

Monitoring and Delivering: Evidence from Dropout Rates in Brazil

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Abstract

This paper provides evidence that randomized audits of municipal funds in Brazil led to an improvement in outcomes for public school students, but were not as effective in improving the provision of all public services. I find causal evidence that the audits led dropout rates in municipal elementary and middle schools to decrease by ten percent (0.34 percentage points). A back-of-the-envelope calculation, ignoring spillovers, 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 designed, in part, to minimize student abandonment. Additionally, I show that the audits' effectiveness increases with municipalities' reliance on conditional cash transfers, and with the quantity of irregularities uncovered during the audit. I also find suggestive evidence that this effect is driven by a disciplining effect on audited mayors, especially in the presence of re-election incentives. These results support the notion that monitoring and accountability can effectively realign the incentives of politicians with the needs of their constituencies.

Keywords: Corruption; Audits; Education; Governance; Accountability

JEL Codes: D04 ; D72; I24; I25; I28; O01

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I Introduction

In the developing world, the tendency for politicians to engage in suboptimal – rent-seeking – behavior is as ubiquitous in casual dialogue as it is on the minds of central governments. As a result, many government entities engage in audits to monitor the behavior and performance of local officials. This paper seeks to answer whether audits serve a corrective role, in addition to its inherently diagnostic purposes. In other words, do audits lead to changes in the quality of the services provided by public officials? Moreover, this paper sets out to answer a broader question about the importance of specific characteristics of the ecosystem in which audits take place. The paper argues that certain activities, or programs, benefit from additional monitoring disproportionately more than others. Though the symptoms of the corruption disease are well understood, the conditions under which audits are effective remedies are still opaque.

In this paper, I investigate the effectiveness of government audits in improving outcomes enjoyed by the residents of an audited municipality. I do so in the context of Brazil’s anti-corruption program, which is a renowned central policy measure aimed at surveilling the use of funds by local governments. From 2003 until 2015¹, the *Controladoria Geral da União* (CGU), Brazil’s federal monitoring branch, randomly selected municipalities and audited their records. 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 for misconduct ([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.

I seek to make two contributions with this paper. First, I seek to show the audits’ effects on outcomes directly enjoyed by residents of a municipality, including some null effects. Secondly, I seek to quantify the degree to which the audits’ ability to impact the aforementioned outcomes depends on symbiotic relationships between government audits and other peripheral government programs. The underlying question the paper seeks to answer is whether the audits led to improvements in the efficacy of public expenditures. To this end, I compare outcomes in audited municipalities relative to those that were not audited. Additionally, I focus on education for a variety of reasons. First, education and social programs, many of which are related to education, were two of the primary areas of focus of the audits; as seen on [Table 1](#), both were audited in over 98% of municipalities. Second, the Brazilian constitution requires municipal governments to spend at least 25% of their yearly budget on educational expenditures. Third, mayors have power over the selection of school administrators

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

²Source: gov.br, 2003

and employees, and the municipalities are responsible for supervising the registration and eligibility of its residents to conditional cash transfer programs. These reasons, along with the availability of extensive education data, motivate 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 elementary and middle school³ students decrease by 0.34 percentage points (ten percent of the average dropout rate in those schools) in audited municipalities. Additionally, we see a decrease in failure rates among elementary and middle school students, and similar patterns for high school students, although the latter are not significant at traditional levels. Moreover, while the audits focused on the use of municipal funds, the results seem to show a spillover effect to state schools as well, as dropout rates also significantly decreased among state-administered schools.

I investigate three potential mechanisms that could drive these effects on education outcomes. The first of these relates to the existing literature about the audits, and it examines the effect that comes from disciplining mayors (Avis et al., 2018, Ferraz and Finan, 2008). The results suggest that the treatment effect on dropout rates is entirely explained by improvements that take place while the audited mayor is still in office; the effects on other, non-education, outcomes tell a similar story. This result is in line with the theoretical framework and empirical results found in the existing literature. Further, I provide some suggestive evidence that the mayoral disciplining effect is especially important for first-term mayors who are eligible for re-election (Ferraz and Finan, 2005).

Secondly, I analyze the relationship between the audit effects and each municipality's reliance on various social programs. As outlined below, the only program which seems to be a relevant mechanism for the audit effects is *Bolsa Família*, a conditional cash transfer program which incentivizes parents to keep their children enrolled in school. The program is in many ways a contemporary of the CGU audits, as they were rolled out almost concurrently and were both prized policies by the sitting president at the time. The *Bolsa Família* program is regarded as a success, and it is estimated to have contributed to a decrease in dropout rates in the range of 0.4-1.2 percentage points (Glewwe and Kassouf, 2012). I estimate that audited municipalities that receive more per capita *Bolsa Família* transfers than the median municipality in the sample experienced approximately 1.6 percentage point decreases in dropout rates after the audits. 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

³Throughout the paper, what I call elementary and middle schools refers *Ensino Infantil* and *Ensino Fundamental* in Brazil, and elementary and middle school and the U.S.

the reliance on *Bolsa Família* is relatively more important than the amount of irregularities found. I also consider the possibility that other school programs (e.g., school transportation) are relevant mechanisms, as well as the possibility that *Bolsa Família* benefits are a proxy for poorer municipalities, but the data support neither hypothesis.

