Monitoring and Delivering: Evidence from Dropout Rates in Brazil*

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December 15, 2023

Abstract

This paper provides evidence that randomized audits in Brazil led to an improvement in some outcomes for public school students, but were relatively ineffective in improving the provision of other public services. I find causal evidence that the audits led to a decrease in the dropout rates in municipal elementary and middle schools by 0.37 percentage points, which corresponds to a ten percent decrease. A back-of-the-envelope calculation suggests that one fewer student dropped out, per school, per two years, as a result of these municipal audits. This amounts to approximately one third of the estimates of the decrease in dropout rates resulting from *Bolsa Família*, a large conditional cash transfer program in Brazil. I also find suggestive evidence that this effect is driven by the audits disciplining audited mayors. Additionally, I show that the effectiveness of the audits in decreasing dropout rates is dependent on a municipality's reliance on conditional cash transfers, and on the amount of irregularities uncovered during the audit. These results support the notion that monitoring can effectively realign the incentives of politicians and their constituencies.

*University of Pittsburgh, Department of Economics. Email: brunokomel@pitt.edu. I am grateful to my advisors, Yasir Khan and Gabriel Tourek for their invaluable guidance and assistance. I also thank the attendants of the Labor and Development as well as the Applied Microeconomics Brown-Bag at the University of Pittsburgh for their helpful comments, specifically Richard Van Weelden, Osea Giuntella, and Andy Ferrara. This research was supported in part by the University of Pittsburgh Center for Research Computing through the resources provided. Specifically, this work used the HTC cluster, which is supported by NIH award number S10OD028483.

I Introduction

While decentralized flexibility is a useful feature of federalism, monitoring local governments is an inherent challenge to this form of government. Given the necessity of principal-agent relationships in a decentralized context, central governments are left with the task of creating incentive structures and monitoring mechanisms to ensure that bureaucrats behave in accordance with the people's interests. Brazil's anti-corruption program is one well-known measure aimed at surveilling the use of funds by local governments. From 2003 until 2015[1], the *Controladoria Geral da União* (CGU), Brazil's federal monitoring branch, randomly selected municipalities and audited their use of funds. The stated purpose of these audits was to "Nourish more rigorous internal control practices among public administrators to ensure the appropriate use of public funds[2]" These random audits have been shown to be effective tools in punishing mayors (Ferraz and Finan, 2008), and in reducing corruption (Avis et al., 2018), but it is not clear whether they accomplished their goal of improving the use of public funds.

This paper studies the downstream effects of the municipal audits in Brazil, specifically as it pertains to education. The underlying question is whether the audits led to improvements in the provision of public goods and services in audited municipalities relative to those that were not audited. I focus on education for a variety of reasons. First, education, and conditional cash transfers related to education, were two of the primary areas of focus of the audits; second, municipalities are required by the constitution to spend at least 25% of their budget on educational expenditures; third, mayors have influence over the selection of school administrators and employees; and lastly, the municipalities are responsible for supervising the registration and eligibility of its residents for conditional cash transfer programs. These reasons, along with extensive education data, allow me to conduct the analysis in this paper.

Leveraging the random timing of the audits, as well as the random selection of the municipalities, I show that dropout rates among primary school[3] students decrease by

[1]After 2015, the CGU continued auditing municipalities, but the practice became more directed and deterministic.

[2]Source: gov.br, 2003

[3]Throughout the paper, what I call primary school refers *Ensino Fundamental* in Brazil, and elementary

0.37 percentage points (ten percent of the average dropout rate) in audited municipalities relative to unaudited ones. This effect is concentrated in municipalities where higher levels of irregularities were unveiled, as well as municipalities with relatively high reliance on *Bolsa Família*, a conditional cash transfer program which incentivizes parents to keep their children enrolled in school. By comparison, Glewwe and Kassouf (2012) estimate that the *Bolsa Família* program led to a decrease in dropout rates in the range of 0.4-1.2 percentage points, thus my results suggest that the program's effectiveness would have been significantly improved if the conditional cash transfers to the citizens had been paired with additional monitoring of the municipal governments. I find some evidence which indicates that the reliance on *Bolsa Família* is relatively more important than the amount of irregularities found. Further, I provide suggestive evidence that the positive effects of the audits come from municipalities where a mayor is audited while still in their first term, seemingly indicating that re-election incentives may be the driving force behind the effectiveness of mayoral accountability, even in the presence of increased monitoring.

In the appendix, I show that the audits did not lead to commensurate improvements to other outcomes related to public goods and services. Specifically, I show that standardized test scores, school infrastructure, hospital procedures (inpatient and outpatient), and some municipal services are unaffected by the audits. These null results suggest that the effectiveness of the audits was contingent on four conditions: (1) the relationship between the outcomes and corruption, (2) the ability of the audits to detect deficient outcomes, (3) the dependence of the outcomes on governmental accountability, and (4) the municipal governments' ability to change these outcomes in the short term (less than four years). In other words, for the outcomes where any of the conditions are not met, we do not observe significant post-audit effects.

Holmstrom and Milgrom (1991) propose a model of a multidimensional setting where the level of difficulty involved in monitoring efforts varies by activity. This setting describes the sort of principal-agent relationship that is ubiquitous in Brazil. As a result of the complexities in these kinds of settings, we find conflicting empirical results. For instance, in Olken (2007) we see an example of additional accountability being effective at improving road-building in

and middle school and the U.S.

Indonesia. On the other hand, Dizon-Ross et al. (2017) offers an example where additional monitoring did not lead to the same kinds of improvements, this time in the context of distributing subsidized health products in Sub-Saharan Africa. Given the multidimensionality of the problem, the effects of the randomized audits in Brazil are unpredictable *ex-ante*.

In establishing a link between the audits and educational outcomes, this paper bridges a connection between several results in the literature. First, as seen in Ferraz and Finan (2008) the timing of release of the audit reports has significant implications for electoral outcomes in the presence of corruption findings. Further, as shown in Ferraz and Finan (2005), mayors with re-election incentives divert fewer funds than those who are term-constrained. These results elicit hypotheses about the audits' ability to select "good" mayors, and to incentivize second-term mayors to behave productively. Additionally, Avis et al. (2018) explored the fact that some municipalities were audited at least twice to show that the audits helped to decrease corruption, and that neighboring municipalities are also positively affected by audits when local media is present. The results found in Ferraz and Finan (2005) and Avis et al. (2018) are consistent with those found in this analysis, as it seems that audited municipalities experienced improvements in outcomes relative to unaudited ones, especially when mayors had re-election incentives.

A second branch of relevant studies focuses on outcomes more closely related to the goods and services and enjoyed by citizens. Ferraz et al. (2012), show that higher levels of corruption are strongly negatively correlated with the academic performance of primary school students. They find that increased corruption is closely related with worse test performance and higher dropout rates. Contrastingly, this paper shows that the audits reverse some of these worse outcomes for children in municipalities with higher levels of revealed corruption. In short, Ferraz et al. (2012) show that the corruption found in audits correlates with worse student outcomes, while I show that some of those outcomes improve after an audit, while others are unchanged. The sample in this paper also differs slightly from that of Ferraz et al. (2012), as I use audit data from lotteries 20-40, and they use data from lotteries 1-16. This difference is due to data availability; only lotteries 20-40 are made available by the CGU.

Other papers in the literature have studied different sets of outcomes, such as health and private sector outcomes. Health outcomes also seem to be impacted by corruption,

but as Lichand et al. (2016) shows, this relationship is not as simple, as it seems that while the anti-corruption program reduced financial malpractice (e.g., over-invoicing, and under-the-table payments), the audits also caused a worsening in health indicators, including total hospital beds. Using slightly different empirical strategies, I find that the audits had no effects on hospital procedures and number of beds.

Additionally, Colonnelli and Prem (2022), shows that the audits impact different firms in heterogeneous ways; for instance, they show that the audits had negative effects on politically connected firms, but led to growth in the number of firms in procurement dominant sectors. Similarly, Colonnelli et al. (2022) show that firms in audited municipalities grow larger post-audit, despite receiving fewer procurement contracts than their matched counterparts in unaudited municipalities.

