Is a well-designed anti-corruption program enough to improve the provision of goods in a country where corruption is endemic?

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Abstract: Brazil's randomized anti-corruption program provides a unique opportunity to study the effect of a well-designed anti-corruption program on health. While previous studies have examined whether the program detected, prosecuted or punished corruption, our study examines the policy-relevant question of whether the anti-corruption program improved health outcomes. We study four childhood health indicators: infant mortality, undernourishment, doctor's appointments, and infant vaccinations. Using panel data from 5560 Brazilian municipalities from 2000 to 2011 we find tiny, short run effects on two measures but reject that there was any lasting, statistically significant, meaningful effects of the anti-corruption program on these outcomes.

JEL classification: H51, H72, I18, O17
Key words: anti-corruption program, primary health, audits, randomization, Brazil

Is a well-designed anti-corruption program enough to curb corruption in a developing country where corruption is endemic?

1. Introduction

The paper addresses the fundamental issue of whether a well-designed anti-corruption program is enough to improve the quantity or quality of government-funded services in an environment of endemic public corruption. We focus here on health care outcomes that are likely to respond quickly to government spending, and are also a large component of federal budgets and ask whether the anti-corruption program is making them better.

The core feature of Brazil's central anti-corruption program, which started in 2003, is that a randomly chosen subset of moderately large municipalities was audited each year, and the results quickly publicized, even before any criminal or civil charges were made. Since these audits were truly random the quasi-experimental design enables us to compare outcomes between audited and unaudited municipalities to see if there is a direct "auditing effect," and compare outcomes between eligible and ineligible municipalities to check for evidence of a "deterrent effect." We employ difference-in-difference for the first effect and regression discontinuity analysis to test the second.

Brazil's Corregedoria-Geral da União (CGU), the Comptroller General's anti-corruption agency, has a mandate to monitor municipalities' usage of funds provided by the central government. As detailed by Ferraz and Finan (2008), the program investigates and monitors central funds in numerous areas, including education, health, and infrastructure. The health sector is the second largest single recipient of centralized funds (after education), with federal funds largely channeled through the Brazilian Public Health System (SUS). The SUS was created by Brazil's 1988 Constitution, which defined a process of decentralization for primary health and prevention services. Although the central government was successful in largely decentralizing the administration of the health sector - mainly employing the Program of Health Family (PSF) according to Rocha and Soares (2010) - decentralization of the financing has happened more slowly. In 2011 the central government still financed about 40% of the provision of health services.

Even though the CGU anti-corruption program is very well designed, the Brazilian legal system is often seen as ineffective at deterring criminal activities. Even if a crime is detected the justice system can be very slow to bring suspected criminals to trial. Additionally, court proceedings seem to fail at successfully convicting and punishing defendants or compensating victims. A final concern is that even if corruption is identified and punished, it may not meaningfully improve public good provision if the punishment is late, largely invisible to voters or other officials, or not severe enough to deter future offenses.

Brazil is an important case in point since it has a large developing economy where the level of corruption has escalated hugely in recent years, despite the numerous transparency measures implemented. This was particularly evident in the huge amount of money deviated from the public sector by corporations and politicians disclosed by the "Car Wash" investigation of 2014, showing that Brazilian corruption is endemic (Rotberg, 2019).

The remainder of this paper is organized as follows. In Section 2 we review the existing literature and describe the chronology of interventions and changes in the Brazilian health sector. Section 3 specifies our hypotheses, describes our panel data, and outlines our empirical strategy. Section 4 presents our results, while Section 5 discusses our main conclusions.

2. **The anti-corruption program and literature**

The CGU program was launched in 2003 following a general emphasis on transparency and public disclosure of information, however, Brazil only passed its own Freedom of Information Act (FOI) in 2011. Since 2011, the number of investigated municipalities by year dropped from around 200 to 60 and has weakened CGU's auditing power as well, which is what motivates us to focus our analysis on early years of the program, up to 2011. The CGU audit reports are available on the Internet shortly after the municipalities have been audited and a great attraction of the data is that the program relies heavily on randomization to promote fairness: a public lottery drawing each year determines which municipalities are selected for an audit. The municipalities eligible for the lottery must have less than 500 000 inhabitants, which represent 99% of all Brazilian municipalities. Figure 1 illustrates the randomness of the audited municipalities from 2003 to 2011 (see Ferraz and Finan (2008) for further details of the program).

