Is corruption good for your health?[1]

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ABSTRACT

This paper assesses whether corruption is good or bad for health outputs and outcomes. Following the introduction of a random-audits initiative in Brazil, we contrast Health programs with different embezzlement opportunities. While differential corruption prevalence between the programs dramatically narrowed after the initiative, outcomes did not differentially improve. Evidence from audited amounts suggests that, after the program, procurement staff holds spending back: the decrease in corruption can be entirely attributed to lower spending for a given set of transfers. Results are consistent with a model in which accidental procurement mistakes labeled as corruption induce underspending.

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1. Introduction

In this paper we empirically examine the consequences of monitoring mechanisms on the prevalence of corruption in local governments in Brazil and we estimate the effects of corruption deterrence on health outputs and outcomes at municipal level. Although a growing literature has investigated corruption drivers, the effects of monitoring mechanisms on politicians' decisions have not been fully understood. Furthermore, while corruption is conjectured to have high social costs, there is limited evidence of its effects on different welfare measures.

The lack of studies measuring the effects of monitoring mechanism on corruption can be mainly explained by the lack of reliable information on corrupt behavior prior the adoption of

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anticorruption programs; see Pande (2011). Following Ferraz and Finan (2008, 2011), we rely on the Brazilian anticorruption program launched in April of 2003, when the federal government, in a joint venture with the national lottery, began to randomly select municipal governments to be audited for their use of federal funds, specially transfers for health programs. The novelty of this program is the fact that auditors follow transfers' paper trail usually up to 3 years (but sometimes more) prior to actual audit.

In order to measure the effects of the audit program on corruption reduction, our research design relies on a difference-in-difference estimator where the lack of a control group, as every municipality are eligible to be audited by the program, is circumvented by splitting the sample of federal transfers according to the incidence of procurement (measured by transfer's percentage of procurement-related actions, listed under Health Ministry's description of the transfer). The key idea is that different transfers are subject to different treatment intensities: there is a much higher scope to reduce procurement problems among procurement-intensive transfers after the program is introduced. Under the assumption of identical potential outcomes for the two sets of transfers, the differences-in-differences estimate, given by the within-municipality variation in corruption across transfers with different procurement intensities, before and after mayors learned they could be audited, captures the causal effect of the random-audits program on corruption. Our results indicate up to a substantial 17-percentage-point decrease in the differential corruption between high- and low-procurement transfers due to the program (out of a baseline incidence of 37% among the procurement-intensive transfers). [**Contextualizar o resultado com a literatura e ressaltar a sua importância**].

Di Tella and Schargrodsky (2003) study the effects from the introduction of crackdown audits on corruption among public hospitals in Buenos Aires. Nonetheless, they cannot observe corruption directly, rather looking at procurement prices for basic inputs, which are argued to differ among hospitals before the crackdown only due to corruption. Moreover, they are primarily interested in the effect of public officials' wages on corruption when there is a positive auditing probability.

To corroborate our results, we also run a series of robustness checks. We also report estimates of treatment effects using municipalities with below-median pre-2003 corruption as a control group. As before, there is a much higher scope to reduce procurement problems after the program is introduced among high-baseline municipalities. Results are robust across different empirical strategies. Furthermore, we also control for a wide array of municipal- and transfer-level characteristics, including team-of-auditors' fixed effects, the number of investigations and the availability of federal transfers. Additional evidence that the narrowing of the corruption gap is driven by the program comes from the timing of effects – increasing over time, in particular within 2003 –, from the geographic pattern of the effects according to the presence of an audited municipality within a 100 km radius, and from the responsiveness of the estimated effects to administrative penalties applied by the Health Ministry. Moreover, including the previous and the subsequent terms in the analysis does not change the results, which are shown not to be driven by a "second-half of the term effect".

We also document that anticipation is responsible for at least half of the total effect of the adoption of the monitoring program, as corruption responses to nearby or own audits are much less dramatic. Moreover, by contrasting program's effect on corruption prevalence within the 2001-2004 term with that over the following term, we are able to disentangle the effect of audits on moral hazard for elected politicians from that on selection of candidates for upcoming elections. Our estimates accrue roughly 75% of the effects to moral hazard.

The possible consequences of the decrease in corruption on health outputs and outcomes are also examined based on a difference-in-differences strategy where the adoption of the random-audit program is used as an exogenous source of variation in corruption. The differences-in-differences estimator is used to assess the within-municipality variation in health outputs and outcomes across transfers with different procurement intensities, before and after mayors learned they could be audited by the program. Surprisingly, health outputs and outcomes became worse with the lower incidence of corruption. This result is linked to the fact that such decrease in corruption has not led to higher compliance; mismanagement has arisen instead. In fact, mismanagement is estimated to have increased one-to-one with the decrease in corruption in response to the introduction of the random-audits program. Irregularities increased particularly for infrastructure (linked to health care facilities and equipment) and medication stock problems. We present evidence that drop in corruption can be entirely attributed to lower spending, for a given supply of funds: spending on procurement-intensive transfers is cut back by about 50% relatively to that on other transfers after the program was introduced. Discretion-intensive transfers faced the highest spending cuts, of the order of 2/3 of the initial budget.

Contrary to Reinikka and Svennson (2003, 2004, 2005), who document substantial effects of a newspaper campaign in Uganda that disclosed the official figures about the central Government's transfers to local primary schools on eliminating resource misallocation, with positive medium-term impacts on schooling outcomes, we show that although corruption dramatically dropped after the program was introduced, the decrease in corruption has not led to higher compliance or better health outcomes. The contradictory results may be explained by the choice of the exogenous source of variation in corruption. While we use the launch of the monitoring program as an instrumental variable, Reinikka and Svennson (2005) use the distance to the closest newspaper outlet. We argue that their choice of instrumental variable is not likely to satisfy the exclusion restriction if the demand for newspapers is a function of average education and if children's outcomes have differential trends in areas with different average education levels. Other differences from our paper stem from their focus on a specific transfer in which beneficiaries are able to track accurately the amount actually cashed in. In contrast, our paper analyzes politician's behavior concerning a wide spectrum of transfers targeted to benefit a disperse group of individuals who have imperfect information about how the outcomes of interest depend upon politicians' decisions.

As a robustness check we also look at alternative measures of health outputs from the notary registries of stillbirths and from household survey data. The results do not change.

The remainder of the paper is organized as follows. Section 2 details CGU's random-audits program. Section 3 describes the data and empirical strategy and presents the results for the first stage; and Section 4 does it for the second stage. Section 5 discusses the mechanism. Section 6 concludes the paper.

2. The Random Audits Program

The program, a joint venture with the national lottery, consists of public random draws to select in each period, the municipalities to be audited by officials from the Office of the Comptroller General (CGU). Auditors analyze accounts and documentation and physically inspect public works and services under implementation, to assess whether federal transfers are effectively applied to their specific ends. CGU's

official website points out that auditors also interact with local councils and municipal entities to stimulate and empower local citizens to play an effective role in monitoring the use of resources from tax revenues.[2]

The program was announced in early 2003. Four municipalities were audited as a pilot in February, followed by the announcement, in March, of the municipalities selected to be audited in the first draw. There was no decree or media announcement of the program prior to 2003.[3]

If it is fair to say that there were monitoring mechanisms prior to its introduction[4], the random-audits program represented not only a substantial increase in the probability of being audited, but also on the intensity of electoral damages conditionally on being exposed, as summary audit reports were systematically disclosed on the internet, and covered in newspapers, television and radio broadcasts.

The fact that voters punish mayors exposed to be engaged in corruption, as pointed out in Ferraz and Finan (2008), alongside with the substantial number of administrative penalties taken forward by the CGU since 2003 and even Brazilian Federal Police operations triggered by evidence from program's audit reports, suggests politicians should respond to the introduction of a technology that allows for information disclosure.

Last, it was not the case that other major institutional changes occurred throughout the same period. The random-audits program is still the most important corruption-deterrence mechanism in Brazil, and the fact that several political attempts have been made to terminate it, reduce the number of municipalities audited or increase the time span between draws only attests its concrete role in exposing corrupt politicians.

3. From audits to corruption

3.1 – Data

Estimation draws upon data from the random-audits program's reports for Health transfers. Although auditors investigate transfers linked to several Ministries, including Education, Transports and Environment, we only have access to data from Health audit reports up to July 2007, assembled by Fundação Getulio Vargas' Center of Politics and Economics of the Public Sector (CEPESP-FGV/SP).[5] Each report was independently catalogued 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 by draw 8, in March, 2004. While the audit took place in 2004, auditors have scrutinized transfers dating back to 2001 (e.g., resources from the federal government transferred on December 2001, which should have been kept in a separate bank account, according to federal norms, were not properly managed by the municipality). Accordingly, auditors reported an irregularity, catalogued in our database as evidence of mismanagement.[6]

Because the value embezzled on the event of corruption is seldom clear from audit reports, we restrict attention to incumbent's extensive margin decision of whether to be corrupt or not (explicitly controlling for the transfer's amount in our estimates at the transfer level).

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

[3] See Appendix B for more details about the program.

[4] In particular, CGU, since 2001, and TCU, even before that, were entitled to scrutinize (and approve, in the latter case) municipal accounts and to crackdown frauds.

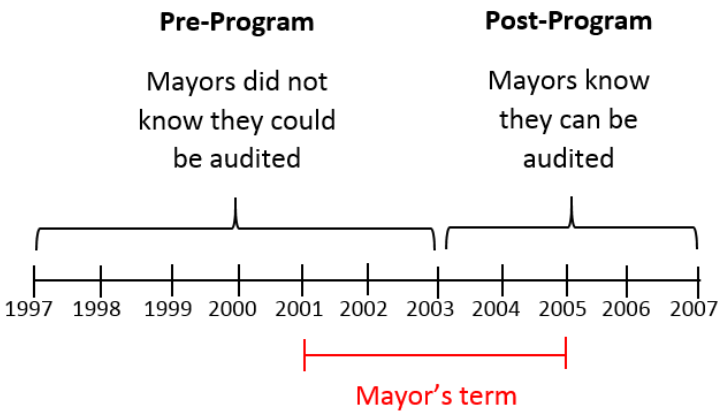
[5] For draw 25 on, with respect to Health transfers, and for all draws with respect to other Ministries, CEPESP did not catalogue the date of the irregularity (or absence of thereof), which is crucial for our research question. This is why we restrict attention to draws 2 to 24 (draw 1 was a pilot).

[6] See Appendix A for the classification list, and Apendix C for more examples of pre- and post-program findings described by audit reports.

Our baseline sample has 10,538 investigations for which we have coded the procurement intensity of the Health program under scrutiny, distributed across 20 Health programs and 1901 municipalities, from 1997 to 2007.[7] Procurement intensity is coded as follows: first, for each of those programs, every action associated to it, according to Ministry of Health's list of actions within each program (*Funcional Programática*), is coded as procurement-related or not, based on whether its description involves terms such as "inputs", "modernization", or "acquisition" (see Appendix B for the complete list); next, for each program, we compute the percentage of actions coded as procurement-related. In our regressions, we use a binary version of this variable, for which a program is coded as procurement intensive if the previous share exceeds or equals 50%. The sensitivity of the results to the chosen cutoff, to the choice of procurement-related terms, and to the discretization of the procurement-intensity variable, is considered in the supplementary material.

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 contains 33 categories of irregularities, ranging from "inadequate documentation" to "off-the-record invoice," 11 of which are coded as evidence of corruption and the remainder 21 as evidence of mismanagement. We follow Ferraz and Finan (2008, 2010) in defining corruption as procurement problems, over-invoicing and off-the-record invoice. The complete classification list is included in Appendix A.

Figure 1 – Timeline



The major difficulty in working with municipal-level data in Brazil is that most information for municipalities is only available from the censuses conducted by the Brazilian Institute of Geography and Statistics ("Instituto Brasileiro de Geografia e Estatística" , IBGE), updated about once every 10 years. For this reason and also to avoid endogeneity concerns, we control only for initial conditions (for the closest year available), except for mayor's attributes and political alignment variables, updated at the beginning of each term.

Data for mayor and administrative attributes come from the 2000 Municipal Information Database ("Base de Informações Municipais", BIM), and demographic and other municipal characteristics are retrieved from the 2000 Census; both surveys are conducted by IBGE. Health information is retrieved from

[7] Those are the 20 most prevalent Health programs in CEPESP's dataset, standing for 92.3% of the total number of observations.

the Information System for Health Public Budgets ("Sistema de Informações sobre Orçamentos Públicos em Saúde," SIOPS) and the National Health Database ("Base de Informações de Saúde," DATASUS). We also control for initial shares of revenue sources from federal transfers overall and in health, and for municipality's total budget (and in Section 5, as a robustness check, for yearly Health transfers by transfer group) using data from the National Treasury.