This paper separates itself from the rest of the literature by considering not only the effects of the audits as a treatment for corruption, but also their connection with the political process via government programs and re-election incentives. The closest study to this paper is the contemporary working paper by Gonzales (2021), which focuses on the effects of the anti-corruption program on the hiring of public employees. Gonzales (2021) finds an increase in the number of public employees in post-audit municipalities, and focusing on educational outcomes, finds that the increased hires do not improve student outcomes. Specifically, Gonzales' paper does not find a significant effect on dropout rates. This paper, on the other hand, finds a significant and robust effect on dropout rates. This discrepancy seems to come from slightly different samples and from different research designs. I use data from 2007-2019 to allow for a four year post-period after the last audit, whereas the data in Gonzales (2021) is limited to 2007-2015, and my main specification omits audits from 2007-2008 to allow for a pre-period of two years[4]. Additionally, I use a stacked difference-in-differences strategy to address the potential issues related to staggered treatments, while Gonzales (2021) analysis of dropout rates uses an event-study strategy without explicitly addressing the staggered treatment. Importantly, this paper does not present a challenge to the validity of the results in Gonzales (2021), as that paper convincingly shows the impact of audits on employment dynamics of schools, but rather, this paper serves as supplementary evidence of how the

[4]My estimates are robust to this choice

audits impacted educational outcomes.

The remainder of the paper is organized as follows: Section II provides a background on the anti-corruption program and other facets of Brazil's institutional context. Section III describes the data used. Section IV elaborates on the conceptual framework at play, and Section V outlines the empirical strategy used. Section VI provides a discussion of the results, and is followed by Section VII, which elaborates on potential mechanisms. Section VIII concludes, and Sections IX and X consist of Appendix and Robustness tables.

II Institutional Context

As mentioned above, Brazil's institutions created an environment with two features that allow for the study of the relationship between the efficacy of public spending and corruption.

II.I Anti-Corruption Program

In 2003, the Brazilian president Luiz Inácio da Silva set in motion the largest official anti-corruption program in the nation's history. With the creation of the office of the General Comptroller of the Union (CGU, following the Portuguese acronym), the Brazilian federal government launched an organized effort to combat corruption by randomly selecting and auditing municipalities to ascertain the propriety of their use of federal funds. The program, which selected municipalities via lottery, began by drawing relatively few towns, only five were selected in the first round in 2003, but grew to select 60 municipalities per round between 2003 and 2015. The frequency of the lotteries, as well as which municipalities were eligible to be selected, varied significantly over time. There were more lotteries per year earlier in the program, and the number dwindled until the program was revamped in 2016, when the CGU began selecting municipalities according to a set of designed parameters. Eligibility rules also changed over time, as the number of eligible municipalities increased almost monotonically since the genesis of the program, starting with municipalities with fewer than 150,000 residents, and reaching municipalities with fewer than 500,000 by 2004. State capitals were always ineligible to be audited in the program. The result of these eligibility rules is that over 99% of Brazil's 5,570 municipalities were eligible to be audited. Between

2003 and 2015, 2,200 audits took place, investigating a total of 1,949 municipalities, some of which were audited multiple times (Avis et al., 2018). See Figure 1 for a depiction of the extensiveness of the audits, as well as the heterogeneity in audit results. Further, to lend credence to the randomness of the selection of the municipalities, see Table 1 for a balance test of some summary statistics related to municipal characteristics and the student outcomes used in this paper[5]. Table 2 provides a summary of the number of municipalities audited in each sector relevant to this paper.

While questions always exist about the seriousness with which a government investigates itself, all evidence seems to support the notion that the anti-corruption program by the CGU was a *bona fide* effort to halt corruption at the municipal level. Upon having its "number" drawn at the lottery, a municipality would be subject to an investigation of the expenditure of funds received from the federal government over the previous 3-4 years by a team 10-15 of well-remunerated auditors[6]. These auditors would spend 1-2 weeks on-site collecting data on municipal accounts, as well as physically investigating construction projects, schools, hospitals, and other establishments subject to the audit; the auditors also interviewed local residents to gather information about the provision of services by the municipal government. Importantly, the funds audited were limited to federal transfers to the municipalities. This issue is especially relevant in the context of schools, as it implies that the audits focused on municipal rather than state schools[7]. Additionally, the specific accounts and expenditures audited varied by lottery, such that municipalities were unable to perfectly predict which sectors would be audited.

After several months, the auditors would submit a comprehensive report to the CGU office at the nation's capital. In the reports, some of which reach 300 pages, the auditors

[5]All outcomes are winsorized at the 1^{st} and 99^{th} percentile.

[6]These auditors are hired through a competitive selection process, a ubiquitous practice for hiring public employees in Brazil.

[7]An important aspect of the Brazilian context is that the responsibility for educating citizens is shared between municipal and state governments in the following fashion: municipal schools are primarily responsible for primary school education (7-14 year-olds, respectively), and the state schools are primarily responsible for secondary and high-school education. This means that there are few state primary schools and even fewer municipal high-schools, but the state and municipal responsibilities overlap at the secondary school level.

provided a detailed account of their findings, including a list of all irregularities [8], amounts audited and estimates of the magnitude of each irregularity (when relevant), photographic evidence, and responses from local government officials about the issues found. The CGU, then would compile all reports, publish them for public access on their website, and send them to the Federal Police (PF), the Federal Court of Accounts (TCU), the Public Federal Ministry (MPF), as well as the relevant local judiciary and legislative branches. While the consequences for irregularities varied greatly, some were severe, including impeachment and prosecution.

II.II Pro-Education Programs

In addition to the anti-corruption measures discussed above, Brazil has a long history of programs which incentivize students to attend, and stay in, school. One of the earliest of these programs started in the 1940s, and focuses on school lunches: The *Programa Nacional de Alimentação Escolar* (PNAE), which translates to National Program of School Meals, has undergone many changes over the years, with[9], but it took its current name and form in 2009. PNAE aims to educate all public school students on proper dietary and nutritional habits, and to offer them meals for the duration of the school-year. Similarly, in 2004, the Ministry of Education instituted the National Program of Support for School Transport (*Programa Nacional de Apoio do Transporte do Escolar* , PNATE) which aims to provide school transportation to public school students in rural areas. The program was structured such that the federal government would transfer funds to the states and municipalities to provide transportation to the students in its regions, and to maintain any infrastructure necessary to make the transportation of students possible, roads notwithstanding. In its inaugural year, the program served around 3.2 million students, growing to approximately 4.7 million at its peak in 2020. Lastly, and most importantly, in 2003, Brazil's president launched the program *Bolsa Família*, which is the country's largest social welfare program. *Bolsa Família* is a conditional cash transfer program, which requires, among other things, that the

[8]Starting in 2006, the CGU started tracking of the severity of irregularities.

[9]For instance, starting in 2006 participating schools were required to have an accompanying nutritionist to help implement the program

recipient's children be enrolled in school and maintain regular attendance. These programs created incentives for children to stay in school, and were all directly audited as part of the randomized audits. This allows for an analysis of the extent to which irregularities in these programs were correlated with outcomes of interest, as well as the effect that revealing these irregularities had on the same outcomes.

III Data

The data for this project comes from an array of sources, causing the periods of analysis to vary by outcome variable and mechanism.

III.I Audits

The data on audit reports comes from the CGU, and it encompasses every audit from the 20^{th} through the 40^{th} lottery, which translates to every audit from 2006 until the end of 2015[10]. I omit audits between 2003 and 2005 because it was in 2006 that the CGU began digitizing the reports and categorizing each infraction according to the account that was audited (e.g., education, health, etc.), as well as according to the severity of the violation.[11] The data also includes resources audited but for which no irregularity was found. In line with Avis et al. (2018), and Brollo et al. (2013), I will refer to these irregularities as broad evidence of corruption, with the acknowledgement that it is difficult to parse exactly which infractions stemmed from rent-seeking behavior by bureaucrats (Banerjee et al., 2012), and which come from mismanagement, incompetence, or malpractice.

