The limited power of information: Monitoring and corruption deterrence–evidence from a random-audits program in Brazil

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ABSTRACT: While corruption is documented to have high social costs, the mechanisms that enable and deter corruption are not entirely understood. This paper takes advantage of the introduction of a random-audits program in Brazil, for which the only practical effect is information disclosure, to assess the effects of monitoring on corruption deterrence: because auditors were entitled to investigate transfers that occurred prior to the time of audit, we are able to compare incumbents' decisions when they did not know they could be audited with their subsequent decisions, when they knew they had a much higher probability of being exposed. We depart from a simple theoretical model to derive empirically testable predictions, which are contingent upon local constraints and embezzlement opportunities, of the effects of the program on the prevalence of corruption. The results are that corruption dramatically dropped after the program was introduced; nevertheless, the decrease in corruption has not led to higher compliance or better health outcomes. Politicians seem to have adapted, and irregularities have only moved to where it is less straightforward to identify corruption.

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KEYWORDS: corruption; information; audits; measurement error.

1. INTRODUCTION

While corruption is documented to have high social costs–from static resource misallocation to distorted dynamic incentives to accumulate production factors whose returns are seized –, the mechanisms that enable and deter corruption are not entirely understood. Corruption poses a major obstacle to the decentralization of public-service provision in developing countries, with costs so high that they eventually supersede the benefits, as in (Olken, 2006), or that they even reverse

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the progressivity of public expenditures, as in (Reinikka and Svensson, 2004). This paper assesses the effects of the introduction of a monitoring mechanism, which increased the probability of local politicians being audited, on corruption prevalence and the effectiveness of local constraints. Taking advantage of the randomness of the interval between the announcement of the monitoring technology and the actual audit, we assess incumbents' responses by exploring within-municipality variation in corruption prevalence for events that occurred before and after mayors became aware of the random-audits program.

A growing literature has relied on randomized audits to investigate corruption drivers (examples are Olken and Barron (2009), which assigned truck drivers to assess the demand for bribes by corrupt officials in Indonesia, and Ferraz and Finan (2010), which explores the Brazilian Government's official random audit reports to test whether lame-duck incumbents embezzle resources to a greater extent). However, the effects of increasing the probability of exposure of incumbent actions due to the monitoring mechanism itself have not been assessed thus far (Pande, 2011).[1]

The Brazilian experience is particularly interesting because the only practical effect of the introduction of the random-audits program was an increase in the probability of information disclosure; to date, no politician exposed by the program to be corrupt has been legally convicted. This fact does not preclude an expectation that the program may have an effect on corruption prevalence. The fact that voters punish mayors exposed to be engaged in corruption, as pointed out in (Ferraz and Finan, 2008), supports the hypothesis that politicians should respond to the introduction of a technology that allows for information disclosure, as long as corruption, - along with other dimension of a politician's overall quality, - is at least partly an endogenous choice of elected officials.[2]

We take advantage of the Brazilian Office of the Comptroller General's ("Controladoria Geral da União", CGU) random-audits program to assess the effects of monitoring on corruption deterrence in health[3]: because auditors were entitled to investigate transfers that occurred prior to the time of audit, we are able to compare differences between incumbents' decisions when they did not know they might be audited with subsequent decisions, when they knew they had a much higher probability of being exposed. We depart from a simple theoretical model to derive empirically testable predictions of the effects of the program on corruption, contingent upon local constraints, such as the presence of media or a well-functioning municipal health council, and embezzlement

[1]Although Zamboni and Litschig (2011) evaluate how procurement practices respond to a major one-time increase in the probability that municipalities are audited by the same program, in their setting the random-audits program was already in place prior to treatment adoption. In particular, as we show, the bulk of corruption incidence linked to misprocurement had already been deterred by 2003, when CGU random audits were introduced. Moreover, the external validity of their findings is very limited, given that municipalities knew the higher auditing probability would be a one-time event, and thus, they might have over-responded to the experiment.

[2]In contrast, if, as in citizen-candidate models, politicians were to have fixed preferences for outcomes, including corruption, then corruption incidence would only possibly change subsequent to the following elections, when voters might choose a higher-quality candidate based upon richer information about incumbent preferences for corruption; see the discussion in Pande (2007).

[3]We only have access to the data on health transfers from the Federal Government to municipalities.

opportunities, - from political alignment with the state's governor to transfer characteristics, such as capital intensity.

The paper most similar to ours is Reinikka and Svensson (2003), which assesses the impact of a newspaper campaign in Uganda that disclosed the official figures about the central Government's transfers to local primary schools, consistent with the findings summarized in Reinikka and Svensson (2004) concerning extensive misallocation of such resources. The main difference is that while the latter focuses on a specific transfer in which beneficiaries are able to accurately track the money that is actually cashed in, this paper analyzes politician 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.[4]

As such, we address a broader question: Can information disclosure to voters deter corruption in public services delivery? While Reinikka and Svensson (2003) documents substantial effects of the campaign on eliminating resource misallocation, with positive medium-term impacts on schooling outcomes (Reinikka and Svensson, 2005), 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: politicians adapted, and irregularities have only moved to where it is less straightforward to identify that corruption has occurred.

This paper also bears some similarities with DiTella and Schargrodsky (2003), which studies the effects from the introduction of auditing on corruption among public hospitals in Buenos Aires. Nonetheless, they cannot observe corruption directly; rather, they observe procurement prices for basic inputs, which are argued to differ among hospitals before the crackdown only due to corruption. Furthermore, they are primarily interested on the effect of public officials' wages on corruption when there is a high probability of an audit. Although this study is similar in spirit to our inquiry, particularly with respect to the interaction of the monitoring mechanism with local constraints, we have direct evidence of corruption incidence and hence are able to more precisely distinguish the effects of the introduction of the auditing technology from other confounding effects.

Additionally, unlike other papers that use data from non-experimentally designed randomized audits, we address mayoral decisions at the transfer level instead of using aggregate measures of corruption at the municipality level. The purpose of using more disaggregated data is twofold. First, we avoid the omitted-variable bias because corruption prevalence can differ across municipalities simply because the composition of transfers presents different embezzlement opportunities for different municipalities. As such, we are able to take into account transfer attributes to test predictions concerning heterogeneous behavior of the mayor in question with respect to opportunities for corruption. Second, because auditors are not perfect, measurement error due to misclassification prevents consistent estimation; see (Hausman, Abrevaya, and Scott-Morton, 1998; Lichand, Lopes, and Medeiros, 2011) for a discussion on the techniques used in this paper. We draw upon the model to deliver empirically testable predictions on the nature of such bias and to correct for measurement

[4]Alternatively, their framework is one of multiple principals and a single agent, whereas ours can be thought of as a single-principal-multiple-agents case, wherein the Federal Government maximizes social welfare.

error at the transfer level, as transfer attributes may affect the auditor's ability to correctly identify irregularities, using standard procedures from the econometrics literature. Finally, we investigate not only whether the program has decreased corruption but also whether incentives were sufficiently strong to foster compliance or whether, conversely, politicians adapted to the new incentive scheme only to avoid facing the negative reputation premium of being exposed as corrupt, while still not channeling resources to the provision of public goods.

The results indicate that up to 82% of the 60.7% decrease in corruption among audited health transfers in Brazil between 2001-02 and 2003-04 can be attributed to the random-audits program. Consistent with the theoretical model, the program's effect is weakly increasing in the strength of local constraints, particularly with respect to the presence of media. Accounting for heterogeneous embezzlement opportunities, it is indeed the case that the program's effect was stronger among capital-intensive transfers, which are more prone to misappropriation of resources compared with labor-intensive transfers. In turn, municipal health councils seem to play an important role: because auditors work together with the local council to assess documentation and performance, better-functioning councils (those that meet with a higher frequency) are linked to higher auditor effectiveness, thus decreasing the probability that corruption eventually escapes official scrutiny.

The substantial fall in corruption has not led, however, to higher compliance rates or better health outcomes. In fact, mismanagement is estimated to have increased in response to the introduction of the random-audits program by at least 50% of the decrease in corruption. Irregularities increased particularly for infrastructure maintenance and medication stock control, for which it is less straightforward to identify that corruption has occurred compared with irregularities in procurement. We conclude that while voters punish corruption episodes, they seem not to take the poor provision of public goods into account; as a result, politicians seem to adapt away from irregularities with higher exposure toward those less likely to be identified with corruption–consistent with the findings of Shleifer and Vishny (1993) –while resources still do not meet their specific ends and performance continues to lag behind.

The remainder of the paper is organized as follows. Section 2 details CGU's random-audits program. Section 3 introduces the economic model for mayoral decisions at the point of each transfer. Section 4 describes the data and empirical strategy. Section 5 presents the results, followed by robustness checks in Section 6. Section 7 extends the discussion to the ordered case, and Section 8 concludes the paper.

2. The Random-Audits Program

Brazil's 1988 Constitution put forward the issue of decentralization, passing on to states and municipalities the mandate of providing public goods such as education and health. Along with it, a system of constitutional transfers ("Fundo de Participação dos Estados" (FPE) and "Fundo de Participação dos Municípios" (FPM)) established the mandatory reallocation of resources from the federal government to sub-national units earmarked to support these increased responsibilities.

Not until CGU's random-audits program, however, did the federal government have the ability to systematically track sub-national governments' use of these resources.

Created in February 2001, the CGU is in charge of the oversight of 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, initiated in April 2003, is a federal government initiative to inhibit corruption among all levels of public management. The program consists of public random draws to select in each period the municipalities to be audited by CGU officials. 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. The 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[5].

The program implementation was announced in early 2003. Four municipalities reached by Fome Zero, the Federal program to fight hunger, were audited as a test case in February, followed by the announcement, in March, of the selected municipalities to be audited according to the program's first draw. Evidence supports the claim that mayors did not anticipate the program because there was no decree or media announcement of the program prior to 2003. However, one might entertain the hypothesis that the creation of CGU in early 2001 and the several institutional reforms that followed it, with the goal of enhancing controls over the proper use of resources at the federal level and beyond, might have already driven mayors away from corruption before the program's implementation. While this would entail a lower baseline corruption prevalence (which would allow us to estimate only a lower bound for the effects of interest), no such anticipation seems to have occurred. A naive analysis–a simple comparison of average incidence –yields that corruption was actually higher in 2002 than 2001, using information from CGU's audit reports[6].

Irregularities documented by auditors are followed up by the public entities responsible for implementing sanctions, including the Prosecutor's Office, the Brazilian Court of Audit ("Tribunal de Contas da União,", TCU), the Federal Police and the municipal legislative houses. Mayors exposed as corrupt are mandated to return the resources embezzled, but apart from that, to date, no politician has been legally convicted following the program's information disclosure.

The program currently audits municipalities of up to a limit of 500,000 inhabitants.[7] The selection process was designed such that the samples would be geographically representative, and selection probabilities currently approximate 1% for each of the 5,526 municipalities (which represents over

[5]For a full description, see http://www.cgu.gov.br/AreaAuditoriaFiscalizacao/ExecucaoProgramasGoverno/Sorteios/index.asp
[6]See Section 4 for a complete description of the database.
[7]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.

99% of Brazilian municipalities or about 70% of the country's population) that currently lie within the maximum population eligibility thresholds.[8]

Given that the random-audit program is administered by a de facto national anti-corruption agency (i.e., the CGU), some may view the program as a politicized tool and thus ineffective for preventing corruption. However, a mechanism for randomly selecting states and municipalities to be audited negates this possibility. The initiative of making random selections through a national lottery 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.

The random-audits program investigates transfers earmarked to execute national health and education policies (mandatory transfers), direct transfers to citizens, and politically negotiated (voluntary) transfers. Once a municipality has been randomly drawn (the lottery is organized by "Caixa Econômica Federal," the same federal institution that runs the national lottery), a service order is issued by the CGU, indicating the transfers that should be audited in each municipality. Then, a team of auditors visits the municipality for sufficient time to investigate irregularities according to the service order.