3.2 Empirical strategy

We start by exploring in Section 3.3 a simple comparison of the average corruption prevalence before and after the program was introduced, where we also display summary statistics of our data. Next, we move on to a different strategy in section 3.4. There, we split the sample according to the procurement-intensity of transfers within each municipality. 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 then other transfers, with a 17% corruption incidence in the baseline period. We then explore a differences-in-differences estimator to assess whether procurement-intensive transfers experienced a systematically higher drop in corruption prevalence following the introduction of the program. Drawing upon observations for the previous and the following terms (1997-2000 and 2005-2008) allows assessing the alternative hypothesis that corruption is systematically different between the two halves of municipal terms.

In Section 3.5 we no longer restrict attention to the anticipation effect – those adjustments following program's announcement early 2003. There, we also consider the effect of actual audits, by increasing the salience of the program to nearby municipalities, on the incidence of corruption across transfers with different embezzlement opportunities. Along those lines, in Section 3.6 we consider the effects of administrative penalties carried on by CGU, following up on the evidence of audit reports, as an additional source of corruption deterrence.

Moving next, in Section 3.7 we decompose program's effects between moral hazard and selection, taking advantage of the fact that municipal elections take place at the end of 2004. The net effect of the program spans, on one hand, higher-powered incentives during 2003-2004, when the decision maker was held fixed, and, on the other hand, higher-powered incentives and selection of a different pool of incumbents for the 2005-2007 period.

As a robustness check, in Section 3.8 we split the sample according to the incidence of corruption in the baseline period (2001-2002). Low-baseline corruption municipalities have up to 16.7% of the investigations coded as evidence of corruption, and an average of 2% in the baseline period, whereas high-baseline ones average 62% of investigations coded as corruption before 2003. The idea is that each group is subject to different treatment intensities: high-baseline municipalities have a much higher scope for reducing corruption after the program is introduced. We then explore a differences-in-differences estimator to assess whether high-baseline municipalities experienced a systematically higher drop in corruption prevalence following the introduction of the program.

Section 3.9 addresses additional robustness checks.

3.3 Results for before vs. after

Table 1 displays summary statistics for all the variables included in our analysis, over the 2001-2004 term. While 19% of all investigations are coded as evidence of corruption, we will show that this share is remarkably different across the two halves of the term, before and after the program was introduced.

Table 1 – Summary statistics (2001-2004)

Variable	Mean
Share of investigations coded as corruption	0.19
Distance to closest State capital (km)	238.38
Share of municipal revenues from transfers	0.82
Share of municipal transfer from SUS	0.09
Share of Health transfer from federal gov.	0.97
Share of SUS transfers from federal gov.	0.16
Per capita GDP (current 1000 R$)	4.81
Population	28316
Per capita Health expenditures (current R$)	135.08
Share of public sector in employment	0.03
Radio station	0.55
Access to internet	0.20
Community clubs	0.67
Share of population below extreme poverty line	0.27
Share of population below poverty line	0.48
Literacy rate	0.77
Average years of schooling	4.03
Mayor from the same party as Governor	0.22
Turnout rate	0.86
Electoral margin	0.16
Mayor's age	49.57
Male mayor	0.93
Mayor incomplete primary school	0.11
Mayor incomplete secondary school	0.14
Mayor complete secondary school	0.25
Mayor from Workers' Party (PT)	0.02
Mayor from Social Democrats' Party (PSDB)	0.18
Mayor serving second term	0.27
Health council exists	0.91
Health council age	8.99
Health council meets at least monthly	0.85
Hospital beds per thousand inhabitants	2.45
Hospitalizations per thousand inhabitants	74.43
Per capita medical consultations	0.93
Per capita emergency procedures	9.86
Immunization rate	78.91
Share of children under 2 with diarrhea	0.05
Share of children under 1 with malnutrition	0.03
Infant mortality	21.06
Share of population enrolled in Family Health Program	0.57
Share of households with access to clean water	0.60

Share of households with access to adequate sanitation	0.23
Total revenue (in 2000, R$)	12400000
Tax revenue (in 2000, R$)	918757
Revenue from current transfers (in 2000, R$)	9409631
Observations	1304

Figure 2 displays the share of investigations coded as evidence of corruption in each year. Visual inspection hints at the effect of the program: while corruption had been systematically above 20% since 1998 (and above 30% in 2002), it fell below 15% and stayed at this new level from 2003 on.

Figure 2 – Share of investigations coded as corruption

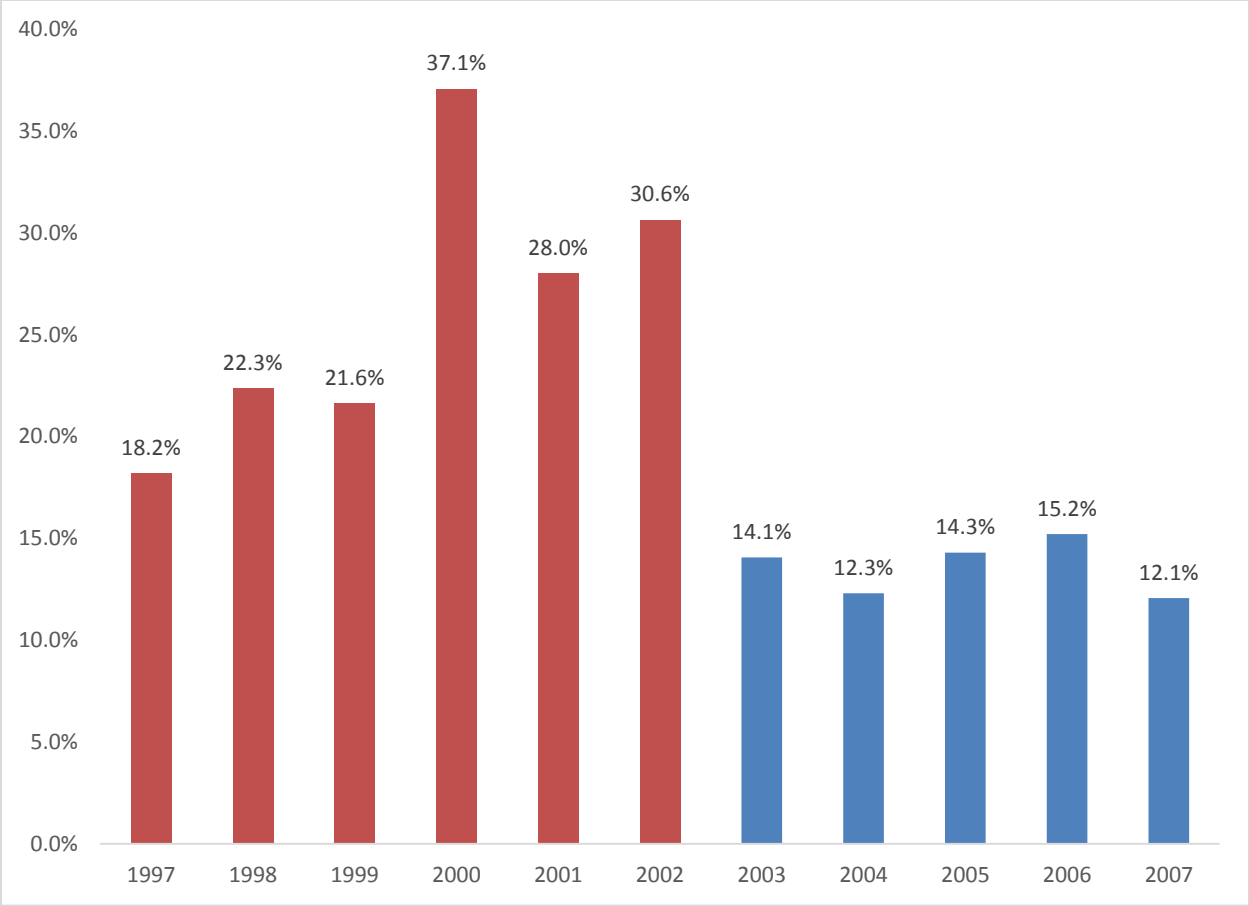


Table 2 presents the regression version of the previous figure. In column (1) there are no controls, and the 16.5% coefficient of Post-2003 stands for the simple difference in averages across the two halves of the term, for all 1304 municipalities with investigations within the 2001-2004 period. Column (2) incorporates geographic and socio-economic covariates, political variables, and mayor's attributes, with only a slight change to the estimated coefficient. Curiously, municipal-level controls are not systematically correlated with corruption except for mayor's party: PT mayors seem to be less associated with corruption, although that must be carefully interpreted, particularly in light of the fact that the party held at that point only a very small fraction of municipal governments, which are by no means expected to be similar with respect to characteristics not included as covariates in our analysis to the ones governed by other parties. Column (3) adds municipal fixed-effects, transforming variables in deviations from their means, such that only the political alignment indicator – which potentially varies in 2003, with State elections – remains as a control, with the Post-2003 coefficient estimated even higher in absolute value (-18.4%).

Table 2 – Corruption: before and after (2001-2004)

	Share of investigations coded as Corruption		
	(1)	(2)	(3)
Post-2003	-0.165***	-0.172***	-0.184***
	[0.0200]	[0.0220]	[0.0268]
ln(distance to closest State capital)		-0.043	
		[0.0314]	
ln(per capita GDP in 2000)		-0.0292	
		[0.0497]	
ln(population in 2000)		-0.0468	
		[0.0832]	
Share of public servants in formal employment		-0.481	
		[0.872]	
Municipality has a radio station		0.0261	
		[0.0241]	
Municipality has internet access		0.0215	
		[0.0335]	
Municipality has community clubs		0.0358	
		[0.0239]	
Literacy rate		0.0577	
		[0.120]	
Share of municipal revenues from transfers		-0.0273	
		[0.0948]	
Share of municipal transfer from SUS		0.071	
		[0.174]	
Share of Health transfer from federal gov.		-0.148	
		[0.118]	
Share of SUS transfers from federal gov.		0.0457	
		[0.0419]	
Total revenue (in 2000, R$)		-0.0157	
		[0.0130]	
Tax revenue (in 2000, R$)		0.00148	
		[0.0279]	
Mayor serving second term		0.017	
		[0.0239]	
Turnout rate		0.0457	
		[0.177]	
Electoral margin		0.085	
		[0.0643]	
Mayor from Workers' Party (PT)		-0.179***	
		[0.0263]	
Mayor from the same party as Governor		0.00792	-0.0276
		[0.0238]	[0.0543]
Mayor's age		-0.00128	

		[0.00113]	
Male mayor		-0.0121	
		[0.0356]	
Mayor incomplete primary school		0.038	
		[0.0363]	
Mayor incomplete secondary school		0.0243	
		[0.0315]	
Mayor complete secondary school		0.038	
		[0.0258]	
Constant	0.297***	0.168	0.316***
	[0.0194]	[0.457]	[0.0218]
Municipality fixed-effects	No	No	Yes
Observations	1304	1116	1304
R-squared	0.0672	0.0975	0.59

Robust standard errors clustered at the municipality level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Columns (1) to (3) are OLS regressions for the sample 2001-2004.

Of course, there are many confounding factors which might have driven corruption down after 2003, regardless of the introduction of the program. In particular, 2003 is the first year of PT's ruling at the national level, and how it should have affected mayors' decisions with respect to managing federal transfers is unclear. More broadly, the documented pattern could be consistently attributed to any aggregate variable that affected municipal Health spending differentially in the second half of the 2001-2004 term. This is why we move next to investigating heterogeneous treatment intensities.