III.II Schools

The primary source of school data used in this paper comes from INEP, which provides school-level abandonment and failure rates starting in 2007. Additionally, I obtained the

[10]Starting in 2016, the CGU began selecting municipalities in a directed, and non-random, fashion.

[11]This assignment can be inconsistent, as the auditors' discretion was used in qualifying the severity of the infraction. What is observable in the data, however, is that medium-level irregularities are significantly more common than severe-level irregularities.

additional data on schools from the *Censo Escolar* (School Census), which is a yearly survey of schools and contains information on school conditions, infrastructure, number of school employees, etc. Importantly, the census categorizes schools according to their source of funding (e.g., municipal, state, federal, private, etc.), allowing for analysis at the school level. The harmonization and availability of data for the census improved dramatically after 2007, thus, I will restrict the analysis to the years of 2007-2019. I also make use of data from the national standardized exam, *Prova Brasil*, which takes place biannually, and since 2009 has tested all public school students in the 5th and 9th grades.

III.III Hospitals

Hospital infrastructure data is maintained by the *Cadastro Nacional de Estabelecimentos de Saúde* (CNES), which is a public system that tracks registration and miscellaneous information about all health establishments in the nation, irrespective of funding source (e.g., municipal, state, private, etc.). These data are available at the hospital level, and are also pre-processed by *Base dos Dados*, and are available from 2005-2015. Additionally, I use data on the amount and cost of inpatient and outpatient procedures from *DATASUS*.

III.IV Municipalities

Data on municipal public services, like electricity, sewage, water delivery, etc. come from the SIAB surveys, which took place until 2015. I use data from 2004-2015, which describes the number of families with access to the various services and forms of infrastructure at the municipal level.

III.V Elections

The election data used in this paper comes from the *Tribunal Superior Eleitoral* (TSE), the Brazilian electoral court, which publishes data on local elections and candidates. The data have been pre-processed by *Base dos Dados* (Dahis et al., 2022). Our electoral dataset covers all municipal elections from 2004-2016, consisting of four electoral terms.

III.VI Others

I also leverage the pre-processed IBGE population estimates for the period 2006-2015, as well as some basic demographic and geographic data about the municipalities from *Base dos Dados*.

IV Conceptual Framework

The first step in thinking about the significance of anti-corruption measures is establishing a negative link between corruption and desirable outcomes. Table 3 and Table A1 lend some support to the hypothesis that where corruption is found, worse outcomes are present. Those tables report the results of the simple fixed effects regression:

$$Y_{it} = \gamma C_{it} + \delta_s + \lambda_t + X_{it} + \epsilon_{it}$$

Where Y_{it} is the school-level, or municipal-level, outcome of interest, C_{it} is the measure of corruption reported, δ_s and λ_t are state and year fixed effects, and X_{it} is a vector of municipal characteristics [12]. Corruption is measured in line with other papers in the literature, and, in this context, effectively means number of audited items that were found to be irregular divided by number of accounts audited. The corruption measures are standardized for ease of understanding. For reference, see Figure 2 for a histogram of education irregularities found per service order. While these regressions fail to establish a causal link between corruption and worse public good provision, they do show that these variables are negatively correlated.

As we can see, in Table 3, higher levels of irregularities in education are predictive of worse student outcomes in municipal primary schools. Column 1 considers all education irregularities, showing that a one standard deviation increase in all education-related irregularities correlates with an increase in dropout rates of almost 1 percentage point (20% of the dependent variable mean). Columns 2 and 3 consider the effects of irregularities related to the school-bus program and the conditional cash transfer (CCT) program, respectively, and column 4 performs a horse-race between those irregularities and other irregularities related to school lunches. Note

[12] I include controls for log population, log GDP, log federal transfers (*Fundo de Participação Municipal*), Gini coeficient, and percentage of residents with a High School degree, and percentage urban.

that the conditional cash transfer irregularities are not included in the education irregularities, whereas both food and transport irregularities are. Secondly, notice that all estimates are positive, and their magnitudes are relatively consistent across columns (with the horse race being slightly noisier). Finally, note in Column 6 that the audits were unsuccessful at capturing issues in (unaudited) state schools; again, this is unsurprising since the focus of the audits were the schools funded by the municipalities, not the state. These results seem to point to the notion that whatever the issues audits captured had a foundation in the "real" world of student outcomes.

To reinforce this point, I approximately replicate the results from Ferraz et al. (2012) in Table A1, showing that standardized test scores among primary school students were lower in municipalities where higher levels of corruption were revealed. Due to data availability at the time of writing, the standardized test score data is aggregated at the municipal level.

Having established the existence of a negative relationship between corruption and the outcomes of interest, the natural next step is to consider whether measures which reduce corruption, also improve those outcomes. As Avis et al. (2018) show, the anti-corruption program was successful at more than just finding corruption, it also reduced it. Given these results, then audited municipalities should, *ceteris paribus*, see an improvement in their public services relative to those which are never audited. It should be acknowledged, however, that it is possible that the audits merely deter mayors from diverting federal funds, and instead substitute into other, subtler, forms of corruption, as suggested by Gonzales (2021). Additionally, it is possible that the audits pose such a threat to mayors, that out of fear of impeachment or indictment, mayors become paralyzed and fail to make changes to their towns.

In the spirit of Holmstrom and Milgrom (1991), we should expect heterogeneity in the effectiveness of the audits to depend on the ease of monitoring the various services. For instance, it is easier for auditors to inspect whether school buses are picking up children from school than it is to ensure that teachers are appropriately incentivized by school principals to educate their students. Further, given their nature as one-shot events, one would expect the effect of the audits to be limited to outcomes which meet certain conditions: the audits must be able to detect the outcome as deficient, the outcome must be distorted by rent-seeking,

the outcome must be sensitive to monitoring, and the outcome must be fixable within a mayoral term. This rationale guides the rest of the analysis, and informs the interpretation of the results we see in Section VI.

V Empirical Strategy: Stacked Difference-in-Differences

My empirical strategy relies on the random, and unanticipated, nature of the audits to identify the causal downstream effects of being audited[13]. As described in Section II, from 2003 until 2015, the number, timing, and frequency of random selections for audits was unknown to the municipalities, preventing any sort of significant anticipation of treatment. Further, the randomness of selections combined with the balance in observable characteristics validates the assumption that, absent the audit, the treated and untreated municipalities would have continued along conditionally parallel paths.

To properly address the issues caused by the staggered treatment of the municipalities, as well as the difficulties triggered by the fact that a municipality becomes untreated after one electoral term, I borrow the strategy in Cengiz et al. (2019), whereby I create a distinct dataset (stack) for each "event." In this case, the event is represented by an audit, and for each year I extract all municipalities audited in that year. The procedure then consists of stacking these event-specific datasets to perform a difference-in-differences analysis via two-way fixed effects within each stack. Thus, for each audit year for which data is available, I create an event-specific dataset made up of: (1) municipalities audited in that given year, and (2) municipalities that are never audited. For instance, for 2009, the sample would be made up of municipalities that are (1) audited in 2009, and (2) never-audited. This strategy allows for the creation of a *clean* control group, which is unaffected by the complications of staggered treatments[14]. Then, using the stacked dataset, the regression equation is:

$$Y_{imth} = \beta A_{m\tau h} + \mu_{mh} + \lambda_{th} + \epsilon_{imth}$$

[13]Table 1 provides summary statistics and a balance test of the variables used in the main analysis.

[14]To ensure the existence of a pre-treatment period, in my main specification I include only municipalities audited after 2009. This is because data on the desired education outcomes is available starting in 2007. The results are robust to the inclusion of the stacks for earlier audited municipalities.