Fortunately for the researchers, for both education and health, all federal transfers are subject of auditing in every municipality drawn, such that there is also no strategic selection with respect to the issuing of service orders. Auditors are then entitled to inspect the entire trajectory of money, 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.[9] As such, even though the program began in 2003, we have information for every year of the mayor's term from 2001 to 2004.[10] Once the auditing is complete, CGU officials describe all irregularities detected for each transfer (if any) in official reports.[11]

It is fair to state that there were monitoring mechanisms prior to the introduction of the random-audits program. In particular, CGU, since 2001, and TCU, even before that, were entitled to scrutinize (and approve, in the case of the latter) municipal accounts and to crack down on frauds in the event of denouncements. However, the CGU program represented both a substantial increase in the probability of being audited, given that municipalities have, on average, a 1% probability of being drawn every two months–the probability of being eventually audited in the course of a four-year term being approximately 21.4% –and of being exposed because summary audit reports began to be systematically disclosed on the internet, with high repercussions following from exposure in newspapers, television and radio.

[8]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.

[9]As such, there is no strategic selection of which transfers should be 'backward-looking' investigated.

[10]We also have data for the 2005-2008 term, which we draw upon to perform some of the robustness checks.

[11]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.

Finally, 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. MODEL

We present the interaction between a mayor and an auditor in a typical transfer, modeling CGU's random-audits program as a marginal increase in the probability of being audited. We explicitly incorporate the possibility that the auditor is not always effective, with a positive probability of misreporting corruption as compliance, although compliance is never misclassified as corruption, following Lichand, Lopes, and Medeiros (2011)[12]. Model's comparative statics allow us to derive empirically testable hypotheses concerning the effects of the random-audits program on corruption prevalence, the interaction of the setup of the monitoring mechanism with local constraints for corruption deterrence and embezzlement opportunities, and the nature of the biases of ignoring measurement error due to the auditor's lack of effectiveness.

Figure 1 schematically presents a sequential interaction between a mayor (M) and an auditor (A) focusing on one particular transfer. First, the mayor decides whether to engage in corruption (C) or not (NC).

We define $q \in (0, 1)$ as the probability of being selected for audit and $p \in [0, 1)$ as the average probability of corruption being misclassified as compliance. Once the mayor has chosen among alternatives, nature (N) decides whether the municipality is selected for audit, occurring with a fixed probability (q), homogeneous through municipalities. Nonetheless, it is insufficient that a municipality is audited in order for irregularities to be exposed. Being exposed depends upon the auditor's effectiveness, which might depend to a greater or lesser extent on local attributes, such as available monitoring technologies.[13]

Four possible outcomes are presented for the mayor at each point in which the auditor is called to play, defined by the interaction between mayor's previous choice and the nature's draw: (i) not being drawn; (ii) being drawn, having no irregularities; (iii) being drawn, having irregularities, but not being exposed; and (iv) being drawn, having irregularities and being exposed. In every case in which the mayor is not selected, gains or losses of reputation are not determined; if the mayor opts out of corruption, there is no change to the prior status (we define 0 as the baseline payoff). If he opts for corruption, he appropriates the amount embezzled (mT, where T is transfer amount and $m \in [0, 1]$ indexes the share of it that can be privately appropriated–an index of "embezzlement

[12]The triangular error structure is not a necessary condition for identification, if the measurement error is not very high in any direction (see Hausman, Abrevaya, and Scott-Morton (1998)).

[13]The fact that the overall exposure probability is not entirely a choice variable for CGU distinguishes this setting from the costly state verification framework, because the principal will not be able to design a contract such that no deviations are observed in equilibrium (Townsend, 1979).

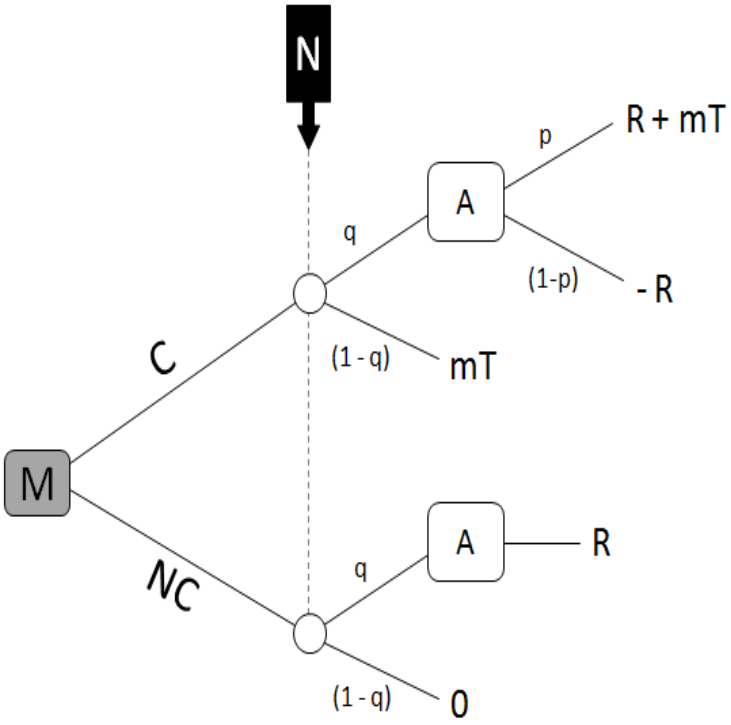

FIGURE 1. Sequential interaction between the mayor and an auditor.

opportunities").[14] If the mayor is selected but the auditor fails to find any irregularities, a gain of positive reputation ($R > 0$) is added to the payoffs discussed above. That case resembles the one in which the auditor finds the incumbent to have chosen compliance. If the auditor is successful in his/her search, then only those mayors who have chosen good management reap reputation gains, while the choice of corruption implies a loss, which we assume to be the same in absolute value, for the sake of simplicity and without great loss of generality ($-R$). It is assumed that mayors detected to have engaged in corrupt behavior are required to return the amount embezzled.

It is clear from the model that, aside from the imposition of returning the amount embezzled once exposed as corrupt, the only enforcement mechanism of the contract between the federal government and municipalities is the negative reputation premium associated with information disclosure of the episode, if the mayor is found to have engaged in corruption.

[14]We claim that the nature of the health program to which each transfer accrues determines to a great extent the opportunities for embezzlement, such that capital-intensive programs are much more prone to corruption than labor-intensive ones, following (Tanzi and Davoodi, 1998).

For our analysis, the necessary hypotheses are that mayors maximize expected utility and that mayors' utility is additive in each choice, which guarantees that we can observe each transfer separately without considering strategic interdependence. [15] Hence, supposing for simplicity that mayors are risk-neutral, the mayor's problem can be summarized by his/her choice for each transfer as:

$$\begin{aligned} &\max_{Y} u^{E}(Y) \\ &s.t.\ Y \in \{C, NC\} \end{aligned} \tag{1}$$

where $u^{E}(Y)$ denotes the mayor's expected utility of choosing Y, such that

$$u^{E}(Y) = \begin{cases} (1 - q(1-p))\, mT - q(1-2p)R\ , \text{ if } Y = C \\ qR\ , \text{ if } Y = NC \end{cases} \tag{2}$$

We model CGU's random-audits program as a marginal increase in the probability of being audited, q. The following propositions condense the testable implications of the program on corruption.

PROPOSITION 1. *The probability of corruption (weakly) decreases with CGU's program.*

Proof. See Appendix A.

This result is not surprising: A higher probability of being exposed disincentives misconduct. What is perhaps less intuitive is that it is not contingent upon the auditor's effectiveness. One may wonder whether a sufficiently high rate of misclassification might induce mayors to bet on a reputation premium. However, if true, while observing only the payoff of being corrupt, one should notice that q also increases the compliance payoff, which is appropriated with certainty whenever the municipality is audited, which offsets incentives to behave strategically upon high misclassification rates.

PROPOSITION 2. *The program' corruption-deterrence effect is (weakly) enhanced by local constraints.*

Proof. See Appendix A.

The intuition for this result is straightforward: with a higher probability of being audited, a higher cost of disclosing corruption (higher benefit of disclosing compliance) or an increase in the auditor's effectiveness implies a higher disincentive for corruption because the overall probability of conduct disclosure increases, leading to higher expected losses from corruption in both cases.

[15] Strategic interdependence might actually be meaningful because reputation losses from non-compliance (auditor's effectiveness) might be concave (convex) in the number of deviation events. Nevertheless, this would complicate the analysis to a greater extent, making it unfeasible to handle the measurement error in audit reports.

PROPOSITION 3. *The effect of CGU's program is higher among transfers with higher opportunities for embezzlement.*

Proof. See Appendix A.

Intuition for this result is rather simple. The probability of being audited acts like a tax on expected gains from embezzlement; because these gains are higher the larger the transfer amount that can be privately appropriated, taxation increases with this share. Thus, controlling for the amount transferred, the introduction of the random-audits program should have implied a larger disincentive for corruption among health programs that displayed higher embezzlement opportunities. Aside from its simplicity, the implications of this result are powerful: the program should benefit the most those municipalities with weaker checks and balances, where opportunities for embezzlement are higher.

Proposition 3 also clarifies the omitted-variable bias: if the "portfolio" of opportunities for embezzlement varies by municipality, then the aggregate-level estimates for the effects of municipal attributes on corruption deterrence will not coincide with transfer-level estimates. In particular, if this distribution is different for transfers audited after the monitoring mechanism was introduced, and if it differs in a fashion correlated with the distribution of local constraints, then the effects of interest are biased when estimated at the municipal level.[16] Taken together with the predictions for the effects of measurement error, which can be contingent upon transfer's characteristics, this result supports the use of transfer-level data instead of aggregate figures for corruption incidence.

The following proposition highlights the effects of measurement error.

PROPOSITION 4. *If misclassification displays a non-zero correlation with the other variables of interest, measurement error introduces an undetermined-nature bias on the effect of CGU's program on observed corruption prevalence, as well as on that of its interaction with local constraints or embezzlement opportunities.*

Proof. See Appendix A.

There are strong reasons to expect such correlations to be different from zero. First, it is clear from the model that variables that condition the auditor's ability to correctly identify corruption events will also constrain politicians' choices. Second, if auditors are better able to identify corruption in events that have occurred in the past, for which documentation is consolidated and for which mayors have less room to contest former findings, then measurement error will be higher for transfers after 2003 (for which at least some investigations will consider irregularities that have occurred in the same year as the audit), in which case it will display a positive correlation with the program's implementation. Furthermore, in that case, not only will the effects be biased but the effects of its

[16]Provided municipal-level 'portfolio' changes are not taken into account.

interactions with local constraints and embezzlement opportunities will also be biased. Accounting for measurement error is thus an essential robustness check of the empirical findings.

In the following sections, these predictions are applied to the data.

4. Data and Empirical Strategy

Estimation draws upon data from random-audits program reports for health transfers. In principle, we restrict attention to transfers for the 2001-2004 period, once we want to hold the decision-maker fixed in each municipality. Although auditors investigate transfers linked to several Ministries, including Education, Transports and Environment, we only have access to data from health audit reports, codified by Fundação Getulio Vargas' Center of Politics and Economics of the Public Sector (CEPESP-FGV/SP)[17].

In our database, transfers are dated according to the auditor's record of the year the action investigated accrues to. For example, consider the audit report for Quissama - RJ, drawn by lottery 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 invested in financial markets, according to federal norms, were not properly managed by the municipality). Accordingly, auditors reported an irregularity, coded in our database as evidence of mismanagement.[18]

When there are multiple years to which a single investigation accrues to–either because a problem is documented to extend for several years or because no irregularity has been found in several years –then we separate this transfer into several observations (one for each year that the investigation concerns), which we treat as independent. Our database is linked to CGU's service orders, such that we are able to precisely determine the monetary value of each transfer.[19] [20] Nonetheless because the value privately appropriated is seldom clear from audit reports, we cannot quantify the amount embezzled on the event of corruption, such that our working variable is discrete-whether corruption

[17]Codified data come from CGU's *complete* reports, which comprehend every transfer originally prescribed by CGU's Service Orders, in contrast to *summary* reports, which include only the major irregularities exposed and hence have been applied to assess the impact of information disclosure on voters' behavior, as in Ferraz and Finan (2008).

[18]See Appendix C for more examples of pre- and post-program findings described by audit reports.

[19]This feature also guarantees that compliance is not underestimated in our data because some auditors just exclude transfers that did not display any irregularity from the complete report; this information can only be recovered by linking the reports to the service orders.