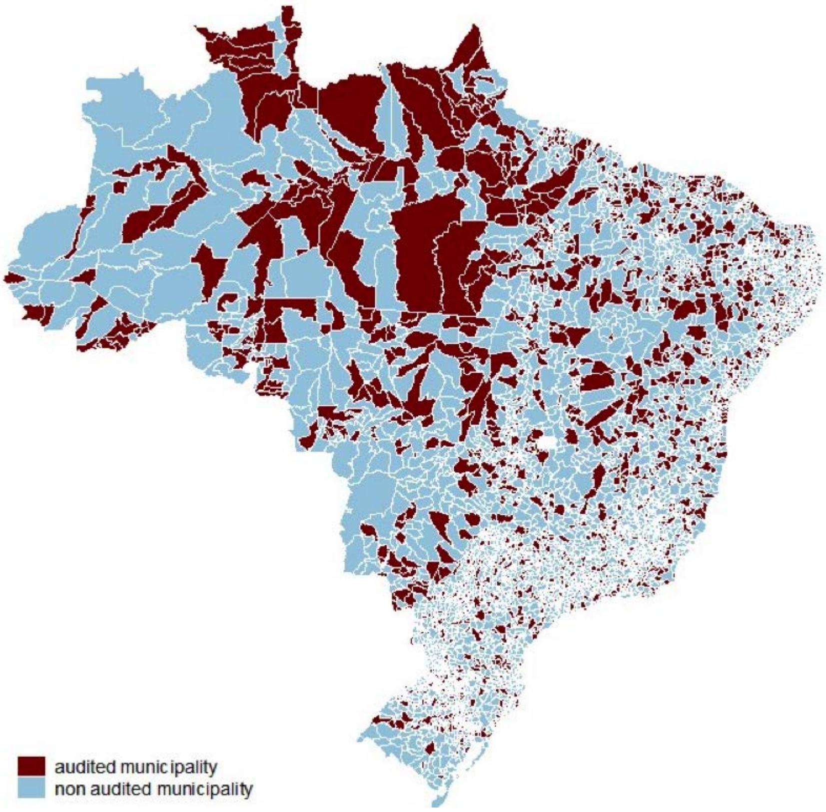

Figure 1 – Municipalities that were audited at from 2003 to 2011

The literature is inconclusive about the benefits of decentralization and corruption, with results depending on specific characteristics of decentralization. Brennan and Buchanan (1980) use a model of inter-jurisdictional competition to illustrate the possibility of a negative relationship between decentralization and corruption, which relies upon the logic of market competition.

According to Fredriksson and Vollebergh (2009), decentralization under a federal system reduces corruption by increasing the number of levels of political officials necessary to bribe in order to obtain large scale benefits. Fisman and Gatti (2002) found that greater decentralization was linked to lower levels of corruption behavior using cross-country data as well. Using data from the United States, Goel and Nelson (2011) find a negative effect of decentralization on local government corruption activities.

Contrary views, however, are offered by Careaga and Weingast (2000), Rodden (2000), Persson and Tabellini (2000), and Fan, Lin and Treisman (2009), who warn that decentralization may not be successful in reducing corruption if decentralized spending by municipalities is not accompanied by decentralized revenue. Acemoglu and Verdier (2000) argue that whenever the government transfers resources to address a market failure, this increases incentives for municipalities to indulge in corrupt behavior: a federal structure requires local bureaucrats to gather information and implement federal policies, creating opportunities for local officials to misreport or misrepresent their findings for their own self-interest.

A different line of argument is that anti-corruption programs are inherently ineffective. Banerjee, Mullainathan and Hanna (2012) argue that there are no guarantees that the bureaucrat responsible for auditing the municipalities' is not himself corrupt. This can occur if the bureaucrat benefits from the existing information asymmetry in the municipalities, or will accept bribes in exchange for ignoring mismanagement.

