal health spending is recorded by the Brazilian Dataset on Municipal Budgets (FINBRA) only for the total budget, not separately for constitutionally mandated transfers. Since these transfers do not legally admit budget leftovers (such that amounts only partially executed cannot be returned to the federal government), there is no other way to know how much municipalities actually spent out of those transfers.¹⁹ Relying on the total health budget is no feasible alternative, for two reasons. First, it would not allow us to resort to the differences-in-differences strategy, since that would require separate data for different sets of transfers. Second, and most importantly, these data are low-quality: subnational units have been systematically accused of including in other expenses in the health budget (in particular, educational expenses) in order to comply with constitutional amendment 29 (EC-29), which requires health expenses to be at least 10% of a municipality's total budget.²⁰

The best we can do with the data we have is to proxy for public spending using audited amounts. Once again, because auditors leave CGU's headquarters with preset service orders, they do not have discretion over which transfers (or how much) to audit. Any systematic differences in audited amounts across transfers over time must accrue to differential spending patterns. More specifically, the idea is that, beyond compliance with documentation and account keeping – with respect to which all transfers are always

¹⁸ See <http://www.contasabertas.com.br/website/arquivos/8134>.

¹⁹ This has led some authors to wrongly assume that budget execution for these transfers is always 100% (Zamboni and Litschig, 2013).

²⁰ <http://www.cmb.org.br/index.php/noticias-gerais/622-a-regulamentacao-da-ec-29-e-o-financiamento-da-saude-no-brasil>.

audited on –, only spending moves a transfer up in its life-cycle and generates further objects of investigations by the anti-corruption program.

We again combine the differences-in-differences estimator with recent nearby audits as our empirical strategy. The dependent variable is the natural logarithm of the audited amount for each investigation, as in the following equation:

$$\ln(\text{Amount})_{mit} = \alpha + \theta_m + \theta_t + \beta \text{Post}_t \times \text{ProcIntensive}_{mit} + \gamma \text{ProcIntensive}_{mit} + \epsilon_{mit} \quad (6)$$

In equation (6), $\ln(\text{Amount})_{mit}$ is the audited amount (in ln) for investigation i at municipality m at year t , $\text{Post}_t = 1$ from 2003 on, and 0 otherwise; $\text{ProcIntensive}_{mit}$ equals 1 for procurement-intensive transfers, and 0 otherwise; θ_m are municipal fixed-effects; θ_t are year fixed-effects; and ϵ_{mit} is an error term. Under the assumption of identical potential *reported* outcomes between high and low procurement-intensity transfers, β identifies the causal effect of the anti-corruption program on public spending. All estimates are from Ordinary Least Squares (OLS) regressions, clustering standard errors at the municipality level. The prediction we are interested in is $\beta < 0$.

Once again, in some specifications, we also include an indicator variable for audits within 75 km of the municipality’s centroid in the previous year, and its interaction with the indicator variable for procurement-intensive transfers. If the coefficient for the interaction agrees with the differences-in-differences estimate, then we can confidently rule out alternative explanations for the latter.

Before we turn to the results, Figure 6 shows, first, that audited amounts for the two sets of transfers followed similar trends over the pre-program period. Second, amounts for both sets of transfers fall substantially after the program, at least until 2005. Third, and most importantly, amounts for procurement-intensive transfers fall significantly more, and the gap between the low and high procurement-intensity transfers is increasing over time.

[Figure 6]

Table 7 considers the empirical regularity hinted at by the previous figure within a regression framework. Columns (1) and (2) restrict attention to the 2001-2004 term, over which we can estimate the effects of the program while holding decision-makers fixed. Column (1) controls for a post-program indicator variable, while column (2) includes year fixed-effects. In both cases, the program decreases spending by about 50%, a huge effect statistically significant at the 10% level. Interestingly, this figure is

within range of the order of magnitude of the negative effects we previously documented on health outputs and outcomes.

[Table 7]

Column (3) includes the previous and subsequent political terms in the analysis, extending it to the universe of investigations for which we have coded procurement intensity. Once again in line with our previous findings, effects increase are substantially larger (about 70%) and still significant at the 1% level. Column (4) includes a second-half of the term indicator, allowing it to differentially affect procurement-intensive transfers. While proximity to elections also significantly deteriorates spending (what is expected, since municipalities cannot procure for most of the final year of the term), the effect of the program on corruption remains significant at the 1% level and remarkably close to column (3)'s estimate.

Finally, columns (5) and (6) include the indicator of recent nearby audits and its interaction with procurement-intensity. We find that audits have a negative and significant effect on public spending of about 32% within 2001-04, and up to 38% when we include the subsequent term. Once more, since the effects of recent nearby audits are in the same direction and order of magnitude of those of the differences-in-differences estimator, we can be confident that results are not driven by alternative explanations. Last, about 75% of the overall effect of the program on public spending comes from its announcement, again consistent with the effects of the program on corruption and health indicators.

To highlight how striking the results of Table 7 are, we can decompose variation in corruption over time as follows:

$$C_t = \frac{Y_t}{T_t} = \left(\frac{Y_t}{X_t}\right) \left(\frac{X_t}{T_t}\right) \quad (7)$$

In equation (7), the first equality defines corruption at time t , C_t as the ratio between the number of corruption events at time t , Y_t , to the number of investigations at time t , T_t . The second equality just rewrites this ratio as the product between corruption events *per dollar spent* at time t , $\frac{Y_t}{X_t}$, multiplied by the average spending per investigation at time t , $\frac{X_t}{T_t}$. Taking logs and differentiating with respect to time yields:

$$\frac{d \ln(C_t)}{d t} = d \frac{\ln\left(\frac{Y_t}{X_t}\right)}{d t} + d \frac{\ln\left(\frac{X_t}{T_t}\right)}{d t} \quad (8)$$

The first term on the right-hand side of equation (8) is the percentage change in corruption if average spending per investigation was held constant; the second term is the percentage change in average spending.

With $\frac{d \ln(C_t)}{d t} \cong -0.18$, according to the results in column (2) of Table 4, and with $\frac{d \ln(X_t)}{d t} \cong -0.49$, according to the results of column (2) of Table 7, corruption per dollar spent must have increased by 31 percentage points; otherwise the share of investigations coded as corruption would have been reduced even further.

Our findings for the effects of the program on corruption and public spending are consistent with a simple model in which local procurement staff first decides whether or not to procure, and then, conditionally upon procuring, decides whether or not to be corrupt. Intuitively, a higher probability of being audited (and exposed as corrupt for being involved in procurement problems) discourages both corruption and spending, particularly among procurement processes involving lower amounts; hence the potentially positive effect of a higher audit probability on corruption per dollar spent. When we incorporate the possibility of accidental procurement mistakes into the model, the discouragement effect on spending is amplified.²¹

5.2 Implementation quality

Since we use audited amounts as proxy for public spending, this subsection provides additional evidence, by looking at the quality of policy implementation, that lower spending is indeed the mechanism linking lower corruption to worse health indicators. We start by decomposing investigations not coded as “evidence of corruption” into mismanagement and compliance.²² We follow the same empirical strategy as equation (3), only replacing the dependent variable on the left-hand-side by indicator variables of mismanagement (equal to 1 if the investigation is coded as evidence of mismanagement, and 0 otherwise) or compliance (equal to 1 if the investigation is neither coded as evidence of corruption nor of mismanagement, and 0 otherwise).

Table 8 presents the results. Columns (1) to (3) restrict attention to the 2001-04 term, while columns (4) to (6) include all investigations between 1997 and 2007 in the analysis. Looking first at the effect of the program’s announcement, we find that the decrease in corruption (columns 1 and 4) translates basically

²¹ See Supplementary Appendix.

²² See Appendix A for the complete list of irregularities and how they are coded in our dataset.

one-to-one into an increase in mismanagement (columns 2 and 5). Compliance (columns 3 and 6) is not significantly affected by the announcement of the anti-corruption program.

[Table 8]

For recent nearby audits, it seems that results are mixed: a little over half of the decrease in corruption seems to translate into higher mismanagement, and the remainder, to higher compliance. Having said that, the marginal effect of actual audits on corruption is small enough that their effect on compliance is not statistically significant at the 10% level.