[20]CGU's audit reports are only linked to service orders from the eighth draw onward, beginning in November, 2003. For this reason, we discard all information derived from reports of audits 1 to 7. One might speculate that this would prevent us from documenting mayors' adjustments to the announcement of the program because those drawn in the very first rounds would have had less room to respond, with part of the budget already executed. However, since the program was announced in January, 2003, municipalities drawn first did not necessarily have less room for adjustment; as a matter of fact, casual observation of the data collected from these reports (with the word of caution that they underestimate compliance, as noted in footnote 18) points out that it was quite the opposite: mayors drawn up to April incurred almost no corruption in 2003–possibly with major procurements placed on hold until it was clear to what extent the program would enforce compliance –, whereas those drawn in the following months displayed corruption levels closer the "new equilibrium" that we document to prevail after the program's introduction.

has taken place or not[21] [22]. Last, we only keep municipalities that had audited transfers for both periods of interest, except for some robustness checks, for which we maintain in the sample the whole content of CGU' audit reports, including observations of the 2005-2008 term.

In doing so, our baseline sample has 3,814 observations, distributed across over 50 health programs and 217 municipalities. Although we have a significant number of observations (968 transfers) for the 2001-2002 period, as somewhat expected, the larger proportion of the sample (2,846 transfers) is concentrated in the 2003-2004 period. CEPESP's database contains 32 categories of irregularities, ranging from "inadequate documentation" to "off-the-record invoice," six of which are coded as evidence of corruption and 26 of which are coded as evidence of mismanagement. The complete classification list is included in Appendix B. We consider both mismanagement and compliance to be *non-corruption* in our baseline specifications, but later on, we examine in greater detail the mayor's decision beyond the binomial case.

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. Thus, for our period of interest, we cannot control for most municipal variables that change over time, only initial conditions. The only controls we observe at the municipality level on an yearly basis are per capita GDP, per capital health expenditure, per capita public servants, immunization rate and child mortality (which we used with a one-year lag, as a proxy for the demand for health services), and the proportion of public servants in the population; political alignment between the mayor and state Governor is observed every two years.[23]

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 the IBGE. Health information is retrieved from 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).

The incidence of corruption for the whole period and that for before and after the introduction of the random-audits program is shown in Panel A of Table 1. It can be observed that while the overall prevalence of corruption for the 2001-2004 period is high, there was an impressive reduction in corruption prevalence, from 17.4% of the transfers during the 2001-2002 period to 6.8% for the later period, a dramatic drop of over 60%. If we break down these figures according to IBGE's sociodemographic regions (Panel B), we can observe that the fall in corruption prevalence was even more pronounced for the Southeast, with an impressive rate above 70%.

[21] However, our municipal-level database weights transfers by their value, and we explicitly control for the transfer amount in our estimates at the transfer level.

[22] In Section 6, we go beyond the binomial definition.

[23] We restrict attention to political parties, without looking at coalitions.

We can dig further into descriptive statistics to assess how corruption incidence varies with municipality or mayor attributes and to what extent the decrease in corruption observed after the introduction of the random-audits program was contingent upon these characteristics. Panels C and D of Table 1 below explore some of these patterns. Of course, the analysis is an unconditional one; it also ignores the issue of misclassification.

Of interest is that for the 2003-2004 period, municipalities with more than 32.4% of the population living with less than USD 1.25 a day in 2000 (the median value for extreme poverty in 2000 for the municipalities in our sample) have a significantly lower proportion of irregularities, with a decrease in corruption incidence 6.5 percentage points higher than that documented for municipalities below the extreme poverty median, which is consistent with higher opportunities for embezzlement in poorer settings.[24] The same is true for electoral competition (i.e., the larger the mayor's winning margin in the previous election, the higher the fall in corruption) and political alignment (i.e., the mayors affiliated with the same political party as the State governor face a high a decrease in corruption and end up with a lower average incidence than politically non-aligned municipalities).

For municipalities with a radio station or a well-functioning municipal health council[25], it is interesting that they actually are documented to have higher corruption incidence before the introduction of the monitoring technology, but the drop is so pronounced after 2003 that they end up with about the same proportion of transfers identified as displaying evidence of corruption than municipalities that lack such local constraints. These patterns are consistent with the predictions from our model, such that the random-audits program might incur decreased corruption, particularly in the presence of local constraints.

If these effects are significant under a conditional analysis, however, we can reject the interpretation that the fall in corruption incidence was merely due to an exogenous trend for Brazilian municipalities because if that were to be the case, it would be difficult to make sense of the heterogeneous fall, conditional on local constraints. On the other hand, the mayor's attributes do not seem to matter much for corruption trends. If the data show that male mayors and those with university degrees embezzle to a greater extent, the drop in corruption between the periods of interest is about the same, irrespective of gender or schooling. It is also worth pointing out that lame-duck incumbents do not seem to embezzle to a different extent than mayors serving first terms and, most important, that the program might not have had a very different effect conditional upon this status. If this effect is maintained throughout conditional analysis, it is a powerful argument against the alternative explanation that it is merely upcoming elections that drive the decrease in corruption prevalence; this argument is seriously considered through a series of robustness checks in Section 6.

Taking advantage of the randomness of the interval between the announcement of the monitoring technology and the actual audit, we assess incumbent responses by exploring within-municipality

[24]This difference is statistically significant, although we do not present standard-errors.

[25]According to our definition, these are councils that meet not less often than once a month.

variation in corruption prevalence for events that took place before and after mayors became aware of the random-audits program. The empirical strategy to assess the effect of the random-audits program on corruption is the following. We depart from a municipal-level linear specification with the proportion of transfers (weighted by their individual amount) for which auditors document evidence of corruption as the dependent variable. Our treatment variable (CGU) assumes 0 for all transfers accruing in the 2001-2002 period, and 1 for those of the 2003-2004 period. Other independent variables are progressively introduced to control for municipal features that might have changed over the same period or for initial conditions (either municipal attributes or mayor characteristics), which might be correlated with corruption prevalence. We then introduce municipal-fixed effects to control for unobservable features that are fixed in time. As such, our baseline specification is the following:

$$\sum_i Y_{m,t,i} = \alpha_0 + \sum_{k=1}^{K} \alpha_k X_{k,m,t} + \delta CGU_{m,t} + \theta_m + \epsilon_{m,t}, \tag{3}$$

where $Y_{m,t,i}$ equals 1 if transfer i in municipality m in period t is reported as evidence of corruption, and 0 otherwise, $CGU_{m,t}$ equals 1 for 2003 and 2004 and 0 otherwise, and δ is the coefficient of interest, i.e., the causal effect of the program on corruption under standard assumptions concerning the correlation of the error-term, $\epsilon_{m,t}$, with the covariates. $\boldsymbol{X}_{m,t} = (X_{1,m,t}, \ldots, X_{K,m,t})'$ is a vector of control variables.

TABLE 1. Descriptive statistics: Corruption incidence

Panel A: Aggregate

	2001-2002	2003-2004	% Variation
% of transfers	17.4%	6.8%	-60.7%
% of amount	17.0%	6.7%	-60.4%

Panel B: Socio-Demographic Regions

	North	Northeast	Southeast	South	Center-West
2001-2002	14.3%	17.6%	20.2%	14.5%	15.8%
2003-2004	8.8%	6.4%	6.0%	9.8%	6.6%
% Variation	-38.2%	-63.6%	-70.2%	-32.7%	-58.5%

Panel C: Municipalities' attributes

	Extreme poverty		Electoral competition		Radio		Health Council Meets Monthly	
	No	Yes	No	Yes	No	Yes	No	Yes
2001-2002	17.5%	17.3%	19.4%	16.2%	15.6%	21.4%	15.8%	17.6%
2003-2004	7.4%	6.2%	7.0%	6.7%	6.8%	6.9%	7.0%	6.9%
% Variation	-57.6%	-64.1%	-63.7%	-58.4%	-56.3%	-67.9%	-55.5%	-60.7%

Panel D: Mayors' attributes

	University degree		Male		Same party as Governor		Lame duck	
	No	Yes	No	Yes	No	Yes	No	Yes
2001-2002	16.7%	17.5%	12.8%	17.5%	17.0%	17.5%	17.2%	17.1%
2003-2004	6.7%							

We eventually introduce year-fixed effects to allow for a heterogeneous effect of the program over the years following its introduction. Moreover, with the goal of assessing the model's predictions concerning heterogeneity of the program's effect with respect to local constraints and embezzlement opportunities, we introduce interactions of our treatment variable with municipal initial conditions for these variables. Local constraints are defined by three sets of regressors. The first set affects the average auditor's effectiveness, $\boldsymbol{x}_i^{(p)}$: age of the municipal health council and an indicator of whether it meets at least on a monthly basis because well-functioning councils should have better organized evidence on health programs' implementation. The second set purports reputation effects, $\boldsymbol{x}_i^{(R)}$: the incumbent's lame-duck status and presence of a radio station because mayors serving second terms should be less concerned with reputational effects, and since media availability amplifies the program's disclosure effect. A third set of covariates is linked to embezzlement opportunities, $\boldsymbol{x}_i^{(m)}$: electoral competition and political participation as well as measures of voters' socioeconomic status, which should affect the extent to which voters use new information to update their political preferences.

Nevertheless, as we have anticipated throughout previous sections, a number of reasons might imply that the estimated effects do not have a causal interpretation. First, even after controlling for municipal attributes, embezzlement opportunities linked to transfer characteristics might be different between the periods. In fact, while transfers from capital-intensive programs are 65.58% of total transfers for the 2001-2002 period, they are only 38.40% of those for the 2003-2004 period. As such, we proceed to the transfer level. Our dependent variable is redefined as a binary indicator of corruption. We estimate a probit model, maintaining municipal-level fixed effects.[26] At the transfer level we are able to account for other factors that affect $\boldsymbol{x}_i^{(p)}$, such as transfer's amount, since larger amounts might make it more difficult to detect specific deviations.

$$Pr\left(Y_{m,t,i}=1\right)=\Phi\left(\alpha_0+\sum_{k=1}^{K}\alpha_k X_{k,m,t}+\sum_{j=1}^{J}\beta_j T_{j,m,t,i}+\delta CGU_{m,t}+\theta_m\right), \tag{4}$$

where $T_{j,m,t,i}$ is a matrix of transfer-level characteristics, and $\Phi()$ is the cumulative normal distribution function. We cluster standard errors at the municipality level to allow for arbitrary correlation of residuals at this level, particularly because only few covariates display variation at the transfer level. Second, we have previously raised the issue of measurement error. At the level of transfers, we are able to correct for measurement error, allowing it to depend on the transfer's characteristics. Third, because we are not addressing an experimental setting, we do not have control observations against which the time variations in corruption could be contrasted so as to dismiss alternative explanations. In particular, the concern that the second half of the term is structurally different from the first half-especially because of upcoming elections-provides an alternative mechanism consistent with a decline in corruption incidence irrespective of the random-audits program.

[26]Following Fernández-Val (2009), probit marginal effects are consistent under low heterogeneity of the average treatment effect.

To address these concerns, we perform a series of robustness checks to enhance comparability of the decision setting and to better outline the mechanism through which the program might have affected mayors' decisions.

5. RESULTS

The results of our estimates at the municipal level are presented in Tables 4 and 5. Column (1) of Table 4 presents the baseline estimate without controlling for any municipal feature, attributing a drop of 8.28% in corruption to CGU's program. Column (2) introduces municipal attributes that vary throughout the course of the mayor's term (dynamic controls), while the program's effect remains very negative and significant, at -7.21%. Next, column (3) introduces initial conditions (static controls), without much difference to the coefficient of the program, estimated at -7.40%. Column (4) adds mayor-level characteristics, which also do not seem to matter much to the program's estimated effect, which remains at nearly the same magnitude.[27]

Our identification strategy allows us to introduce municipal-level fixed-effects in column (5), retaining the dynamic controls. In fact, it seems that there is a compositional change in the sample of audited transfers with respect to non-observable municipal features before and after 2003 because the program's effect is somewhat reduced but is still very significant at the 5% level, with a point estimate of 5.44%. Lastly, column (6) introduces year-fixed effects to assess the heterogeneity of the program's effect over the course of the years following its implementation. While the estimated effect is negative for 2003, it is only significant for 2004, with a point estimate of -6.24%, significant at the 10% level.[28] It must also be noted that none of the controls are consistently significant through specifications, while the program's effect is only very sensitive to the introduction of municipal-level fixed effects. In turn, Table 5 evaluates the model's propositions with respect to local constraints and embezzlement opportunities by displaying the results of a regression of interactions of the treatment with municipal initial conditions. In the results, none of the interactions are significant. While these results do not reject the model-because the predictions were of a weakly increasing effect-it might simply be the case that embezzlement opportunities linked to transfers' attributes prevent consistent estimation of the effects of interest, with the same holding true for the effects of the program presented in Table 4.