3.4 Results for low procurement-intensity transfers as a control group

In this section, our empirical strategy consists of a differences-in-differences estimator given by the following:

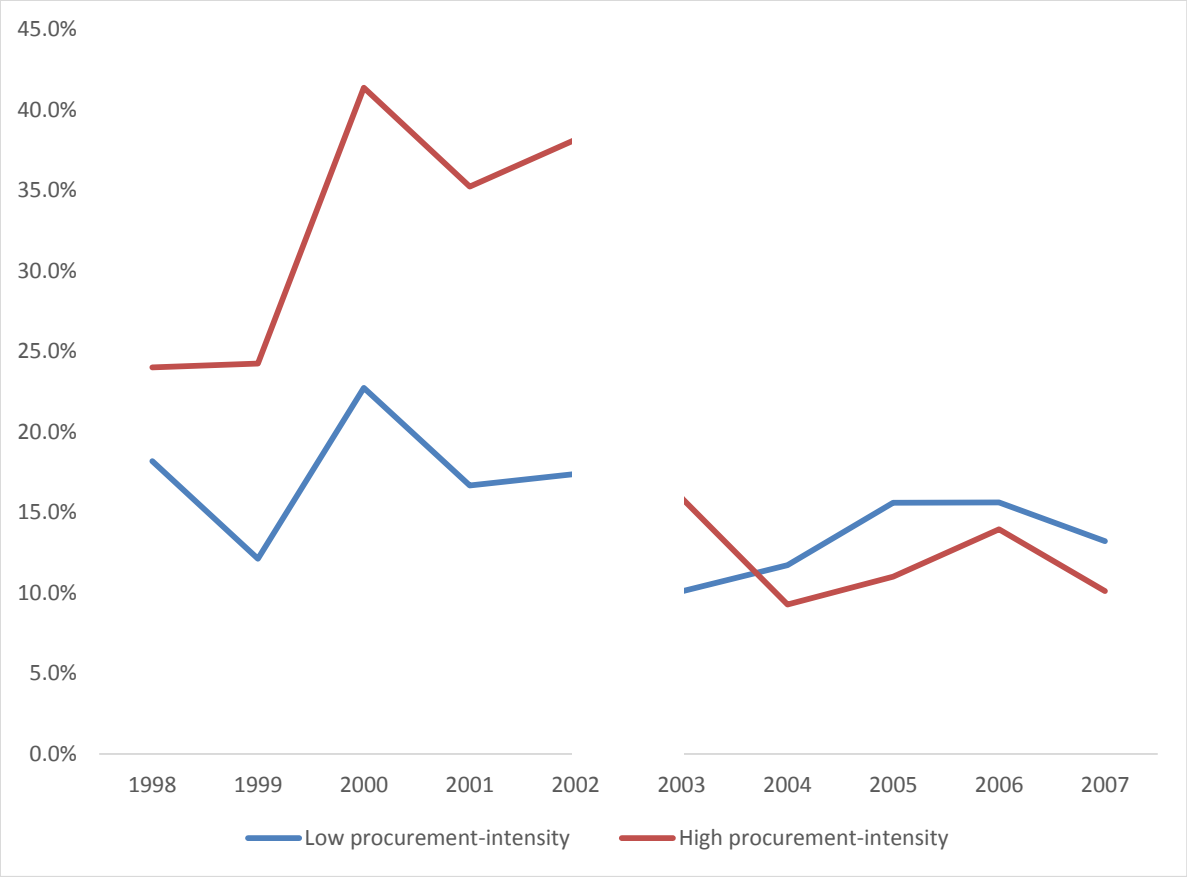
$$(2)\ PctCorruption_{j,m,t} = \beta PostProgram_t \ x\ ProcIntensive_j + \alpha_j + \alpha_m + \alpha_t + \gamma X_{m,t} + \epsilon_{j,m,t}\ ,$$

where $PctCorruption_{j,m,t}$ is the % of investigations among transfers of procurement-intensive status j in municipality m at year t coded as corruption, $PostProgram_t = 1$ from 2003 on, and 0 otherwise, $ProcIntensive_j = 1$ for procurement-intensive transfers, and 0 otherwise, α_j is a procurement-intensive transfer indicator variable, α_m are municipal fixed-effects, α_t are year fixed-effects, $X_{m,t}$ is a set of covariates for municipality m at year t, and $\epsilon_{m,k,t}$ is an error term. Under the assumption of identical potential outcomes between procurement-intensive transfers and else, β identifies the causal effect of the introduction of the random-audits program on the incidence of corruption.

Even though the identification assumption cannot be tested, it is instructive to assess whether the two groups had parallel trends before 2003. Figure 3 displays the yearly averages of corruption incidence for each group, hinting at the effects of the program: not only do averages for both groups seem to have fallen after 2003, but the gap between different transfers – those strongly associated with procurement and those

that are not – decreased substantially after the program was introduced. Visually, from 2004 on, corruption within procurement-intensive transfers became even less prevalent than within other transfers.

Figure 3 – Share of investigations coded as corruption



Before moving on to a regression analysis, Table 3 motivates the differences-in-differences estimator by pair-wise hypothesis testing of differences in corruption incidence, between low and high procurement-intensity transfers, before and after the introduction of the program. While the difference between treatment and control was of 19 percentage points, pre-2003, that gap was eliminated on the following period – a substantial decrease of 20 percentage points.

Table 3 – Conditional averages and pair-wise hypothesis testing

	Corruption		
	Low procurement-intensity	High procurement-intensity	Difference [Low - High]
Before 2003	0.17	0.37	-0.19***
	[0.02]	[0.03]	[0.04]
After 2003	0.12	0.12	0.00
	[0.01]	[0.01]	[0.01]
Difference [Before - After]	0.05**	0.25***	**-0.20*** **
	[0.03]	[0.03]	**(0.03)**

We start with municipal-level data for the 2001-2004 period in column (1) of Table 4 and assess how the estimated coefficient is affected by the introduction of a progressively richer set of controls. With a control group, now we are able to move further than last section results by including year fixed-effects from column (2) onwards. We also control for transfers' procurement-intensity (the continuous variable used to define the indicator variable which assigns treatment and control status in this setting) and the amount spent, augmenting the sample to the whole 1997-2007 period from column (3) onwards, adding term fixed-effects. Columns (4) and (5) include a second-half of the term placebo and its interaction with the procurement-intensive transfer indicator. Column (5) adds draw fixed-effects, in order to control for idiosyncratic team-of-auditor effects. Of course it does not make sense in this case to add Health program fixed-effects, since identification comes from exploring over time variation in corruption incidence across Health programs with different procurement intensities.

Also here, the estimated effects are stably documented across specifications, ranging from 16.7 to 19.6 percentage points, similar to the one computed in the naïve exercise in Table 3. Once again, procurement intensity is robustly associated with higher corruption incidence, whereas the amount spent shows no systematic relationship with corruption.

Table 4 –Differences-in-differences with low procurement-intensity transfers as a control group

	Share of investigations coded as Corruption				
	(1) Municipal-level [2001-2004]	(2) Transfer-level [2001-2004]	(3) Transfer-level [1997-2007]	(4) Transfer-level [1997-2007]	(5) Transfer-level [1997-2007]
Post-2003 x	-0.178***	-0.169***	-0.167***	-0.196***	-0.195***
High procurement intensity	[0.0387]	[0.0339]	[0.0330]	[0.0382]	[0.0382]
High procurement intensity	0.214***	0.174***	0.135***	0.135***	0.135***
	[0.0394]	[0.0405]	[0.0384]	[0.0384]	[0.0389]
Procurement intensity		0.0610**	0.0765***	0.0752***	0.0728**
		[0.0304]	[0.0290]	[0.0289]	[0.0294]
ln(transfer amount)		0.00379	0.00386	0.00338	0.00295
		[0.00478]	[0.00367]	[0.00372]	[0.00368]
Second-half of the term x				0.0421**	0.0434**
High procurement intensity				[0.0208]	[0.0207]
Second-half of the term				-0.239***	-0.279***

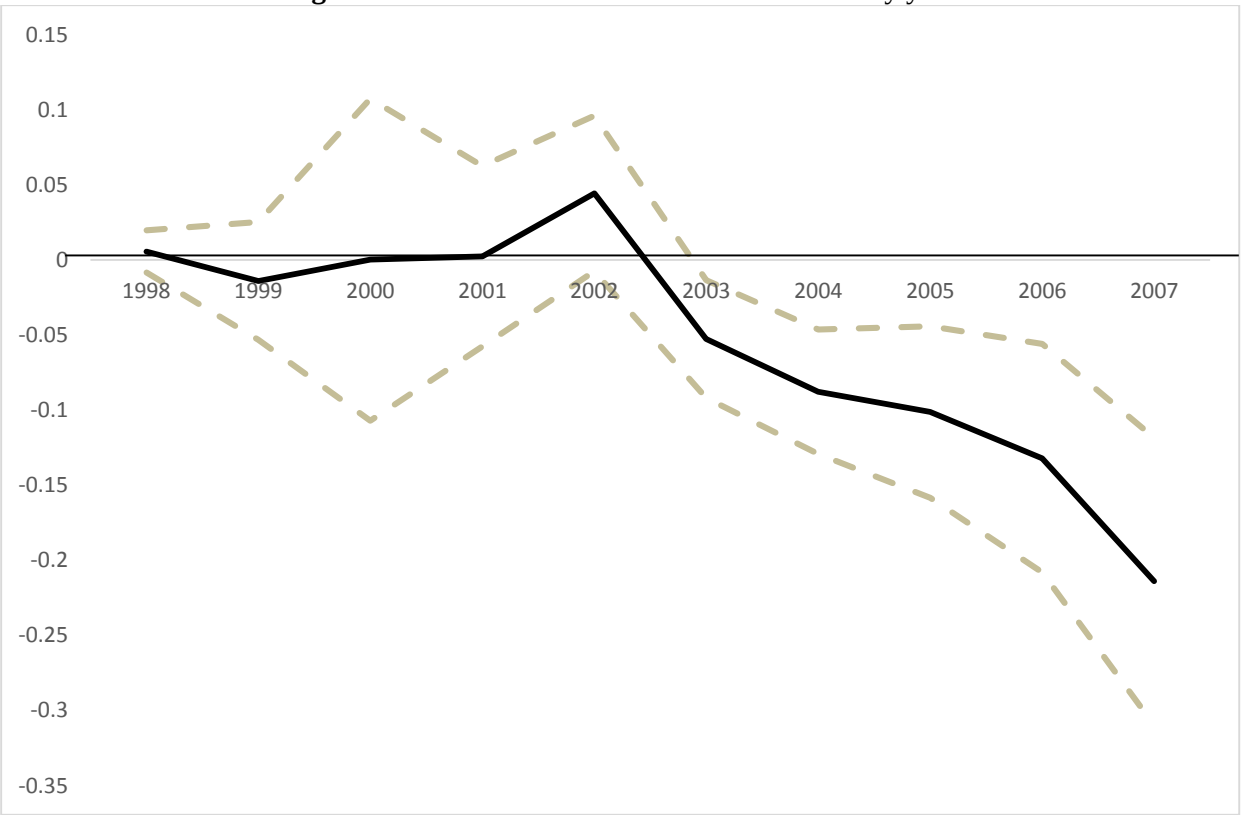
				[0.0487]	[0.0462]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes
Term fixed-effects	No	No	Yes	Yes	Yes
Draw fixed-effects	No	No	No	No	Yes
Observations	1,932	7,072	10,538	10,538	10,538
R-squared	0.555	0.243	0.196	0.196	0.201

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Column (1) is a GLS regression with the number of investigations used to build the municipal-level figures as weights. Columns (2) – (5) are OLS regressions. All municipal-level controls are included.

Next, in Figure 4 we allow the differences-in-differences coefficient to vary by each year in order to get traction at the timing of the effects. This time, not only do the effects becomes negative only post-2003, but, also, coefficients are not statistically different from zero up until that year, backing up the identification hypothesis of parallel trends, required for the differences-in-differences strategy to estimate a causal effect of the program on the incidence of corruption.

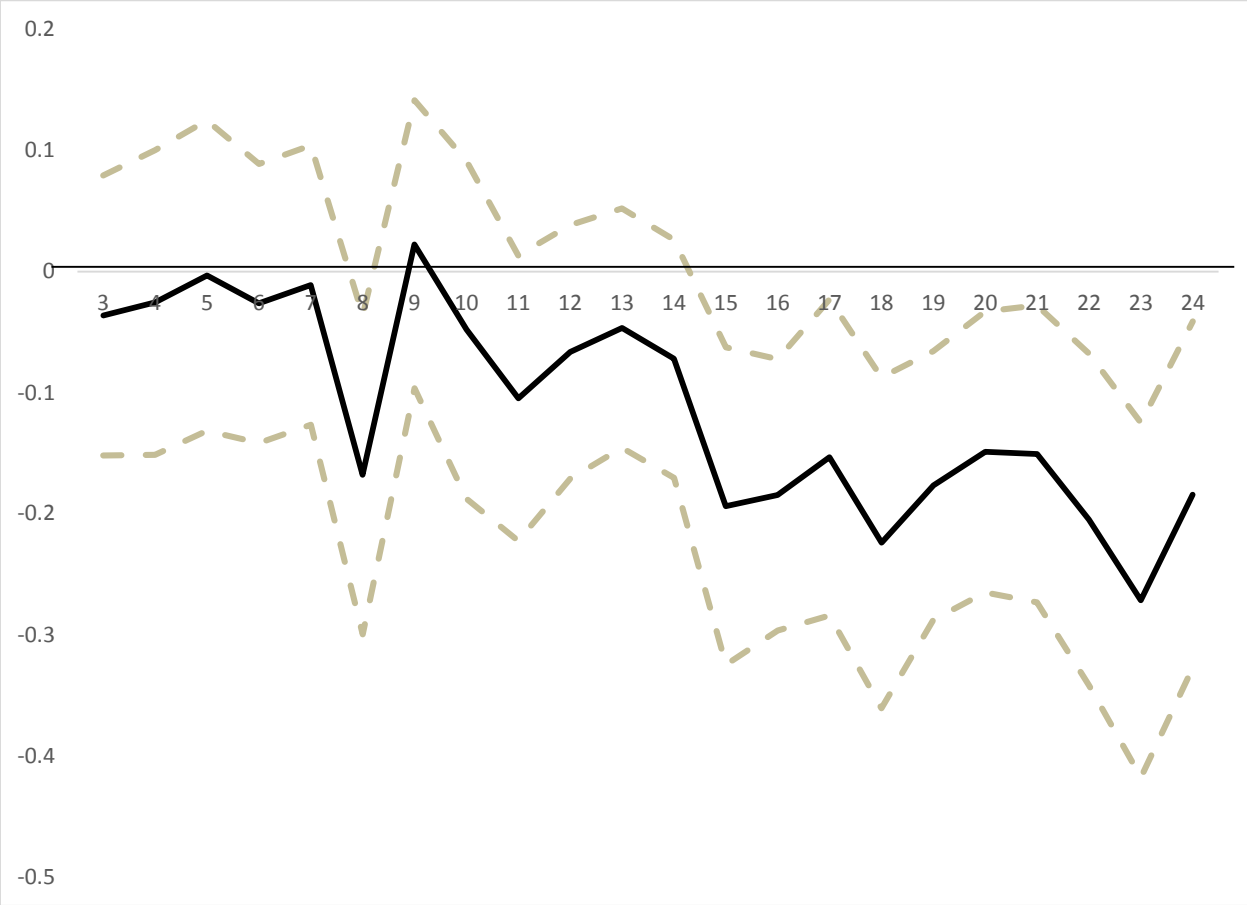
Figure 4 – Differences-in-differences coefficient by year



Notes: All municipal- and transfer-level controls, municipal-, year- and term-fixed effects are included. The dashed lines stand for the 95% confidence interval, computed from robust standard errors clustered at the municipal level.