Where Y_{imth} represents the relevant school-level outcomes for municipality m, in year t, subject to event h, μ_{mh} are municipality-stack fixed effects, λ_{th} are year-stack fixed effects, and $A_{m\tau h}$ is an indicator which equals 1 for the four years following the audit year and the audit year itself.[15] The use of municipality-stack and year-stack fixed effects ensures that a treated observation is only compared to its appropriate clean control group. The identifying assumption for this strategy is that given the random, unanticipated, nature of the audits, the difference-in-differences coefficient on the treatment variable identifies the causal effect of the audits. Thus, the causality of the results below is supported by the fact that this a widely accepted assumption in the literature, which is also in line with the balance tests discussed above.

VI Results

As seen in Section IV, higher levels of corruption correlate with worse school outcomes. In this section, I examine whether the randomized audits effectively resolved the frictions caused by corruption and mismanagement.

VI.I Dropout and Failure Rates

The results of the main specification (Table 4) seem to clearly indicate that students in municipal primary schools in audited municipalities experienced a decrease in dropout rates relative to the control group.[16] The result, a 0.037 decrease in dropout rates, may seem small in magnitude, but it is large in relative size, making up approximately 10 percent of the dependent variable mean. This means that in a country where several measures have been taken to incentivize children to go to school, fiscal accountability led to an additional

[15]These results are robust to the use of school-stack fixed effects as well as different choices of treatment horizons (3-6 years).

[16]Note that this effect is concentrated in municipal schools, whose funds were audited as part of the anti-corruption program, in contrast with the unaudited state schools. This can be seen in Table 5, where one can observe that state schools saw no changes in dropout rates after the audits. Additionally, the fact that the result is only visible for primary schools is due to the fact that most municipal schools focus on elementary and middle school education, with relatively few municipal high schools.

0.37 percentage point decrease in abandonment. This result is robust to the use of school fixed effects (Table R2).

To further contextualize the significance of these results, consider Glewwe and Kassouf (2012), which estimates that the impact of the *Bolsa Família* program on dropout rates was around a 0.4 percentage point decrease. They further estimate that the overall effect was around a 1.2 percentage point decrease, assuming that the program had no spillovers to students whose parents did not receive the conditional cash transfers. The effect of the audits then, although more localized to a few audited municipalities, is close to 30 percent of the overall effect of the entire *Bolsa Família* program.

Additionally, audited municipalities saw a similar decrease in failure rates. This decrease, although bigger in absolute size, is relatively smaller as a percentage of the dependent variable mean, totaling approximately 6.2 percent. While also important, failure rates are complicated due to the highly subjective nature of the decision to grant passing grades to students. Furthermore, there is no mechanism external to the school which would allow me to discern the source of the effect, in contrast with abandonment rates, where reliance on the aforementioned pro-education programs allows me to estimate which programs were more important to the results. As such, the remainder of the discussion will focus on the more objective decrease in dropout rates.

VI.II Schools, Hospitals, and Municipal Services

In addition to showing what the audits accomplished, it is important to highlight that which they did not. As mentioned above, given the nature of the audits and the dynamics of local governments, one would expect the effect of the audits to be concentrated in areas where accountability is important for the delivery of services. Additionally, since these audits represent single monitoring events, rather than a continuous type of accountability[17], we would expect the results to be limited to outcomes which can be improved in the short-run. These are the results we find when considering the appendix tables A2, A3, A4, and A5. In tables A2 and A3, we see that even in the education sector, which was frequently audited,

[17]Importantly, audit risk represents the continuous sort of accountability useful for deterrence, as shown in Zamboni and Litschig (2018).

the audits did not lead to changes in the performance of students in standardized tests, nor did they lead to improvements in school infrastructure or personnel. Similarly, as seen in Table A4, hospital and healthcare outcomes were largely unchanged. Although healthcare procedures could respond to accountability in the short-run, as did dropout rates, their magnitude is more likely driven by demand by healthcare, which is orthogonal to the audits; additionally, demand for health services is not contingent on conformance with government programs, as is the demand for education, which is spurred by CCTs. Lastly, considering the municipal services in Table A5, we see mild improvements in electricity and trash disposal[18], which can be addressed by relatively small efforts by the municipality (e.g., connecting houses to the main power line, or including more homes in their trash collection route), but we do not see any improvement in areas that would require bigger infrastructure investments like public water and public sewage access. These results further suggest that the effectiveness of accountability is limited to outcomes rectifiable in the short-run.

VI.III Robustness

I perform several robustness tests to ensure the validity of the results. First, I follow Callaway and Sant'Anna (2021) to estimate the coefficient on the treatment variable while properly accounting for the staggered nature of the treatment. As shown in Table R1, while the point estimates are smaller, they show the same signs and patterns seen in the Stacked DiD analysis, and considering the size of the standard errors, the point estimates are not statistically different. Additionally, I also perform the Stacked DiD analysis using school fixed effects for both municipal and state schools (Table R2 and Table R3). Note that, as with the Callaway and Sant'Anna estimators, the signs tell the same story as the regressions with municipality fixed effects. Further, note that the coefficient on failure rates is no longer significant, even though its magnitude is largely unchanged; note also that the magnitude of the treatment effect on dropout rates is the same as in the same as in Table 3. Lastly, to check for whether the results are driven by outliers, I run the same regression with school fixed effects, but I winsorize the data at the 60^{th} and 50^{th} percentiles to show that the significance

[18]"Open" trash corresponds to an illegal practice of garbage collection in Brazil, which consists of disposing trash in large open areas.

of the coefficients is not driven by a few outliers but rather by many impacted schools; in this case, the magnitudes decrease, as expected, but the coefficients are still significant.

VII Mechanisms

To properly understand the results above, I investigate three mechanisms that could potentially drive the estimates. I test: (1) whether the effects of the audits were driven by reliance on the programs which were audited (e.g., *Bolsa Família, PNATE*); (2) whether the effects of the audits were concentrated in municipalities where a greater quantity of irregularities was found; or (3) whether the effects of the audit were driven by disciplining the mayors in office at the time of the audit, rather than creating a lasting impact on the municipality.

VII.I Reliance on School Programs

One plausible mechanism for the decrease in dropout rates is that municipalities which rely heavily on the government programs designed to incentivize school attendance are the ones responsible for the effect. This possibility is in line with the idea that those programs would function more effectively in high-accountability environments. This is reasonable because the success of the *Bolsa Família* program, for instance, depends almost exclusively on the local governments' efforts in ensuring funds are distributed *only* to eligible citizens, but also in ensuring they are, in fact, distributed. To investigate this possibility, I control for a measure of reliance on either school-buses, or school-transportation, more generally. For each school, I use the School Census to calculate the percentage of students who use school-transportation in the year prior to the audit. Similarly, for *Bolsa Família*, I take the total value of CCT benefits paid to each municipality in the year prior to the audit, divide it by the municipality's population estimate in that year. For both analyses, I use an indicator variable which equals one if the school, or municipality, had higher values than the median in the two aforementioned categories.

As seen in Table 7, interacting these measures of reliance on the programs sheds some light on their relative importance. Notice first in column one that the DiD coefficient on

"Audited" is mostly unaffected, but that the coefficient on the interaction term is insignificant and small. On the other hand, the interaction coefficient on the per-capita CCT measure is both significant and relatively large, indicating that municipalities with a higher reliance on *Bolsa Família* than the median experienced a noticeable decrease in abandonment rates[19] . This indicates that the municipalities which benefit the most from the conditional cash transfer program were also those who benefited most from the downstream effects of the audits.[20] It is likely, but untestable in the data, that the effect is partly driven by a more strict enforcement of the CCT school attendance requirements after the audits, whereby the municipal authorities become aware of previously lax monitoring and address these issues upon being found in irregular standing. This suggests that the *Bolsa Família* program would have been significantly more effective if accompanied by increased monitoring.