Finally, there are studies exploring the literature on transparency, and in particular Freedom of Information (FOI) legislation, which finds mixed evidence on their effectiveness. Escaleras, Lin and Register (2010) examine panel data on 128 countries that includes 46 countries adhered to FOI in 2003 as a way of promoting greater transparency in the public sector and do not discover any significant effect of developed countries incorporating FOI on the level of corruption. More controversially, the authors find a perverse effect of transparency on corruption for developing countries. Costa (2012) use the FOI adoption to study perceived corruption and quality of governance. He finds an increase in the corruption perceived and decrease of governance quality perceived only in the initial years of the reform, with no significant decrease in the long term.

Several studies examine corruption specifically in Brazil. Peixoto et al. (2012) find that the number of corruption incidents in Brazil's Southeast region between 2004 and 2010 declined when municipalities gained control over the provision of basic health services. Ferraz and Finan (2008) find that an increase in the number of corruption charges identified by the CGU program reduced the probability of an incumbent being reelected in the municipal election results. Hence local officials may not want to identify corruption, even if only higher level bureaucrats are involved, since they may be blamed. Ferraz, Finan and Moreira (2012) analyze whether the reduction in allocated funds due to corruption and misuse affects students outcomes in Brazil. They find that missing resources significantly constrains school quality. Test scores on national standardized exams and exam pass rates are significantly lower in more corrupt than less corrupt municipalities, while dropout rates are significantly higher.

Lichand, Lopes and Medeiros (2015) analyzed the association between the number of corruption cases discovered by Brazil's CGU anti-corruption program and health and non-health spending outcomes. Consistent with our findings, they find that health outcomes were largely unaffected or if anything became worse after detection of corruption. The Lichand et al. study uses much of the same data and is closest to the current study. Both Ferraz, Finan, and Moreira (2012) and Lichand et al. focus on the effects of corruption, not on the overall effects of an audit or an audit program as we study here. The audit program may influence corruption through a deterrent effect even among municipalities that are not audited (and hence where corruption is not detected), thus we examine whether the CGU government audit program - which entails both actual audits and the threat of audits - affects health outcomes regardless of whether corruption is detected, publicized, prosecuted, or punished.

3. Data and Methodological Strategy

We conceptualize the CGU anti-corruption program as changing the relative price of corruption (i.e., it attractiveness) and the resulting provision of public goods through two mechanisms. One mechanism is that municipalities change this price due to the threat of being audited and corruption detected, which we call the "*deterrent effect.*" In this case, auditing should make more resources available and outcomes should improve in all municipalities at risk of an audit. A different mechanism is that officials only change their behavior if they are audited, which we call the "*auditing effect.*" We acknowledge that it is plausible that changes in resources available and outcomes only change when a municipality is audited and mismanagement is found. Detection makes it more likely that corrupt officials are removed from office or new policies and procedures are implemented that make corruption more difficult (Myers, 1983). But detected and remedied corruption are themselves endogenous to numerous characteristics of the municipality and legal system which are not necessarily directly addressed by the CGU auditing program. We therefore focus on the reduced-form analysis of the impact of the occurrence of a CGU audit, which is exogenous and directly amenable to policy change.

We use annual municipality data between 2000 and 2011 from DATASUS, an information system of the Brazilian Health Ministry; Table A1 describes our variables, and sources. Our four dependent variables are the infant mortality rate, the rate of undernourished children, the number of child care appointments per inhabitant and the percentage of children up to age 1 who are vaccinated.

3.1. Difference-in-Differences (auditing effect)

To assess whether an audit affects the health indicators only when municipalities are actually audited, we measure the auditing effect using difference-in-differences (DD) models. In our setting the first difference is between audited and unaudited municipalities among eligible municipalities with less than 500 000 inhabitants, while the control group comprises municipalities with more than 500 000 inhabitants not included in the CGU audit program.

Equation (1) describes the DD model, used when the treatment occurs in different dates (9 time periods of intervention) and more than one control group is used (Imbens and Wooldridge 2009).

$$y_{igt} = \delta_g + \rho p_{gt} + \boldsymbol{x}_{igt}\boldsymbol{\theta}_{igt} + \alpha_i + \lambda_t + v_{gt} + \mu_{igt} \qquad (1)$$

where y_{igt} is the health indicator municipality i, group g, and in year t; α_i is the municipal fixed effect; λ_t is a full set of year dummies; δ_g is a full set of group effects; p_{gt} is group/time period covariates (our policy variable of interest), a dummy program variable with value 1 after the municipality was audited, and 0 otherwise; $\boldsymbol{x}_{it}$ is a matrix of control variables capturing observed characteristics of the municipalities, v_{gt} is a time and group unobserved effect, and μ_{igt} is the individual-specific error.