A natural extension of the previous exercise is to allow the differences-in-differences coefficient to vary by draw. Because corruption is supposed to be at least somewhat sticky – when mayors and the municipal staff learn about the program, it might take some time to adjust –, one would expect to see the effects kick-in only after a few draws. Consistent with that hypothesis, Figure 5 shows a clear downward slope for the coefficients, particularly from draw 9 onwards. Moreover, it documents corruption to be quite sticky: it took almost 2 years for the diff-in-diff coefficient to differ systematically from zero (lottery 14 was drawn on November, 17, 2004).

Figure 5 – Differences-in-differences coefficient by draw



Notes: All municipal- and transfer-level controls, municipal-, year- and term-fixed effects are included. The dashed lines stand for the 95% confidence interval, computed from robust standard errors clustered at the municipal level.

3.5 Anticipation vs. salience

An additional test of whether the effects previously documented can be attributed to the program explores insights from behavioral economics (Kahneman, 2003). If the program becomes more salient to incumbents when a nearby municipality is audited, then this event should lead to an additional differential reduction in corruption across the two groups. To assess this hypothesis, we add two variables to our differences-in-differences specification:

$$(3)\ PctCorruption_{j,m,t} = \beta PostProgram_t \ x\ ProcIntensive_j + \phi AuditWithin100km_{m,t}\ x\ ProcIntensive_j + \omega AuditWithin100km_{m,t} + \alpha_j + \alpha_m + \alpha_t + \gamma X_{m,t} + \epsilon_{j,m,t}\ ,$$

where $AuditWithin100km_{m,t} = 1$ if a municipality (possibly m itself) is audited in a year previous to t within 100 km of the centroid of municipality m, and 0 otherwise.[8] We are interested in whether ϕ has a negative and significant effect on the prevalence of corruption, along the same lines as β.

We experiment with different definitions of 'previously audited' in Table 5. In columns (1) and (2), it stands for an audit within the 100 km radius in year t-1; in columns (3) and (4), it stands for an audit within this radius in any year prior to t; and in columns (5) and (6), it is instead a smooth-decay function of the distance to the closest audited municipality in year t-1 (ranging from 0 to 1, with 1 if the municipality itself is audited).

Results are quite robust across specifications, and computing the estimated effects at the averages of the proximity variables yield nearly identical results for the "salience effect" of the program. While the "anticipation effect", given by (Post-2003 x High procurement intensity), ranges from 13.2 to 16.5 percentage points, the "salience effect" is about 4 percentage points per year at which a nearby audit takes place.

Table 5 – Anticipation and salience effects

	Share of investigations coded as Corruption					
	(1) 2001-2004	(2) 1997-2007	(3) 2001-2004	(4) 1997-2007	(5) 2001-2004	(6) 1997-2007
Post-2003 x High procurement intensity	-0.150***	-0.144***	-0.150***	-0.132***	-0.164***	-0.165***
	[0.0346]	[0.0332]	[0.0346]	[0.0334]	[0.0340]	[0.0329]
High proc. intensity x Audit within 100 km in t-1	-0.0424**	-0.0399**				
	[0.0187]	[0.0173]				
Audit within 100 km in in t-1	0.136**	0.0486				
	[0.0652]	[0.0324]				
High proc. intensity x Audit within 100 km in any previous year			-0.0424**	-0.0601***		
			[0.0187]	[0.0188]		
Audit within 100 km in any previous year			0.136**	0.117**		
			[0.0652]	[0.0501]		
High proc. intensity x 1 - exp(1/distance to closest audit in t-1)					0.333**	0.0966**
					[0.134]	[0.0471]
1 - exp(1/distance to closest audit in t-1)					2.015**	0.801

[8] Only 12 municipalities in our sample are audited more than once from 2003 to 2007, what restricts our ability to assess the independent effect of being audited on corruption.

					[0.789]	[0.807]
Mean of proximity variables at 2004/2007	0.87	0.65	0.87	0.96	0.99	0.10
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Term fixed-effects	No	Yes	No	Yes	No	Yes
Observations	7,072	10,538	7,072	10,538	7,072	10,538
R-squared	0.245	0.197	0.245	0.198	0.243	0.197

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Columns (1), (3) and (5) are OLS regressions with the 2001-2004 sample, and columns (2), (4) and (6) are OLS regressions with all observations (1997-2007), controlling for second-half of the term effects. All municipal-level controls are included.

The results in Table 5 incidentally allow answering an interesting side question: what share of the effect of the random-audits program accrues to *ex-ante* anticipation, and how much to *ex-post* salience? Table 6 computes the share of the anticipation effect drawing upon previous estimates. Broadly speaking, at least half of in the total effect can be attributed to anticipation.

Table 6 – Anticipation effect

	Previous years with audits within 100km (by 2004)		**Previous years with audits within 100km (by 2007)**				
	0	1	0	1	2	3	4
Total effect	-15.0%	-19.2%	-14.4%	-18.4%	-22.4%	-26.4%	-30.4%
Anticipation effect (% of total effect)	100.0%	78.0%	100.0%	78.3%	64.3%	54.6%	47.4%

Notes: Calculations use Table 9's columns (1) and (2) coefficients for (Post-2003 x High proc. intensity) as *anticipation effect* and [(Post-2003 x High proc. Intensity) + (Post-2003 x High proc. Intensity x Audit within 100 km in t-1) x (Previous years with audits within 100 km)] as *total effect*.

3.6 Punishment

Next, we assess whether actual punishments carried forward by CGU after 2003 (administrative penalties for public servants, ranging from reallocation to a different function to termination from public office) affect the salience of the program and induce further differential decreases in corruption prevalence. Table 7 documents that a significant part of the effect seems to operate along that dimension: the (Post-2003 x High procurement intensity) coefficient is reduced with respect to previous estimates, and the scaling up of administrative penalties following the program, particularly within the Health Ministry, is negatively correlated with the incidence of corruption.

Table 7 – Administrative penalties

	Share of investigations coded as Corruption	
	(1) [2001-2004]	(2) [1997-2007]
Post-2003 x High procurement intensity	-0.0936***	-0.0890**
	[0.0323]	[0.0323]
High proc. intensity x Administrative penalties in State, t-1	0.000143	-0.000280
	[0.000384]	[0.000464]
High proc. intensity x Administrative penalties within Health, t-1	-0.00106***	-0.000711**
	[0.000282]	[0.000285]
Administrative penalties in State, t-1s	-0.000367	-0.000970**
	[0.000435]	[0.000424]
Administrative penalties within Health, t-1	-0.00137	-0.000237
	[0.000943]	[0.00117]
Mean of all punishments by State 2004/2007	26.1	62.4
Punishments within Health Ministry at 2004/2007	101	192
Municipality fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Term fixed-effects	No	Yes
Observations	7,072	10,538
R-squared	0.244	0.198

Robust standard errors clustered at the State level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Notes: Column (1) is an OLS regression with the 2001-2004 sample, and column (2) is an OLS regression with all observations (1997-2007), controlling for second-half of the term effects. All municipal-level controls are included.

3.7 Moral hazard vs. Selection

Next, we assess program's effect on moral hazard relative to that on selection. The idea is that audits might curb corruption through two main mechanisms: either by generating incentives for incumbent politicians to comply with regulation, or by inducing the replacement of current incumbents by more disciplined ones on the following elections. Towards that goal, we contrast the effect of the program within the term, on 2003-04, to that on the following term, from 2005 onwards. In column (2), we control for past audits in order not to confound the difference with the salience effects described in the section 3.5.

Table 8 documents that the introduction of the program drove corruption down by about 13.76 percentage points due to incentives for elected mayors (74.3% of the effect of the program), whereas it led corruption to decrease another 4.75 percentage points (25.7% of the effect of the program) due to selection of more disciplined mayors for the 2005-2008 term.

Table 8 – Moral hazard vs. Selection

	Share of investigations coded as Corruption	
	(1) [1997-2007]	(2) [1997-2007]
Post-2003 x High procurement intensity	-.15936***	-.13795***
	[.0323953]	[.033325]
Post-2005 x High procurement intensity	-.06778**	-.04745**
	[-.0677793]	[.0214761]
Post-2003	-.10385***	-.09569***
	[.0236841]	[.023465]
Post-2005	-.0301602	-.0238865
	[.0213323]	[.0216559]
Audit within 100 km in any previous year x High procurement intensity		-.04085**
		[.0185584]
Audit within 100 km in any previous year		-.02087
		[.0190838]
Municipality fixed-effects	Yes	Yes
Year fixed-effects	No	No
Term fixed-effects	Yes	Yes
Observations	10,538	10,538
R-squared	0.192	0.193

Robust standard errors clustered at the State level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Column (1) and (2) are OLS regressions with all observations (1997-2007), controlling for second-half of the term effects. All municipal-level controls are included.

3.8 Results for low-baseline corruption municipalities as a control group

As a robustness check, in this section, our empirical strategy consists of a differences-in-differences estimator given by the following:

$$(4)\ PctCorruption_{m,t} = \beta PostProgram_t \ x\ HighBaseline_m + \alpha_m + \alpha_t + \gamma X_{m,t} + \epsilon_{m,t}\ ,$$

where $PctCorruption_{m,t}$ is the % of investigations in municipality m at year t coded as corruption, $PostProgram_t = 1$ from 2003 on, and 0 otherwise, $HighBaseline_m = 1$ for the high-baseline group, and 0 otherwise, α_m are municipal fixed-effects, α_t are year fixed-effects, $X_{m,t}$ is a set of covariates for municipality m at year t, and $\epsilon_{m,t}$ is an error term. Under the assumption of identical potential outcomes between low- and high-baseline municipalities, β identifies the causal effect of the introduction of the random-audits program on the incidence of corruption.

Table 9 below displays the results. Columns (1) and (2) are for municipal-level averages, while columns (3) to (7) are at the transfer-level, controlling for transfer characteristics, such as procurement-intensity and amount. Starting in column (4), we augment the sample to the whole 1997-2007 period adding term fixed-effects, in order to test the perhaps most immediate confounding factor to the introduction of the program: a second-half of the term placebo (=1 in 1999-2000, 2003-2004 and 2007-2008, and =0 otherwise). From column (5) onwards we interact such placebo with the above median baseline corruption indicator, so as to assess whether there is something special about the post-2003 period different from the usual forces of the political cycle. Column (6) adds draw fixed-effects, in order to control for idiosyncratic team-of-auditor effects, and column (7) adds health program fixed-effects, so as to enhance comparability by controlling for transfer characteristics which are not captured by its procurement intensity or amount spent.

The estimated coefficient is quite stable across specifications, ranging from 39.5 to 49.4 percentage points, similar to the one computed in the naïve exercise in Table 3. It is interesting that, despite controlling for the binary indicator of high procurement intensity, its intensive variable is still robustly associated with higher-corruption: moving from just the cutoff level of procurement intensity (50%) to 100% is estimated to increase the incidence of corruption in about 3 percentage points. Conversely, the amount spent does not seem to be systematically correlated with corruption, conditional on all other variables.