VII.II Number of Irregularities Found

A different mechanism that may be relevant relates to the results of the audit reports themselves. Under the assumption that corruption, or the level of irregularities, in a municipality stays constant absent the audits, we can take advantage of the staggered treatment to compare early vs. late treated municipalities while controlling for the number of irregularities which were *eventually* revealed. This implicitly assumes that revealed corruption is akin to a symptom of an underlying "room for improvement," which is medicated upon audit. In other words, we can observe the heterogeneity in audit effects depending on the level of corruption found. These results can be found in Table 8, where we can see, in the first column, that the effect of the audits is concentrated in municipalities where higher levels

[19]Columns 3-5 of that table provide a robustness test using "microregion" fixed effects to avoid the collinearity problem caused by the municipal fixed effects on Column 3. Microregions are a set of administrative regional divisions in Brazil, which are slightly larger than municipalities. For reference, Brazil is divided across 26 states (and one federal district, Brasília), 5,570 municipalities, and approximately 558 microregions. Note that the analysis loses power with the use of microregion fixed effects, but the results are largely the same when we consider an idicator for whether a municipality is above the median level CCT per capita in the year prior to the audit.

[20]These results are robust to controlling for municipal GDP, suggesting that it is not only a matter of municipal poverty, but also of engagement with *Bolsa Família*.

of irregularities in education were revealed.

Adding the coefficients, we conclude that a municipality with one irregularity saw no change in the percentage of dropouts. Given that the median number of irregularities found in education was less than one, and the upper bound was four, one can conclude that the higher the underlying level of corruption in a municipality, the more it benefited from the additional monitoring. Furthermore, the coefficients on the interaction terms in columns 2 and 3, while noisy, paint a similar picture. A reasonable interpretation of these results is that the treatment worked well where underlying fiscal issues would eventually be found.

VII.III Mayoral Accountability and Incentives

The final, and perhaps most important, mechanism which I consider is the differential response by the mayors who were audited in relation to those who were not. This analysis requires a complete reframing of the problem to consider mayors as the unit of treatment rather than municipalities. Thus, in order to investigate this mechanism, I create a panel of mayors and an accompanying indicator variable which is equal to one if that specific mayor is audited, independent of the audit history of the municipality. Conversely, once an audited mayor leaves office, those observations are no longer considered treated, and thus are dropped from the stacked sample. Given this relatively different sample composition, I consider this to be suggestive evidence of the underlying mechanism rather than a proper causal estimate of the average treatment effect. Naturally, I replicate the mechanism analyses above (interactions with government program reliance and with number of irregularities found) using the mayor-treatment design. The results from these regressions can be found in Tables 9, 10 and 11.

This analysis suggests that the response by audited mayors is the primary dynamic in play in this context. This result is immediately clear from the fact that when one considers the mayor as the unit of treatment, the audit effect on dropout rates becomes larger (more negative), and maintains its significance. Moreover, the fact that the other regression results on the mechanisms are substantively unchanged, seems to suggest that the audit effects observed above are explained by the response of the mayor in office at the time of the audit.

Given the mayor's ultimate responsibility for the findings of the audits, it is important

to consider not only the audit-status of the municipality, but also the term-status of the mayor. Another important result in the literature, which is consistent with theory, is that re-election incentives serve as a powerful deterrent against corruption (Ferraz and Finan, 2005). As such, I disaggregate the analysis of the effect of the audits by whether the mayor in power was in their first or second-term[21]. These results can be found in Table 12, and are highly suggestive of the fact that mayors' response to the audits is largely driven by re-election incentives. As seen in Column 1, only municipalities where first-term mayors were audited – and irregularities were found – experienced a differential decrease in dropout rates. This is in line with the view that first-term mayors may try to take advantage of their extra years in office to ameliorate the issues found by the audits, whereas second-term mayors have no incentive to do so.

VII.IV Comparing Mechanisms

In light of these three significant mechanisms, we can compare their relative importance. To do this, consider Table 13, which presents the results of a specification interacting the treatment variable with the indicators for above median *Bolsa Família* reliance and for above median number of irregularities in education. As seen on column 1, the pattern that emerges suggests that reliance on CCTs is more important to the downstream effect of the audits than the number of irregularities found. One can see this from the fact that the interaction with the CCT reliance indicator is large and significant, unlike that of the irregularities' indicator. While the triple interaction term is negative – as expected – it is also imprecise and noticeably larger than the earlier coefficient; the magnitude suggests that the triple interaction is relevant, but less important than the sole reliance on *Bolsa Família*. Further, in column 2, we see that when we consider the mayor-treatment interactions, the same overall pattern appears, which points to the fact that, together, both the mayor-treatment and the reliance on the CCT program drive the results on dropout rates.

[21]Note that in Brazil, mayors are term-limited, and may only serve two consecutive terms.

VIII Conclusion

Brazil's anti-corruption program, distinguished by its large scale centralized random audits, appears to have had positive consequences not only by decreasing *measured* corruption, but also by improving more tangible outcomes. These effects are only present in outcomes that can be expected to improve due to increased monitoring, and are concentrated in municipalities where (1) a relatively large number of irregularities is found, (2) the citizens rely heavily on conditional cash transfer programs, and (3) the mayor has re-election incentives. Audited municipalities saw a significant decrease in dropout rates among public primary school students who attend institutions funded by the municipality. This effect constitutes an approximate 10% decrease in abandonment for the relevant students, which in a back-of-the-envelope calculation translates to approximately one less student dropping out per school every two years[22]. This decrease in dropout rates, given that it was caused by work done by teams of around ten people, is not trivial, and quite favorable from the perspective of reward-to-effort ratios. The results in this paper point to what the audits *could not* accomplish, as seen by the fact that school infrastructure, hospital services, and municipal services were largely unaffected, but they also show that under the right conditions, extra monitoring can be a powerful mechanism to realign the incentives of politicians and bureaucrats with those of the populace.

[22]The mean number of students in municipal primary schools in a given year in the sample is 196, multiplied by an average dropout rate of 3.1% implies that approximately 5.5 students dropped out per school per year, whereas the accounting for a lower average dropout rate (0.031 - 0.0037) yields an average of 4.9 students dropping out each year.

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Table 1: Summary Statistics

	Eligible	Audited	Difference	95% CI
Panel A: Municipal Characteristics				
Population	25,389.41 (49,712.49)	24,131.23 (46,883.84)	1,258	-393, 2,909
GDP	297,413.66 (912,958.84)	347,343.08 (1,259,497.48)	-49,929	-110,114, 10,256
Perc. of Pop. with Secondary Ed.	0.17 (0.09)	0.18 (0.09)	-0.01	-0.01, 0.00
Gini Coef.	0.52 (0.07)	0.51 (0.07)	0.01	-0.01, 0.02
Perc. Urban	0.58 (0.21)	0.59 (0.21)	-0.01	-0.03, 0.00
Panel B: Student Outcomes				
Abandonment Rate - Primary School	0.0341 (0.0683)	0.0297 (0.0635)	0.00	0.00, 0.00
Failure Rate - Primary School	0.1022 (0.1107)	0.0921 (0.1042)	0.01	-0.01, 0.03
Abandonment Rate - High School	0.0748 (0.0905)	0.0678 (0.0846)	0.01	-0.01, 0.03
Failure Rate - High School	0.0884 (0.0830)	0.0888 (0.0851)	0.00	0.00, 0.00