To check the robustness of our results, we conduct a placebo test simulating a random program variable instead of the original CGU drawn program variable, p_{it}, for the statistically significant models.

3.2. Regression Discontinuity Design (deterrent effect)

To learn the deterrent effect, we follow Imbens and Lemieux (2008) and use a sharp regression discontinuity design (SRDD) having as the threshold the municipal population of 500,000 on the selected health indicators. In this case the set of eligible municipalities, with populations less than the cutoff, is the treated group, and more populated municipalities, not eligible for the CGU program, are the control group.

To estimate robust SRDD models we follow Cattaneo e Escanciano (2017) and Calonico et al. (2016). Equation (1) shows the SRDD model estimated by the cut-off. Where τ_{SRD} measures the average treatment effect.

$$\tau_{SRD} = \mathrm{E}[Y_{it}(1) - Y_{it}(0)|X_{it} = \bar{x}] = \lim_{x \downarrow c} E[Y_{it}|X_{it} = x] - \lim_{x \uparrow c} E[Y_{it}|X_{it} = x] \quad (2)$$

The local polynomial approach τ_{SRD} is estimated by Equation (3), where Y_i is the selected primary health indicators i in time t; T_i is 1 if the municipality has population lower than 500000 (is eligible to be audited) and 0 the contrary case; X_{it} is our assignment variable, the municipal population i at time t, c is the logarithm of 500000 since we employ the log of the municipal population in our estimates, and $\boldsymbol{Z}$ is a vector of covariates. All covariates are information available before the film release date (Table A1).

$$Y_i = \alpha + \tau_{SRD}.T_i + (X_i - c).\beta_1 + T_i \cdot (X_i - c) \cdot \gamma_i + \boldsymbol{Z'\theta} + \varepsilon_i \quad (3)$$

4. Results and Discussion

Figure 2 shows the evolution of the selected health indicators respectively between eligible and non-eligible municipalities and between audited and non-audited municipalities. These illustrations reveal a considerable improving trend in all four measures throughout the sample period, which includes three years before the CGU program, but a small impact of the program specifically in 2003. The figures also illustrate common trends of the selected health indicators among subgroups of municipalities. The trends are the very similar, although one can see a hint of improvement in right column of Figure 2 for infant mortality and numbers of children vaccinated between the audited and unaudited groups over time.

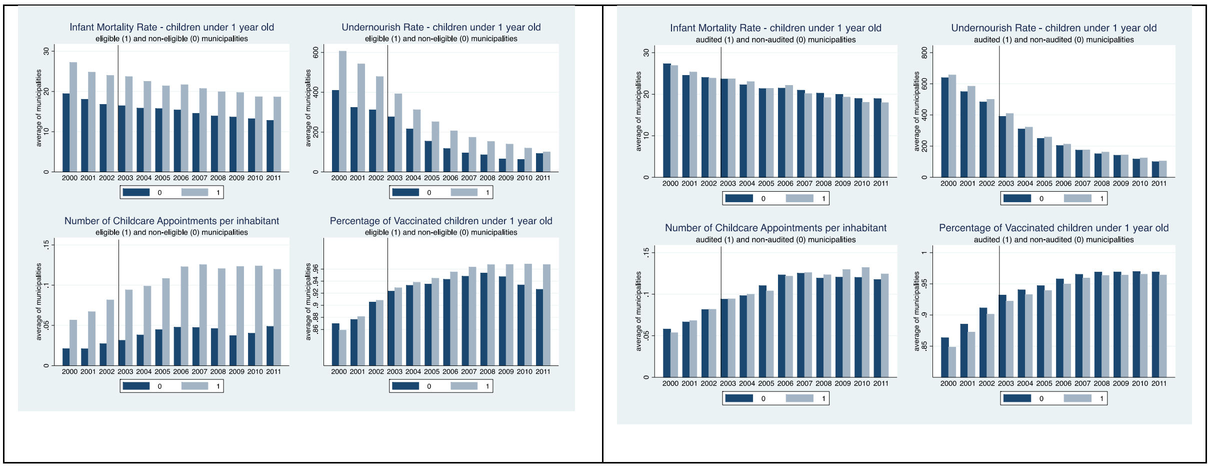

Figure 2 - The evolution of the selected health indicators between eligible and non-eligible and between audited and non-audited municipalities

4.1. Difference-in-Differences (auditing effect)

Table 1 shows DD estimates for the health indicators according to equation (1). Results suggest that the program has an auditing effect only on the infant mortality rate, and we find no significant CGU program effect on the remaining health indicators.