Table 9 –Differences-in-differences with below-median baseline corruption as a control group

	Share of investigations coded as Corruption						
	[2001-2004]				[1997-2007]		
	Municipal-level (1)	Municipal-level (2)	Transfer-level (3)	Transfer-level (4)	Transfer-level (5)	Transfer-level (6)	Transfer-level (7)
Post-2003 x High baseline corruption	-0.417***	-0.410***	-0.395***	-0.411***	-0.494***	-0.471***	-0.464***
	[0.0350]	[0.0350]	[0.0311]	[0.0305]	[0.0594]	[0.0574]	[0.0559]
Post-2003	0.0357**						
	[0.0176]						
Procurement intensity			0.0847***	0.0568***	0.0574***	0.0599***	
			[0.0207]	[0.0186]	[0.0209]	[0.0212]	
ln(transfer amount)			0.00175	0.00164	0.000892	-0.000150	-0.00858
			[0.00570]	[0.00445]	[0.00497]	[0.00491]	[0.00670]
Second-half of the term x High baseline corruption					0.0874	0.0667	0.0835
					[0.0554]	[0.0532]	[0.0533]
Second-half of the term					-0.250***	-0.287***	-0.260***
					[0.0696]	[0.0721]	[0.0698]
Municipality fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed-effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Term fixed-effects	No	No	No	Yes	Yes	Yes	Yes
Draw fixed-effects	No	No	No	No	No	Yes	Yes
Health program fixed-effects	No	No	No	No	No	No	Yes
Observations	990	990	4,989	6,224	6,224	6,224	6,224
R-squared	0.670	0.679	0.251	0.212	0.212	0.219	0.241

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Columns (1) and (2) are GLS regressions with the number of investigations used to build the municipal-level figures as weights. Columns (3) – (7) are OLS regressions. Columns (1) to (3) use data from the 2001-2004 sample, whereas columns (4) to (7) use all the data between 1997 and 2007. All municipal-level controls are included.

Of course, treatment and control status are not randomly assigned in this setting and, not surprisingly, differ systematically in some important dimensions – as documented in the supplementary material, which tests differences between groups in covariates over the baseline period (2001-2002). High-baseline municipalities are, on average, larger municipalities: they have larger population and higher tax revenues, a larger supply of public goods, more educated mayors and host a municipal Health council with higher probability. Taking such differences into account is crucial for not confounding the causal effects of the program with that of differential trends in corruption due to systematic differences in baseline covariates. In the supplementary material we also control for interactions of all baseline characteristics with liner trends in column (1), quadratic in column (2) and cubic in column (3). The estimated coefficient is quite stable across specifications, of the order of 40 percentage points.

3.9 Additional robustness checks

This section addresses additional concerns that the effects previously documented might be due to other confounding factors. First, it could be that corruption has not become relatively lower among procurement-intensive transfers, but that corrupt municipalities are especially good at "erasing the paper trail" (or at preventing auditors from investigating, perhaps by bribing or threatening them) of transfers with higher embezzlement opportunities. Why that would be the case only for investigations post-2003 is puzzling, but, still, it would be good to rule out that possibility to the extent of our powers. To assess that hypothesis, columns (1) and (2) in Table 9 control for the number of investigations among transfers of procurement-intensive status j in municipality m at year t.

An additional concern is linked to the findings in Brollo (2010) that the federal government reduces voluntary transfers to municipalities exposed as corrupt. Although that effect, taken literatlly, would be irrelevant in our case, since we restrict attention to mandatory, constitutional transfers, throughout, if a version of that argument held for the release of funds of constitutional transfers (differentially for procurement-intensive ones), it could be that procurement-intensive transfers display less corruption after 2003 simply because they are more severely constrained afterwards.

Unfortunately, there is no record of the supply of funds assigned to the triple (Health program, municipality, year) in Brazil. However, we can proxy for that variable with municipal-level funds released yearly for two major Health transfers, one for a group of programs which can use those funds for a variety of purposes, including procurement, and another which explicitly preclude municipalities from using its funds to procure goods or services.[9] We control for the category-wide federal transfers (ln) towards municipality m at year t in columns (3) and (4) of Table 9.

[9] There is only municipal-level yearly information for three categories of Health transfers: Basic Attention (which encompasses other Health programs beyond Basic Attention itself, such as Basic Pharmacy), Medium and High Complexity, and Strategic Actions. Since pages 55-56 of DENASUS' "*Manual de auditoria na gestão dos recursos financeiros do SUS*", 2004, explicitly preclude the utilization of funds for the Medium and High Complexity category for procurement, we coded it as non-procurement intensive, and use the Basic Attention category as procurement-intensive for this purpose. The Strategic Actions category is not included because it is a less systematic transfer, restricted to about 10% of Brazilian municipalities each year.

Table 10 – Number of investigations and supply of funds

	Share of investigations coded as Corruption			
	(1) [2001-2004]	(2) [1997-2007]	(3) [2001-2004]	(4) [1997-2007]
Post-2003 x High procurement intensity	-0.164***	-0.160***	-0.155**	-0.140**
	[0.0336]	[0.0327]	[0.0675]	[0.0702]
Number of investigations	0.00486***	0.00499***		
	[0.00167]	[0.00141]		
ln(supply of funds by transfer group)			-0.00375	-0.00389
			[0.00448]	[0.00402]
Municipality fixed-effects	Yes	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes	Yes
Term fixed-effects	No	Yes	No	Yes
Observations	7,072	10,538	5,222	7,923
R-squared	0.245	0.198	0.269	0.224

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Columns (1) and (3) are OLS regressions with the 2001-2004 sample, and columns (2) and (4) are OLS regressions with all observations (1997-2007), controlling for second-half of the term effects. All municipal-level controls are included.

Results are very robust to the inclusion of either the number of investigations or the supply of federal transfer by category: the estimated coefficient ranges from 14 to 16.4 percentage points across specifications. Although investigations are indeed positively correlated with corruption prevalence, the supply of transfers is not systematically associated with the latter.

Additional robustness checks, such as the sensitivity of results to the procurement-intensity cutoff, to the choice of procurement-related terms, and to the discretization of the procurement-intensity variable, is considered in the supplementary material.

4. From corruption to outcomes

4.1 Data

We now turn to the second-stage of our estimation procedure. Having documented that the program decreases corruption significantly and substantially, we exploit this exogenous source of variation to assess how the program affected health outputs and outcomes. Data for health outputs and outcomes are from DATASUS, restricting attention to the municipalities and periods for which we have data on the prevalence of corruption.

4.2 Empirical strategy

First, in section 4.3 we assess the cross-sectional relationship between corruption and health outputs and outcomes for the baseline period (2001-2002). Next, in section 4.4, we split the sample according to the procurement-intensity of transfers within each municipality. We explore a differences-in-differences

estimator to assess whether procurement-intensive transfers experienced a systematically different variation in health outputs and outcomes following the introduction of the program.

Identification still requires high and low procurement-intensity transfers not to have different potential outcomes in what comes to outcomes and outputs after 2003. To minimize concerns with differential trends, we also include an interaction of high-procurement intensity with an indicator for past audits within 100 km as independent variable. Since audits are random, they satisfy the identification assumption by design.

Section 4.5 presents robustness checks.

4.3 Cross-sectional relationship

In column (1), the dependent variable is the share of the population in municipality m at year t assisted by the Family Health program ("*Programa Saúde da Família*", PSF); in column (2), the share of the population under 1 year old diagnosed with malnutrition by PSF's family doctors; in column (3), the share of the population under 2 years old with diarrhea episodes; in column (4), the supply of hospital bed per 1,000 inhabitants (ln); in column (5), the share of the population immunized[10]; and in column (6), infant mortality per 1,000 under 1 year old (ln).

OLS estimates, presented in Table 10 are at odds with the evidence in Ferraz, Finan and Moreira (2012): corruption is positively correlated with Health outputs or outcomes: higher corruption is associated with more hospital beds per 1,000 inhabitants and lower infant mortality. Of course, such estimates capture differences across low- and high-corruption municipalities; in particular, as documented in the supplementary material, more corrupt municipalities are have larger population, have higher tax revenues and better access to public goods in general.

Table 10 – Pre-program (2001-02) correlation between corruption and Health outputs and outcomes

	(1) % population covered by Family Health program	(2) Hospital beds per 1,000 inhabitants	(3) Immunization (shots / 1,000 inhabitants)	(4) % under 1-year-old children with malnutrition	(5) % under 2-year-old children with diarrhea	(6) Infant mortality per 1,000 live births
Share of investigations coded as Corruption	-0.0984	1.503*	1.212	-0.00560	-0.0124*	-4.307*
	[0.0619]	[0.789]	[1.554]	[0.00599]	[0.00699]	[2.219]
Mean (2001-02)	0.49	2.46	79.22	0.06	0.04	21.52
Standard deviation (2001-02)	0.38	3.59	11.68	0.05	0.04	17.40
Observations	386	403	403	386	386	403
R-squared	0.431	0.284	0.220	0.374	0.323	0.259

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

[10] DATASUS considers several vaccines in this indicator; see http://tabnet.datasus.gov.br/cgi/pni/notatecnicaCobertura.pdf

4.4 Results for low-procurement intensity transfers as a control group

In order to documents the effects of the program on health outputs and outcomes, we need to address the challenge of comparing outputs and outcomes of different programs. This is because identification of causal effects arises from contrasting high- and low-procurement intensity transfers, before and after 2003. To do so, we convert all variables to z-scores, so that they all have mean 0 and variance 1 over the 2001-2002 period. This procedure yields comparability, normalizing the units of estimated coefficients to standard deviations.

Our empirical strategy consists of a differences-in-differences estimator given by the following:

(5) $Z_{j,m,t} = \beta PostProgram_t x\ ProcIntensive_j + \alpha_j + \alpha_m + \alpha_t + \gamma X_{m,t} + \epsilon_{j,m,t}$, where

$Z_{j,m,t} = \frac{1}{n_i}\sum_i \frac{Y_{i,j,m,t} - \overline{Y}_{i,j,t<2003}}{\sigma_{i,j,t<2003}}, j \in \{\text{high,low}\}$, and $Z_{j,m,t}$ stands for the average of all outputs and outcomes linked to high- or low-procurement intensive programs.

To base our decision of which variables to assign to high- or low-procurement intensive z-scores, we draw upon Health Ministry's official M&E indicators, which assign indicators for each health transfers. Whenever the same indicator applied to multiple transfers, unless they were all high-procurement intensity or low-procurement intensity, we did excluded that variable in the analysis. That process yielded the following: z-scores for high procurement-intensive transfers average hospital beds per 1,000 inhabitants, immunization, and water and sanitation coverages; z-scores for low procurement-intensive transfers average coverage of the Family Health program and medical consultations.

Since the identification still requires that z-scores for both groups have identical potential outcomes over the post-2003 period, Figure 6 gauges preexisting differential trends across the groups. Although the data is noisy, it seems that z-scores for both groups were moving similarly during the 2001-2002 period. The groups, however, largely diverge from 2003 on, with low procurement-intensity z-scores steadily increasing, while high procurement-intensity z-scores end up below their pre-2003 level.

Figure 6 – Z-scores by year and procurement-intensity

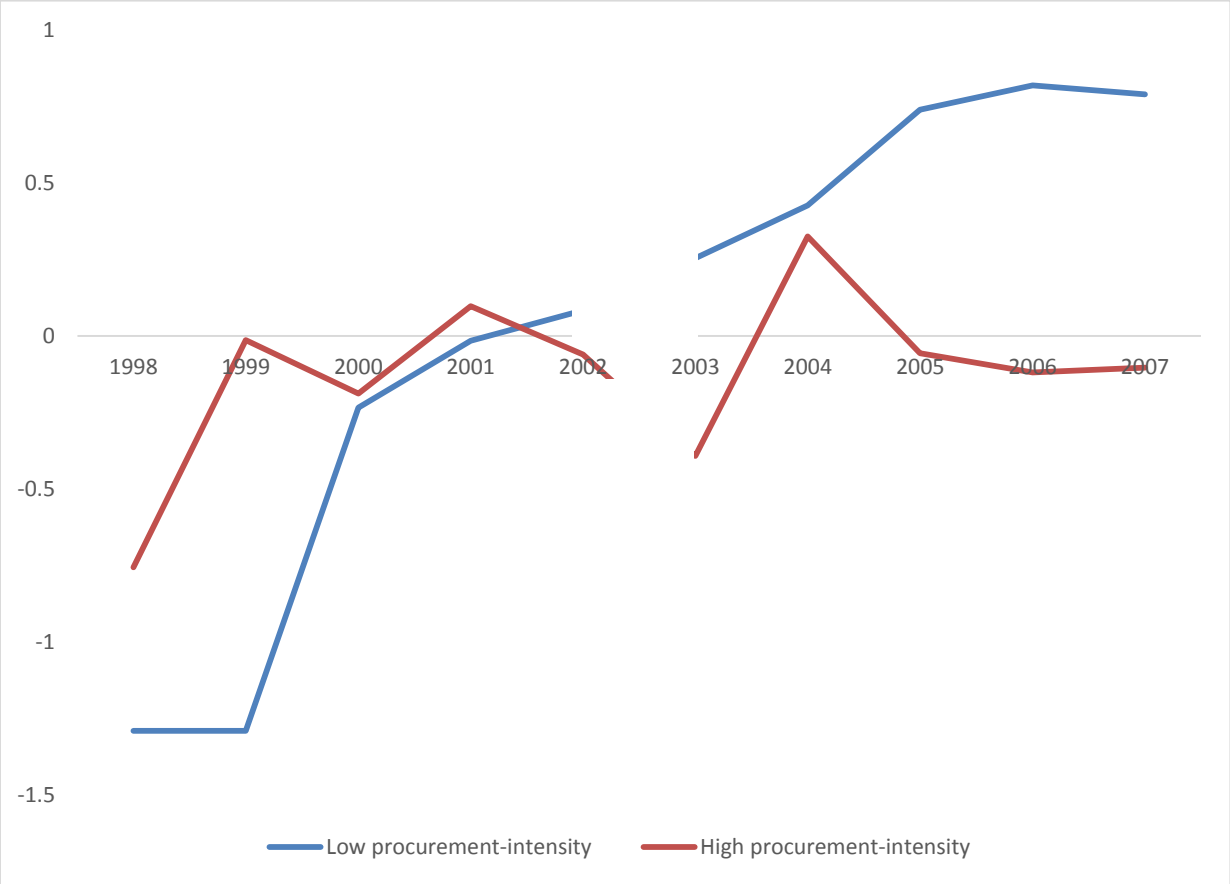


Table 11 presents the regression version of Figure 6, controlling for potential confounders. Z-scores are documented to have decreased significantly after the introduction of the program, by at least 0.3 (considering only 2001 to 2004) and at most nearly 0.5 standard deviations (considering the 1997-2007 period). Such effects are quantitatively very large: as a benchmark, deworming school-age kids in Kenya increased weight-for-age by 0.09 after one year (Kremer and Miguel, 2004). In column (3) we add an indicator of past audits interacted with high-procurement intensity. Since audits are random, this source of exogenous variation does not require the hypothesis of identical potential outcomes. Since the estimate for this variable's effect on z-scores is not statistically significant, we can confidently state that the random-audits program did not have a positive effects on health outputs and outcomes.