The third mechanism that I investigate relies on the heterogeneity in irregularities found in the audit reports. In this analysis, I find that the municipalities where the most irregularities were found benefited most from the audits; each additional irregularity (per service order) found corresponded to a decrease in dropout rates of 0.69 percentage points. In other words, the audit effects are a function of irregularities found, whereby the treatment effects are concentrated in those municipalities that stood to benefit the most from more accountability. This is an important result, as it suggests that even in cases when the average audit effect for the full population of municipalities is small or zero, the audits may lead to significant improvements in towns where high levels of corruption can be found.

The appendix shows 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, and some municipal services are unaffected by the audits. Importantly, some services experienced marginal improvements. These 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 relationship between the outcomes and 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 is not met, we do not observe significant post-audit effects.

This paper relates to research on principal-agent dynamics, especially as it pertains to incentives. Inherent to this analysis is the heterogeneity in the difficulty of observing and measuring mayoral performance and corruption. Mayors have a myriad of responsibilities, which correspond to varying mappings between effort and results. For instance, enforcing internal controls is more likely to reduce instances of noncompliance with the requirements in conditional cash transfers than it is to accelerate, or improve, the completion of sewage projects. Theoretically, the optimal matching of incentives to activities should reflect this disparity by assigning variable incentives to tasks that are easily measured and observable, and fixed incentives to ensure that the other kinds of tasks are completed ([Holmstrom and Milgrom, 1991](#)). The same reasoning applies to efforts to discipline agents for malpractice in different areas, which vary in the observability and measurability dimensions. The underlying theoretical framework leads to important conclusions about the efficacy of audits in

reducing government wrongdoing in a setting where audits are used to detect irregularities in disparate sectors. Namely, agents are likely to respond to changes in intensity of accountability only in areas where they would also respond to intensity of rewards (i.e., areas where effort is measurable and observable). This heterogeneity in expected effectiveness is confirmed by the conflicting empirical results about the efficacy of monitoring and accountability (Avis et al., 2018, Bobonis et al., 2016, Dizon-Ross et al., 2017, Ferraz and Finan, 2011, Olken, 2007). These conflicting results highlight the value of considering the complexities of incentive structures across settings, as well as the types of outcomes considered.

A necessary consideration for the analysis in this paper is whether the audits are effective monitoring instruments, or in other words, if the audits are able to detect instances of deficient delivery of public goods and services. Prior research has shown that higher levels of corruption are strongly negatively correlated with the academic performance of primary school students, and that increased corruption is closely related with worse test performance and higher dropout rates (Ferraz et al., 2012). I approximately replicate these results for the outcomes considered in this paper and arrive at the same conclusion: the fiscal irregularities highlighted in the audits effectively detected worse municipal outcomes.

In establishing a link between the audits and educational outcomes, this paper bridges some gaps in the literature. First, empirical results have shown that the timing of release of the audit reports has significant implications for electoral outcomes in the presence of corruption findings (Ferraz and Finan, 2008). Further, mayors with re-election incentives divert fewer funds than those who are term-constrained (Ferraz and Finan, 2005). Additionally, from the fact that some municipalities were audited at least twice by the CGU, we know that the audits led to a decrease in corruption, and that even neighboring municipalities were positively affected by the audits (Avis et al., 2018). These results elicit hypotheses about the audits' ability to select "good" mayors, and to incentivize second-term mayors to behave productively, but do not address whether municipal constituencies benefitted from the audits.

Additionally, the audits have also been shown to be relevant for outcomes related to health and the private sector. Health outcomes also seem to be impacted by corruption, but 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 (Lichand et al., 2016). Additionally, the audits impacted firms heterogeneously; the audits had had negative effects on politically connected firms, but led to growth in the number of firms in procurement dominant sectors (Colonelli and Prem, 2022).

Similarly, firms in audited municipalities grew larger post-audit, despite receiving fewer procurement contracts than their matched counterparts in unaudited municipalities ([Colonnelli et al., 2022](#)).

This paper separates itself from the existing 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 is the contemporary work [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, including 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 years⁴. Additionally, I use a stacked difference-in-differences strategy to address the potential issues related to staggered treatments, and I consider the interplay between the audits and other school and social programs. 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 additional evidence of how the 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 employed. Section VI provides a discussion of the results, and is followed by Section VII, which elaborates on potential mechanisms. Section VIII concludes.

II Institutional Context

As mentioned above, Brazil's institutions created an environment with two central features that allow for the study of the relationship between the efficacy of public spending and corruption: (1) the CGU anti-corruption program, and (2) a variety of education and social programs designed to incentivize children to meet the country's educational goals.

⁴My estimates are robust to this choice

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 a concerted effort to combat local 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 desired parameters.