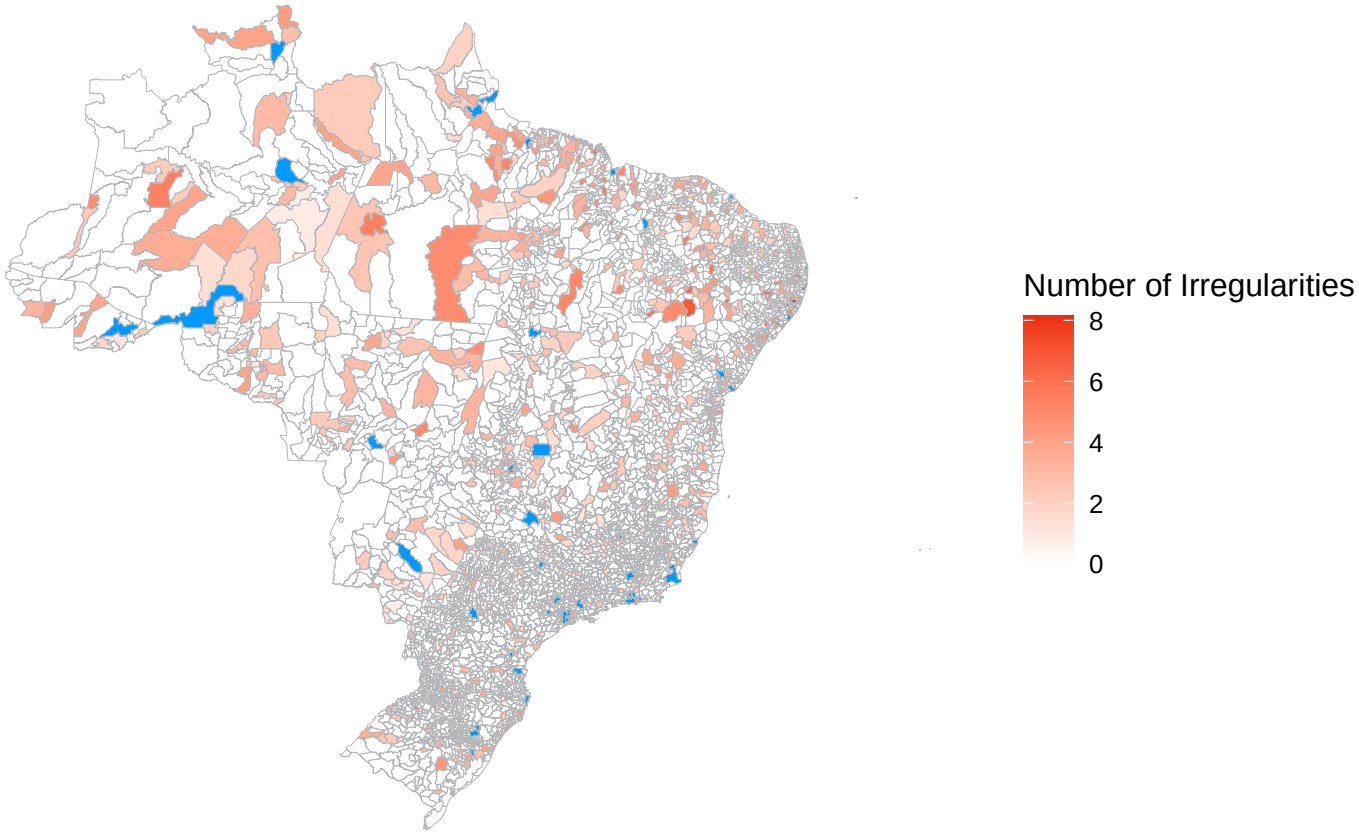
Note: This table shows the means and standard deviations of the municipal characteristics used in this paper, grouped by treatment status. Audited municipalities represent those municipalities which have been audited at any point in our sample, whereas eligible municipalities include those which were, at any point, eligible to be audited.

Table 2: Accounts Audited

Total Audits	Education	Health	Social Services	Bolsa Família	Sanitation
1132	1111	1098	1123	261	43
	(98.14%)	(96.99%)	(99.20%)	(23.10%)	(3.79%)

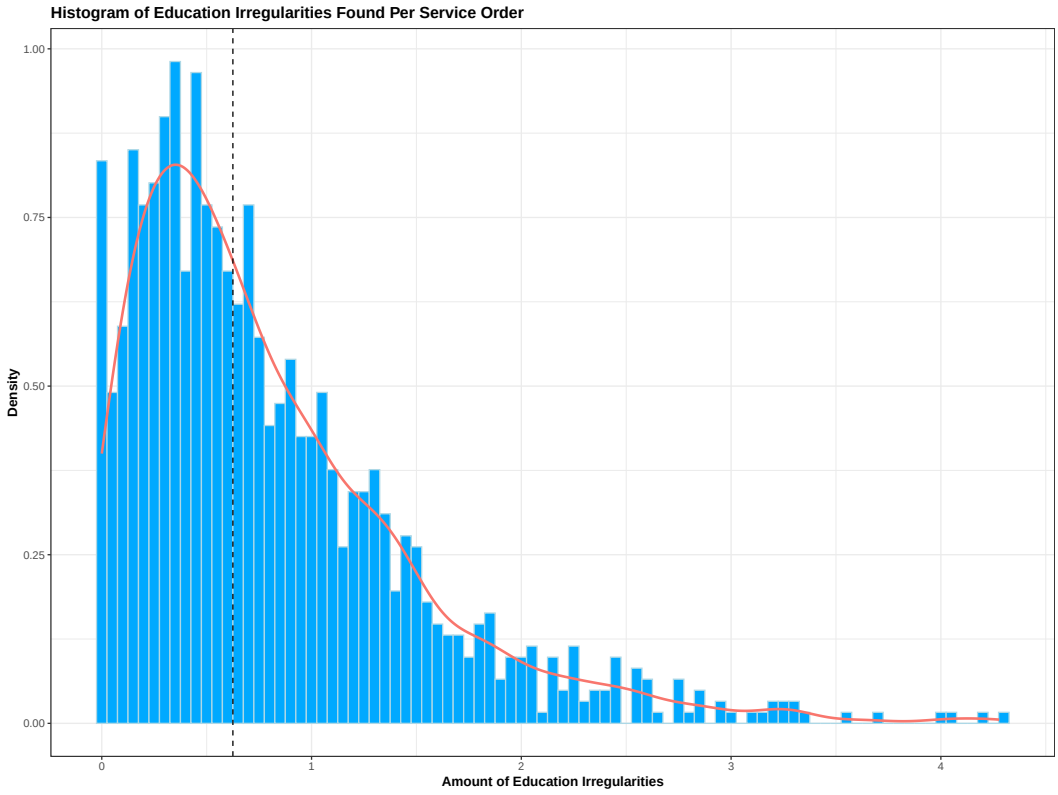
Note: This table shows a summary of which kinds of accounts were audited as part of the randomized audits from 2006-2015. Note that the unit of observation is a municipality-audit, and it includes only the first time the municipality was audited in the relevant period. Additionally, Bolsa Família is a subset of the social programs which were audited.

Figure 1: Map of Brazil - Audits



Note: This map depicts all Brazilian municipalities, and is colored according to their audit status in the sample. The audited municipalities are shaded in accordance with the number of irregularities revealed for each audit order. In other words, the color represents the number of irregularities found for each account audited, with darker red representing more irregularities. Municipalities in blue were not eligible to be audited due to being state capitals or due to exceeding the population limit (500,000 residents). Municipalities in white were not audited in the period between 2006 and 2015.

Figure 2: Histogram of Education Irregularities



Note: This histogram plots the distribution of irregularities found when education-related accounts were audited.

Table 3: Corruption Effects

	Dropout Rates - Municipal				Failure Rates - Municipal	Dropout Rates - State
	(1)	(2)	(3)	(4)	(5)	(6)
Std. N. Irreg. (Educ)	0.0090***				0.0024	0.0018
	(0.0027)				(0.0041)	(0.0019)
Std. N. Irreg. (Transport)		0.0074***		0.0067**		
		(0.0023)		(0.0026)		
Std. N. Irreg. (CCT)			0.0043*	0.0036		
			(0.0025)	(0.0023)		
Std. N. Irreg. (Food)				0.0009		
				(0.0023)		
Mean of DV	0.0473	0.0473	0.0473	0.0473	0.1284	0.0481
Num. obs.	22082	22082	22082	22082	22082	4139
N Clusters	976	976	976	976	976	820

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the coefficients on the standardized variable for the different irregularity types in a regression of the dependent variables listed on the column headers. Only municipal primary schools are included in the sample. All regressions use state and year fixed effects, as well as various municipal characteristics (log GDP, log population, percentage of residents with secondary education, percentage urban, and Gini coefficient). Standard errors are clustered at the municipality level. Dependent variable means are reported below the coefficients.