Table 11 –Health outputs and outcomes: low vs. high procurement-intensity, before and after

	(1) [2001-2004]	(2) [1997-2007]	(3) [1997-2007]
Post-2003 x High procurement-intensity	-0.309*** [0.107]	-0.494*** [0.0948]	-0.495*** [0.103]
High proc. intensity x			0.00140

Audit within 100 km in previous year			[0.0977]
Audit within 100 km in previous year			-0.109
			[0.0897]
Municipality fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Term fixed-effects	No	Yes	Yes
Observations	1,870	2,699	2,699
R-squared	0.556	0.513	0.514

Notes: Columns (1) is an OLS regression for the 2001-2004 sample, with dependent variable given by z-scores of immunization for procurement-intensive transfers, and of Family Health coverage (% population) otherwise. Columns (2) and (3) are OLS regressions for the 1997-2007 sample, with dependent variable given by the average of z-scores of immunization and per thousand hospital beds, for procurement-intensive transfers, and of Family Health coverage (% population) otherwise. All z-scores are normalized by variable's 2001-02 mean and standard deviation. All municipal-level controls are included, as well as a second-half of the term indicator for the even-numbered columns.

4.5 Robustness checks [INCOMPLETE]

Removing one variable at a time from the computation of z-scores to assess whether results are driven by any one specific output or outcome.

5. Mechanism: In what sense is corruption 'greasing the wheels'?

The negative results in the previous section might be seen as evidence that corruption "greases the wheels" of public service provision. There might be at least two reasons for this to be the case. First, by making corruption more costly, audits might have induced public officials to "skim off the bottom", or engage in other distortionary behavior in order to keep their economic rents. Since embezzling from procurement has become very costly, bureaucrats possibly revert to embezzling from purchased vaccines or medication stocks, for instance, reselling them instead of distributing them to the public. Second, as the random-audits program makes misprocurement extremely dangerous for incumbents (who can be imprisoned for it) and procurement staff (who can be terminated from public office), procurement staff might have held spending back, leading to under-provision of public goods and services. Although it might be the case that the two stories might have taken place to a lesser or greater extent, we explore different dimensions of our rich dataset to try to shed light on the mechanisms.

Section 5.1 explores another dimension of audit reports: irregularities coded as mismanagement, which we have not distinguished from compliance so far – pooling them as instances of absence of corruption. Because our data has very detailed information on the sources of mismanagement, we can look at differential variation across high- and low-procurement intensive transfers, before and after 2004, for different mismanagement categories in order to gauge which hypothesis is more likely to explain such pattern. Section 5.2 provides a more thorough analysis of the second hypothesis, by looking at differential

trends in spending for different procurement-intensity transfers after the introduction of the program. Section 5.3 further explores this angle, documenting whether the effects in 5.2 vary systematically with the extent to which bureaucrats can exercise discretion for different transfers.

Next, Section 5.4 entertains an alternative hypothesis: that the lack of positive effects of the random-audits program on health outputs and outcomes is the artifact of previous overspending. If that is the case, the marginal dollar might not have any effect on public service delivery, so results would not be surprising. The section presents evidence that largely contradicts that claim. Last, Section 5.5 takes stock of the discussion about mechanisms.

5.1 Mismanagement

We start by decomposing investigations not coded as "evidence of corruption" into mismanagement and compliance.[11] Table 12 documents that the decrease in corruption (column 1) basically translates one-to-one to an increase in mismanagement (column 2); compliance (column 3) is not significantly affected by the program nor by actual audits nearby.

Table 12 – Corruption, mismanagement and compliance

	(1) Corruption	(2) Mismanagement	(3) Compliance
Post-2003 x	-0.150***	0.153***	-0.00262
High procurement-intensity	[0.0346]	[0.0446]	[0.0294]
Audit within 100 km in t-1 x	-0.0424**	0.0247	0.0177
High proc. intensity	[0.0187]	[0.0253]	[0.0199]
Audit within 100 km in t-1	0.136**	-0.116	-0.0198
	[0.0652]	[0.0734]	[0.0314]
Municipality fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Observations	7,072	7,072	7,072
R-squared	0.245	0.251	0.301

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Columns (1) to (3) are OLS regressions for the 2001-2004 sample. All municipal-level controls are included.

This result makes it less surprising that the program did not enhance outputs and outcomes – but it is curious that switching to mismanagement might have made things worse. To shed further light on that issue, Table 13 decomposes mismanagement in six categories. In column (1), resource diversion stands for misapplication of resources mean to be used for one program towards other programs (within Health or not); in column (2), Health council problems encompass precarious conditions or insufficient frequency of

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

meetings; in column (3), performance problems stand for complaints from final users about frustrated appointments or admissions, lack of availability of medication or low quality of health services; in column (4) infrastructure and stock problems refer to physical problems with health units (including mobile units, such as ambulances) or medication not properly kept in stock or ins and outs not properly accounted for; in column (5), human resources problems encompass problems with public servants' wages or professionals not working along program's guidelines; and in column (6), documentation problems range from receipts not properly kept to invoices with erased or illegible information.

Table 13 –Mismanagement categories

	(1) Resource diversion	(2) Health council problems	(3) Performance problems	(4) Infrastructure and stock problems	(5) Human resources problems	(6) Documentation problems
Post-2003 x High procurement-intensity	-0.0614**	0.0289*	-0.0404	0.156***	0.0169	0.0906**
	[0.0260]	[0.0162]	[0.0324]	[0.0280]	[0.0147]	[0.0360]
High proc. intensity x Audit within 100 km in t-1	-0.0328*	0.00420	0.0188	0.0106	0.00790	0.0414*
	[0.0175]	[0.00875]	[0.0250]	[0.0251]	[0.0147]	[0.0235]
Audit within 100 km in t-1	-0.0346	-0.0142	-0.0215	0.0398	-0.00578	-0.0915**
	[0.0385]	[0.0221]	[0.0456]	[0.0482]	[0.0222]	[0.0424]
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	7,072	7,072	7,072
R-squared	0.237	0.125	0.136	0.210	0.124	0.165

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Columns (1) to (6) are OLS regressions for the 2001-2004 sample. All municipal-level controls are included.

Results show that resource diversion among high-procurement intensive transfer decreases significantly as a result of both the announcement of the program and nearby audits, mimicking the movement of corruption itself. Conversely, Health council problems, infrastructure and stock problems and documentation problems increase systematically more for procurement-intensive transfers. The two latter categories are the most quantitatively important reactions to the random-audits program.

In particular, the differential rise in infrastructure and medication stock problems among procurement-intensive transfers could perfectly explain why outcomes do not improve or even become worse even though corruption is now lower. Which mechanism explains it – "skimming off the bottom" or cutting back spending – is however unclear, as both could possibly play out given this patter. Section 5.2 turns to spending in order to disentangle the two of those.

5.2 Spending

To analyze differential trends in spending across different transfers after the introduction of the program, a major challenge is that the transfers which are object of this paper – constitutional transfers from the federal government to municipalities – do not admit leftovers from a legal standpoint. This means that all transferred resources must be executed, otherwise they cannot be held until the next fiscal year and return to the federal government. For our sake, this creates one critical hurdle: for those transfers, spending is not recorded in fiscal books. Since we know for a fact that execution is far below 100% (see section 5.4), we rely on an alternative measure of execution: audited amounts.

To justify it, remember that auditors leave CGU's headquarters with pre-set service orders, so that 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.

Having introduced the dependent variable that we will be using throughout this section and the following one, we turn to our empirical strategy. We rely on a differences-in-differences estimator, comparing high- and low-procurement intensity transfers, before and after 2003. Figure 7 shows that the audited amounts (in logs) for the two sets of transfers followed similar trends for the pre-program period. Both fall substantially after the program, however, that for procurement-intensive transfers falls significantly more, and keeps trending down while low procurement-intensity transfers trend up from 2003 onwards.

Figure 7 – ln(amount) by year and procurement-intensity

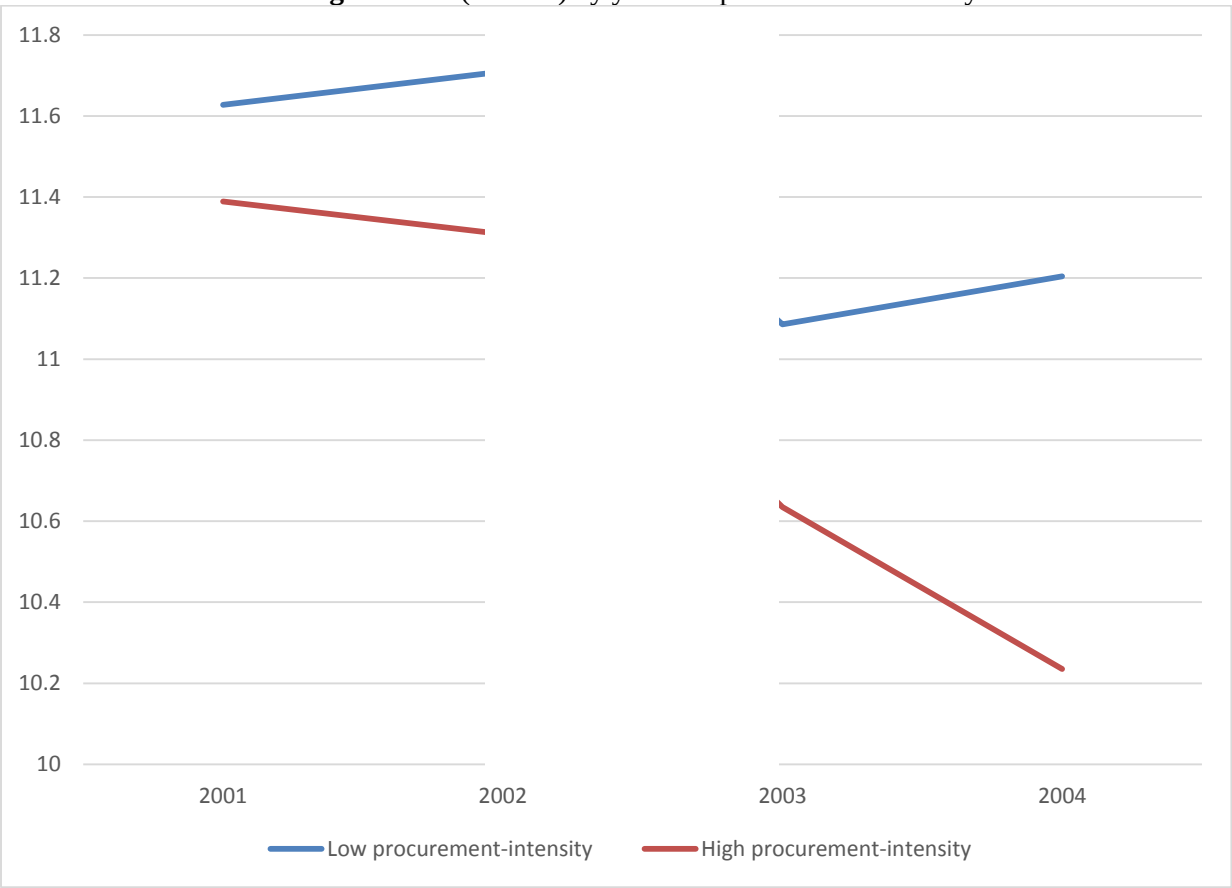


Table 15 presents the regression version of Figure 7, controlling for potential confounders, including the number of investigations and the differential supply of funds across the two sets of transfers, which we introduced in section 3.9. In all columns, the program is documented to have had a very significant and negative effect on spending among health transfers.