Eligibility rules also changed over time, as the number of eligible municipalities increased almost monotonically from the genesis of the program. At first, only municipalities with fewer than 150,000 residents were eligible, but the criteria expanded to include all municipalities with fewer than 500,000 by 2004. State capitals were always ineligible to be audited in the program. While seemingly stringent, the eligibility criteria were such that over 99% of Brazil’s 5,570 municipalities were eligible to be audited. Between 2003 and 2015, over 2000 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. Moreover, [Table 1](#) provides a summary of the number of municipalities audited in each sector relevant to this paper. Further, to lend credence to the randomness of the selection of the municipalities, see [Table 2](#) for the summary statistics related to municipal characteristics and the student outcomes used in this paper⁵.

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 identify and halt corruption at the municipal level. The process was as follows: upon having its “number” drawn by the lottery, a municipality would be subject to an investigation covering expenditures of funds received from the federal government over the previous 3-4 years. These investigations were conducted by a team 10-15 of well-remunerated auditors⁶, who would spend 1-2 weeks

⁵All outcomes are winsorized at the 1st and 99th percentile.

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

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 restricted to those related 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 schools⁷. 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 reached 300 pages, the auditors provided a detailed account of their findings: this included a list of all irregularities⁸, amounts audited and estimates of the magnitude of each irregularity (when relevant), photographic evidence, and responses from local government officials seeking to address the issues found. The CGU, then would compile all reports, publish them for public access on their website, and forward 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 so that any necessary action could be taken. While the consequences for irregularities varied greatly, some were severe, including impeachment and prosecution.

II.II Education Programs

In addition to the anti-corruption measures discussed above, Brazil has a long history of implementing programs which incentivize students to attend 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 years⁹, 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 Escolar*, PNATE) which aims

⁷An 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.

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

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

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 PNATE program served around 3.2 million students, growing to approximately 4.7 million at its peak in 2020.

Lastly, in 2003, Brazil launched the *Bolsa Família* program, which, in addition to being regarded as a success, remains the country's largest social welfare program. *Bolsa Família* is a decentralized federal conditional cash transfer program, which requires, among other things, that the recipient's children be enrolled in school and maintain regular attendance¹⁰. Another set of conditions for receipt of transfers focuses on health outcomes of children of enrollees: pregnant mothers must receive prenatal care¹¹, children must be vaccinated, and the children's height and weight must progress in accordance with growth charts created by the Ministry of Health. The program is decentralized in that, while it is funded at the federal level, monitoring and enforcement happen at the municipal level. The mayor is responsible for appointing a local program manager, who is expected to handle the various administrative duties related to running the program locally, including hiring of personnel, enforcement of the conditions of the program, and handling of noncompliance¹². This is an essential aspect of the program, as it highlights the mayor's responsibility for its effectiveness and its compliance with federal requirements as well as the mayor's culpability for the results of the audits of accounts related to *Bolsa Família*.

These programs created incentives for children to stay in school, and were all directly audited as part of the randomized audit, allowing 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.

¹⁰Students in elementary and middle school must maintain an attendance record of at least 85% and high school students must maintain at least a 75% attendance rate

¹¹This sort of care is provided by the state, free of charge.

¹²Source: Ministry of Development and Social Assistance (*Ministério do Desenvolvimento e Assitência Social, Família, e Combate à Fome*)

III.I Audits

The data on audit reports comes from the CGU, and it encompasses every audit from the 20th through the 40th lottery, which translates to every audit from 2006 until the end of 2015¹³. 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.¹⁴ 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, acknowledging 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 (dropout) and failure rates starting in 2007. Additionally, I obtained the 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 on student performance on the national standardized exam, *Prova Brasil*, which takes place biannually. Since 2009. *Prova Brasil* has tested all public school students in the 5th and 9th grades on their Portuguese and Mathematics skills.

III.III Municipalities

Data on municipal public services such as electricity, sewage, and water delivery come from the SIAB surveys, which took place until 2015. I use data from 2004-2015, containing information on the number of families per municipality with access to the various services and forms of infrastructure.

¹³Starting in 2016, the CGU began selecting municipalities in a directed, and non-random, fashion.

¹⁴This 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.

III.IV Elections

The data on mayoral terms and elections 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 mayoral terms.

III.V Miscellaneous

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 adequacy of anti-corruption measures depends on the notion that corruption leads to undesirable outcomes. [Table 3](#)¹⁵ lends support to the hypothesis that where corruption is found, worse outcomes are present. Those tables report the results of the fixed effects regression:

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

Where Y_{imt} is the school-level, or municipal-level, outcome of interest, C_{mt} is the measure of corruption reported, δ_s and λ_t are state and year fixed effects, and X_{mt} is a vector of municipal characteristics¹⁶. 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 (number of service orders). For reference, see [Figure 2](#) for a histogram of education irregularities found per service order. As outlined below, while these regressions are not designed to establish a causal link between corruption and worse public good provision, they do show that revealed corruption is negatively correlated with desirable outcomes.

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 additional education-related irregularity (per service order) correlates with an increase in dropout rates of over one percentage point (over 20% of the dependent variable mean).

¹⁵Tables A3 and A5 show similar results for the outcomes discussed in the appendix.

¹⁶I approximately follow [Ferraz et al. \(2012\)](#) by including controls for log population, log GDP, log federal transfers (*Fundo de Participação Municipal*), Gini coefficient, and percentage of residents with a High School degree, and percentage urban.