Table 4 – DD models

VARIABLES	Log. Infant Mortality Rate	Log. Rate of undernourished children under 1 year old	Log. Number of childcare appointments per inhabitant	Rate of Vaccinated children under 1 year old
p_g	-0.024**	-0.014	-0.001	-0.00002
	(0.012)	(0.020)	(0.002)	(0.002)
Observations	47,767	47,638	51,301	55,181
R-squared	0.045	0.473	0.116	0.332
Number of municipalities	5,360	5,225	5,245	5,277

*** p<0.01, ** p<0.05, * p<0.1, Robust standard errors in parentheses. See covariates at Table A.1

As we find a 5% significant effect of the CGU program on the infant mortality rate, we conducted a sensibility analysis for this health indicator. We randomly drew 1000 samples of "selected municipality" that mimic the real CGU lottery and estimate difference-in-differences regressions replicating equation (4.5) 1000 times. We replicate it replacing the policy variable, p_{it}, by the program placebo that was randomly chosen. We also replace the treated group, and the non-treated but eligible group, according to each random draw conducted. This sensitivity analysis (results not shown here) suggests no effects under the "placebo policies".

To evaluate the duration of the auditing effect on the infant mortality rate through the time we estimate equation (1) replacing the policy variable, p_{tg}, with dummies for different years of elapsed time since the year of the audit. The results show significant effects only at the year of the auditing, Table 2.

Table 2 – DD models – Duration of Effect

VARIABLES	year of auditing Log. Infant mortality rate	1 year after Log. Infant mortality rate	2 years after Log. Infant mortality rate	3 years after Log. Infant mortality rate	4 years after Log. Infant mortality rate	5 years after Log. Infant mortality rate
Modified p_{tq}	-0.0337**	-4.56e-05	-0.00599	0.0162	0.00603	-0.0140
	(0.0154)	(0.0155)	(0.0155)	(0.0170)	(0.0174)	(0.0191)
Observations	48,217	48,217	48,217	48,217	48,217	48,217
R-squared	0.046	0.046	0.046	0.046	0.046	0.046
Number of municipalities	5,391	5,391	5,391	5,391	5,391	5,391

*** p<0.01, ** p<0.05, * p<0.1, Robust standard errors in parentheses. See covariates at Table A.1

4.2. Regression Discontinuity Design (deterrent effect)

Figure 3 shows the population histogram and its adjusted kernel density for all years and municipalities as well the CGU program's cutoff point. The municipal population variable is continuous at the cutoff for investigation and the majority of municipalities are eligible to be audited; as we already mentioned, 99% of the smallest Brazilian municipalities in population are eligible to be audited by the CGU program.

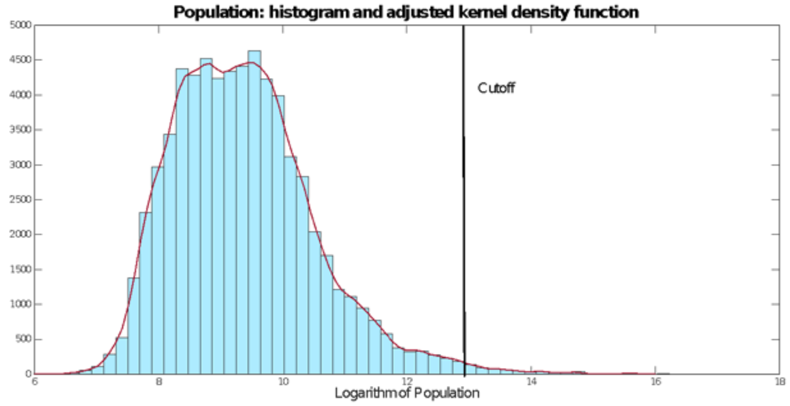

Figure 3 – Municipal Population histogram and kernel density

Figures 4 shows the scatterplots of the health indicators for the years 2003-2011 against the municipal population and its cutoff region. We also include a fourth-degree polynomial-adjusted curve for each side of the cutoff. In the left side of each figure, green dots are the health indicator values of non-audited municipalities and black dots represent these values for the audited municipalities. The polynomial curves on both sides of the cutoff fail to detect meaningful discontinuities in the childcare appointments and the vaccinated children indicators.