Table 15 – Spending as dependent variable

	ln(transfer amount)		
	(1)	(2)	(3)
Post-2003 x	-0.490***	-0.455***	-0.562**
High procurement-intensity	[0.133]	[0.133]	[0.272]
Number of investigations		0.0234***	0.0253***
		[0.00749]	[0.00866]
ln(supply of funds by transfer group)			-0.0279
			[0.0277]
Municipality fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Observations	7,072	7,072	5,222
R-squared	0.416	0.419	0.446

Robust standard errors clustered at the municipal level in brackets
*** p<0.01, ** p<0.05, * p<0.1

Notes: Columns (1) to (3) are OLS regressions for the 2001-2004 sample. All municipal-level controls are included.

To highlight how striking the results of Table 15 are, we can decompose of the variation in corruption over time as follow:

$$(6)\ C_t = \frac{Y_t}{T_t} = \left(\frac{Y_t}{X_t}\right)\left(\frac{X_t}{T_t}\right)$$

The first equality defines corruption at time t, C_t as the ratio of corruption events at time t, Y_t, to the number of transfers 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 dollar spent per transfer ratio at time t, $\frac{X_t}{T_t}$. Taking logs and differentiating with respect to time,

$$(7)\ \frac{d\ln(C_t)}{d\,t} = d\,\frac{ln\left(\frac{Y_t}{X_t}\right)}{d\,t} + d\,\frac{ln\left(\frac{X_t}{T_t}\right)}{d\,t}$$

The first term on the right-hand side is the percentage change in corruption if spending per transfer was held constant; the second term is the percentage change in spending per transfer.

With $\frac{d\,ln(C_t)}{d\,t} = -0.3$, according to the results of Table 6 for a municipality with nearby audits in every year up to 2007, and $\frac{d\,ln(X_t)}{d\,t} = -0.5$, approximately, according to the results of Table 15, corruption per dollar spent must have actually increased by 20 percentage points to prevent a further decrease in the share of investigations coded as corruption.

5.3 Discretion-intensity

The previous section advanced a clear argument in support of the hypothesis that the negative effect of the program on outcomes must be primarily due to spending cuts. To further explore this mechanism, in this section we assess whether spending was particularly reduced for those transfers which bureaucrats understood as having higher room for discretion.

We start by defining discretion-intensive transfers as those with above-median cases of resource diversion problems unrelated to corruption in the pre-2003 period (see table A4 in Appendix A).[12] Such problems reflect attempts by bureaucrats to utilize money earmarked for a particular action to fund another action within the same health program, an action from another health program, or an action from a program outside of health. Our interpretation is that transfers which presented systematic attempts to divert resources to other programmatic actions are those for which bureaucrats exercised the highest levels of discretion.

The empirical strategy is to interact the high-procurement indicator with a high-discretion indicator, as outline by Table 16. We are particularly interested in the coefficient for the interaction term (Treatment 2).

Table 16 – Procurement-intensity vs. discretion-intensity

	Low Discretion	**High Discretion**
Low procurement-intensity	Control group	
High procurement-intensity	Treatment 1	Treatment 2

Table 17 documents that the effects in the previous section are entirely driven by discretion-intensive transfers, for which spending differentially decreased by 66.85 percentage points between high- and low-procurement transfers. This results reinforces the interpretation that procurement staff held spending back afraid of accidentally misprocuring after the introduction of the program.

[12] Different from our procurement-intensity definition, in this case we cannot draw upon programmatic descriptions to code this variable. Nevertheless, our definition reflects an empirical measure of discretion at a period when mayors did not know they could be audited.

Table 17 – Spending as dependent variable

	ln(transfer amount) (1)
Post-2003 x High procurement-intensity	-.03602 [.1656674]
Post-2003 x High discretion-intensity	.06336 [.174313]
Post-2003 x High procurement-intensity x High discretion-intensity	-.66851*** [.1516437]
Municipality fixed-effects	Yes
Year fixed-effects	Yes
Observations	7,072
R-squared	0.423

Notes: OLS regression for the 2001-2004 sample. All municipal-level controls are included.

5.4 Overspending

An alternative explanation for the documented patterns following the introduction of the random-audits program is that, before the program, public officials provided health outputs above their optimal level in order to create opportunities for embezzlement.[13] If that statement is correct, the marginal dollar might not have any effect on public service delivery, so that reduced spending and lower provision of health outputs could even increase efficiency.

Table 18 below suggests that is not likely to be the case. Comparing hospital beds per 1,000 inhabitants, immunization, and access to water and sanitation for Brazil relatively to other comparison countries and regions, by no means Brazil seems to be overproviding health outputs before or after the program.

Table 18 – Health statistics for Brazil and comparison countries/regions

Panel A

	Hospital beds per 1,000 inhabitants		% of population immunized for measles	
	2000-2002	**2003-2012**	**2000-2002**	**2003-2012**
Argentina	4.1	4.4	91.7	96.8
Brazil	2.6	2.4	98.0	99.0
Chile	2.6	2.2	96.0	92.6
China	2.5	3.1	84.7	93.7

[13] For a discussion and evidence about how fiscal spending relates to corruption, see, for instance, Goel and Nelson (1998).

East Asia & Pacific (all income levels)	3.4	3.6	83.9	89.9
Europe & Central Asia (all income levels)	7.1	6.0	91.2	93.3
European Union	6.3	5.7	89.7	92.5
Latin America & Caribbean (all income levels)	.	2.1	94.0	94.1
Middle East & North Africa (all income levels)	.	1.6	90.0	89.7
Mexico	1.1	1.5	95.7	96.4
OECD members	5.6	5.0	91.7	93.4

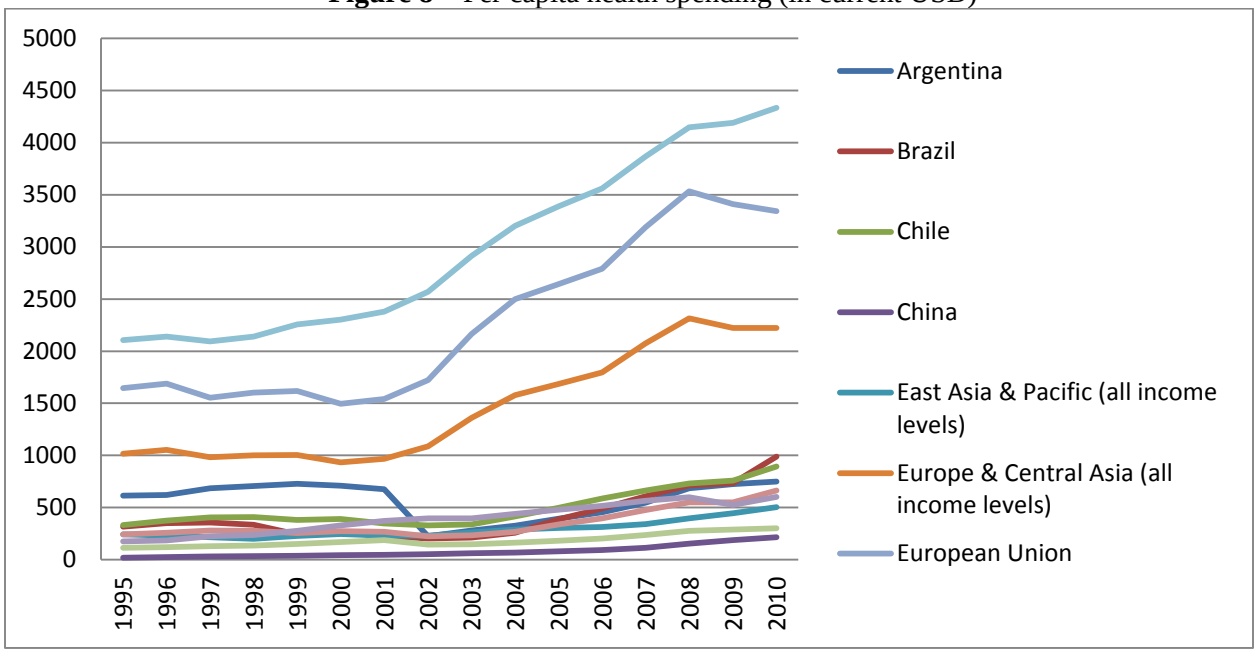
Panel B

	% of rural households with drinking water		% of rural households with sanitation	
	2000	**2012**	**2000-2002**	**2003-2012**
Argentina	81.1	95.3	82.5	99.4
Brazil	75.7	85.3	39.5	49.2
Chile	67.8	91.3	69.3	89.3
China	70.4	84.9	35.5	55.8
East Asia & Pacific (all income levels)	71.7	84.8	41.5	59.0
Europe & Central Asia (all income levels)	90.8	94.1	84.3	88.5
European Union	96.9	99.7	92.7	99.6
Latin America & Caribbean (all income levels)	71.8	82.5	48.9	63.1
Middle East & North Africa (all income levels)	79.3	83.5	67.5	81.1
Mexico	73.4	90.8	55.0	79.0
OECD members	93.9	97.9	91.2	94.9

Source: World Bank.

Moreover, Figure 8 displays per capita health expenditures between 1995 and 2010, placing Brazil in the same range as other Latin American countries.

Figure 8 – Per capita health spending (in current USD)



Source: World Bank.

What is more, budget execution rates by Brazilian municipalities are strikingly low in general, and particularly so for health spending. Between 2011 and 2013, municipalities executed only 8% of budgeted health transfers from the federal Government.[14] It is hard to make the case for why would the federal Government systematically over-allocate 92% of its funds to municipal offices, often ran by competing parties.

How then would one reconcile what we claim to be underspending with the traditional logic of overspending by politicians in office? A simple model, outlined in Appendix D, pins down the logic behind these results. Mayors must procure in order to provide public goods and services. Procurement generates opportunities for embezzlement, what creates a tendency for overspending among high-amount transfers – those for which the benefits of embezzlement exceed the expected costs of being caught. However, to the extent that accidental misprocurement is also labeled as corruption, the opposite tendency holds for low-amount transfers: mayors will procure less often than optimally, underproviding public goods and services as a result.

5.5 Discussion

Our interpretation of the results are not that they make the case for corruption, but that they document a trade-off between precluding corruption and incentivizing spending. The simple model in Appendix D suggests that one would do better by restricting audits to high-amount transfers (the ones for which embezzlement is more likely), and that capacity building is a necessary complement to a random-audits

[14] http://portal.cfm.org.br/index.php?option=com_content&view=article&id=24589:apos-tres-anos-governo-conclui-apenas-11-das-acoes-em-saude&catid=3

program like the one we analyze (or even a substitute, as a lower likelihood of accidentally misprocuring also decreases the probability of corruption for high-amount transfers, by making the alternative safer).

However, the implementation of capacity building programs is challenging. In 2006, CGU instituted a capacity-building program called Strengthening Public Management ("Fortalecimento da Gestão Pública", FGP).[15] Even though we cannot assess the impacts of that program, as there were no municipalities in our sample drawn by FGP and subsequently audited, Lopes (2011) finds no significant effect of the program on the prevalence of corruption.

Due to budget constraints, the random-audits program has been recently downscaled. From 5,000 investigations per year in 2010, it fell down to 2,000 in 2011 and 1,000 in 2013. The need to reduce audits has led CGU to concentrate investigations in State capitals, focusing on large transfers (those more likely to involve overspending, according to the model). Because that process was driven by the need to cut down on costs, rather than by a diagnostics that it is efficiency-enhancing, it is unclear whether the previous setup would be resumed once budget constraints become less important.

6. Concluding remarks

Jim Kim, president of the World Bank, has claimed that "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 No. 1".[16] This paper suggests that such extreme views should be taken with a grain of salt. As Leys (1965, p.220) anticipated, "even in the case of petty bribery or extortion, it is relevant to ask, what is the alternative?".

We assessed the effects of the introduction of a monitoring mechanism, Brazilian Office of the Comptroller General's ("Controladoria Geral da União", CGU) random-audits program, which increased the probability of local politicians being audited, on the prevalence of corruption to assess the effects of monitoring on corruption within health transfers, and on health outputs and outcomes.

Results are that the gap in corruption between transfers with different embezzlement opportunities dramatically narrowed after the program was introduced; nevertheless, the decrease in corruption has not led to higher compliance nor better health outputs and outcomes. In fact, mismanagement is estimated to have increased one-to-one with the decrease in corruption in response to the introduction of the random-audits program. Irregularities increased particularly for infrastructure and stock problems. Our analysis of the dramatic cutback in spending following the introduction of the program – so large as to account entirely

[15] Municipalities must enroll to be eligible for in-house training tailored to public officials, in particular procurement staff. Municipalities are drawn to be trained also based on a random selection mechanism. Currently, training includes topics in public administration, ethics, governance principles and compliance. The program is typically one-week long, comprising five days and approximately 30 hours of course load. Ideally, the program targets local government employees - managers, specialist officers and technical support staff -, but, in some cases, local community leaders are invited to participate. On average, considering data from 2005 to 2010, 13 public officials are capacitated in each training period, in each municipality, and enrollment rates are usually related to the size of the local government. For more details, see Lopes (2011).