Columns 2 and 3 consider the effects of irregularities related to the school-bus program and conditional cash transfer (CCT) programs¹⁷, respectively, and Column 4 performs a horse-race between those irregularities and other irregularities related to school lunches. Note that the conditional cash transfer, or social programs, irregularities are not included in the education irregularities, whereas both food and transport irregularities are subcategories of education. Secondly, notice that all estimates are positive, and their magnitudes are relatively consistent across columns (with the horse race estimates 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 states. These results seem to point to the notion that the audits captured issues which were consequential in the “real” world of student outcomes.

To reinforce this point, I approximately replicate the results from [Ferraz et al. \(2012\)](#) in Panel A of [Table A1](#), showing that standardized test scores among primary school students were lower in municipalities where higher levels of corruption were revealed. Note that all estimates are negative, and the coefficients on education irregularities for the outcomes for 5th grade students are larger, in absolute value, and significant. A different pattern emerges in Panel B, when we consider the number of irregularities across all social programs. In these regressions, the large and significant coefficients are those in the regressions using data on 9th graders. Again, the coefficients on 5th graders are commensurate with those in Panel A, and are all negative, but it is notable that the effect of social programs is larger on 9th graders. While there are many interpretations for this result, the combination of the two panels seems to suggest that CCT irregularities have stronger consequences later in the students’ academic careers, *vis-à-vis* the earlier effect of education irregularities. This combination perhaps results from the fact that students in municipalities with worse provision of social programs are relatively less incentivized to attend school regularly, an effect which leads to an accumulated learning disparity over the students’ academic lives.

Having established the negative relationship between corruption and the outcomes of interest, the natural next step is to consider whether measures, designed to 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 succeeded in reducing it. Given these results, audited municipalities should, *ceteris paribus*, see an improvement in their public services relative to those which are never audited. It is possible, however, 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,

¹⁷The choice using irregularities related to all social programs is made due to power, as there are fewer instances of Bolsa Família irregularities than there were irregularities across all social programs. The results presented are qualitatively the same as those using only Bolsa Família irregularities, but are more precise.

it is also 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, or investments into, their towns. While these two hypotheses are plausible, the results discussed in Section V seem to favor the earlier interpretation, that municipalities see some improvements in public services.

We should moreover expect heterogeneity in the audits' impact across outcomes, as the results of the audits would be influenced by the ease, or difficulty, in inspecting the evidence related to the various audited services (Holmstrom and Milgrom, 1991). For instance, it is arguably easier for auditors to ascertain that conditional cash transfers are being paid out without regard for the agreed upon conditions, by looking at supporting evidence, than it is for them to establish that underground sewage projects were carefully built to match the specifications of approved work orders. Thus, I propose an intuitive framework to create expectations about audit effectiveness. Given that the present analysis considers only the effects of the first audit, which is akin to single monitoring shocks, we would expect different results than if the treatment consisted of a permanent and continuous expansion in monitoring across all activities all the time. Therefore, for any set of outcomes, the effectiveness of the audits as a remedial measure for effectiveness in public spending depends on: (1) the auditor's ability to detect deficient outcomes via fiscal irregularities, (2) the impact that rent-seeking behavior may have on the outcomes, (3) the sensitivity of the outcomes to additional monitoring, and (4) the possibility for the outcomes to be ameliorated in one mayoral term. Each of these conditions is easily rationalizable, but they are all essential for the rest of the analysis, and their sum informs the interpretation of the results we see in Section VI.

V Empirical Strategy

The empirical strategy for this analysis relies on the random, and unanticipated, nature of the audits to identify the causal downstream effects of being audited¹⁸. As described in Section II, from 2003 until 2015, the number, timing, and frequency of random selections for audits were 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 in the mechanism analyses triggered by the fact that a municipality becomes untreated

¹⁸Table 1 provides summary statistics and a balance test of the variables used in the main analysis.

after an audited mayor leaves office, I follow a similar strategy to Cengiz et al. (2019), whereby I create a distinct dataset (stack) for each “event.” For the purposes of this paper, the event corresponds to an audit, and for each year I create a subsample made up of all municipalities audited in that year, as well as all municipalities which are never audited¹⁹. 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. This strategy allows for the creation of a *clean* control group, which is unaffected by the complications of staggered treatments²⁰. Then, using the stacked dataset, the regression equation is:

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

Where Y_{imth} represents the relevant school-level, or municipal-level, outcomes for municipality m , in year t , related to event h , μ_{mh} are school-stack (or municipality-stacked) 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. Note that for student and school outcomes, I primarily school-stack fixed effects in the main specification.²¹ 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 average causal effect of the audits²².

VI Results

VI.I Education

The results of the main specification are presented in Table 4, and indicate that elementary and middle schools in audited municipalities experienced a decrease in dropout rates relative to the control

¹⁹Estimates are robust to the inclusion of not-yet-treated units.

²⁰To 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.

²¹These results are robust to the use of school-stack or municipality-stack fixed effects as well as different choices of treatment horizons (3-6 years).