Table 4: Stacked DiD: Audit Effects

	Primary School		High School	
	Dropout Rate	Failure Rate	Dropout Rate	Failure Rate
Audited	−0.0037**	−0.0062*	−0.0065	−0.0179
	(0.0017)	(0.0033)	(0.0155)	(0.0193)
Mean of DV	0.0296	0.0935	0.0685	0.0875
Num. obs.	3295610	3295610	14115	14115
Num. groups: Municipality × Stack	25782	25782	2150	2150
Num. groups: Year × Stack	60	60	60	60

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Only municipal schools are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table 5: Stacked DiD: Audit Effects (State Schools)

	Primary School		High School	
	Dropout Rate	Failure Rate	Dropout Rate	Failure Rate
Audited	−0.0029	−0.0049	−0.0053	0.0018
	(0.0019)	(0.0038)	(0.0034)	(0.0029)
Mean of DV	0.0296	0.0935	0.0685	0.0875
Num. obs.	797070	797070	587240	587240
Num. groups: Municipality × Stack	23128	23128	25787	25787
Num. groups: Year × Stack	60	60	60	60

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Only state schools are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table 6: Stacked DiD: Audit Effects by Grades

	Audited Treatment			Mayor Audited Treatment		
			Primary School			Primary School
	1st-5th Grade	5th-9th Grade	1st-9th Grade	1st-5th Grade	5th-9th Grade	1st-9th Grade
Audited	−0.0038*	−0.0033	−0.0037**			
	(0.0021)	(0.0027)	(0.0017)			
Mayor Audited				−0.0046***	−0.0033	−0.0045***
				(0.0018)	(0.0020)	(0.0015)
Mean of DV	0.0250	0.0393	0.0296	0.0250	0.0393	0.0296
Num. obs.	3198454	1023389	3295610	3198454	1023389	3295610
Num. groups: School × Stack	454859	202315	463928	454859	202315	463928
Num. groups: Year × Stack	60	60	60	60	60	60

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (municipality audited in the last four years) disaggregated by the level of education of the students (listed on the column headers). The effects for the full sample (elementary and middle school students), which is used in the rest of the paper) is included for comparison. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications.

Table 7: Mechanism: Reliance on School Programs

	Munic. FE		Microregion FE	
	(1)	(2)	(3)	(4)
Audited	−0.0040**	0.0104***	−0.0053***	0.0049***
	(0.0020)	(0.0011)	(0.0018)	(0.0009)
Above Median Perc. Students in Shcool Bus	0.0065***		0.0051***	
	(0.0004)		(0.0004)	
Audited X Above Median Perc. Students in Shcool Bus	0.0011		0.0024	
	(0.0013)		(0.0016)	
Above Median Per Capita CCT Value				0.0023**
				(0.0009)
Audited X Above Median Per Capita CCT Value		−0.0169***		−0.0112***
		(0.0021)		(0.0019)
Mean of DV	0.0296	0.0296	0.0685	0.0875
Num. obs.	3295610	3295610	3295610	3295610
Num. groups: Municipality × Stack	25782	25782		
Num. groups: Year × Stack	60	60	60	60
Num. groups: Microregion × Stack			3758	3758

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with various covariates. Only municipal primary schools are included in the sample. Standard errors are clustered at the municipality level, and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table 8: Mechanism: Number of Irregularities

	Primary School Dropout Rates		
	Educ. Irreg.	School Transport Irreg.	CCT Irreg.
Audited	0.0081*	0.0037	0.0053
	(0.0046)	(0.0046)	(0.0047)
Audited × Qty. Educ. Irreg.	−0.0071***		
	(0.0025)		
Audited × Qty. School Transport Irreg.		−0.0121	
		(0.0162)	
Audited × Qty. CCT Irreg.			−0.0111
			(0.0083)
Mean of DV	0.0296	0.0296	0.0296
Num. obs.	78619	78619	78619
Num. groups: Municipality × Stack	581	581	581
Num. groups: Year × Stack	21	21	21

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with the number of irregularities found in different categories (Education, School Transportation, or *Bolsa Família)*. Only municipal primary schools are included in the sample. Additionally, the sample is restricted to audited municipalities, where the number of revealed irregularities is known. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table 9: Mechanism: Mayor Audit Effects

	Primary School		High School	
	Dropout Rate	Failure Rate	Dropout Rate	Failure Rate
Mayor Audited	−0.0045***	−0.0025	−0.0142	−0.0275**
	(0.0015)	(0.0028)	(0.0202)	(0.0125)
Mean of DV	0.0296	0.0935	0.0685	0.0875
Num. obs.	3295610	3295610	14115	14115
Num. groups: Municipality × Stack	25782	25782	2150	2150
Num. groups: Year × Stack	60	60	60	60

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (mayor was audited in the last four years). Only municipal schools are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table 10: Mechanism: Mayor Effects × Reliance on School Programs

	Municipality FE		Microregion FE	
	(1)	(2)	(3)	(4)
Mayor Audited	−0.0050***	0.0047***	−0.0064***	0.0031***
	(0.0018)	(0.0010)	(0.0018)	(0.0010)
Above Median Perc. Students in Shcool Bus	0.0065***		0.0051***	
	(0.0003)		(0.0004)	
Mayor Audited X Above Median Perc. Students in Shcool Bus	0.0013		0.0025	
	(0.0016)		(0.0017)	
Above Median Per Capita CCT Value				0.0022**
				(0.0009)
Mayor Audited X Above Median Per Capita CCT Value		−0.0113***		−0.0105***
		(0.0020)		(0.0019)
Mean of DV	0.0296	0.0296	0.0296	0.0296
Num. obs.	3295610	3295610	3295610	3295610
Num. groups: Municipality × Stack	25782	25782		
Num. groups: Year × Stack	60	60	60	60
Num. groups: Microregion × Stack			3758	3758

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (mayor audited in the last four years) interacted with various covariates. Only municipal primary schools are included in the sample. Standard errors are clustered at the municipality level, and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table 11: Mechanism: Mayor Effects × Number of Irregularities

	Primary School Dropout Rates		
	Educ. Irreg.	School Transport Irreg.	CCT Irreg.
Mayor Audited	0.0002	−0.0019	−0.0015
	(0.0035)	(0.0037)	(0.0037)
Mayor Audited × Qty. Educ. Irreg.	−0.0045**		
	(0.0022)		
Mayor Audited × Qty. School Transport Irreg.		−0.0130	
		(0.0132)	
Mayor Audited × Qty. CCT Irreg.			−0.0070
			(0.0063)
Mean of DV	0.0296	0.0296	0.0296
Num. obs.	55390	55390	55390
Num. groups: Municipality × Stack	362	362	362
Num. groups: Year × Stack	21	21	21

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (mayor audited in the last four years) interacted with the number of irregularities found in different categories (Education, School Transportation, or *Bolsa Família)*. Only municipal primary schools are included in the sample. Additionally, the sample is restricted to audited municipalities, where the number of revealed irregularities is known. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table 12: Mechanism: Mayor Effects by Term-Status

	Primary School Dropout Rates	
	First Term	Second Term
Mayor Audited	0.0077^{***}	-0.0029
	(0.0025)	(0.0031)
Mayor Audited × Qty. Educ. Irreg.	-0.0062^{**}	-0.0020
	(0.0024)	(0.0032)
Mean of DV	0.0296	0.0296
Num. obs.	55390	55390
Num. groups: Municipality × Stack	362	362
Num. groups: Year × Stack	21	21

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (mayor audited in the last four years) interacted with the number of irregularities found in different categories (Education, School Transportation, or *Bolsa Família)*. The column headers, list which category of mayor is considered in each column (e.g., the first column only includes first-term mayors). Only municipal primary schools are included in the sample. Additionally, the sample is restricted to audited municipalities, where the number of revealed irregularities is known. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications.