There are several other reasons why these effects might not be causal. In addition to transfer-level heterogeneity, measurement error due to auditors' mis-sorting of irregularities might be at stake; alternatively, the drop in corruption may have resulted from a mere exogenous falling trend, especially because we do not have a control group against which we can track the evolution of mayors' decisions over time, or from upcoming elections, since we are comparing very different

[27]We miss some observations as we include additional controls because of missing data for some of the covariates.
[28]Estimation precision declines with fewer observations for each treatment year.

decision settings. Thus, the mayors' behavior during the first two years of the term might be fundamentally different from that during the last two years, especially if voters are perceived to highly discount the past. The next section carefully investigates each of these concerns.

6. ROBUSTNESS CHECKS

6.1. **Transfer-level heterogeneity.** At the transfer level, we are able to introduce transfer-level characteristics, whereby compositional change over time might confound the effects of the program estimated at the municipal level. Mainly, we add the transfer amount–as stated in CGU's service orders –and each transfer's capital intensity, according to the attributes of the program that they are a part of.[29] Table 6 displays the results for probit marginal effects.[30] We lose all observations for the 87 municipalities that had 0% corruption prevalence for every year between 2001 and 2004 due to municipal-level fixed effects, which explains why all tables reporting probit marginal effects display about 900 less observations, but results are not sensitive to estimating without municipal dummies.

Once holding the sample constant, controlling for transfer-level attributes does not substantially affect program's estimated effect: column (1) attributes a 10.7% decrease in corruption to the introduction of CGU's program, significant at the 1% level, which is close to the 11% to 12% range of the municipal-level regressions that exclude those 87 cities. Most interestingly, though, interactions with initial conditions are now significant: column (2) shows that the program's effect is estimated as stronger for those municipalities closer to the state capital (which have a higher probability of being audited by State auditors independently of the Federal program), with higher voters' turnout and older municipal health councils, consistently with model's predictions for the power of local constraints. In contrast, column (1) does not back up model's prediction for a stronger effect of the program among transfers with higher embezzlement opportunities because the coefficient of the interaction of the treatment with transfers' capital intensity is not significant.

Nonetheless, columns (3) to (6) display the results for capital- and labor-intensive transfers separately, with remarkably different results: the fall in corruption is documented to have been concentrated among capital-intensive transfers, with a much smaller and not statistically significant effect among labor-intensive ones. With respect to local constraints, an interesting pattern arises: First, labor-intensive transfers dominate the overall effect and display a significant effect of the interactions of the program with turnout and distance to the state capital. Second, local constraints seem to work in opposite directions for the two types of transfers, such that the estimated effect for the pooled sample is zero. This pattern is apparent for the proportion of the population below the poverty line and average educational attainment. We should, however, turn to the other identification concerns to observe whether such patterns survive other robustness checks.

[29]Thus, for instance, a transfer to build a hospital or to buy medication is classified as capital-intensive, while other transfers targeted at hiring personnel for the Family Health program are classified as labor-intensive. See Appendix B for the full classification list and a description of the main health programs that are funded with federal transfers.

[30]Although we do not present them for the sake of conciseness, all estimates are also ran using a logit specification, which yields nearly identical results and is available upon request. As such, these results are compatible with the ones presented in the next subsection, which draws upon a logit specification to correct for the potential presence of measurement error.

6.2. **Measurement error: Binomial case.** Next, we move on to assess whether the presence of measurement error due to the auditors' potential lack of effectiveness prevents us from interpreting the previous results as causal ones. The literature on measurement error on the categorical dependent variable has greatly advanced in terms of identification, although applications have mostly been confined to labor-market transitions because employment status is argued to be often miscoded by survey respondents (e.g., Hausman, Abrevaya, and Scott-Morton, 1998). Although some previous results for bounds have been available now for a long time (see Bound, Brown, and Mathiowetz, 2001, for a survey of this literature), the literature has only recently stated identification results for binary dependent variables.

Hausman, Abrevaya, and Scott-Morton (1998) delivers identification results and estimation methods for known misclassification structures and semi-parametric results for arbitrary misclassification, as long as it does not depend on covariates. Lewbel (2000) states identification results for misclassification arbitrarily correlated with covariates, while Lichand, Lopes, and Medeiros (2011) introduces identification results for binary and multinomial models under weaker hypotheses, exploring the triangular error structure of audit models.

On the basis of these results, we estimate a logit model , with the mayor's decision between compliance and corruption as the dependent variable and with the covariates presented in last section, parametrically modified to account for the fact that corruption might be mis-sorted as compliance, as the following:

$$\begin{cases} Pr(\hat{Y}=0) = Pr(Y^*=0) + p\left[x_i^{(p)}\right] Pr(Y^*=1), \\ Pr(\hat{Y}=1) = \left\{1 - p\left[x_i^{(p)}\right]\right\} Pr(Y^*=1), \end{cases} \tag{5}$$

where we index $\{NC, C\}$ by $\{0, 1\}$.

A central discussion concerns which variables should be included in the auditor's effectiveness equation and whether these variables should also be included in the mayor's choice equation. With respect to the latter question, the model states that the mayor's decision accounts for every variable that potentially affects the auditor's effectiveness *at the time of his/her decision*. That is, while, on the one hand, most variables included in the latter should also be part of the former, variables that affect $p(x_i^{(p)})$ after the incumbent's choice has been made cannot condition it, by definition. Thus, we are able to specifically estimate whether the auditor's precision is a decreasing function of the lag between the date of investigation and that of when the irregularity has taken place.

Last, we do not include municipal-level fixed effects due to the non-linear specification, but rather maintain the whole set of municipal- and transfer-level controls throughout. Table 7 displays the results. Even with sizeable average estimated misclassification rates, of the order of no less than 20%, the effect of the program is remarkably similar to our baseline results of -7.7% for the full sample and is significant at the 1% level. When we breakdown the analysis by capital intensity, the drop in corruption attributed to the program is high and statistically significant for both capital- and labor-intensive transfers. Interestingly, point estimates are lower for the former and are higher for

the latter in comparison with the estimation that ignores measurement error; CGU random audits are documented to have decreased corruption by 12% and 6.6%, respectively, in line with model's prediction that information disclosure should have decreased corruption by the most among transfers with higher opportunities to embezzle.

With respect to the interactions with local constraints, the results also support the model's predictions, with more pronounced drops in corruption for municipalities more distant from the state capital and with lower electoral turnout and worse-functioning municipal health councils. For two of these variables, coefficient signs are reversed with respect to the estimates that ignore measurement error. When we breakdown the analysis by capital intensity, the effect of local constraints seems to operate mainly through labor-intensive transfers. Moreover, despite still displaying a negative and sizeable coefficient, the interaction of CGU's program with the presence of an internet provider (or, for the matter, of any other indicator of media presence) is no longer significant. We can take advantage of our estimates to also assess how local constraints or transfer attributes affect auditors' effectiveness in correctly identifying corruption. Three main variables stand out as systematically relevant for misclassification: the transfer amount, which increases measurement error among capital-intensive transfers, such that it may indeed be more difficult to identify specific deviations in larger transfers; the quality of municipal health councils, such that councils that meet monthly or more frequently are linked to a significantly lower probability of misclassification; and the lag between the dates of investigation and irregularity, with older transfers displaying almost no measurement error, which is concentrated among irregularities that are investigated without lag (see Figures 4 and 5), for which documentation is still not consolidated, leaving more room for mayors to contest auditors' findings.

6.3. **Exogenous falling trend.** In our sample, there are no control observations to account for counterfactual trends. Because treatment varies only between periods, our findings might simply reflect an overall declining trend in corruption among municipalities in Brazil, instead of the effect of the program. However, not only it is difficult to make sense of such a pronounced natural falling trend (on the order of 60%), but also if that were the case, there would be no reason why one should expect a differential effect of the program according to local constraints. Because an exogenous trend might not account for that stylized fact predicted by the model, we can dismiss that alternative explanation.

6.4. **Elections.** Upcoming elections might have precisely the same effects as those predicted for the introduction of the random-audits program, both with respect to discouraging corruption engagement and to igniting local constraints. It is indeed the case that average corruption prevalence is documented to be lower in 2004 than in 2003. While this occurrence might, in principle, be attributed to the treatment's duration, it is also compatible with the electoral explanation.

Figure 2 above seems to dismiss this concern because average corruption incidence remains around the 5% level over the first three years of the next term, in what seems to be the new equilibrium level under the random-audits program. Nevertheless because the pool of audited transfers is

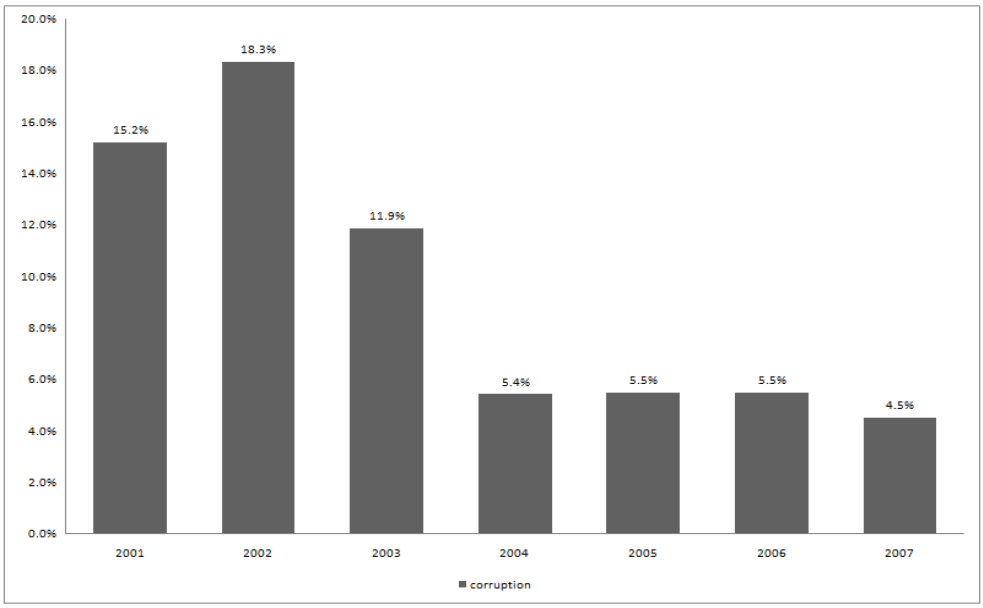

FIGURE 2. Corruption prevalence

different for each year, we must carry on with a conditional analysis to assess whether the effect of the program previously estimated does not confound the effect of elections.

We address this concern in two ways. First, we restrict attention to mayors serving second terms during the 2001-2004 term. While it is not true that those mayors are not interested in the electoral outcomes, both because of career concerns and possible political involvement in succession, it is reasonable to assume that elections should play a less important role for these group, as the evidence in Ferraz and Finan (2010) suggests, for corruption levels.

The results are displayed in Table 8. Column (1) shows that program's effect is estimated as -10.7% homogenously for mayors serving first and second term because the interaction of the treatment with the indicator of lame duck status is not statistically different from zero. Allowing for different interactions with initial conditions, however, columns (2) and (3) show a different pattern: while the presence of radio implied a higher decrease in corruption for lame ducks, running counter to the intuition that they should be less concerned with information disclosure, the health council's operation was associated with the drop in corruption among mayors serving a first term (the older the council, the higher the drop in corruption following the program's introduction, consistently with the interpretation of the effects of higher opportunities of embezzlement).

In sum, it does not seem that the overall effect of the program differed significantly among mayors based upon their ability to run for reelection, which would dismiss the alternative explanation of a drop in corruption merely due to upcoming elections, but we cannot discard this entirely because the mechanics behind this drop are documented to have been contingent on a lame-duck status.