²²I perform robustness tests using various differences-in-differences estimators in the robustness appendix.

group.²³ While the 0.34 percentage point decrease in dropout rates, may seem small in magnitude, it is relatively large, making up approximately 10 percent of the dependent variable mean. This is an important result as it constitutes improvement in an area where previous measures, such as *Bolsa Família*, had already generated sizable advancements; thus, under the reasonable assumption of diminishing returns to interventions targeting school attendance, these audits are making contributions where marginal improvements are hard to come by. As discussed below, this result is robust to a variety of modifications, including limiting the sample to a balanced panel of schools, changes in the level of fixed effects, changes in clustering of standard errors, and various difference-in-differences estimators (Tables R1-R4).

Table 4 also shows results for failure rates, which can be interpreted as the dual of grade progression, as well as results for high schools. While these results tell a similar story, that audits led to improvements in student outcomes, they are less robust than the effect on dropout rates for elementary and middle school students. Additionally, failure rates are complicated by the fact that they represent decisions by the schools, which are plausibly independent of testable external mechanisms, making it unfeasible to draw conclusions about what drives the observed effect. For these reasons, I focus on the analysis of the drivers of the effect of audits on dropout rates. It should be noted that the effect on high-schools is also negative and is even larger than the effect on elementary and middle schools, but this effect is not significant at conventional levels; this is likely the result of the relative infrequency of municipal high schools²⁴. Table 5 shows that results for state schools are similar, suggesting the existence of possible monitoring spillovers, since the state schools were not directly audited.

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

In addition to showing what the audits accomplished, it is important to highlight that which they

²³Note 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.

²⁴High schools are traditionally funded and maintained by state governments, and elementary and middle schools by municipal governments.

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 accountability²⁵, we would expect the results to be limited to outcomes which can be improved in the short-run. Considering Tables A2, A3, and A4, we see that the audits themselves did not have a significant effect on standardized test scores, or on the number of classrooms in each of the schools, nor did they impact the number of teachers working in them. The only noticeable change comes from the increase in number of middle school students enrolled, an effect which is likely due to the negative relation between irregularities and number of elementary students enrolled seen in Table A3.

I perform several robustness tests to ensure the validity of the effect on dropout rates. The first procedures performed are displayed on Table R1, where column 1 restricts the sample to create a balanced panel of schools in which every school listed existed for all relevant years (two years before treatment and four years afterwards), and for which the percentage of dropouts is available for every relevant year. Column two, on the other hands, uses data aggregated up to the municipal level by INEP, and thus uses municipal fixed effects. In both columns we see that the main result is unchanged, indicating that the sample of schools used in the main regression estimates is representative. Additionally, I use the estimators proposed by Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Wing et al. (2024) to estimate the average treatment effect over the four years after the audit period at both the school level Table R2, and the municipal level Table R3. From those tables it is evident that, while magnitudes change slightly, the results hold the same weight and interpretation. Further, Table R4 illustrates that the results are robust to the choice of clustering at the municipal level. It is also reasonable to argue that the error terms are correlated within a municipality-year, but assuming so yields the same conclusions. Finally, Table R5 combines municipal and state schools to show that the combined effects for both categories of school are unchanged.

VI.II Public Services

As the audits also focused, albeit to a lesser extent, on miscellaneous municipal public services, I show here some evidence that the audits also contributed to improvement in some of these areas²⁶.

²⁵Importantly, audit risk represents the continuous sort of accountability useful for deterrence, as shown in Zamboni and Litschig (2018). Thus, it is sensible that audit risk presents an incentive for long-run change, but experiencing an audit only changes short-run outcomes.

²⁶The audits also investigated accounts related to health expenditures, but there was no measurable impact from the audits on the number or total cost of inpatient or outpatient procedures in municipal hospitals. These results are

[Table A6](#) shows that municipalities experienced mild improvements in services which are relatively less invasive, namely trash collection and electricity, but did not see improvements in areas where sizeable infrastructure investments would have been necessary (water and sewage). This is in line with the framework laid out in Section IV. We can see from Table A6 that garbage services are significantly improved: the percentage of households who dispose of their trash in landfills decreases, while the percentage of those with access to trash collection increases. This is likely the result of audits increasing scrutiny for landfills, which are an illegal manner of disposing of garbage in Brazil. As a response, municipalities seem to add households to their garbage pick-up route to combat these issues. A similar pattern is observed in column 5, where the number of households with access to electricity increases; this is most likely due to households being connected to existing electricity grids, rather than the construction of new ones (see the mayor-audit section in the mechanisms' discussion below).

VII Mechanisms

The results thus far indicate that the audits led to small, but noticeable, improvements across some municipal outcomes. To properly understand the effect on educational outcomes, specifically, the decrease in dropout rates, 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. Due to the nature of the data on municipal services, one's ability to target mechanisms through which the audit effect work are limited, thus, for those outcomes, I am only able to consider the mayoral discipline mechanism.