To shed further light on what ensued after the Brazilian anti-corruption program was introduced, still resorting to the empirical strategy described by equation (3), we look separately at each category of irregularities within mismanagement in Table 9, including all investigations between 1997 and 2007. 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 below-required frequency of meetings. In column (3), *performance problems* stand for complaints from final users about frustrated consultations or admissions, about lack of availability of medication, or about 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 ins and outs not properly accounted for. In column (5), *human resources problems* represent problems with late wages, with missing personnel, or with the composition of health teams relative to transfer's guidelines. 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.

[Table 9]

First, the results show that mismanagement increased particularly for infrastructure and medication stock problems. This pattern is consistent with plummeting public spending. With more precarious facilities (possibly fewer hospital beds) and with missing medication (possibly fewer vaccines), it is not surprising that health indicators linked to procurement-intensive transfers deteriorate after the program. Second, we also find a significant and sizable increase in documentation and accounting problems. This is suggestive

that local procurement staff might have tried to tamper with evidence for past procurement problems, an additional piece of evidence for the extent to which bureaucrats diverted energy from public service delivery after the program was announced.

Third, we find that resource diversion significantly decreased after the program. This finding potentially raises a different concern: reshuffling resources across different programs does not necessarily captures resources from public goods' provision, but, possibly, reallocates those resources to local priorities (which local bureaucrats might be better equipped to identify). Last, evidence for performance problems is mixed: while, to some extent, the announcement of the program seems to discipline the quality of service delivery to end users, recent nearby audits significantly increase these problems.

Overall, the evidence suggests that the Brazilian anti-corruption program significantly decreased corruption, but for all the wrong reasons. In response to the introduction of the program, local procurement staff reduced spending within earmarked federal transfers by at least 50%. As a result, infrastructure and medication suffered, bureaucrats felt more constrained about channeling resources towards local needs, and the quality of health services significantly deteriorated. These negative effects do not seem to result from temporary adjustments: not only did they not fade in the subsequent political term, but also became stronger and more precisely estimated.

5.3 Robustness checks

One channel potentially missing from the previous analysis is voluntary transfers. Brollo (2010) argues that lower federal support towards municipalities' budget might be central to the electoral punishment documented by Ferraz and Finan (2008) once a municipality is exposed as corrupt. In our setting, this might imply that municipalities' total health budgets become lower on average after the program, a possibility that could also deteriorate the quality of public services. Having said that, this channel would only matter for our results if voluntary transfers have differential trends before and after the program across transfers of different procurement-intensities.

While there is no annual data on municipal-level voluntary funds *released to each transfer* that we could link to procurement-intensity, we have annual data on municipal-level voluntary funds towards two sets of health transfers: *Basic Attention*, which can use those funds for a variety of purposes, *including procurement*, and *Medium- and High-Complexity*, whose guidelines explicitly preclude municipalities from using its funds to procure goods or services.²³ For the purposes of our analysis, we code these sets of

²³ *Basic Attention* encompasses Basic Attention in Health and other health transfers, such as Basic Pharmacy. Pages 55-56 of DENASUS' "*Manual de auditoria na gestão dos recursos financeiros do SUS*" (2004) explicitly preclude the utilization of funds for *Medium- and High-Complexity* transfers for procurement. Although data for voluntary

transfers as high and low procurement-intensity, respectively. The Supplementary Appendix shows that voluntary transfers were systematically affected by the anti-corruption program.

Last, to provide additional evidence about the mechanism, the Supplementary Appendix investigates whether public spending was particularly reduced within sets of transfers for which bureaucrats used to exert higher discretion at baseline. We first define a *discretion-intensive* indicator, equal to 1 for transfers with above-median share of resource diversion problems unrelated to corruption before the program, and 0 otherwise. We then generate a triple interaction, multiplying the discretion-intensive indicator with the procurement-intensive indicator and the post-program indicator. The hypothesis we test is whether corruption is further reduced by the program within discretion-intensive transfers, beyond its differential effect working through procurement-intensity. Consistent with the mechanism and with the results for resource diversion in Table 9, we find that corruption is further reduced among discretion-intensive transfers.

5.4 Relation to the literature

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).²⁴ However, there is limited evidence of its effects (Banerjee, Hanna and Mullainathan, 2013; Pande, 2007). 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.²⁵

There are at least two reasons why cracking down 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). Second, because it is hard to separate corruption from discretion (Bandiera, Pratt and Valletti, 2009; Huntington, 1968; Leys, 1965; Leff, 1964), discouraging the former often discourages the latter. This paper provides first-hand evidence for these two

transfers towards *Strategic Actions* transfers is also available, we do not include it because such voluntary funds are not systematic, accruing to only about 10% of Brazilian municipalities each year.

²⁴ 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.

²⁵ For a theoretical perspective, see Shleifer and Vishny (1993) and Banerjee, Hanna and Mullainathan (2012).

mechanisms. After the introduction of the anti-corruption program, we observe both plummeting public spending and less reshuffling of resources across different transfers.

Analyzing the causal effects of corruption is challenging. 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, 2013; 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, 2004; 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 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 externally valid and that allows for counterfactual analysis under weaker assumptions than previous papers. 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 its causal effects on corruption prevalence and on public service delivery. That allows us to go beyond the findings of Olken (2007) and Bobonis, Fuertes and Schwabe (2015), which document the effects of monitoring on corruption but do not analyze its effects on the quality of public goods.

Ferraz and Finan (2008) exploit variation induced by the Brazilian anti-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. None of these papers estimate the effect of the program on corruption or on downstream outcomes.

Our findings are at odds with those of Reinikka and Svensson (2004), 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. One possible explanation for this difference is that while Reinikka and Svensson (2004) consider a single transfer that is delivered directly to schools, this paper considers the

universe of earmarked federal transfers to municipalities. Before benefitting citizens, such funds must be used by local bureaucrats to procure goods and services, subject to a complex set of procurement guidelines. This is the typical budget implementation process in developing countries, and the effects we find are consistent with the effects of monitoring bureaucratic performance documented elsewhere (Rasul and Rogger, 2015).

6 Discussion and concluding remarks

In this paper, we have documented that the Brazilian anti-corruption program significantly decreased corruption, but made health indicators significantly worse. In response to the introduction of the program, local procurement staff seem to have reduced spending within constitutionally mandated transfers by at least 50%. As a result, infrastructure and medication suffered, bureaucrats felt more constrained about channeling resources towards local needs, and the quality of health services significantly deteriorated.

These negative effects do not seem to result from temporary adjustments. Not only do effects persist in the subsequent political term; they also become stronger and more precisely estimated. Moreover, it is not the case that unspent resources are reallocated to other local priorities (that would be coded as resource diversion) or to other municipalities (constitutionally mandated transfers cannot be returned to the federal government). While there are no official data for budget leftovers with respect to these transfers, evidence for other federal health programs which rely on decentralized budget execution is consistent with extremely low execution rates. All in all, our findings suggest that extreme views such as that held by Jim Kim, president of the World Bank, who calls corruption “developing world’s public enemy number 1”, should be taken with a grain of salt.

Our results provide first-hand evidence that cracking down corruption may hurt public service delivery. Does this mean corruption is welfare-improving in this setting? On the one hand, it may be that local bureaucracies have more accurate information about the local demand for public goods or about the quality of local suppliers, along the lines of Huntington (1968). In this case, being able to deviate from transfers’ guidelines by diverting resources or targeting procurement processes to specific vendors – all recorded by auditors as evidence of corruption – would improve welfare. On the other hand, it may be that bureaucrats are motivated by other objectives, such as extracting personal rents from vendors, or minimizing implementation effort. In that case, corruption would not necessarily improve welfare. Better public services would come at the cost of redistribution towards bureaucrats, which can generate a broad set of distortions (from vendors’ incentives to outbid other bribe offers to individuals’ perceptions about

trustworthiness in society at large) that might out-weight its benefits. Our results for implementation quality provide preliminary evidence that both mechanisms might be at play in this setting.

While the Brazilian anti-corruption program represents 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. These findings suggest that policies that expand the scope of desirable outcomes beyond formal procedures, that differentiate between active and passive waste, and that support local procurement staff in complying with complex guidelines might be important steps towards balancing incentives between procuring and making proper use of public funds.