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

for the corruption decrease – is evidence that raising the costs to misprocurement without providing guidance on good procurement practices can actually make outcomes worse.

Despite being the highest per capita GDP country in Latin America, Brazil fares fairly poorly in Health (in 2010, it was only the 10th out of 20 Latina American countries with respect to infant mortality, and 13th in what comes to life expectancy at birth, according to the World Health Organization). Given that decentralization of federal spending from 1996 on increased municipal Health spending seven-fold (from 0.1% of GDP in 1995 to 0.72% in 2010), we believe this paper sheds light on some critical constraints for bringing country's Health outcomes more in line with its higher-income status.

Overall, our results suggest corruption 'greases the wheels' of public service delivery in a setting where assessments of the quality of public management do not go beyond procurement problems. Creating a legal framework along the lines of draft law 931/2007, which proposed expanding the scope of irregularities for which administrative penalties can be applied, would be an important step towards balancing incentives between procuring, on the one hand, and making proper use of public funds upon procurement, on the other.

Future research should clarify the connections between corruption, public management, and outcomes, and the extent to which information and capacity building about best practices can improve public service delivery.

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Appendix A – Classification lists

Table A1 – List of irregularities

Panel A: Corruption

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

Panel B: Mismanagement

Category	Irregularity
Resource diversion	Unconfirmed payments
Resource diversion	Diversion of resources for other goals
Resource diversion	Diversion of resources for other goals within Health
Resource diversion	Diversion of resources for other goals within Program
Resource diversion	Under-application of resources
Health council	Irregular Composition
Health council	Irregular Operation
Health council	Poor infrastructure and work conditions
Performance	Unaccomplished goals
Performance	Unfinished projects
Performance	Poorly evaluated services to health system users
Supplies and facilities	Precarious facilities
Supplies and facilities	Signs, logos and so on 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	Incomplete or inadequate documentation

Table A2 – Procurement-intensity by program

Health Ministry Code	Health Program	% of action coded as procurement-related
0119	SANEAMENTO BÁSICO (Sanitation)	100.00
0004	QUALIDADE E EFICIÊNCIA DO SUS (Quality and efficiency of the Unified Health System)	54.17
0005	ASSISTÊNCIA FARMACÊUTICA (Pharmaceutical Assistance)	50.00
0013	VIGILÂNCIA EPIDEMIOLÓGICA E AMBIENTAL EM SAÚDE (Epidemiological and environmental surveillance in Health)	50.00
0002	PREVENÇÃO E CONTROLE DE DOENÇAS TRANSMITIDAS POR VETORES (Prevention and control of vector-transmitted diseases)	38.46
1214	ATENÇÃO BÁSICA EM SAÚDE (Basic Attention in Health)	4.55
0001	SAÚDE DA FAMILIA (Family Health)	0.00
0023	ATENDIMENTO AMBULATORIAL, EMERGENCIAL E HOSPITALAR (Admission, emergency and hospital services)	0.00
1335	TRANSFERÊNCIA DE RENDA COM CONDICIONALIDADES (Conditional Cash Transfer)	0.00

Table A3 – List of procurement-related terms

- “PAB Fixo” (Fixed part of the Basic Attention Program)
- “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)

Table A4 – Discretion-intensity by program

Health Ministry Code	Health Program	% of resource diversion problems (except over-invoicing and off-the-record invoicing) in 1997-2002
0023	ATENDIMENTO AMBULATORIAL, EMERGENCIAL E HOSPITALAR (Admission, emergency and hospital services)	16.0
1214	ATENÇÃO BÁSICA EM SAÚDE (Basic Attention in Health)	15.0
0119	SANEAMENTO BÁSICO (Sanitation)	14.7
0005	ASSISTÊNCIA FARMACÊUTICA (Pharmaceutical Assistance)	12.6
0001	SAÚDE DA FAMILIA (Family Health)	10.4
1335	TRANSFERÊNCIA DE RENDA COM CONDICIONALIDADES (Conditional Cash Transfer)	7.1
0004	QUALIDADE E EFICIÊNCIA DO SUS (Quality and efficiency of the Unified Health System)	6.7
0002	PREVENÇÃO E CONTROLE DE DOENÇAS TRANSMITIDAS POR VETORES (Prevention and control of vector-transmitted diseases)	5.2
0013	VIGILÂNCIA EPIDEMIOLÓGICA E AMBIENTAL EM SAÚDE (Epidemiological and environmental surveillance in Health)	0.0

Appendix B – More details about CGU Random Audits 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 random-audits program is a federal government initiative to inhibit corruption across all levels of the public administration.

The program currently audits municipalities up to 500,000 inhabitants.[17] 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 represents over 99% of Brazilian municipalities

[17] It 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 drawn, 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.

or about 70% of the country's population) which currently lie within the maximum population eligibility thresholds.[18, 19]

The random-audits program investigates transfers earmarked to execute national health and education policies (mandatory constitutional 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 lifecycle": (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 tranfers, from the Federal Treasury's account to its current stage under the municipality's discretion, whether in previous years or under a previous political ruling. As such, even though the program began in 2003, we have information for every year of the mayor's term from 2001 to 2004 (our full dataset extends for the 1997-2007 period). Once the audit is complete, CGU officials describe all irregularities detected for each transfer (if any) in official reports.[20]

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.

Appendix C – Examples from audit reports

Examples of pre-program findings (2001-2002)

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.

[18] Population thresholds were the following for past draws: 250,000 inhabitants for the third draw, 300,000 inhabitants for the fourth to the eighth, and 500,000 inhabitants for the ninth to the 26th. Some of these draws also displayed a minimum threshold of 10,000 inhabitants.

[19] Given that the random-audits program is administered by a de facto national anti-corruption agency, one might be concerned that the program is used as a politicized tool, and thus ineffective for preventing corruption. However, the random selection mechanism run through the national lottery (organized by "Caixa Econômica Federal") has eliminated political influence over eligible municipalities, given the transparency needed for an anti-corruption program to be credible; see Ferraz and Finan (2008) for a thorough test of the hypothesis of randomness of the selection mechanism.

[20] 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.

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

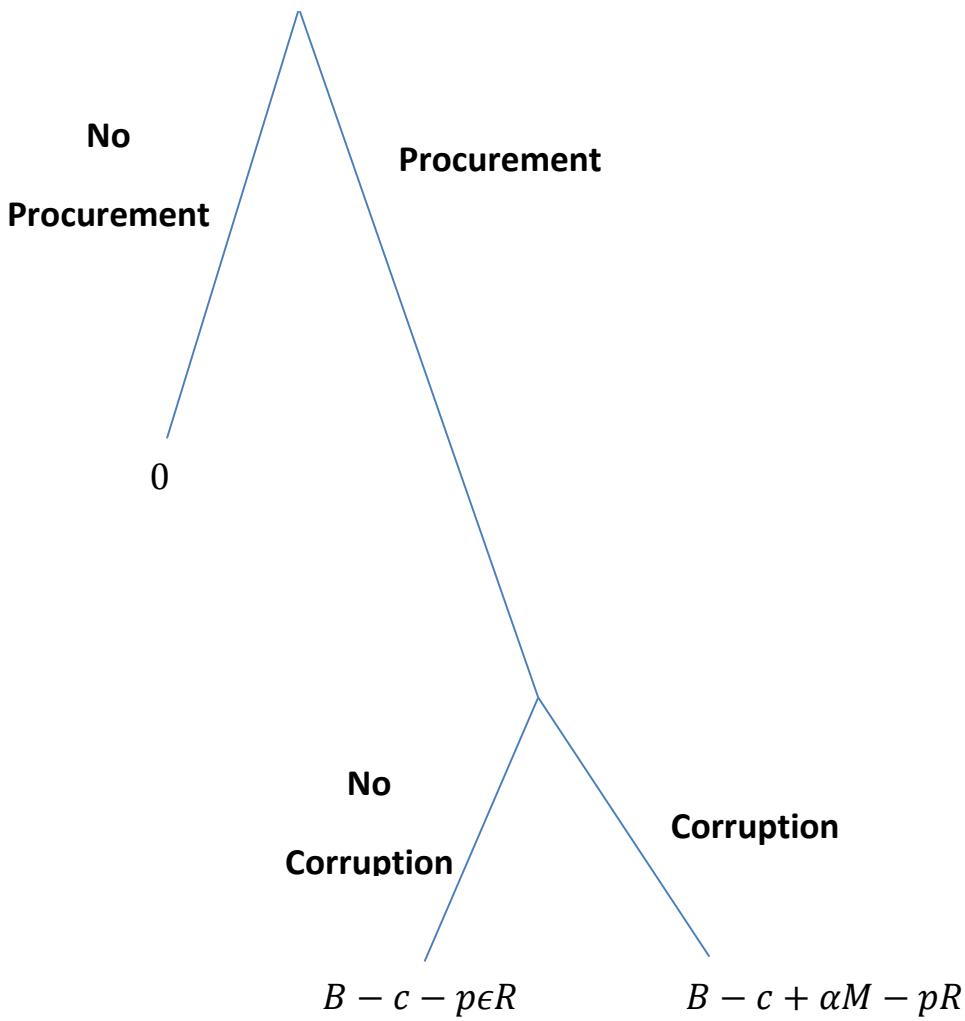
When evaluating the medication accessibility to citizens of Londrina in 2006, auditors found out that several prescribed drugs in local health units were out of stock. Evidence was provided by interviews with patients and local employees (nurses), who reported lack of, for example, Clorana, Dipirona, 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 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 overbilling, an evidence of corruption. This irregularity occurred in 2004, in Poloni, São Paulo, drawn by lottery 17.

Appendix D – A simple model

Consider a mayor deciding whether to procure a public good, and then, upon procuring it, whether or not to embezzle money, according to the extensive-form game depicted below:



$B > 0$ is the reputational payoff from providing the good, c the effort cost of procurement, $c \sim G(\,)$, p is the audit probability, $R > 0$ is the reputational cost of being exposed as corrupt, $\epsilon \in [0,1)$ is the probability of running into procurement problems by mistake, $M > 0$ is the amount transferred and $\alpha \in [a_{Low}, a_{High}]$ is the share of it which can be embezzled.

Case 1: Optimal spending ($\alpha_{High} < \frac{p(1-\epsilon)R}{M},\ \epsilon = 0$)

At the final sub-game – upon having procured the good –, the mayor is corrupt if and only if:

$$B - c + \alpha M - pR \geq B - c$$

The mayor is corrupt if $\alpha M \geq pR$, what does not happen for any transfer. The expected payoff of procurement is then $B - c$.

Hence, the mayor procures if $B \geq c$, and the probability of procurement is $G(B)$.

Case 2: Overspending ($\alpha_{Low} \geq \frac{p(1-\epsilon)R}{M},\ \epsilon = 0$)

At the final sub-game – upon having procured the good –, the mayor is corrupt if and only if:

$$B - c + \alpha M - pR \geq B - c$$

The mayor is corrupt if $\alpha M \geq pR$, what happens for all transfers. The expected payoff of procurement is then $B - c + \alpha M - pR$.

Hence, the mayor procures if $B + \alpha M - pR \geq c$, and the probability of procurement is $G(B + \alpha M - pR)$. Because $G(\,)$ is a c.d.f., it is (weakly) increasing, so that $G(B + \alpha M - pR) > G(B)$. The result is overspending.

Case 3: Underspending ($\alpha_{High} < \frac{p(1-\epsilon)R}{M},\ \epsilon > 0$)

At the final sub-game – upon having procured the good –, the mayor is corrupt if and only if:

$$B - c + \alpha M - pR \geq B - c - p\epsilon R$$

The mayor is corrupt if $\alpha M \geq p(1-\epsilon)R$, what does not happen for any transfer

Hence, the mayor procures if $B - p\epsilon R \geq c$, and the probability of procurement is $G(B - p\epsilon R)$. Because $G(\,)$ is a c.d.f., it is (weakly) increasing, so that $G(B - p\epsilon R) \leq G(B)$. The result is underspending.

Case 4: Heterogeneity ($\alpha_{Low} < \frac{p(1-\epsilon)R}{M} \leq \alpha_{High},\ \epsilon > 0$)

The probability of corruption is

$$\begin{cases} 1, if\ \alpha \geq \dfrac{p(1-\epsilon)R}{M} \\ 0, otherwise \end{cases}$$

The probability of procurement is

$$\begin{cases} G(B + \alpha M - pR), if\ \alpha \geq \dfrac{p(1-\epsilon)R}{M} \\ G(B - p\epsilon R), otherwise \end{cases}$$

Transfers with high embezzlement opportunities display overspending, while those with low embezzlement opportunities display underspending.

Increases in p have two effects: decrease the probability of corruption, for transfer with high-embezzlement opportunities, but also decrease the probability of procurement for all transfers.

Decreases in ϵ, conversely, decrease the probability of corruption, for transfer with high-embezzlement opportunities, and increase the probability of procurement for transfers with low-embezzlement opportunities.