Thus, we move on to our second strategy: we include audited transfers accruing in the 2005-2006 period in our analysis to conduct a more thorough counterfactual analysis of the mayor's behavioral change in face of the introduction of the random-audits program, by enhancing the comparability of mayors decision settings. Of course, we cannot maintain municipal-fixed effects under this strategy because very few municipalities have audited transfers in any particular year of both terms, but we retain all covariates to control for municipal- and transfer-level heterogeneity.

The results, shown in Table 9, are very similar for the overall effect of the program, estimated at around 9%, while corruption is documented to have fallen more drastically in poorer municipalities, where embezzlement opportunities are higher, once federal transfers are a much more important budget source. When we break down transfers by capital intensity, we observe that the point estimate is again higher for capital-intensive ones but also significant for labor-intensive ones. While local constraints do not play a large role among the former, the presence of media is relevant for the latter, with corruption falling the most among labor-intensive transfers in municipalities with a radio station and internet provider.

The bottom line is that there is no support for the hypothesis that the drop in corruption after 2003 was merely the result of upcoming elections. Not only does corruption remain at what seems to be a new equilibrium under the random-audits program but also adding observations from the 2005-2006 period, which can be argued to be more directly comparable with the 2001-2002 baseline period, does not change the sign nor the magnitude of the treatment's effect. The effect of local constraints, conversely, varies substantially throughout specifications, but the presence of media seems to be more robustly associated with a stronger fall in corruption following the introduction of CGU's program, especially that of internet, and among labor-intensive transfers.[31]

7. CORRUPTION, MISMANAGEMENT AND COMPLIANCE

Thus far, we have shown that CGU's random-audits program has had a robust and sizeable effect on decreasing corruption prevalence. Nevertheless, the alternative to corruption is not necessarily compliance, such that the binomial framework can be too simplistic to provide a clear understanding of the extent to which the program indeed allowed for the agency problem to be resolved.

Next, we investigate how the program has affected mayors' decisions among corruption, mismanagement and compliance. The theoretical model presented in Section 3 can be readily extended to incorporate the choice of mismanagement: if each transfer is indexed by both an appropriable monetary value ($m.T$, using model's notation) and a cost of compliance, whereas there is also a reputational cost of mismanagement (though smaller than that of corruption), then the incumbent's decision can be re-written to include these additional parameters, as in Lichand, Lopes, and Medeiros (2011).

Figure 3 below, which reproduces Figure 2 but also accounts for mismanagement, shows, first, that mismanagement is always very high, with compliance only seldom being attributed to mayors,

[31]This finding is in line with the findings of (Ferraz and Finan, 2008), which show that electoral punishment following evidence of corruption is higher in the presence of media.

but second and more strikingly, that mismanagement prevalence has increased almost to the same extent that corruption has decreased following the introduction of the program.

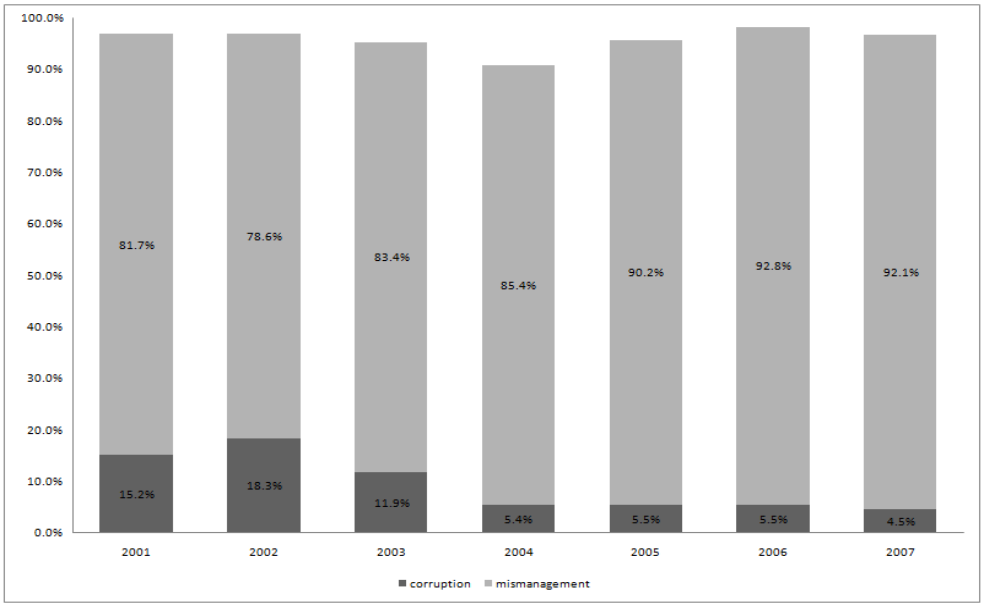

FIGURE 3. Corruption and mismanagement prevalence

The following subsections assess whether this pattern is maintained throughout a conditional analysis and, if so, whether we can attribute that pattern to politicians adapting to the new incentive scheme–so as to only avoid the reputational premium of being exposed as having engaged in corruption –or if costs of compliance are so high that mismanagement skyrockets even though mayors are trying to channel resources to public services delivery.

7.1. **Ordered model.** First, we run an ordered-probit model for the mayor's choice among corruption, mismanagement and compliance. We do not include municipal dummies due to the specification's non-linearity but instead include all available municipal- and transfer-level controls. The results here presented correspond to the 2001-2004 period, but adding observations of the 2005-2008 period does not significantly affect estimated coefficients.

Table 10 attributes an effect of -7.74% of the program on corruption, remarkably close to that obtained on the binomial specifications. Nonetheless, results are far from showing that compliance dominates as a substitute for corruption: mayors' decisions are approximately split between mismanagement and compliance, with the former prevailing over the latter for capital-intensive transfers, for which mismanagement is documented to have increased by an additional 0.5% in response to the program.

Even more striking are the effects of the interactions of the program's effect with local constraints: the presence of media seems really to be a key ingredient linked to information disclosure for deterring corruption but is also highly correlated with the rise of mismanagement that at least partially

continues preventing resources from financing public goods. We take these results as evidence that politicians have mainly adapted away from irregularities that might be linked to corruption, with the major part of resources still not reaching their specific ends, but assess that hypothesis in greater detail ahead.

7.2. **Four hypotheses about the rise of mismanagement.** We have shown that corruption has at least partially been replaced by mismanagement, which is especially true for capital-intensive transfers, but can this really be taken at face value? We here raise four hypotheses about the rise of mismanagement, which are linked to very different implications concerning the nature of mismanagement that arises in response to a fall in corruption. As such, we are able to draw upon our extremely detailed database to attempt to disentangle how likely each hypothesis is in light of the categories of mismanagement which prevalence increased by the most after CGU's program was introduced.

First, because auditors operate in close cooperation with municipal health councils, mayors might have just captured them to prevent irregularities from being exposed. If so, auditors' records of corruption would indeed decrease (although actual corruption could remain at the baseline level), whereas mismanagement linked to the council's structure and operation would increase in turn. Second, an alternative explanation is that mayors learned to better disguise corruption into mismanagement. As such, corruption, which is mainly linked to procurement in our analysis, would decline, while mismanagement in procurement would rise, conversely.

Third, it might be the case that mayors did indeed try to properly use federal transfers to provide public goods, but compliance costs are very high, especially due to the transition from a dysfunctional system to one that actually executes the resources budgeted. Under this scenario, corruption would fall, followed by an increase in mismanagement linked to documentation and/or performance.

Fourth, maybe mayors found that 'stealing at the entrance' is dominated by 'stealing at the exit': if, on the one hand, it is more straightforward to assess that corruption has taken place in procurement, on the other hand, once items have been purchased, it is much harder to disentangle embezzlement from mismanagement with respect to medication stocks, for instance.

To assess which hypothesis has the highest empirical likelihood, we run a multinomial logit model for the decision among corruption, mismanagement and compliance, breaking down mismanagement in seven partitions: (i) procurement problems, (ii) municipal health council problems, (iii) infrastructure problems, from precarious facilities to stock of medication control, (iv) human resources (HR) problems, from lack of qualified staff to irregularities in public servants' payroll, (v) documentation problems, (vi) performance problems, and (vii) other.

The results, presented in Tables 11 and 12, strongly support the fourth hypothesis. First, council problems have not significantly increased following CGU's program. Second, procurement problems have displayed a very sizeable and significant fall, about the same dimension as that of corruption itself, which even enhances the argument that the increase in compliance is very modest. Third, performance problems do indeed increase (by about 3.5%), which suggests that some rightful

implementation attempts might have taken place, but the lion's share of mismanagement increases is due to infrastructure problems (which have increased by 14.3 percentage points).

Combining the fall of corruption and mismanagement in procurement, the program is estimated to have had a first-order deterring effect on misconduct for about 20% of the audited transfers. What is striking is that for over 75% of those, resources were still diverted: the increase in compliance is only about 4%, a meager figure that cannot be explained by performance problems following due execution of budgeted resources.

Of interest, in Table 12, we can observe that mismanagement in procurement fell by the most in municipalities with an internet provider, consistently with what was documented for corruption before, and that mismanagement in infrastructure increased is by far concentrated in municipalities where electoral competition is lower, in line with model's predictions for local constraints and with very relevant implications for public policy.

7.3. **Health outputs and outcomes.** Another way to examine this problem is to focus on health outcomes to investigate whether these faced any significant improvement following the introduction of CGU's program. In line with Reinikka and Svensson (2005), we estimate the elasticity of outcomes to corruption, here, using the random-audits program as an instrumental variable to corruption prevalence at the municipal level. We use as dependent variables immunization rates and child mortality[32], controlling for the logarithm of municipal per-capita health expenditures. However, one might argue that these outcomes display high persistence in time and thus might take longer to show any positive effect of the fall in corruption. As such, we also use four measures of health outputs as dependent variables: shots, hospital beds, of families with treated water, and % of families enrolled in the Family Health program.[33]

Table 13 presents the reduced-form regressions and Table 14, the ones instrumenting average corruption prevalence in the municipality with CGU. We only present the results for the 2001-2004 period but have ran the same regressions including 2005-08 observations and without municipal-level fixed-effects, with nearly identical results. It is clear from the tables that the program's effect on health outputs and outcomes is, at best, meager: the elasticity of shots and immunization with respect to corruption has the "wrong sign" (that is, the decrease in corruption is documented to have decreased shots and immunization rates), and that of treated water and enrolled families, though negative and significant, is economically irrelevant (if corruption has fallen by at most 10%, that would mean that treated water has expanded to no more than 0.25% of the families, and family health coverage to no more than 0.45% of the families).

[32]Both immunization rate and child mortality are defined in terms of 1000 inhabitants.

[33]All health variables were retrieved from DATASUS and are available on a yearly basis. Hospital beds were measured under a different system prior to 2004; we have attempted to make both series compatible but could not retrieve information for 2004. The percentage of families with access to treated water is calculated from the universe of families enrolled in the Family Health program; as such, there might be a selection issue due to the expansion of the cadaster, which we ignore for simplicity, especially because family health coverage remains very low over the entire period.

8. CONCLUDING REMARKS

This paper assesses the effects of the introduction of CGU's Random-Audits Program, which increased the probability that local politicians would be audited, on corruption prevalence and the effectiveness of local constraints. Results are that up to 82% of the 60.7% decrease in corruption among audited health transfers in Brazil between 2001-02 and 2003-04 can be attributed to the random-audits program. Consistently with the theoretical model, the program's effect is weakly increasing in the strength of local constraints, particularly with respect to the presence of media and quality of municipal health councils. Accounting for heterogeneous embezzlement opportunities, it is indeed the case that the program's effect was stronger among capital-intensive transfers, which are more prone to misappropriation of resources in contrast to labor-intensive ones.

The substantial fall in corruption has not led, however, to higher compliance rates or better health outcomes. Combining the fall of corruption and mismanagement in procurement, if for about 20% of the audited transfers the program is estimated to have had a first-order deterring effect on misconduct, over 75% of those resources were still diverted: the increase in compliance is only approximately 4%, a meager figure that cannot be explained by performance problems following due execution of budgeted resources. Irregularities increased particularly for infrastructure maintenance and medication stock control, for which it is less straightforward to identify that corruption has occurred compared with irregularities in procurement. Worse still, mismanagement in infrastructure increase was by far concentrated in municipalities where electoral competition is lower.