Some recent advances have moved the country in that 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 secretariats. 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 – organized around Law 8.666 – 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. Finally, there has not been significant progress in expanding the scope of irregularities for which administrative penalties can be applied since 2007; Draft Law 931 still waits to be voted upon Congress.

Despite being the highest per capita GDP country in Latin America, Brazil fares poorly in health indicators. In 2010, according to the World Health Organization, Brazil only placed 10th out of 20 Latin American countries with respect to infant mortality, and 13th with respect to life expectancy at birth. Given that the decentralization of federal spending, from 1996 on, increased municipal health budgets seven-fold (from 0.1% of GDP in 1995 to 0.72% in 2010), we believe this paper sheds light on some critical constraints to bringing the country’s health outcomes more in line with its development stage.

Our findings for the effects of the program on corruption and public spending are consistent with a model in which local procurement staff decides whether or not to procure, and then, conditionally upon procuring, decides whether or not to be corrupt. Such a model has at least two normative implications.²⁶ First, audits should focus on investigations involving larger amounts (with the largest embezzlement opportunities), since those involve the bulk of embezzlement, and since, within those transfers, audits

²⁶ See the Supplementary Appendix.

discourage procurement by the least. Second, supporting local procurement staff, building capacity to decrease the probability of accidental procurement mistakes, may both decrease corruption (passive waste) and increase public spending (and hence downstream outcomes).

Interestingly, in line with the first normative implication, in 2014 and 2015 the anti-corruption program has restricted audits to larger municipalities (for which transfers involve larger amounts). The reason, however, was of a different nature: given the fragile fiscal situation of the federal government, CGU's budget only allowed investigating municipalities closer to the state capital, where auditors are based. CGU has indicated it would like to return to its all-encompassing strategy once the fiscal situation is alleviated.

The second normative implication seems to have been taken on board by CGU. In 2006, it launched the Public Management Strengthening ("*Fortalecimento da Gestão Pública*", FGP) program, which randomly draws municipalities for capacity building, particularly with respect to procurement guidelines – provided through in-site training over the course of a week, combined with complementary online materials. In fact, Lopes (2011) provides preliminary evidence that FGP significantly decreases corruption within health transfers. This is a striking result for such a short-length in-site capacity-building intervention.

Given these findings, the optimal design of capacity-building interventions to disseminate best practices among local procurement staff, and the extent to which those interventions can improve public service delivery, are promising avenues for future research.

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Appendix A – Classification lists

Table A1 – List of irregularities

Panel A: Corruption	
<u>Category</u>	<u>Irregularity</u>
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	Overinvoicing
Resource diversion	Off-the-record payments
Panel B: Mismanagement	
<u>Category</u>	<u>Irregularity</u>
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 (<i>Saneamento básico</i>)	100.00
0004	Quality and efficiency of the Unified Health System (<i>Qualidade e eficiência do SUS</i>)	54.17
0005	Pharmaceutical Assistance (<i>Assistência Farmacêutica</i>)	50.00
0013	Epidemiological and environmental surveillance in Health (<i>Vigilância epidemiológica e ambiental em Saúde</i>)	50.00
0002	Prevention and control of vector-transmitted diseases (<i>Prevenção e controle de doenças transmitidas por vetores</i>)	38.46
1214	Basic Attention in Health (<i>Atenção básica em Saúde</i>)	4.55
0001	Family Health (<i>Saúde da família</i>)	0.00
0023	Admission, emergency and hospital services (<i>Atendimento ambulatorial, emergencial e hospitalar</i>)	0.00
1335	Conditional Cash Transfer (<i>Transferência de renda com condicionalidades</i>)	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 [<i>imputed</i>].
0005	Pharmaceutical Assistance	(i) Distribution of specific medication [<i>unavailable</i>].
0013	Epidemiological and environmental surveillance in Health	(i) Immunization shots per thousand inhabitants [<i>imputed</i>].
0002	Prevention and control of vector-transmitted diseases	(i) Immunization against Hepatitis B [<i>unavailable</i>]; (ii) Incidence of HIV, tuberculosis, and leprosy [<i>unavailable</i>]; (iii) Population share covered by dental health teams [<i>unavailable</i>];
1214	Basic Attention in Health	(iv) Population share covered by family health teams.
0001	Family Health	(i) Population share covered by family health teams. (i) Medical consultations per thousand inhabitants;
0023	Admission, emergency and hospital services	(ii) Hospital admissions per thousand inhabitants; (iii) Transplantations per thousand inhabitants [<i>unavailable</i>].
1335	Conditional Cash Transfer	–

Notes to Table A4:

1. Source of *non-imputed* 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 – VIGISUS²⁷”, *modernization of the national health surveillance system*), which we proxy with immunization;
4. *Unavailable* indicators have no annual municipal-level data available in DATASUS.

²⁷ <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.²⁸ 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.

²⁸ 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.²⁹

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", TCU), the Federal Police, and municipal legislative houses.

²⁹ 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

Examples of pre-program findings (1997-2002)

COMPLIANCE

When evaluating the implementation of a public program for treatment of Hansen's disease (leprosy) in 2001-02 in Alvaraes, State of Amazonas, auditors found no evidence of irregularity. The local health unit, maintained by the Unified Health System (SUS), operates in compliance with legislation, keeping patients' records and providing vaccination, counseling, and staff training. We code this finding as a case of compliance (in the biennium 2001/2002). The municipality was drawn by lottery 2.

MISMANAGEMENT

When evaluating the municipal inventory of medical supplies in 2002, auditors found no formal controls to be in place concerning stock inflows or outflows. As a consequence, there was no way to determine either the quantity of stocked medication or the quantity delivered to each local health unit. We code this irregularity as mismanagement related to medication stock control. This irregularity occurred in 2002, in Amajari, State of Roraima, municipality drawn by lottery 2.

CORRUPTION

When evaluating the procurement process to purchase two mobile health units (modified vehicles that operate as small health units), in 2002 by the municipal government of Dourados, auditors found evidence of fraud. The only public outbidder, Santa Maria Comércio e Representações Ltda., does not legally exist according to the local branch of the Federal Revenue Secretariat in Cuiabá, State of Mato Grosso do Sul. Despite this fact, the municipal government has concluded the public bid and paid the company the agreed-upon amount. Unfortunately, auditors could not find further evidence of what may have encouraged the fraud. We code this irregularity as an evidence of ghost firm, a clear indication of corruption. This irregularity occurred in 2002, in Dourados, Mato Grosso do Sul, drawn by lottery 4.

Examples of post-program findings (2003-2007)