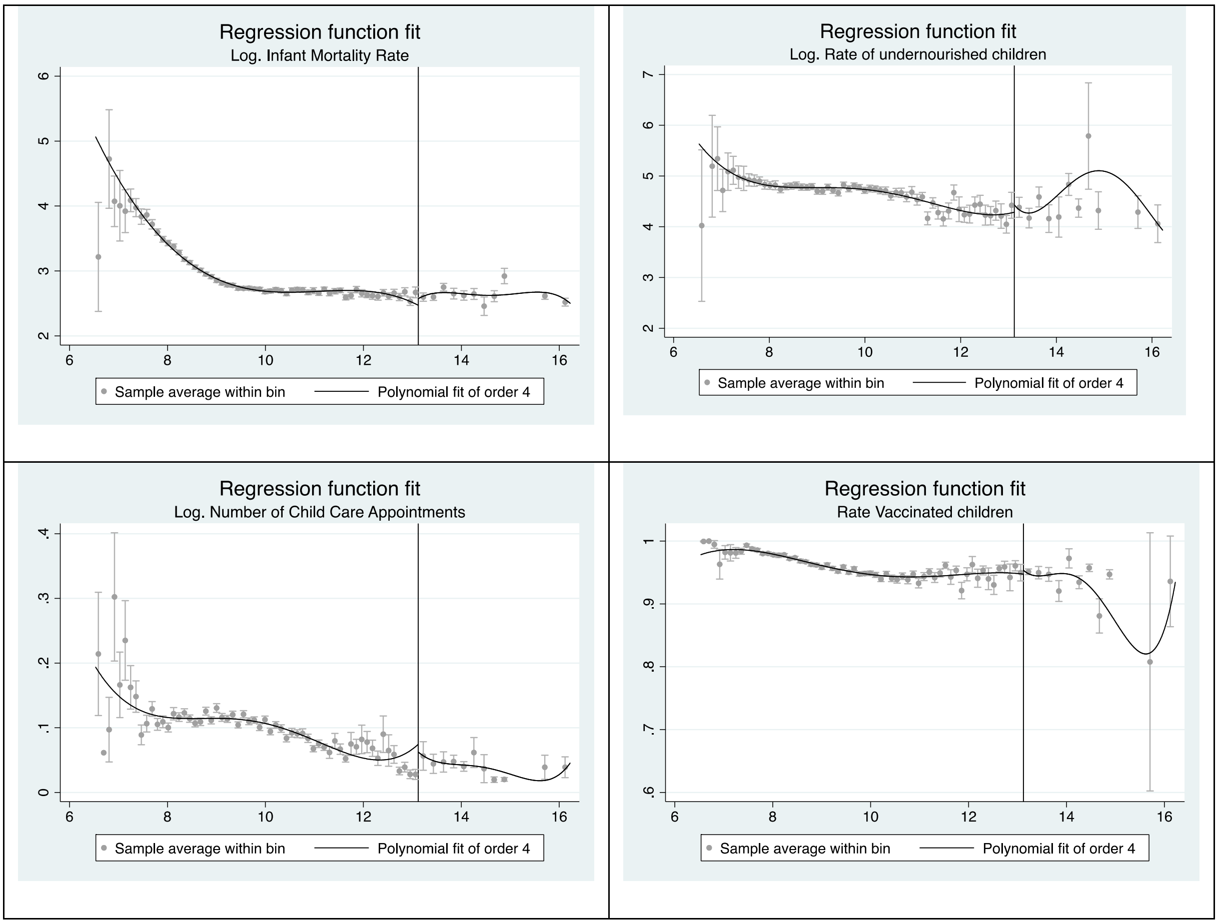

Figure 4 – Primary health care indicators and municipal population at cutoff

Table 3 provides robust estimates of the deterrent effect for different subsets of years and-cutoff using the triangular kernel. To check whether there was any transitory effect in 2003 when the program was implemented, we estimate SRDD including and excluding 2003 when the initial cutoff was 300 000 instead of 500 000, but fail to find any meaningful change. We also estimate the SRDD for 2003 using 300 thousand as the threshold, in this case, we find a significant effect for child vaccinations just for 2003, suggestive of a small, short-run effect on this outcome.