We conclude that while voters punish corruption episodes, they seem not to take poor provision of public goods into account; as a result, politicians seem to adapt away from irregularities with higher exposure toward those less likely to be associated with corruption. The fact that this happens particularly for municipalities with a higher presence of media highlights that an information-disclosure program has limited power to work alone: information allows voters to punish corrupt politicians, as in (Ferraz and Finan, 2008); as a result, rational politicians hide better. Without additional enforcement mechanisms, resources still do not meet their specific ends, and performance continues to lag behind.

Our results are in sharp contrast to that of Reinikka and Svensson (2003) and Reinikka and Svensson (2005), probably because while the latter focuses on a specific transfer, whereby beneficiaries are able to accurately track the money that is actually cashed in, this paper analyzes politician's behavior concerning a wide spectrum of transfers, which are targeted to benefit a disperse group of individuals who have very imperfect information about how the outcomes of interest depend upon politicians' decisions. Therefore, information alone seems to be insufficient to support effective public services delivery, which still remains a major challenge in developing countries trying to implement decentralization: the gains from having public services provision closer to citizens seem still to be overcome by the costs of getting money where it is supposed to be.

One possibility is for information disclosure to be less focused on evidence of corruption but rather more on performance. Perhaps then voters would feel more confident about linking poor

quality of public services to incumbent's decisions. Of course, this would still rely on electoral punishment as the only enforcement mechanism for the proper application of federal transfers. An alternative would be to enforce legal action backed up by program's evidence if there is substantial proof that resources were diverted. While this alternative might work together with electoral incentives–as since 2010, a Brazilian law[34] prevents politicians who have been previously convicted to run for elections –it is very unlikely that something on these terms would be implemented, especially given the several political attempts that have been made to terminate CGU's program, reduce the number of municipalities audited or increase the time span between draws.

Conversely, strengthening municipal councils to both increase auditors' effectiveness and empower local citizens seems to be a low-hanging fruit: increasing the frequency of the meetings is documented to have a sizeable effect on decreasing misclassification of corruption events, generating the right incentives for proper allocation of federal resources. Along these lines, a parallel agenda should be that of enhancing local constraints and closing down embezzlement opportunities. We have shown these to matter; in particular, infrastructure problems following the drop in corruption have risen by the most in municipalities where electoral competition is lower. Empowering self-enforcing corruption controls can certainly have high returns to enhancing the quality of public services delivery.

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APPENDIX A: PROPOSITIONS' PROOFS

PROOF OF PROPOSITION 1: First, let us define Y^* as $\text{argmax}_Y\, u^E(Y)$. The probability of choosing corruption for a particular transfer, $Pr(Y^* = C)$, can be written as $Pr\left(u^E(C) > u^E(NC)\right)$; substituting for the expressions in 2,

$$Pr(Y^* = C) = Pr\left((1 - q(1-p))\, mT - 2q(1-p)R > 0\right) \tag{A-1}$$

For any $p \in [0, 1)$, it follows that

$$\frac{\partial[(1 - q(1-p))\, mT - 2q(1-p)R]}{\partial q} = -(1-p)(mT + 2R) < 0 \tag{A-2}$$

Hence,

$$\frac{\partial Pr(Y^* = C)}{\partial q} \leq 0 \quad \blacksquare \tag{A-3}$$

PROOF OF PROPOSITION 2: We can use A-2 to find the cross-derivatives of the net payoff of corruption with respect to the probability of being audited, q, and local attributes that determine reputation gains, R, or auditor's effectiveness, $(1 - p)$.

Thus,

$$\text{(A-4)} \qquad \begin{cases} \dfrac{\partial^2[(1-q(1-p))\,mT - 2q(1-p)R]}{\partial q\,\partial R} = -2(1-p) < 0\,, \\ \dfrac{\partial^2[(1-q(1-p))\,mT - 2q(1-p)R]}{\partial q\,\partial(1-p)} = -(mT+2R) < 0\,. \end{cases}$$

These, in turn, imply that

$$\text{(A-5)} \qquad \begin{cases} \dfrac{\partial^2 Pr(Y^*=C)}{\partial q \partial R} \leq 0 \quad , \\ \dfrac{\partial^2 Pr(Y^*=C)}{\partial q \partial(1-p)} \leq 0 \quad \blacksquare \end{cases}$$

PROOF OF PROPOSITION 3: By the same logic of the proof of Proposition 2,

$$\text{(A-6)} \qquad \frac{\partial^2[(1-q(1-p))\,mT - 2q(1-p)R]}{\partial q\,\partial m} = -(1-p)T < 0\,,$$

which implies that

$$\text{(A-7)} \qquad \frac{\partial^2 Pr(Y^*=C)}{\partial q \partial m} \leq 0 \quad \blacksquare$$

PROOF OF PROPOSITION 4: Let us first define $Pr(\hat{Y}=C) = (1-p)Pr(Y^*=C)$ as the data observed by the econometrician, contaminated with measurement error. It is easy to see that

$$\text{(A-8)} \qquad \frac{\partial Pr(\hat{Y}=C)}{\partial q} = (1-p)\frac{\partial Pr(Y^*=C)}{\partial q} - \frac{\partial p}{\partial q} Pr(Y^*=C)\,,$$

As long as $\frac{\partial p}{\partial q} \neq 0$, it follows that $\frac{\partial Pr(\hat{Y}=C)}{\partial q}$ can be lesser or greater, in absolute value, than the effect on the absence of measurement error, and can even have a different sign if $\frac{\partial p}{\partial q}$ is sufficiently negative .

Now, even if $\frac{\partial p}{\partial q} = 0$, using A-8 to obtain the cross-derivative of $Pr(\hat{Y}=C)$ with respect to q and $(1-p)$,

$$\text{(A-9)} \qquad \frac{\partial^2 Pr(\hat{Y}=C)}{\partial q \partial(1-p)} = (1-p)\frac{\partial^2 Pr(Y^*=C)}{\partial q \partial(1-p)} + \frac{\partial Pr(Y^*=C)}{\partial q}\,,$$

which also can be lesser or greater, in absolute value, than the effect on the absence of measurement error, since both terms on the RHS are negative (using the results from Propositions 1 and 2). $\blacksquare$

9. APPENDIX B: CLASSIFICATION LISTS

TABLE 2. 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 outbidder
Resource diversion	Overbilling
Resource diversion	Off the record invoice

Panel B: Mismanagement

Category	Irregularity
Health council	Municipal Health Council: composition
Health council	Municipal Health Council: operation
Health council	Municipal Health Council: infrastructure and work conditions
Procurement	Lack of divulgation
Procurement	Documents set with different dates
Procurement	Other procurement problems
Procurement	Irregular class
Procurement	No realization
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
Performance	Unaccomplished goals
Performance	Unfinished projects
Performance	Poorly evaluated services to health system users
Infrastructure	Precarious facilities
Infrastructure	Signs, logos and so on not properly set
Infrastructure	Lack of medical supplies
Infrastructure	Stock control of medication
Infrastructure	Maintenance of medication
Human Resources	Professionals that don't fulfill worktime requirements
Human Resources	Staff training
Human Resources	Staff composition
Human Resources	Public servants' payments
Documentation	Incomplete or inadequate documentantion
Other	Other sources of irregularities

TABLE 3. Capital-intensity by Health program

Panel A: Capital-intensive transfers
Assistência Farmacêutica e Insumos Estratégicos
Saneamento Básico - Melhorias Sanitárias
Farmácia Básica
Construção e Ampliação ou Melhoria dos Serviços de Abastecimento da Água para Controle de Agravos
Implantação, aparelhamento e adequação de unidades de saúde do Sistema Único de Saúde
Implantação de Melhorias Sanitárias Domiciliares para Controle de Agravos
Implantação, ampliação ou melhoria de sistema público de esgotamento sanitário para a prevenção e controle de agravos
Unidade Móvel de Saúde
Saneamento Básico e Resíduos Sólidos
Modernização e Adequação de Unidades de Urgência/Emergência e de Gestantes de Alto risco do Sistema Único de Saúde – SUS
Saneamento Básico - Esgoto
Saneamento Básico – Água
Modernização das Unidades de Saúde do SUS
Implantação e Ampliação ou Melhoria de Sistemas de Coleta, Tratamento e Destinação Final de Resíduos Sólidos para Controle de Agravos
Aquisiçã de Grupo Gerador de Energia Elétrica
Aquisição e Equipamentos e Materiais Permanentes
Panel B: Labor-intensive transfers
Atenção Básica em Saúde
Vigilância Epidemiológica e Ambiental em Saúde
Saúde da Família
PAB - Fixo
Controle da Tuberculose e Outras Pneumopatias de Interesse Sanitário - Nacional
Controle da Hanseníase e Outras Dermatoses - Nacional
Atendimento a População com medicamentos para Tuberculose e outras Pneumopatias - Nacional
Prevenção e Controle de Doenças Transmissíveis
Controle de Zoonozes - Nacional
Bolsa Alimentação
Implantação de Serviço Ambulatorial de Saúde Mental e Prevenção ao Uso de Álcool e Drogas – Nacional
Atendimento Ambulatorial, Emergencial e Hospitalar no SUS
Endemias
Doenças Transmitidas por Vetores
Atendimento Assistencial Básico referente à parte fixa do Piso de Atenção Básica
Atendimento Assistencial Básico nos Municípios Brasileiros
Implantação e Ampliação de Unidades de Saúde do SUS
Prevenção e controle de doenças transmitidas por vetores
Unidades de Saúde do SUS
Atenção à Saúde da População nos Municípios e Est. Plen.(Hospital)
Prevenção e Controle de Doenças Transmissíveis
Combate à doença de chagas
Tuberculose
Ações de Combate às Carências Nutricionais
Vigilância Epidemiológica e Ambiental Em Saúde
Agente Comunitário de Saúde
Qualidade e Eficiência do Sistema Único de Saúde
Erradicação do Aedes Aegypti do Brasil
Qualidade do Sangue
Assistência à População Indígena
Campanha Educativa de Prevenção das doenças Sexualmente Transmissíveis – DST e da Síndrome da Imunodeficiência Adquirida – AIDS - Nacional

10. APPENDIX C: CODING THE AUDIT REPORTS

This appendix shows examples of audits' findings, including cases of compliance, mismanagement and corruption in both periods, before CGU's program was introduced and afterwards.

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.

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 Comercio e Representacoes Ltda., does not exist legally according to the local branch of the Secretariat of the Federal Revenue in Cuiaba, 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-present)

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 Goias, Goias. 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 Soquimica Laboratorios 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.

11. Appendix D: Description of the main Brazilian Health programs

11.1. **Basic Attention in Health.** The Basic Attention in Health program is a set of actions in health, including prevention, diagnosis, medical treatment and rehabilitation. Its focus is on the most common health problems that affect the Brazilian population, following the basic principles of universality, accessibility, humanization, equity and social participation.

Formally, the Health Family program is part of the Basic Attention in Health program, according to the Sistema ênico de Saê de (SUS or Unified National Health System).

Financial rule: The total amount transferred by the federal government ranges from R$ 10.00 (ten reais) to R$ 18.00 (eighteen reais), per capita, on a monthly basis.

The actions financed include:

(1) Medical appointments in basic specialties;
(2) Basic dental care;
(3) Ambulatory and home health care under the Family Health Program;
(4) Vaccination;
(5) Minor surgeries;
(6) Emergency care services.

11.2. **Basic Pharmacy.** Under the Basic Pharmacy program, states and municipalities use a national basic list of medicines, but are allowed to add non-listed drugs. The drugs related to basic attention in health can be found on the National List of Essential Drugs (Relação Nacional de Medicamentos Essenciais), in which are listed the medicines related to the most common diseases in Brazil.

The Ministry of Health transfers funds on a monthly basis, and the distribution of medicines is under the responsibility of municipalities.

Financial rule: The total transferred amount is, at least, R$2.00 (two reais) per capita, per year, of which R$ 1.00 is transferred by the federal government, and at least R$ 1.00 by state and municipal governments altogether.

11.3. **Family Health Program.** The Family Health Program in Brazil is considered by the Ministry of Health to be the most basic priority of the Basic Attention in Health Program organization. Its main purpose is to reach Brazilian families with quality health care, improving general well-being.

Assistance is provided by members of family health teams, composed of doctors, nurses, nurses' aides, community-based health agents and dentists), at local health units or at home. Family health teams and local populations tend to foster a relationship of confidence and trust, making it easier for agents to develop their work. It is important to highlight that each family health team is responsible for taking care of a fixed number of families, on a specific geographic location.