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

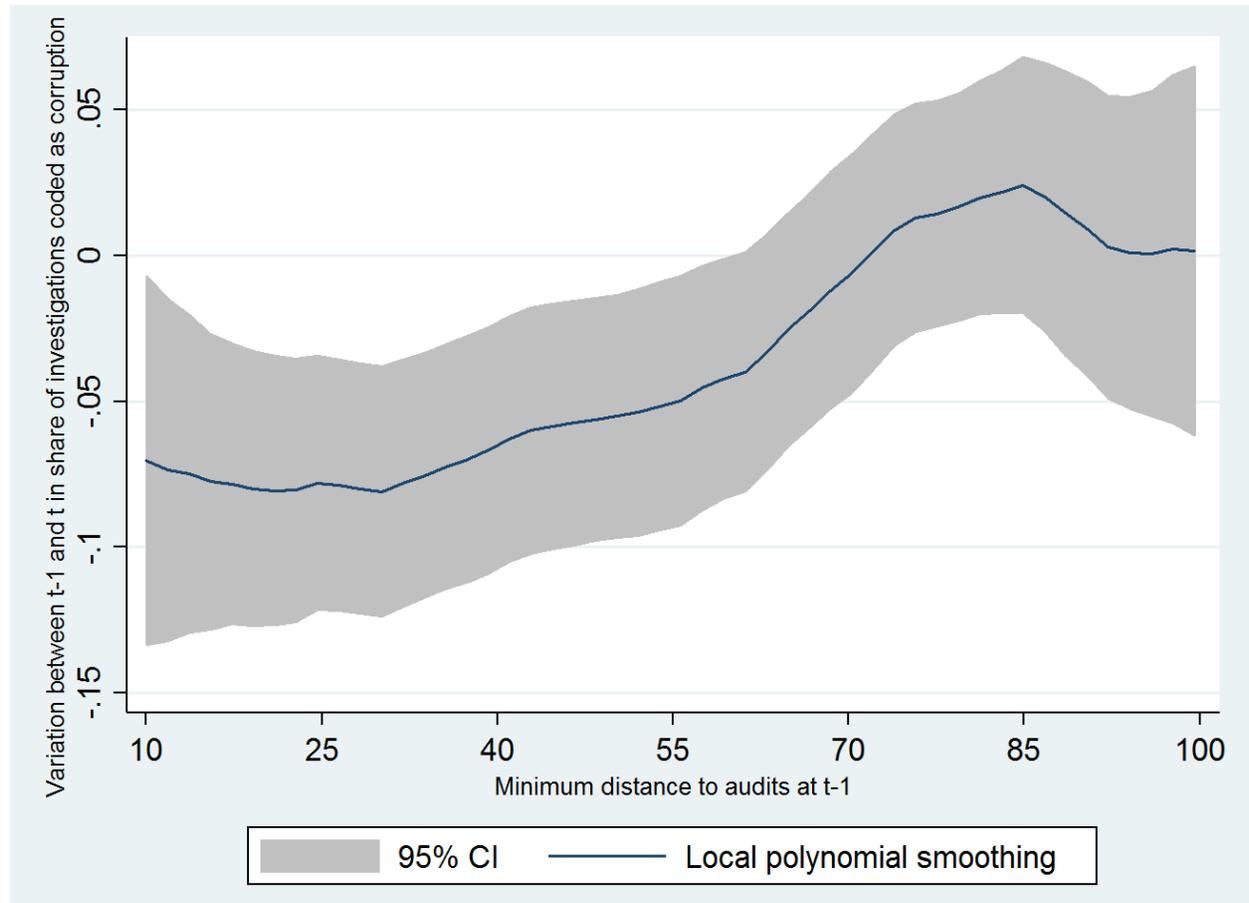
When evaluating the medication accessibility to citizens of Londrina in 2006, auditors found out that several prescribed drugs in local health units were out of stock. Evidence was provided by interviews with patients and local employees (nurses), who reported lack of, for example, Clorana, Dipirona, 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 – Figures

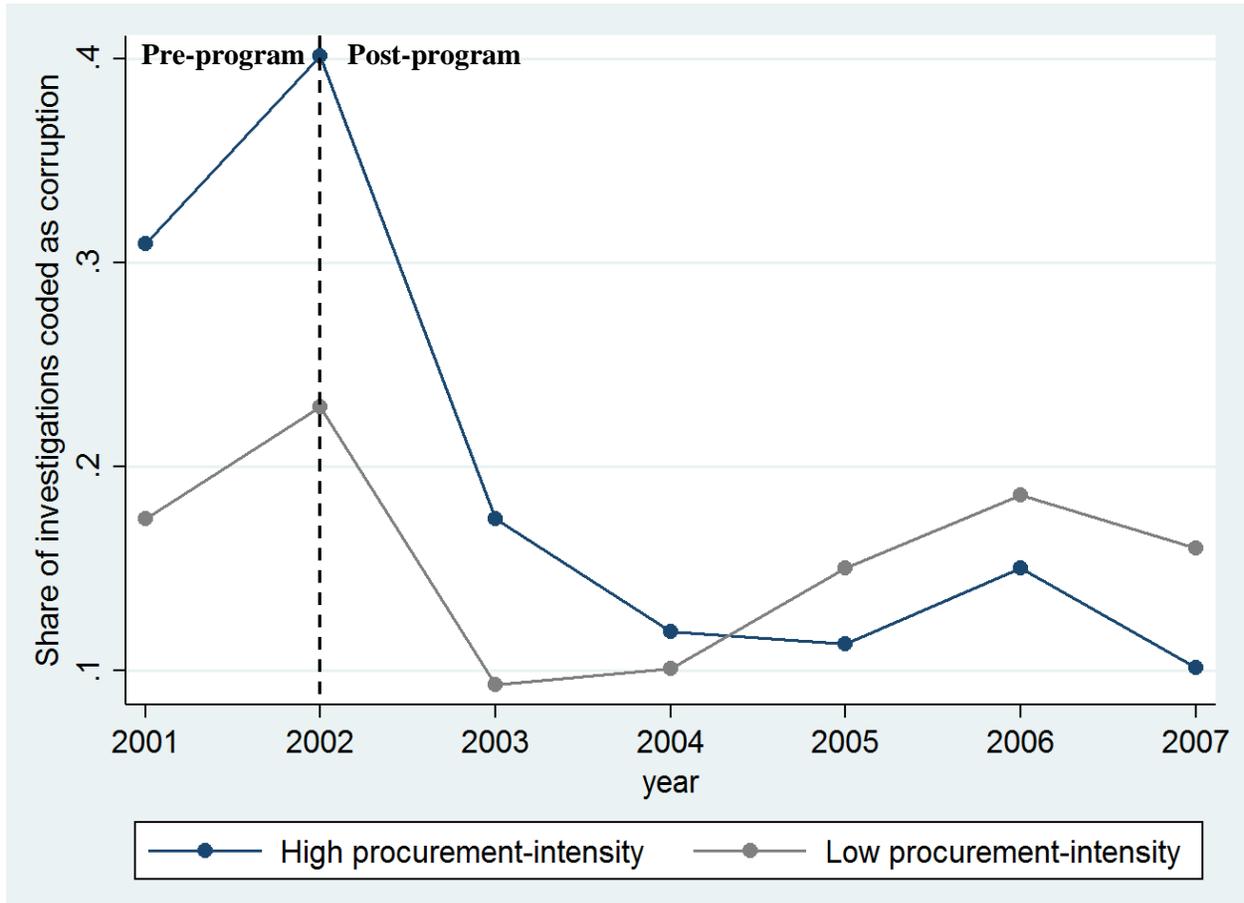
Figure 1 – Non-parametric estimate of the effect of distance to audits in the previous year on corruption



Notes on Figure 1:

1. Non-parametric estimate of the relationship between municipality centroid's minimum distance to audits in the previous year and within-municipality variation in corruption in the subsequent year;
2. Local polynomial smoothing using the number of investigations at each municipality and year as weights;
3. Data includes all municipalities within the 10-100 km range from audits in the previous year, between 1997 and 2007;
4. 95% confidence intervals; standard errors not clustered.

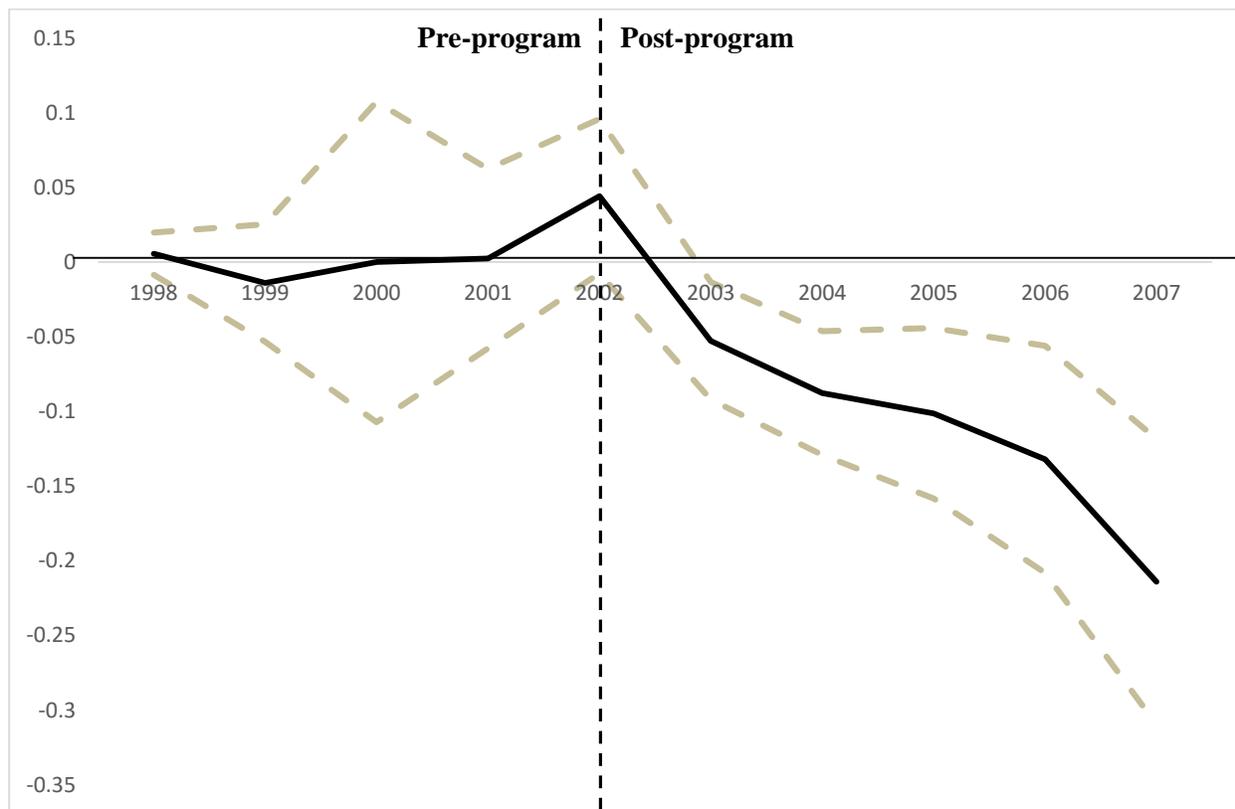
Figure 2 – Share of investigations coded as corruption by year and procurement-intensity