Table 3 – Results in SRDD on cut-off of eligible municipalities – Robust Estimates

	Log. Infant Mortality Rate	Log. Rate of Undernourished children, age <1	Log. Number of Child Care Appointments per Inhabitant	Rate Vaccinated children, age <1
Estimates 2003-2011				
Robust	0.0248	0.0633	0.0454	-0.0082
	(0.075)	(0.280)	(0.033)	(0.011)
Observations	35,266	35,754	41,002	42,502
Estimates 2004-2011				
Robust	0.0722	0.0340	0.0557	-0.0044
	(0.073)	(0.298)	(0.035)	(0.010)
Observations	30,923	31,365	36,639	37,712
Estimates 2003 using cutoff of 300 thousand				
Robust	0.1198	0.2505	-0.0084	0.0846***
	(0.075)	(0.311)	(0.011)	(0.023)
Observations	4,343	4,389	4,363	4,790

*** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. All covariates included and year dummies included. See Table A2 for estimates with no controls.

In summary, we just find a small auditing effect for infant mortality rate and transitory deterrent effect on the rate of vaccinated children in 2003. No effect was found for any indicators in the long-run. Yet, since the robust estimates using SRDD can also be viewed as tests for auditing effect as the threshold splits eligible municipalities that could be audited or not, we can generalize the robust estimates for both auditing and deterrent effects. This is not inconsistent with the existing literature on transparency, and in particular Freedom of Information (FOI) legislation, which finds mixed evidence on their effectiveness (Escaleras, Lin and Register 2010).

Our results suggest that Brazil's well-designed anticorruption program did meaningfully improve outcomes, and are consistent with Banerjee, Mullainathan, and Hanna (2012) who argue that anti-corruption programs are intrinsically ineffective. Further validation of our findings are the huge corruption schemes disclosed in subsequent years, notably including the "Car Wash" Inspection of 2014 that found involvement of prominent politicians, civil servants, and businessmen in the country (Rotberg (2019).

5. Conclusion

Brazil's anti-corruption program, which used strict randomization and emphasized speedy publication of findings, provides a unique opportunity to study the effect of a well designed anti-corruption program on four important health indicators. Previous studies have documented that the program detected, prosecuted and punished corruption (e.g., Finan and Ferraz, 2011), and found that municipalities with corruption had worse health outcomes (Lichand et al. (2015). Our study, however, examines the policy-relevant question of whether the CGU anti-corruption program itself actually improved any of four health indicators, which is not necessarily implied by either of the previous sets of studies.

Despite having significant statistical power, we do not find clear evidence that the randomized audit program had any meaningful effect on our four health outcomes. Although we do find evidence of a tiny one year statistically significant effect of audits on infant mortality and of a one year deterrent effect for rate of vaccine children, the magnitudes are small, and could be due to chance.

Our mostly null long-run findings are robust to a number of alternative specifications. Moreover, we find that the beneficial effects of an audit are short-lived, with no evidence that benefits continue after the year of the random audit. Robust regression discontinuity results of the deterrent effect - as well as of the audit effect, since the audited municipalities are a subsample of the eligible municipalities – point to the same direction.

Because the Brazil's health system decentralization is typical of developing countries, where there is a gap between financial and management decentralization, understanding the value of attempts to curb corruption and improve the quality of health outputs is potentially important for other countries. Because the CGU program is very expensive, our poor results for the health sector indicate that other approaches will be necessary to improve results.

Our results do not help us understand whether the poor performance improving health outcomes comes from problems with the implementation of the anti-corruption program or from other institutional problems. One example of an implementation problem is the way the reports of corruption are disclosed. Dissemination of results is done via a 200-page document that describes the irregularities found in the audited municipality, according to the type of health program, without a common methodological pattern. It could be that more standardized and simpler reporting methods would have a greater impact. In addition, the number of municipalities audited by year decreased significantly in the end of our sample, reducing the relative cost of corruption.