The program was established in 1993, and presently takes care of more than a hundred million people.

Financial rule: The National Health Fund transfers to municipalities, on a monthly basis, per family health team, an amount that ranges from R$ 2,801.00 to R$ 5,400.00, depending on the size of the population under coverage. For each newly implemented health family team, the Ministry of Health provides an additional R$ 10,000.00 transfer.

12. Appendix E: Regression outputs

TABLE 4. Baseline results

VARIABLES	Baseline (1)	Dynamic (2)	Static (3)	Mayor (4)	Mun FE (5)	Year FE (6)
CGU	-0.0828***	-0.0721***	-0.0740***	-0.0737***	-0.0544**	
	[0.0179]	[0.0187]	[0.0198]	[0.0197]	[0.0234]	
Year 2002						0.0216
						[0.0350]
Year 2003						-0.0246
						[0.0343]
Year 2004						-0.0624*
						[0.0371]
mayor_gov		-0.0202	-0.0192	-0.0188	-0.0458	-0.0440
		[0.0209]	[0.0217]	[0.0217]	[0.0512]	[0.0513]
ln(pc_gdp)		0.0307*	0.0190	0.0192	0.0125	0.0204
		[0.0163]	[0.0213]	[0.0210]	[0.125]	[0.125]
ln(pc_health_expenditure)		-0.0433	-0.0398	-0.0443	-0.156*	-0.141
		[0.0324]	[0.0333]	[0.0322]	[0.0839]	[0.0924]
immunization$_{t-1}$		0.508	0.542	0.635	1.218	0.734
		[0.851]	[0.899]	[0.883]	[1.389]	[1.379]
child_mortality$_{t-1}$		-0.209	-0.250	-0.152	-0.798*	-0.689
		[0.230]	[0.255]	[0.273]	[0.470]	[0.467]
pc_public_servants		-1.162	-0.812	-0.919	-0.819	-0.831
		[0.786]	[0.906]	[0.942]	[2.235]	[2.257]
ln(distance_capital)			0.00571	0.00554		
			[0.00479]	[0.00483]		
radio			0.0193	0.0163		
			[0.0295]	[0.0302]		
internet			0.0187	0.0192		
			[0.0308]	[0.0316]		
clubs			0.0352	0.0350		
			[0.0222]	[0.0223]		
poverty			0.172*	0.157		
			[0.0958]	[0.103]		
schooling			0.0276	0.0251		
			[0.0174]	[0.0193]		
turnout			0.145	0.164		
			[0.202]	[0.205]		
electoral_margin			0.0467	0.0493		
			[0.0649]	[0.0662]		
health_council_age			0.000448	0.000411		
			[0.00293]	[0.00303]		
health_council_monthly_meets			-0.00464	-0.00428		
			[0.0244]	[0.0246]		
mayor_age				0.000820		
				[0.00124]		
mayor_male				0.0343		
				[0.0239]		
mayor_high_school				-0.0258		
				[0.0313]		
mayor_university				-0.0135		
				[0.0325]		
lame_duck				-0.00148		
				[0.0231]		
Constant	0.154***	0.103	-0.319	-0.359	0.698	0.591
	[0.0188]	[0.142]	[0.338]	[0.352]	[1.092]	[1.111]
Observations	687	682	674	672	682	682
R-squared	0.033	0.059	0.076	0.083	0.511	0.515
Municipal-level fixed-effects	No	No	No	No	Yes	Yes

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

TABLE 5. Local constraints

VARIABLES	(1)
CGU*log_distance_capital	-0.00234
	[0.00630]
CGU*radio	-0.0534
	[0.0553]
CGU*internet	-0.00834
	[0.0652]
CGU*clubs	-0.0373
	[0.0409]
CGU*poverty	-0.0341
	[0.213]
CGU*schooling	-0.00376
	[0.0349]
CGU*turnout	-0.159
	[0.399]
CGU*elec_margin	-0.0534
	[0.102]
CGU*health_council_age	-5.61e-05
	[0.00578]
CGU*health_council_monthly_meets	0.0112
	[0.0479]
Constant	0.615
	[1.135]
Observations	674
R-squared	0.516
Municipal-level fixed-effects	Yes
Municipal-level controls	Yes

Robust standard errors in brackets

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

Note: Municipal-level controls included are the ones presented in Table 2.

TABLE 6. Transfer-level heterogeneity

	Pooled sample		Capital-intensive		Labor-intensive	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
CGU	-0.107***	0.0478	-0.214***	-0.637	-0.0191	0.299
	[0.0203]	[0.509]	[0.0516]	[0.746]	[0.0285]	[0.522]
CGU*K_intensive	0.00107	0.00499				
	[0.0217]	[0.0207]				
CGU*ln(distance_capital)		0.0302**		0.0405		0.0643*
		[0.0153]		[0.0370]		[0.0342]
CGU*radio		0.00183		-0.0660		0.123
		[0.0464]		[0.0946]		[0.0770]
CGU*internet		-0.0293		0.0835		-0.127***
		[0.0510]		[0.109]		[0.0472]
CGU*clubs		-0.0373		-0.117		-0.0207
		[0.0377]		[0.101]		[0.0695]
CGU*poverty		-0.202		0.739*		-0.863***
		[0.176]		[0.412]		[0.306]
CGU*schooling		-0.0160		0.137*		-0.122**
		[0.0276]		[0.0697]		[0.0474]
CGU*turnout		-0.633*		0.315		-1.505***
		[0.358]		[0.633]		[0.567]
CGU*elec_margin		-0.125		-0.116		-0.221
		[0.0889]		[0.157]		[0.136]
CGU*health_council_age		-0.00861**		-0.0106		-0.00589
		[0.00429]		[0.0123]		[0.00736]
CGU*health_council_monthly_meets		0.0473		0.385**		0.0397
		[0.0642]		[0.194]		[0.121]
Observations	2,747	2,717	1,024	999	1,054	1,054
Municipal-level fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipal-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Transfer-level controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets

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

Notes: (1) Municipal-level controls included are the ones presented in Table 2;

(2) Transfer-level controls are transfer's amount and an indicator of whether the transfer is capital- or labor-intensive.

TABLE 7. Measurement error

	Pooled sample		Capital-intensive		Labor-intensive	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
CGU	-0.0774***	0.1912***	-0.1206***	-0.3636	-0.0659***	0.1843***
	[0.0207]	[0.0191]	[0.0180]	[13.8008]	[0.0196]	[0.0101]
CGU*K_intensity	-0.0173***	-0.0074***				
	[0.0059]	[0.0013]				
CGU*ln(distance_capital)		-0.0166***		0.0059		-0.0424***
		[0.0045]		[0.0219]		[0.0051]
CGU*radio		0.0083		0.0221		0.0209
		[0.0142]		[0.7252]		[0.0193]
CGU*internet		-0.0308		-0.0556		-0.0351
		[0.0188]		[1.4061]		[0.0245]
CGU*clubs		0.8333		0.9385		0.7144
		[0.5330]		[18.0885]		[0.7904]
CGU*poverty		-0.3714		-0.8551		-0.0282
		[0.5120]		[12.9978]		[0.6363]
CGU*schooling		-0.8335		-0.2408		-0.7145
		[0.5808]		[16.5528]		[0.8294]
CGU*turnout		0.0106**		0.0038		0.0016
		[0.0050]		[0.1987]		[0.0079]
CGU*elec_margin		0.0035		-0.0158		0.0028
		[0.0061]		[0.0584]		[0.0019]
CGU*health_council_age		-0.0105		0.0114		0.0514
		[0.0199]		[0.5094]		[0.0424]
CGU*health_council_monthly_meets		0.0309***		0.0179		0.0418**
		[0.0064]		[0.3204]		[0.0197]
			Measurement error			
health_council_age	-0.0006	0.0200	0.0097	0.0027	-0.0191	-0.0217
	[0.1542]	[0.1263]	[0.0329]	[0.0250]	[0.2669]	[0.2596]
health_council_monthly_meets	0.0098	-0.3766***	-0.3968	-0.0822**	0.8078	0.8575
	[0.0986]	[0.0844]	[0.3105]	[0.0375]	[3.5747]	[2.5850]
ln(amount)	0.0102	0.0032	0.0079	0.0793***	0.0824*	0.093*
	[0.0267]	[0.0296]	[0.3529]	[0.0185]	[0.0495]	[0.0501]
lag_investigation	-0.5097	-0.2246***	-0.2724	-0.036***	-0.3143***	-0.2829***
	[1.7156]	[0.0363]	[0.1897]	[0.0092]	[0.0985]	[0.0726]
Observations	3,728	3,728	1,622	1,622	2,106	2,106
Log-Likelihood	-1108.40	-1105.20	-527.94	-522.70	-545.61	-542.69
Average measurement error	0.2658	0.3837	0.1947	0.8458	0.2428	0.2591
Municipal-level fixed-effects	No	No	No	No	No	No
Municipal-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Transfer-level controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets

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

Notes: (1) Municipal-level controls included are the ones presented in Table 2;
(2) Transfer-level controls are transfer's amount and an indicator of whether the transfer is capital- or labor-intensive.

TABLE 8. Elections: Lame-duck incumbents

VARIABLES	Pooled sample (1)	Second-term (2)	First-term (3)
CGU	-0.107***	-0.741***	0.137
	[0.0243]	[0.170]	[0.700]
CGU*lame_duck	-0.000102		
	[0.0389]		
CGU*ln(distance_capital)		0.0113	0.0308
		[0.00956]	[0.0188]
CGU*radio		-0.0316**	0.0608
		[0.0160]	[0.0722]
CGU*internet		0.0353	-0.0611
		[0.0491]	[0.0658]
CGU*clubs		-0.00482	-0.0359
		[0.0196]	[0.0467]
CGU*poverty		0.0642	-0.275
		[0.127]	[0.213]
CGU*schooling		0.00644	-0.0141
		[0.0187]	[0.0367]
CGU*turnout		0.143	-0.789*
		[0.228]	[0.439]
CGU*elec_margin		-0.0349	-0.126
		[0.0499]	[0.131]
CGU*health_council_age		-0.000243	-0.0131***
		[0.00259]	[0.00473]
CGU*health_council_monthly_meets		-0.0218	0.123*
		[0.0216]	[0.0702]
Observations	2,747	650	2,067
Municipal-level fixed-effects	Yes	Yes	Yes
Municipal-level controls	Yes	Yes	Yes
Transfer-level controls	Yes	Yes	Yes

Standard errors in brackets

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

Notes: (1) Municipal-level controls included are the ones presented in Table 2;
(2) Transfer-level controls are transfer's amount and an indicator of whether the transfer is capital- or labor-intensive.

TABLE 9. Elections: 2001-2006

	Pooled sample		Capital-intensive		Labor-intensive	
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
CGU	-0.0902***	0.154	-0.100***	0.0836	-0.0710***	0.151
	[0.00888]	[0.185]	[0.0106]	[0.296]	[0.0129]	[0.356]
CGU*ln(distance_capital)		-0.00421		-0.00272		-0.00320
		[0.00343]		[0.00351]		[0.00527]
CGU*radio		-0.0154		0.0143		-0.0377**
		[0.0183]		[0.0289]		[0.0181]
CGU*internet		-0.0288		0.0104		-0.0532***
		[0.0200]		[0.0357]		[0.0200]
CGU*clubs		0.00560		-0.00526		-0.00452
		[0.0236]		[0.0269]		[0.0354]
CGU*poverty		-0.147*		-0.0157		-0.216
		[0.0873]		[0.115]		[0.132]
CGU*schooling		-0.0168		-0.00396		-0.0297
		[0.0150]		[0.0195]		[0.0231]
CGU*turnout		-0.244		-0.114		-0.113
		[0.173]		[0.218]		[0.268]
CGU*elec_margin		-0.0404		-0.0334		-0.00946
		[0.0440]		[0.0543]		[0.0821]
CGU*health_council_age		-0.00281		-0.00440		-0.00243
		[0.00263]		[0.00340]		[0.00327]
CGU*health_council_monthly_meets		0.0166		0.0220		0.0404
		[0.0265]		[0.0337]		[0.0375]
Observations	8,326	8,326	3,065	3,065	5,261	5,261
Municipal-level fixed-effects	No	No	No	No	No	No
Municipal-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Transfer-level controls	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in brackets

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

Notes: (1) Municipal-level controls included are the ones presented in Table 2;

(2) Transfer-level controls are transfer's amount and an indicator of whether the transfer is capital- or labor-intensive.