Notes on Figure 2:

1. Weighted average of the share of investigations coded as corruption, by year and by the high procurement-intensity indicator, equal to 1 if procurement-intensity is 50% or higher, and 0 otherwise;
2. Number of investigations at each municipality and year used as weights;
3. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A.

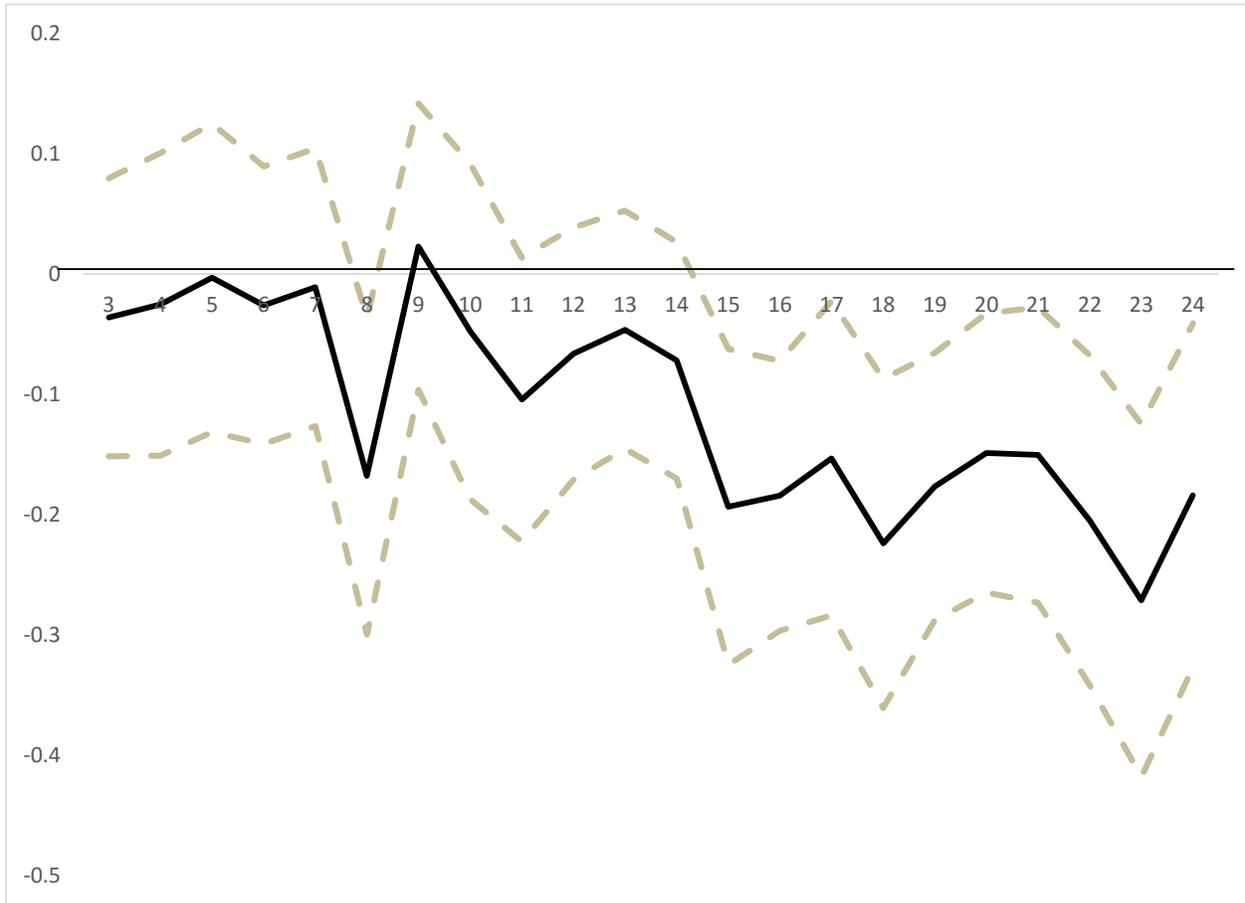
Figure 3 – Differences-in-differences coefficient by year



Notes on Figure 3:

1. Regression coefficients of interactions of year indicators with the high procurement-intensity indicator, equal to 1 if procurement-intensity is 50% or higher, and 0 otherwise, on the share of investigations coded as corruption;
2. Ordinary Least Squares (OLS) regression including municipality fixed-effects and year fixed-effects;
3. Dashed lines represent 95% confidence intervals, computed from robust standard errors clustered at the municipal level;
4. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A.

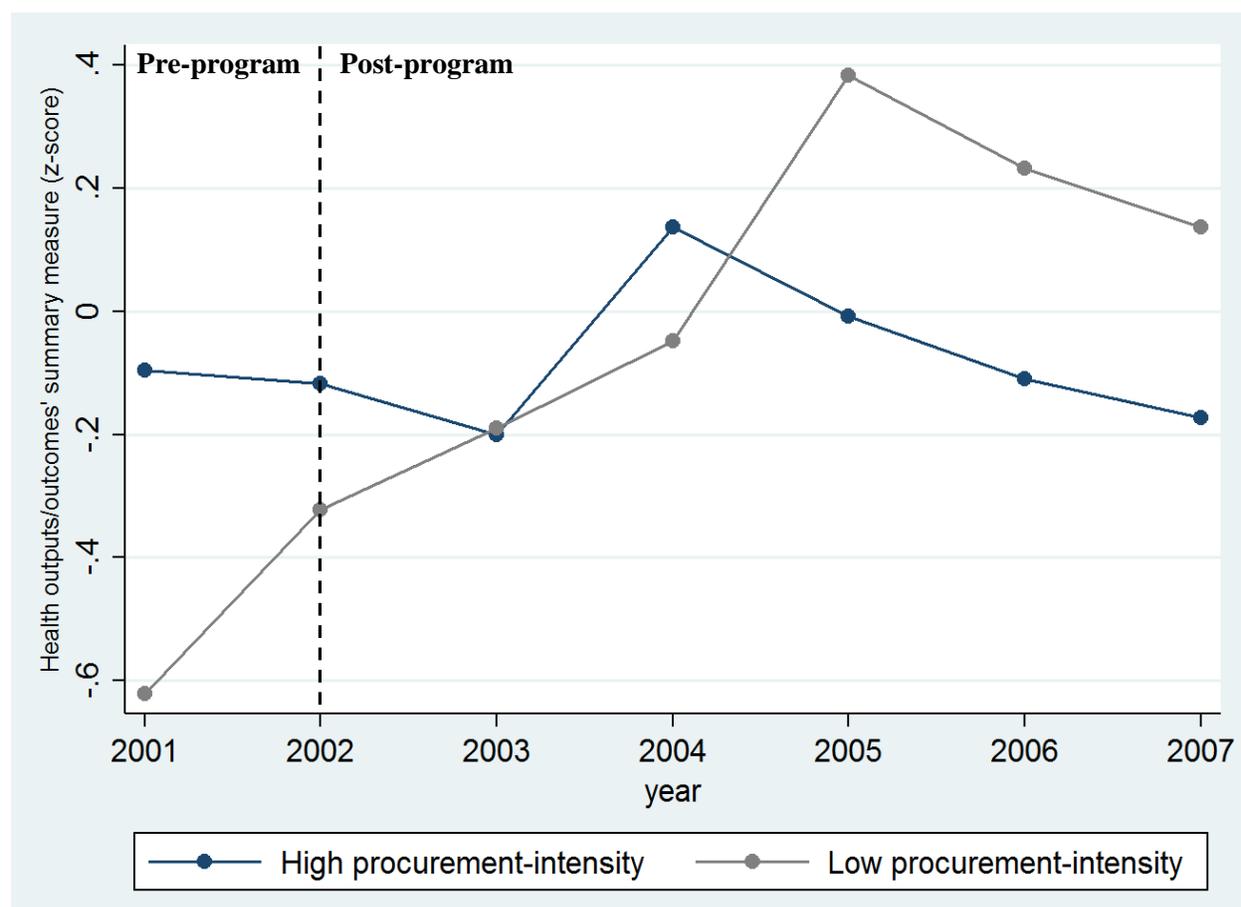
Figure 4 – Differences-in-differences coefficient by draw