Table 13: Mechanism:

Bolsa Família vs. Irregularities

	Audited	Mayor Audited
	(1)	(2)
Audited	0.0107***	0.0051***
	(0.0013)	(0.0012)
Audited X Above Median Per Capita CCT Value	−0.0137***	−0.0076**
	(0.0033)	(0.0031)
Audited X Above Median Education Irregularities	−0.0009	−0.0011
	(0.0021)	(0.0018)
Audited X Above Median Per Capita CCT Value X		
Above Median Education Irregularities	−0.0038	−0.0045
	(0.0043)	(0.0039)
Mean of DV	0.0296	0.0296
Num. obs.	3295610	3295610
Num. groups: Municipality × Stack	25782	25782
Num. groups: Year × Stack	60	60

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (municipality audited in the last four years) interacted with two indicator variables, one for whether the municipality was found to have more irregularities in education than the median audited municipality, and another indicator for whether the municipality was one above the median in *Bolsa Família* benefits distributed per capita. Only municipal primary schools are included in the sample, i.e., the full sample from Table 3 is included. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications.

Table 14: Mechanism:
Amount of Irregularities

	(1)
Audited	0.0054
	(0.0043)
Audited × 1+ Irreg.	−0.0076
	(0.0049)
Audited × 2+ Irreg.	−0.0127**
	(0.0054)
Audited × 3+ Irreg.	−0.0129*
	(0.0066)
Mean of DV	0.0296
Num. obs.	78619
Num. groups: Municipality × Stack	581
Num. groups: Year × Stack	21

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (municipality audited in the last four years) interacted with the number of irregularities found in Education, separated by how many irregularities were found. The sample is restricted to audited municipalities, where the number of revealed irregularities is known. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications.

IX Appendix

Table A1: Corruption Effects: Standardized Tests

	Portuguese		Math	
	5th Grade	9th Grade	5th Grade	9th Grade
Std. N. Irreg. (Educ)	−0.0943***	−0.0652	−0.0976***	−0.0652
	(0.0317)	(0.0408)	(0.0341)	(0.0420)
Mean of DV	−0.0246	−0.0201	−0.0226	−0.0207
Num. obs.	513	398	513	398
N Clusters	497	386	497	386

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table approximately replicates Ferraz et al. (2012). This table shows the coefficients on the standardized variable for the different irregularity types in a regression of the dependent variables listed on the column headers (scores in math and in Portuguese). Scores are standardized using the sample mean and sample standard deviation from the untreated municipalities. Only municipal elementary and middle schools are included in the sample. This table uses data from the 2007, 2009, 2011, 2013, and 2015 iterations of the national standardized test. All regressions use state and year fixed effects, as well as various municipal characteristics (log GDP, log population, percentage of residents with secondary education, percentage urban, and Gini coefficient). Standard errors are clustered at the municipality level. Dependent variable means are reported below the coefficients.

Table A2: Stacked DiD: *Prova Brasil*

	Portuguese		Math	
	5th Grade	9th Grade	5th Grade	9th Grade
Audited	0.3293	−0.0760	0.5246	−0.1642
	(0.7930)	(0.7456)	(0.8791)	(0.6820)
Mean of DV	179.1539	233.4205	197.1953	240.4855
Num. obs.	427559	186503	427559	186503
Num. groups: School × Stack	161888	74199	161888	74199
Num. groups: Year × Stack	21	21	21	21

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Only municipal schools are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table A3: Stacked DiD: School Outcomes

	All Schools	Elementary and Middle Schools			High Schools		
	N. Classrooms	N. Enrolled	N. Teachers	N. Cohorts	N. Enrolled	N. Teachers	N. Cohorts
Audited	0.0001	0.0325	−0.0075	−0.0023	0.0150	0.0101	0.0110
	(0.0046)	(0.0239)	(0.0103)	(0.0075)	(0.0143)	(0.0149)	(0.0078)
Mean of DV	6.4528	33.8996	2.3417	1.9567	137.2134	8.2637	6.0549
Num. obs.	7211393	7220024	7210018	7220024	7220024	7210018	7220024
Num. groups: Municipality × Stack	58157	58157	58157	58157	58157	58157	58157
Num. groups: Year × Stack	83	83	83	83	83	83	83

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Only municipal schools are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table A4: Stacked DiD: Hospital Outcomes

	Inpatient		Outpatient		Infrastructure
	N. of Procedures	Value of Procedures	N. of Procedures	Value of Procedures	Number of Beds
Audited	0.0327	0.0242	0.0305	−0.0833	−0.0045
	(0.0533)	(0.0648)	(0.0473)	(0.1612)	(0.0048)
Mean of DV	1.9876	2.4040	8.6725	6.5487	0.1408
Num. obs.	342332	342332	258681	258681	7603836
Num. groups: Municipality × Stack	45883	45883	29612	29612	36914
Num. groups: Year × Stack	90	90	70	70	77

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Only municipal hospitals are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table A5: Stacked DiD: Public Services

	State-run			Municipality-run	
	% Public Water	% Public Sewage	% W/ Electricity	% Trash "Open"	% Trash Collected
Audited	0.0010	−0.0030	0.0078***	−0.0082***	0.0040
	(0.0023)	(0.0024)	(0.0022)	(0.0020)	(0.0025)
Mean of DV	0.6633	0.2981	0.9206	0.1314	0.6451
Mean N. of Families	4823	4823	4823	4823	4823
Num. obs.	238622	230074	239202	224472	239109
Num. groups: Municipality × Stack	36318	36068	36353	35129	36351
Num. groups: Year × Stack	69	69	69	69	69

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Outcomes are shown in column headers, and represent the percentage of families with access to the various services/amenities listed, with data from 2004-2015. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

X Robustness Tests

Table R1: Callaway and Sant'Anna (2021) Estimator

	Dropout Rate		Failure Rate	
	Primary School	High School	Primary School	High School
ATT (Audited)	−0.0016***	−0.0121	−0.0055***	0.0537
	(0.0006)	(0.0271)	(0.0012)	(0.0335)

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the Callaway and Sant'Anna (2021) coefficient estimator on the treatment variable (audited in the last four years) for each of the outcome variables listed on column headers. Only municipal schools are included in the sample. Standard errors are clustered at the school level, and school-level-stack and year-stack fixed effects are used in all specifications. The estimation procedure assumes parallel trends conditional on log GDP, log Population, and percentage of urban residents for each municipality; estimates are robust to these choices.

Table R2: Stacked DiD: School Fixed Effects

	Primary School		High School	
	Dropout Rate	Failure Rate	Dropout Rate	Failure Rate
Audited	−0.0037**	−0.0059*	−0.0047	−0.0207
	(0.0017)	(0.0035)	(0.0157)	(0.0192)
Mean of DV	0.0296	0.0935	0.0685	0.0875
Num. obs.	3295610	3295610	14115	14115
Num. groups: School × Stack	463928	463928	3818	3818
Num. groups: Year × Stack	60	60	60	60

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Only municipally funded schools are included in the sample. Outcome variables are logged and listed on the column headers. Standard errors are clustered at the municipality level, and school-level-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table R3: Stacked DiD: School Fixed Effects (State)

	Primary School		High School	
	Dropout Rate	Failure Rate	Dropout Rate	Failure Rate
Audited	−0.0022	−0.0047	−0.0053	0.0020
	(0.0019)	(0.0038)	(0.0036)	(0.0031)
Mean of DV	0.0296	0.0935	0.0685	0.0875
Num. obs.	797070	797070	587240	587240
Num. groups: School × Stack	107086	107086	76356	76356
Num. groups: Year × Stack	60	60	60	60

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years). Only state schools are included in the sample. Standard errors are clustered at the municipality level, and school-level-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.

Table R4: Percentile Sensitivity

	60th Percentile	50th Percentile
	(1)	(2)
Audited	−0.0002**	−0.0000**
	(0.0001)	(0.0000)
Mean of DV	0.0296	0.0296
Num. obs.	3969917	3969917
Num. groups: School × Stack	603151	603151
Num. groups: Year × Stack	71	71

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Note: This table shows the difference-in-differences coefficients on the treatment variable (audited in the last four years) for different levels of winsorization of the outcome variable (dropout rates). In all specifications, only the top of the distribution is winsorized at the percentile listed on the column header. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level, and municipal-stack and year-stack fixed effects are used in all specifications. Dependent variables are listed on the column headers.