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 disproportionately affected by the audits. This possibility is in line with the idea that those programs would function more effectively with additional monitoring or accountability. Considering the *Bolsa Família* program, for available upon request.

instance, which depends almost exclusively on local governments' efforts to ensure that funds are distributed *only* to eligible citizens, and to ensure that funds are indeed distributed. To investigate this possibility, I control for a measure of the schools' reliance on two different programs designed to keep children enrolled in schools: one is the PNATE program which promises school transportation to all students, and the other is the *Bolsa Família* conditional cash transfer program. First, as a measure of reliance on school-transportation, I use the School Census to calculate the percentage of students in each school who use school-transportation in the year prior to the audit. Similarly, for *Bolsa Família*, I take the total value of transfer paid per capita to each municipality in the year prior to the audit. For both, I use an indicator variable which equals one if the school (PNATE), or municipality (*Bolsa Família*), had higher values than the median in the two aforementioned categories.

As seen in [Table 6](#), interacting these measures of reliance on the programs sheds light on their relative importance. Notice first in column (1) that the DiD coefficient on “Audited” is mostly unaffected, but that the coefficient on the interaction term is insignificant and smaller, indicating that the impact of the audits is not driven by reliance on school transportation. On the other hand, the interaction coefficient on the per-capita CCT measure is both significant and relatively large, suggesting that municipalities which rely more heavily on *Bolsa Família* than the median municipality experienced a significantly larger decrease in abandonment rates²⁷. 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. I take these results as suggestive evidence that the audit effect is partly driven by stricter enforcement of the school attendance requirements for conditional cash payments after the audits. In other words, the municipal authorities seem to become aware of previously lax monitoring practices and address these issues upon being found in irregular standing. While this is not the only possible interpretation, it is one that fits the institutional context. Support from this hypothesis comes from the fact that *Bolsa Família* payments are not affected by the audits ([Table A9](#)), thus considering that audits reduce corruption ([Avis et al., 2018](#)) and transfer payments remain constant, it is reasonable to conclude that monitoring practices improve. This suggests that the *Bolsa Família* program, while a success in its own right, would have been significantly more effective if accompanied by increased accountability for its local officers.

A different, viable, interpretation is that the audit treatment effect is concentrated in poor munic-

²⁷Columns 3-4 of that table provide a robustness test using “microregion” fixed effects to avoid the potential collinearity problems caused by the school fixed effects on Columns 1-2. 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 qualitatively the same when we consider an indicator for whether a municipality is above the median level CCT per capita in the year prior to the audit.

ipalities and the effects on [Table 6](#) are explained by the fact that reliance on *Bolsa Família* is a proxy for poverty. To address this possibility, I interact the measure of reliance on *Bolsa Família* with municipal GDP in [Table A13](#). As seen by the interaction terms, the interaction between the treatment variable and reliance on *Bolsa Família* remains negative and significant in both columns, whereas the interaction between treatment and GDP (per capita, column 1, and $\log(GDP)$, in column 2) is positive in both regressions. These results lead credence to the conjecture that *Bolsa Família*, not poverty *per se*, is the relevant mechanism.

VII.II Number of Irregularities Found

A second relevant mechanism relates to the results of the audit reports themselves. Under the assumption that municipal corruption, measured by the number of irregularities found in a given audit report, remains constant in the absence of an audit, we can take advantage of the staggered treatment to compare early and late treated municipalities while controlling for the number of irregularities which would *eventually* be 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 estimate the heterogeneity in audit effects by the amount of corruption found. These results can be found in [Table 7](#), where we can see, in the first and third columns, that the effect of the audits is concentrated in municipalities where higher levels of irregularities in education and conditional cash transfers were revealed. Adding the coefficients in columns (1) and (3), 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 these categories was less than one, and the upper bound was approximately four, one can conclude that the higher the underlying level of corruption in a municipality, the more it benefited from the accountability shock²⁸. Considering these results in light of the evidence that audits reduce corruption ([Avis et al., 2018](#)) suggests that the reduction in corruption trickles down to some improved outcomes in the municipalities.

An alternative way to consider this, is displayed in [Table 8](#). Here, I interacted the audited variable with an indicator for whether the number of irregularities per audit service order fell in intervals between zero and three or more, with [0,1) being the omitted category. For this analysis, I include both the sample of schools considered in the regressions above, restricting the period from 2009 to 2019 to guarantee a two-year pre-period for the treated municipalities, and also one additional regression, including the municipalities audited in 2008 to overcome some of the precision lost from the

²⁸See figures 2-4 for histograms of the irregularities per service order rev lead in each of these categories.

sample restriction. Note that the results are qualitatively equivalent, but the latter is more precise. We can see from this analysis that the municipalities on the right tail of the irregularity distribution are the ones which especially benefit from the audits. Further, considering that the median number of irregularities per service order in education was just under one ([Figure3](#)) it's notable that the coefficient on the interaction between 1+ irregularities is negative and marginally significant, while the ones for 2+ and 3+ are even larger in absolute size, while maintaining the negative sign and gaining precision. Assuming that the auditors performed their duties with approximately similar effort and diligence across audits, leads to the conclusion that the places which committed more infractions experienced the most significant improvements from the audits. A different way to look at these results is to say that the driver of this effect is the *revealing* of corruption, or irregularities, which disciplines mayors and nudges them to improve outcomes. While it is not possible to test for which hypothesis is correct, the notion that the municipalities with worse practices stood to benefit the most is in line with the results we find in tables [3](#) and [A5](#), where we see that more irregularities are positively correlated with worse outcomes.