Notes on Figure 4:

1. Regression coefficients of interactions of draw indicators with the high procurement-intensity indicator, equal to 1 if procurement-intensity is 50% or higher, and 0 otherwise, on the share of investigations coded as corruption;
2. Ordinary Least Squares (OLS) regression including municipality fixed-effects and year fixed-effects;
3. Dashed lines represent 95% confidence intervals, computed from robust standard errors clustered at the municipal level;
4. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A.

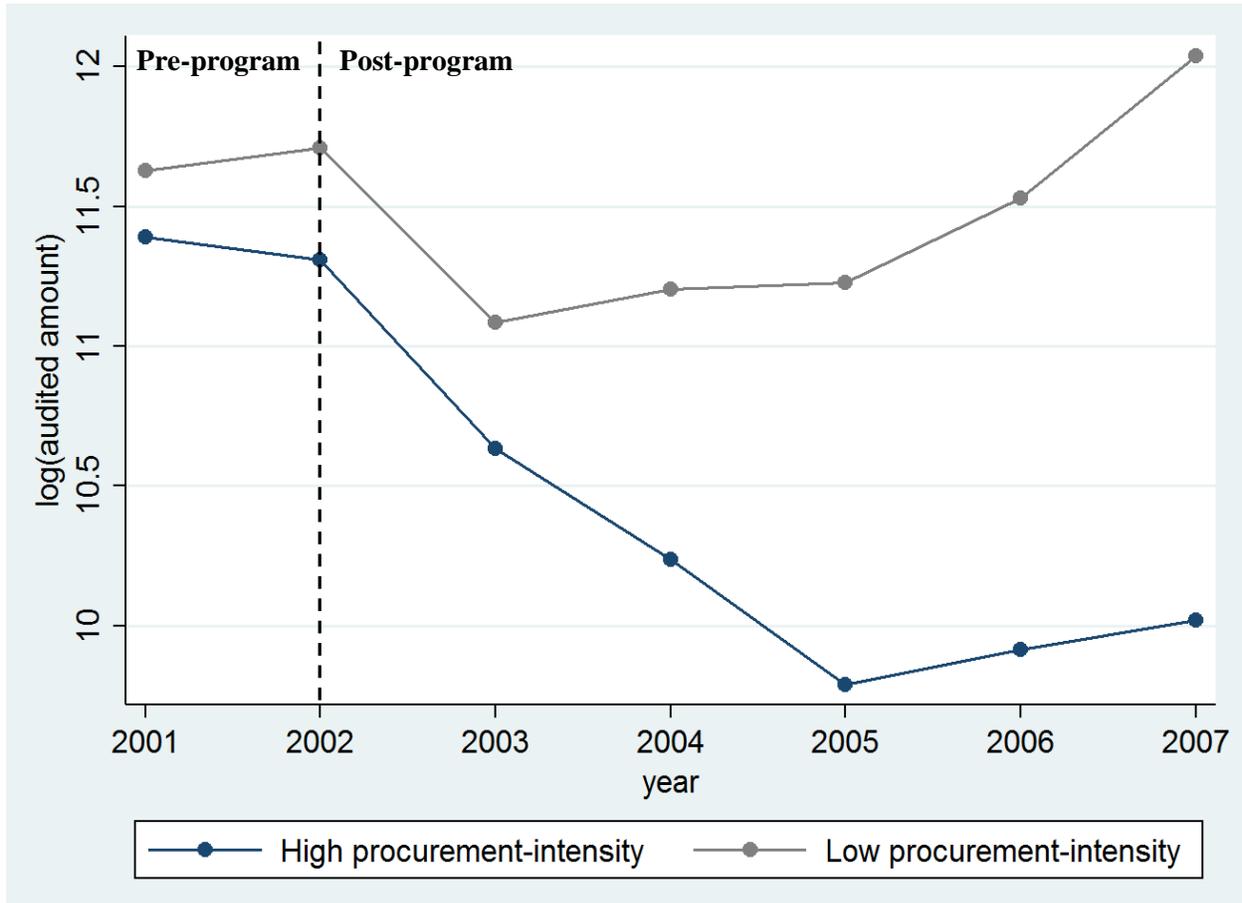
Figure 5 – Average health outputs’ and outcomes’ summary measure by year and procurement-intensity



Notes on Figure 5:

1. Weighted average of the summary measure for health outputs and outcomes, by year and by the high procurement-intensity indicator, equal to 1 if procurement-intensity is 50% or higher, and 0 otherwise;
2. Number of investigations at each municipality and year used as weights;
3. The summary measure for high procurement-intensity transfers includes hospital beds per thousand inhabitants, immunization, the household share connected to the general sewage network, and the household share with connected to piped water;
4. The summary measure for low procurement-intensity transfers includes medical consultations and the population share covered by the Family Health program;
5. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A.

Figure 6 – Average public spending (proxied by audited amounts) by year and procurement-intensity



Notes on Figure 6:

1. Weighted average of audited amount per investigation, by year and by the high procurement-intensity indicator, equal to 1 if procurement-intensity is 50% or higher, and 0 otherwise;
2. Number of investigations at each municipality and year used as weights;
3. There is no data on budget execution for constitutionally mandated health transfers. For this reason, we resort to audited amounts. The idea is that, beyond compliance with documentation and account keeping, with respect to which all transfers are always audited on, only spending moves a transfer up in its life-cycle and generates further objects of investigations by the anti-corruption program;
4. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A.

Appendix E – Tables

Table 1 –Balance tests for audits within 75km in the previous year

	Audits within 75 km in t-1 = 1	Audits within 75 km in t-1 = 0	Difference [1 - 0]	Difference [1 - 0] (municipality and year fixed-effects)
ln(distance to closest State capital)	2.206 [0.0238]	2.425 [0.0296]	-0.219*** [0.0364]	-
ln(per capita GDP in 2000)	0.518 [0.0189]	0.548 [0.0280]	-0.030 [0.0321]	-
ln(population in 2000)	4.170 [0.0276]	4.185 [0.0354]	-0.015 [0.0416]	-
Share of public servants in formal employment (in 2001)	0.040 [0.00100]	0.038 [0.00127]	0.002 [0.00147]	-
Municipality has a radio station (in 2001)	0.533 [0.0297]	0.585 [0.0458]	-0.052 [0.0510]	-
Municipality has internet access (in 2001)	0.180 [0.0217]	0.201 [0.0427]	-0.021 [0.0456]	-
Municipality has community clubs (in 2001)	0.642 [0.0291]	0.650 [0.0437]	-0.008 [0.0488]	-
Literacy rate (2000)	0.753 [0.0086]	0.774 [0.0010]	-0.0206* [0.0124]	-
Share of municipal revenues from transfers (in 2000)	0.824 [0.00652]	0.816 [0.0108]	0.008 [0.0114]	-
Share of municipal transfer from SUS (in 2000)	0.093 [0.00468]	0.082 [0.00433]	0.0107* [0.00613]	-
Share of Health transfer from federal government (in 2000)	0.967 [0.00508]	0.958 [0.00831]	0.009 [0.00887]	-
ln(total revenue in 2000, R\$)	15.660 [0.0555]	15.710 [0.0744]	-0.045 [0.0860]	-
ln(tax revenue in 2000, R\$)	11.520 [0.116]	11.720 [0.152]	-0.200 [0.180]	-
Health council meets regularly (in 2000)	0.886 [0.0170]	0.878 [0.0308]	0.008 [0.0339]	-

Notes on Table 1:

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;
2. 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;
3. Number of investigations at each municipality and year used as weights.
4. *** p<0.01, ** p<0.05, * p<0.1.