TABLE 10. Ordered Probit marginal effects

	(1)			(2)		
VARIABLES	Compliance	Mismanagement	Corruption	Compliance	Mismanagement	Corruption
CGU	0.0391***	0.0383***	-0.0774***	-0.261*	0.0965	0.165
	[0.0107]	[0.00362]	[0.0117]	[0.150]	[0.149]	[0.298]
CGU*K	0.0150	0.00519**	-0.0202	0.0120	0.00439*	-0.0164
	[0.0136]	[0.00202]	[0.0153]	[0.0137]	[0.00265]	[0.0162]
CGU*ln(distance_capital)				0.00167	0.000756	-0.00242
				[0.00320]	[0.00145]	[0.00465]
CGU*radio				0.0244	0.00645***	-0.0309*
				[0.0194]	[0.00192]	[0.0185]
CGU*internet				0.0231	0.00611***	-0.0292
				[0.0233]	[0.00174]	[0.0226]
CGU*clubs				0.0169	0.00881**	-0.0257
				[0.0169]	[0.00403]	[0.0207]
CGU*poverty				0.0187	0.00848	-0.0272
				[0.0678]	[0.0307]	[0.0985]
CGU*schooling				-0.00398	-0.00180	0.00579
				[0.0118]	[0.00535]	[0.0171]
CGU*turnout				0.159	0.0720	-0.231
				[0.122]	[0.0562]	[0.177]
CGU*elec_margin				0.0457	0.0207	-0.0664
				[0.0356]	[0.0164]	[0.0516]
CGU*health_council_age				-0.00193	-0.000874	0.00280
				[0.00185]	[0.000851]	[0.00269]
CGU*health_council_monthly_meets				0.00479	0.00227	-0.00706
				[0.0174]	[0.00677]	[0.0242]
Observations	3,728	3,728	3,728	3,728	3,728	3,728
Municipal-level fixed-effects		No			No	
Municipal-level controls		Yes			Yes	
Transfer-level controls		Yes			Yes	

Standard errors in brackets

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

Notes: (1) Municipal-level controls included are the ones presented in Table 2;

(2) Transfer-level controls are transfer's amount and an indicator of whether the transfer is capital- or labor-intensive.

TABLE 11. Multinomial Logit marginal effects

	Compliance	Mismanagement							Corruption
VARIABLES		Procurement	Council	Infrastructure	HR	Documentation	Performance	Other	
CGU	0.0401***	-0.101***	0.00571	0.143***	0.0263**	-0.0128	0.0347**	-0.0465***	-0.0895***
	[0.0129]	[0.00652]	[0.00733]	[0.0229]	[0.0123]	[0.0171]	[0.0176]	[0.0134]	[0.00862]
Observations	3,728	3,728	3,728	3,728	3,728	3,728	3,728	3,728	3,728
Municipal-level fixed-effects					No				
Municipal-level controls					Yes				
Transfer-level controls					Yes				

Standard errors in brackets

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

Notes: (1) Municipal-level controls included are the ones presented in Table 2;

(2) Transfer-level controls are transfer's amount and an indicator of whether the transfer is capital- or labor-intensive.

TABLE 12. Multinomial Logit marginal effects

	Compliance	Mismanagement							Corruption
VARIABLES		Procurement	Council	Infrastructure	HR	Documentation	Performance	Other	
CGU	0.0424	0.239*	0.0160	0.127	-0.698	0.104	0.102	0.0579	0.00962
	[0.296]	[0.142]	[0.240]	[0.541]	[0.430]	[0.379]	[0.376]	[0.324]	[0.186]
CGU*ln(distance_capital)	0.00764	-0.0238***	-0.0134	0.000824	-0.00128	0.0111	-0.0143	0.0272***	0.00597
	[0.00716]	[0.00700]	[0.00991]	[0.00800]	[0.0137]	[0.00951]	[0.00984]	[0.00902]	[0.00788]
CGU*radio	0.0637	-0.0219	0.0326	-0.0557	-0.0108	0.0390	-0.00313	-0.0213	-0.0224
	[0.0570]	[0.0175]	[0.0470]	[0.0409]	[0.0286]	[0.0491]	[0.0424]	[0.0316]	[0.0212]
CGU*internet	0.0144	-0.0520***	-0.0137	0.130	-0.0293	-0.0131	0.0446	-0.0579*	-0.0233
	[0.0394]	[0.0127]	[0.0124]	[0.0899]	[0.0208]	[0.0528]	[0.0609]	[0.0335]	[0.0257]
CGU*clubs	0.0379	0.0143	0.00456	-0.0631	-0.0135	0.0211	-0.0268	0.0147	0.0108
	[0.0349]	[0.0224]	[0.0178]	[0.0554]	[0.0263]	[0.0406]	[0.0393]	[0.0366]	[0.0290]
CGU*poverty	-0.0921	-0.134	0.0326	0.133	0.165	-0.372**	0.0541	0.304*	-0.0897
	[0.124]	[0.0911]	[0.0788]	[0.238]	[0.155]	[0.185]	[0.184]	[0.162]	[0.114]
CGU*schooling	-0.0256	-0.0127	0.00643	-0.0144	0.0273	-0.0536*	-0.00804	0.0769***	0.00368
	[0.0230]	[0.0158]	[0.0137]	[0.0429]	[0.0262]	[0.0324]	[0.0326]	[0.0289]	[0.0196]
CGU*turnout	-0.109	0.0534	0.208	0.554	0.490*	-0.652**	0.391	-0.476*	-0.460**
	[0.240]	[0.171]	[0.168]	[0.422]	[0.277]	[0.333]	[0.323]	[0.281]	[0.212]
CGU*elec_margin	0.00393	0.0164	-0.00597	0.297**	-0.0860	-0.300***	0.0559	0.145*	-0.127**
	[0.0694]	[0.0463]	[0.0482]	[0.141]	[0.0655]	[0.0972]	[0.0944]	[0.0875]	[0.0618]
CGU*health_council_age	-0.00718*	-0.00204	0.00135	0.00227	0.000503	-0.00350	0.00234	0.00883**	-0.00258
	[0.00367]	[0.00253]	[0.00233]	[0.00662]	[0.00359]	[0.00517]	[0.00505]	[0.00423]	[0.00310]
CGU*health_council_monthly_meets	0.0235	-0.00895	-0.0414***	0.0258	0.0366	-0.0584	0.0221	0.000528	0.000217
	[0.0340]	[0.0226]	[0.0158]	[0.0574]	[0.0413]	[0.0433]	[0.0461]	[0.0379]	[0.0301]
Observations	3,728	3,728	3,728	3,728	3,728	3,728	3,728	3,728	3,728
Municipal-level fixed-effects					No				
Municipal-level controls					Yes				
Transfer-level controls					Yes				

Standard errors in brackets

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

Notes: (1) Municipal-level controls included are the ones presented in Table 2;

(2) Transfer-level controls are transfer's amount and an indicator of whether the transfer is capital- or labor-intensive.

TABLE 13. Health outputs and outcomes: Reduced-form regressions

	Health outputs				Health outcomes	
	ln(shots/1000 inhab.)	ln(hospital beds/1000 inhab.)	% treated water	% cadastred	Immunization	Child mortality
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
CGU	-0.111***	0.0384	0.0250***	0.0236***	-0.00445***	-0.000691
	[0.0279]	[0.0422]	[0.00892]	[0.00458]	[0.000672]	[0.00109]
ln(pc_health_expenditure)	-0.212***	0.125	0.0321*	0.0344***	0.0106***	-0.00604**
	[0.0636]	[0.127]	[0.0177]	[0.00918]	[0.00226]	[0.00283]
Constant	7.806***	-0.341	0.456***	0.0243	0.0322***	0.0489***
	[0.284]	[0.591]	[0.0848]	[0.0418]	[0.0103]	[0.0128]
Observations	9,119	2,272	8,704	9,120	9,120	9,025
R-squared	0.799	0.965	0.977	0.866	0.733	0.701
Municipal-level fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

TABLE 14. Health outputs and outcomes: IV regressions

	Health outputs				Health outcomes	
	ln(shots/1000 inhab.)	ln(hospital beds/1000 inhab.)	% treated water	% cadastred	Immunization	Child mortality
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Corruption	1.093***	-1.251	-0.254***	-0.233***	0.0439***	0.00682
	[0.326]	[1.904]	[0.0960]	[0.0544]	[0.00976]	[0.0108]
ln(pc_health_expenditure)	-0.160**	0.0826	0.0211	0.0234**	0.0127***	-0.00572*
	[0.0696]	[0.128]	[0.0183]	[0.0105]	[0.00266]	[0.00309]
Constant	7.413***	-0.293	0.664***	0.102**	0.0152	0.0469***
	[0.327]	[0.595]	[0.0863]	[0.0494]	[0.0125]	[0.0145]
Observations	9,119	2,272	8,704	9,120	9,120	9,025
R-squared		0.748	0.922	0.420		0.688
Municipal-level fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

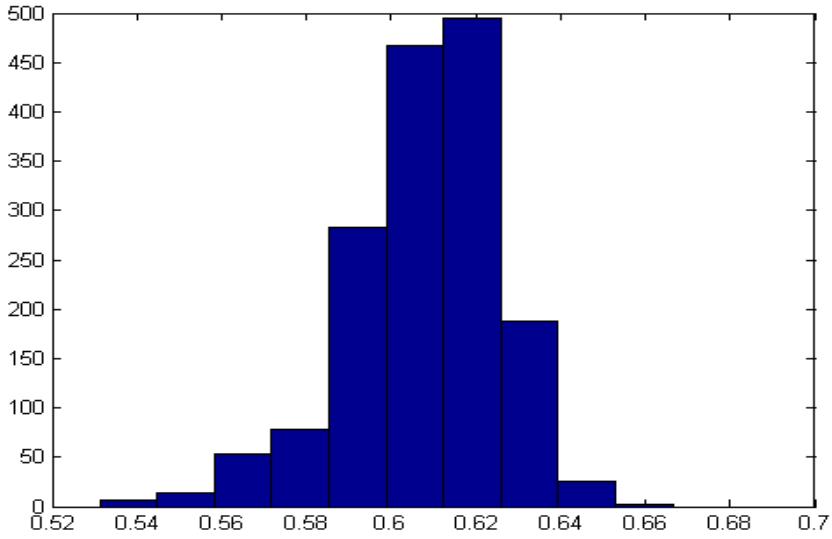

FIGURE 4. Histogram of estimated measurement error among transfers with no lag between irregularity and investigation

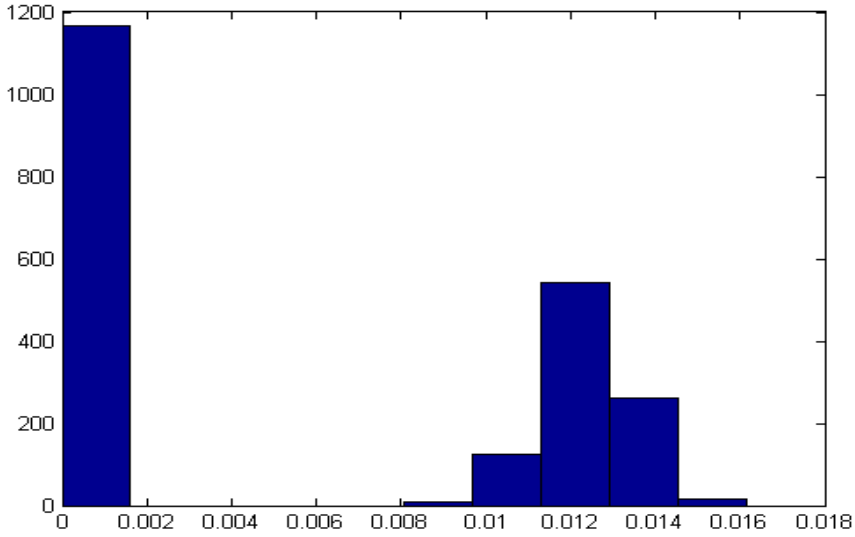

FIGURE 5. Histogram of estimated measurement error among transfers with lag between irregularity and investigation