VII.III Mayoral Accountability and Incentives

The final, 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 reframing of the problem to consider mayors as the unit of treatment rather than municipalities; this is equivalent to measuring the audit effects that materialize while the audited mayors are still in office. To investigate this mechanism, I create a panel of mayors and an accompanying indicator variable which is equal to one if a 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^{[29](#)}. 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](#).

The coefficients suggest that the response by audited mayors is essential in this context. Notice that the audit effect on dropout rates has approximately the same magnitude when we restrict the sample to audited mayors as it was in the full sample, suggesting that the effects are driven by the

²⁹ Audited mayors who leave office and then become mayors in other municipalities at a later date are only considered “treated” in the period immediately after the audit.

periods before a new mayor takes over. Moreover, the fact that the regression results on the other mechanisms are substantively unchanged seems to suggest that the audit effects observed above are induced by the response of the mayor in office at the time of the audit.

If re-election incentives serve as a powerful deterrent against corruption ([Ferraz and Finan, 2005](#)), then we should expect to see heterogeneous effects of the audits by term status and by the number of irregularities listed in the reports. To check for this, I run a triple-differences regression interacting indicators for treatment (mayor being audited), number of irregularities revealed, and term status.³⁰. These results can be found in [Table 12](#), and are suggestive of the fact that mayors' response to the audits is largely driven by re-election incentives. As seen in both columns, the audit effects on second-term mayors' do not vary with the number of irregularities, as they do for first-term mayors (the omitted category is an unaudited mayor). The table further shows that we can draw the same conclusions from looking at the education or CCT irregularities found. These results are rationalized by the hypothesis 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, it is useful to compare their relative importance. First, it should be noted that, across the board, the evidence suggests that the gains from the audits take place in the periods while audited mayors are still in office. This can be seen in tables [9](#), [10](#) , and [11](#). Thus, with the knowledge that the disciplining of mayors is always relevant, we can consider which mechanism drives the results, reliance on *Bolsa Família* or the number of irregularities. To perform this comparison, I create an indicator for whether an audited municipality would eventually be in the top fifty percentiles of municipalities in terms of education irregularities ($\mathbb{1}\{\text{A.M. Irreg.}\}$); the results for education and CCT irregularities are equivalent. Using this indicator, I run a triple differences specification to estimate the heterogeneity in treatment effect across these two categories. The specification for tables [13](#) and [14](#) is:

³⁰Note that in Brazil, mayors are term-limited, and may only serve two consecutive terms.

$$\begin{aligned}
Y_{imth} = & \beta_1 A_{m\tau h} + \beta_2 A_{m\tau h} \times \mathbb{1}\{\text{A.M. CCT pc}\} + \beta_3 A_{m\tau h} \times \mathbb{1}\{\text{A.M. Irreg.}\} \\
& + \beta_4 \mathbb{1}\{\text{A.M. CCT pc}\} \times \mathbb{1}\{\text{A.M. Irreg.}\} \\
& + \beta_5 A_{m\tau h} \times \mathbb{1}\{\text{A.M. CCT pc}\} \times \mathbb{1}\{\text{A.M. Irreg.}\} \\
& + \varphi_1 \mathbb{1}\{\text{A.M. CCT pc}\} + \varphi_2 \mathbb{1}\{\text{A.M. Irreg.}\} \\
& + \mu_{ih} + \lambda_{th} + \epsilon_{imth}
\end{aligned}$$

Where the betas are the coefficients of interest. As seen in tables 13 and 14, when comparing the two mechanisms, the signs on β_2 and β_3 remain negative, and their magnitudes remain approximately the same as when the regressions are run separately (Tables 6 and 7). This suggests that they are both relevant mechanisms, but the added precision and larger magnitude of the indicator for municipal reliance on CCTs suggests that the latter is more important. The fact that the triple-interaction coefficient, β_5 is insignificant, albeit bearing a negative sign, suggests that the combination of the two mechanisms is not as relevant as they are individually. In other words, if a given municipality was either in the top fifty percentiles of *Bolsa Família* recipients, or it is in the top fifty percentiles of irregularities, it enjoyed larger benefits from the audits than those which were not in either category. 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 *Bolsa Família* program drive the decreases in dropout rates.

VIII Conclusion

Brazil's anti-corruption program, distinguished by its large scale centralized random audits, appears to have decreased *measured* corruption, and also improved tangible outcomes. These effects are only present in outcomes that could be improved in the short run, and which could be improved as a result of increased monitoring. Further, the effects are concentrated in municipalities where (1) a relatively high number of irregularities is found, (2) the residents rely heavily on conditional cash transfer programs, and (3) the mayor has re-election incentives. Furthermore, the improvements in outcomes, when present, mostly took place while the audited mayor was still in office. Most notably, audited municipalities experienced a significant decrease in dropout rates among elementary and middle school students who attend institutions funded by the municipality. This effect constitutes an approximate 10% decrease in abandonment for the relevant students. A back-of-the-envelope cal-

culation suggests that approximately one fewer student dropped out per school every two years as a result of the audits³¹. Considering that the spillover effects on state schools yielded similar results, and the fact that there were approximately 240,000 municipal and state public schools in the eligible municipalities in 2015, it is evident that the effect of the audits was all but negligible. Additionally, the decrease in dropout rates, given that it was caused by work done by teams of approximately ten auditors, is not trivial, and yields quite a favorable reward-to-effort ratio.