Table 1 (continued) – Balance tests for audits within 75km in the previous year

	Audits within 75km in t-1 = 1	Audits within 75km in t-1 = 0	Difference [1 - 0]	Difference [1 - 0] (municipality and year fixed-effects)
Mayor serving second term	0.235 [0.0248]	0.274 [0.0417]	-0.040 [0.0465]	-0.038 [0.0447]
Turnout rate	0.868 [0.00363]	0.848 [0.00509]	0.0190*** [0.00602]	0.000 [0.00737]
Electoral margin	0.141 [0.00801]	0.136 [0.0140]	0.005 [0.0152]	0.000 [9.45e-10]
Mayor from Workers' Party (PT)	0.074 [0.0156]	0.041 [0.0180]	0.032 [0.0206]	0.068 [0.0654]
Mayor from the same party as Governor	0.211 [0.0232]	0.287 [0.0410]	-0.0758* [0.0457]	-0.090 [0.0759]
Mayor's age	49.250 [0.546]	46.480 [0.916]	2.772*** [1.038]	0.139 [1.659]
Male mayor	0.910 [0.0188]	0.968 [0.0145]	-0.0577*** [0.0218]	-0.050 [0.0609]
Mayor elementary school drop-out	0.078 [0.0138]	0.083 [0.0213]	-0.005 [0.0252]	-0.019 [0.0599]
Mayor high-school drop-out	0.142 [0.0190]	0.162 [0.0305]	-0.020 [0.0341]	-0.023 [0.0508]
Mayor high-school graduate	0.240 [0.0251]	0.305 [0.0451]	-0.065 [0.0499]	-0.029 [0.0650]

Notes on Table 1:

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;
2. 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;
3. Number of investigations at each municipality and year used as weights;
4. *** p<0.01, ** p<0.05, * p<0.1.

Table 2 – Effect of audits within 75 km in the previous year on corruption

	Investigation coded as corruption			
	(1)	(2)	(3)	(4)
Audits within 75 km in t-1	-0.0543*** [0.0155]		-0.0118 [0.0253]	
Own audit in t-1		-0.0716* [0.0369]		-0.0155 [0.0422]
Other audits within 75 km in t-1		-0.0525*** [0.0151]		-0.0112 [0.0248]
Post-program	-0.205*** [0.0201]	-0.205*** [0.0201]		
Municipality fixed-effects	Yes	Yes	Yes	Yes
Year fixed-effects	No	No	Yes	Yes
Observations	11,419	11,419	11,419	11,419
Number of clusters	731	731	731	731
R-squared	0.182	0.182	0.186	0.186

Notes on Table 2:

1. All columns are regressions with dependent variable equal to 1 if the investigation is coded as corruption, and 0 otherwise. See Appendix A for the definition of corruption;
2. Columns (1) to (4) are Ordinary Least Squares (OLS) regressions;
3. Robust standard errors in brackets, clustered at the municipal level;
4. *** p<0.01, ** p<0.05, * p<0.1;

Table 3 – Differences-in-differences unconditional averages

Share of investigations coded as corruption			
	Low procurement- intensity	High procurement- intensity	Difference [High - Low]
Pre-program	0.17 [0.02]	0.37 [0.03]	0.20*** [0.04]
Post-program	0.12 [0.01]	0.12 [0.01]	0 [0.01]
Difference [Post - Pre]	-0.05** [0.03]	-0.25*** [0.03]	-0.20*** [0.03]

Notes on Table 3:

1. The table presents weighted averages of the share of investigations coded as corruption, by high and low procurement-intensity and by period – before and after the anti-corruption program. The third column presents the differences between the two sets of transfers within each period, and the third row presents the differences between the two periods within each set of transfers. The bottom-rightmost cell presents the unconditional differences-in-differences estimate of the effect of the program;
2. Number of investigations at each municipality and year used as weights;
3. High procurement-intensity equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
4. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A;
5. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 – Differences-in-differences estimates of the anti-corruption program on corruption

	Investigation coded as corruption					
	2001-04		1997-2007		2001-04	1997-07
	(1)	(2)	(3)	(4)	(5)	(6)
Post-program x Procurement-intensive	-0.179*** [0.0338]	-0.178*** [0.0333]	-0.180*** [0.0324]	-0.176*** [0.0323]	-0.159*** [0.0343]	-0.156*** [0.0334]
Post-program	-0.0942*** [0.0234]					
Procurement-intensive	0.215*** [0.0345]	0.214*** [0.0339]	0.186*** [0.0321]	0.187*** [0.0321]	0.214*** [0.0341]	0.187*** [0.0322]
Second-half of term x Procurement-intensive				-0.0829*** [0.0315]		
Second-half of term				-0.223*** [0.0516]		
Audits within 75km in t-1 x Procurement-intensive					-0.0451** [0.0192]	-0.0470*** [0.0171]
Audits within 75km in t-1					0.0215 [0.0476]	0.00706 [0.0264]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	No	Yes	Yes	Yes	Yes	Yes
Observations	7,072	7,072	10,538	10,538	7,072	10,538
Number of clusters	623	623	728	728	623	728
R-squared	0.238	0.242	0.195	0.195	0.242	0.196

Notes on Table 4:

1. All columns are regressions with dependent variable equal to 1 if the investigation is coded as corruption, and 0 otherwise. See Appendix A for the definition of corruption;
2. Columns (1) to (6) are Ordinary Least Squares (OLS) regressions;
3. Robust standard errors in brackets, clustered at the municipal level;
4. High procurement-intensity equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
5. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 – Correlation between corruption and municipal-level characteristics in 2001-02

	Correlation with the share of investigations coded as corruption
ln(distance to closest State capital)	-0.00597 [0.0492]
ln(per capita GDP in 2000)	0.00917 [0.0626]
ln(population in 2000)	0.0748 [0.0482]
Share of public servants in formal employment (in 2001)	-1.68 [1.143]
Municipality has a radio station (in 2001)	0.0426 [0.0371]
Municipality has internet access (in 2001)	0.109* [0.0555]
Municipality has community clubs (in 2001)	0.0407 [0.0370]
Literacy rate (2000)	-1.79E-05 [0.00141]
Share of municipal revenues from transfers (in 2000)	-0.23 [0.168]
Share of municipal transfer from SUS (in 2000)	0.155 [0.240]
Share of Health transfer from federal gov. (in 2000)	-0.0337 [0.189]
ln(total revenue in 2000, R\$)	0.0225 [0.0239]
ln(tax revenue in 2000, R\$)	0.00632 [0.0123]
Health council meets regularly (in 2000)	0.0394 [0.0481]
Mayor serving second term	0.0485 [0.0481]
Turnout rate	-0.305 [0.272]
Electoral margin	0.0824 [0.0838]

Notes on Table 5:

1. Coefficients of Ordinary Least Squares (OLS) regressions with the share of investigations coded as corruption as independent variable, including a constant term;
2. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 (continued) – Correlation between corruption and municipal-level characteristics in 2001-02

	Correlation with the share of investigations coded as corruption
Mayor from Workers' Party (PT)	-0.0672 [0.140]
Mayor from the same party as Governor	0.019 [0.0497]
Mayor's age	-0.00291 [0.00179]
Male mayor	-0.0195 [0.0577]
Mayor elementary school drop-out	-0.0846 [0.0528]
Mayor high-school drop-out	-0.0494 [0.0529]
Mayor high-school graduate	0.0892** [0.0434]
Hospital beds per thousand inhabitants	0.00764 [0.00545]
Immunization shots per thousand inhabitants	0.00114 [0.00148]
Share of households with proper sewage disposal	0.0607 [0.0959]
Share of households connected to piped water	0.0473 [0.0859]
Population share covered by Family Health program	-0.0655 [0.0439]
Medical consultations per thousand inhabitants	-0.00714 [0.0120]
Share of under-1-year-olds diagnosed with malnutrition	-0.252 [0.401]
Share of under-2-year-olds with diarrhea episodes	-0.243 [0.376]
Under-1-year-old infant mortality per thousand	-0.000704 [0.000942]

Notes on Table 5:

1. Coefficients of Ordinary Least Squares (OLS) regressions with the share of investigations coded as corruption as independent variable, including a constant term;
2. *** p<0.01, ** p<0.05, * p<0.1.