Institutional issues may also underlie the poor performance of the anti-corruption auditing program. Others have pointed to the failure of the Brazilian justice system to punish crimes efficiently because of its very slow pace. The instability of Brazilian public institutions is another challenge. Improving the justice system and public administration will take more effort than financial audits.

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Appendix A.1

Table A-1 – Variables description and sources

Variable description and Source	Obs	Mean	Std. Dev	Min	Max
Health indicators					
Logarithm of infant mortality rate under 1 year per 10,000 children (DATASUS)	50794	2.907	0.595	0.243	6.908
Logarithm of undernourished children under 1 year per 10,000 children (DATASUS)	53741	5.106	1.189	0.137	7.597
Logarithm of the number of appointment of children health care per inhabitant (DATASUS)	57531	0.100	0.106	2.0E-06	0.989
Rate of vaccinated children under 1 year (DATASUS)	62151	0.939	0.089	0.013	1.000
Policy variables					
Dummy for year of audit (CGU)	67104	0.028	0.164	0.000	1.000
Dummy =1 for each year after an audit (CGU)	67104	0.154	0.361	0.000	1.000
Forcing variable					
Logarithm of population - DATASUS	66695	9.376	1.146	6.534	16.223
Control variables					
Logarithm of GDP (in thousand of Reais) per capita (prices of 2000) – IBGE	66695	1.628	0.596	0.495	5.297
Logarithm of municipal health expenditure per inhabitant (prices of 2000) – DATASUS	65796	4.834	0.536	0.087	7.109
Logarithm of municipal health expenditure per inhabitant (prices of 2000) financed by own municipalities resources (DATASUS)	65793	4.742	0.836	0.000	7.679
Logarithm of school public expenditure per inhabitant (prices of 2000) (IPEA)	63039	5.230	0.481	0.017	10.80
Percentage of population covered by PSF/ESF (DATASUS)	65432	65.30	40.052	0.000	100.0
Logarithm of number of bed per 10 thousand inhabitant (DATASUS)	66736	2.109	1.569	0.000	6.282
Percentage of population covered by health insurance (ANS)	64758	0.061	0.116	4.0E-05	8.595
Percentage of central transfers (from SUS) out of total health expenditure of municipality (DATASUS)	66756	37.99	18.451	0.000	100.0
Percentage of enrolled students at elementary school older than the expected age for each grade (INEP)	66531	30.18	17.488	0.000	88.80
Percentage of enrolled students at high school order than the expected age for each grade (INEP)	66432	43.27	23.408	0.000	100.0

ANS – National Regulatory Agency for Private Health Insurance and Plans. www.ans.gov.br
CGU – Corregedoria Geral da União’s (CGU) anti-corruption program. http://www.cgu.gov.br
DATASUS – Health Informatics Department of the Brazilian Ministry of Health. www.datasus.gov.br
IBGE – Brazilian Institute of Geography and Statistics. www.ibge.gov.br
INEP – National Institute for Educational Studies and Research "Anísio Teixeira". www.inep.gov.br
IPEA – Brazilian Institute of Applied Economic Research. www.ipeadata.gov.br

Table A2 – Results in SRDD on cut-off of eligible municipalities – Robust Estimates

	Infant Mortality Rate	Rate of Undernourished children, age <1	Number of Child Care Appointments per Inhabitant	Percentage of Vaccinated children, age <1
Estimates 2003-2011				
Robust	-0.0780	-0.1196	0.0409	-0.0120
	(0.129)	(0.413)	(0.035)	(0.011)
Observations	40,096	40,297	45,966	47,851
Estimates 2004-2011				
Robust	-0.0437	-0.1207	0.0457	-0.0075
	(0.136)	(0.418)	(0.037)	(0.008)
Observations	35,440	35,551	41,258	42,681
Estimates 2003 using cutoff of 300 thousand				
Robust	0.0037	-0.1761	-0.0280*	0.0674***
	(0.097)	(0.314)	(0.016)	(0.022)
Observations	4,656	4,746	4,708	5,170

*** p<0.01, ** p<0.05, * p<0.1; Robust standard errors in parentheses. No covariates included.