Table 6 – Effect sizes of the anti-corruption program on health outputs and outcomes summary measure

	Health outputs/outcomes summary measure					
	2001-04		1997-2007		2001-04	1997-07
	(1)	(2)	(3)	(4)	(5)	(6)
Post-program x Procurement-intensive	-0.118	-0.125	-0.288***	-0.279***	-0.126	-0.209***
	[0.0830]	[0.0825]	[0.0775]	[0.0777]	[0.0875]	[0.0797]
Post-program	0.127**					
	[0.0524]					
Procurement-intensive	0.232***	0.242***	0.234***	0.236***	0.241***	0.236***
	[0.0876]	[0.0873]	[0.0812]	[0.0814]	[0.0873]	[0.0811]
Second-half of term x Procurement-intensive				-0.194*		
				[0.115]		
Second-half of term				1.024***		
				[0.219]		
Audits within 75km in t-1 x Procurement-intensive					0.00394	-0.151*
					[0.100]	[0.0849]
Audits within 75km in t-1					-0.0956	0.00423
					[0.0967]	[0.0587]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	No	Yes	Yes	Yes	Yes	Yes
Observations	1,874	1,874	2,703	2,703	1,874	2,703
Number of clusters	611	611	725	725	611	725
R-squared	0.604	0.611	0.516	0.517	0.611	0.518

Notes on Table 6:

1. All columns are regressions with dependent variable equal to 1 if the investigation is coded as corruption, and 0 otherwise. See Appendix A for the definition of corruption;
2. Columns (1) to (6) are Generalized Least Squares (GLS) regressions, with the number of investigations in each municipality and year used as weights;
3. Robust standard errors in brackets, clustered at the municipal level;
4. High procurement-intensity equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
5. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A;
6. *** p<0.01, ** p<0.05, * p<0.1;
7. *Corr*(health summary measure, share of federal transfers in mun. health budget) = 0.186, over the 2001-02 period for low procurement-intensity transfers.

Table 7 – Differences-in-differences estimates of the anti-corruption program on public spending

	ln(audited amount)					
	2001-04		1997-2007		2001-04	1997-07
	(1)	(2)	(3)	(4)	(5)	(6)
Post-program x Procurement-intensive	-0.496***	-0.490***	-0.711***	-0.677***	-0.359***	-0.511***
	[0.134]	[0.133]	[0.126]	[0.125]	[0.135]	[0.128]
Post-program	-0.374***					
	[0.0965]					
Procurement-intensive	-0.371***	-0.377***	-0.439***	-0.434***	-0.372***	-0.431***
	[0.133]	[0.132]	[0.123]	[0.123]	[0.131]	[0.123]
Second-half of term x Procurement-intensive				-0.683***		
				[0.248]		
Second-half of term				-0.855***		
				[0.283]		
Audits within 75km in t-1 x Procurement-intensive					-0.316**	-0.382***
					[0.123]	[0.102]
Audits within 75km in t-1					0.286	0.196**
					[0.174]	[0.0962]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	No	Yes	Yes	Yes	Yes	Yes
Observations	7,072	7,072	10,538	10,538	7,072	10,538
Number of clusters	623	623	728	728	623	728
R-squared	0.416	0.416	0.412	0.413	0.418	0.414

Notes on Table 7:

1. All columns are regressions with investigation's audited amount (in log) as dependent variable;
2. Columns (1) to (6) are Ordinary Least Squares (OLS) regressions;
3. Robust standard errors in brackets, clustered at the municipal level;
4. High procurement-intensity equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
5. Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A;
6. *** p<0.01, ** p<0.05, * p<0.1.

Table 8 – Differences-in-differences estimates of the anti-corruption program on implementation quality

	[2001-04]			[1997-2007]		
	(1) Corruption	(2) Mismanagement	(3) Compliance	(4) Corruption	(5) Mismanagement	(6) Compliance
Post-program x						
Procurement-intensive	-0.159*** [0.0343]	0.162*** [0.0442]	-0.00276 [0.0283]	-0.156*** [0.0334]	0.151*** [0.0415]	0.00449 [0.0254]
Procurement-intensive	0.214*** [0.0341]	-0.224*** [0.0430]	0.0099 [0.0260]	0.187*** [0.0322]	-0.199*** [0.0399]	0.012 [0.0247]
Audits within 75km in t-1 x						
Procurement-intensive	-0.0451** [0.0192]	0.0235 [0.0257]	0.0216 [0.0194]	-0.0470*** [0.0171]	0.0268 [0.0208]	0.0201 [0.0135]
Audits within 75km in t-1	0.0215 [0.0476]	-0.00257 [0.0522]	-0.0189 [0.0270]	0.00706 [0.0264]	-0.00617 [0.0275]	-0.000889 [0.0126]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,072	7,072	7,072	10,538	10,538	10,538
Number of clusters	623	623	623	728	728	728
R-squared	0.242	0.25	0.301	0.196	0.217	0.29

Notes on Table 8:

- Columns (1) and (4) are regressions with dependent variable equal to 1 if the investigation is coded as corruption, and 0 otherwise; columns (2) and (5) are regressions with dependent variable equal to 1 if the investigation is coded as mismanagement, and 0 otherwise; and columns (3) and (6) are regressions with dependent variable equal to 1 if the investigation is coded as compliance (neither evidence of corruption, nor of mismanagement), and 0 otherwise. See Appendix A for the definition of corruption and mismanagement;
- Columns (1) to (6) are Ordinary Least Squares (OLS) regressions;
- Robust standard errors in brackets, clustered at the municipal level;
- High procurement-intensity equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
- Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A;
- *** p<0.01, ** p<0.05, * p<0.1.

Table 9 – Diff-in-diff estimates of the anti-corruption program within mismanagement (1997-2007)

	(1)	(2)	(3)	(4)	(5)	(6)
	Resource diversion	Health council problems	Performance problems	Infrastructure and stock problems	Human resources problems	Documentation or accounting problems
Post-program x						
Procurement-intensive	-0.0598** [0.0235]	0.0243 [0.0149]	-0.0677** [0.0301]	0.211*** [0.0263]	0.0289* [0.0148]	0.0623* [0.0332]
Procurement-intensive	-0.00849 [0.0213]	-0.0741*** [0.0142]	0.0925*** [0.0276]	0.00927 [0.0229]	-0.0336** [0.0134]	-0.165*** [0.0310]
Audits within 75km in t-1 x						
Procurement-intensive	0.00739 [0.0164]	-0.00406 [0.00744]	0.0459** [0.0197]	0.0127 [0.0208]	0.00411 [0.0129]	-0.00569 [0.0190]
Audits within 75km in t-1	-0.0285 [0.0207]	-0.0035 [0.00963]	-0.00442 [0.0184]	0.0281 [0.0210]	0.00722 [0.0133]	-0.0122 [0.0203]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,538	10,538	10,538	10,538	10,538	10,538
Number of clusters	728	728	728	728	728	728
R-squared	0.143	0.109	0.117	0.186	0.124	0.128

Notes on Table 9:

- Columns (1) to (6) are 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;
- Columns (1) to (6) are Ordinary Least Squares (OLS) regressions;
- Robust standard errors in brackets, clustered at the municipal level;
- High procurement-intensity equals 1 if procurement-intensity is 50% or higher, and 0 otherwise;
- Procurement-intensity is coded as the share of actions under each transfer that include procurement-related words; see Appendix A;
- *** p<0.01, ** p<0.05, * p<0.1.