The heterogeneity in treatment effects highlights an important aspect of the audits' effectiveness: the municipalities which stood to benefit the most from the audits were the ones which experienced the largest gains. As shown above, the towns where the highest levels of corruption were found also had the highest dropout rates in the sample. Therefore, it is salient that those municipalities also experienced the largest decrease in dropouts. Similarly, the municipalities where residents were most reliant on social programs, like Bolsa Família, which arguably represent the country' most vulnerable towns, also benefited disproportionately. Moreover, the audits from the anti-corruption program also had positive effects on disparate outcomes, as seen by the improvement in garbage services and access to electricity. These results are not trivial in evaluating the success of the anti-corruption policy in achieving its stated goal of improving the use of public funds.

The results in this paper underscore both what the audits *could not* accomplish, and that which they could. As seen by the fact that school infrastructure, and some municipal services, were largely unaffected, it is clear that the audits' potential was limited to a set of outcomes are corrigible in a short time horizon. But one should not ignore that the results presented here also show that, under the right conditions, additional monitoring can be a powerful tool to realign the incentives of politicians and bureaucrats with those of the citizenry.

³¹The mean number of students in municipal primary schools in a given year in the sample is 170, multiplied by an average dropout rate of 3.2% implies that approximately 5.4 students dropped out per school per year, whereas accounting for a lower average dropout rate (0.032 - 0.0034) yields an average of 4.9 students dropping out each year.

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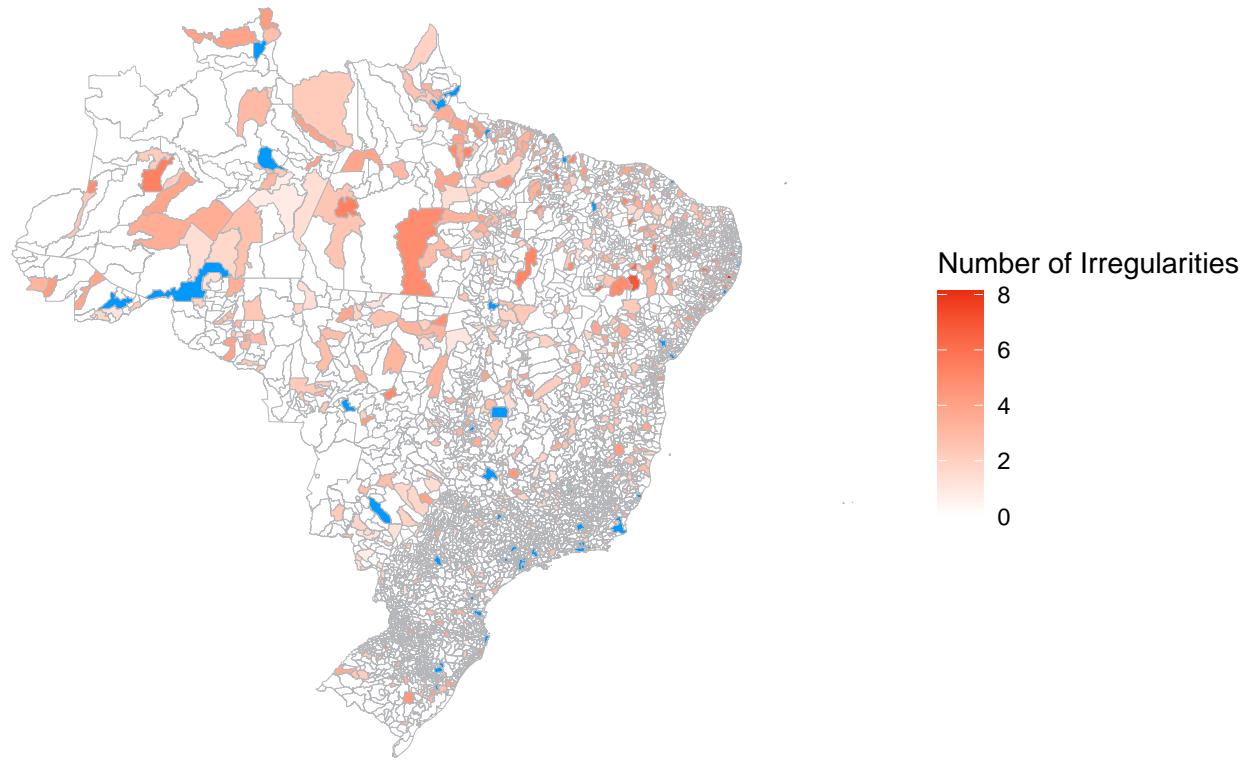
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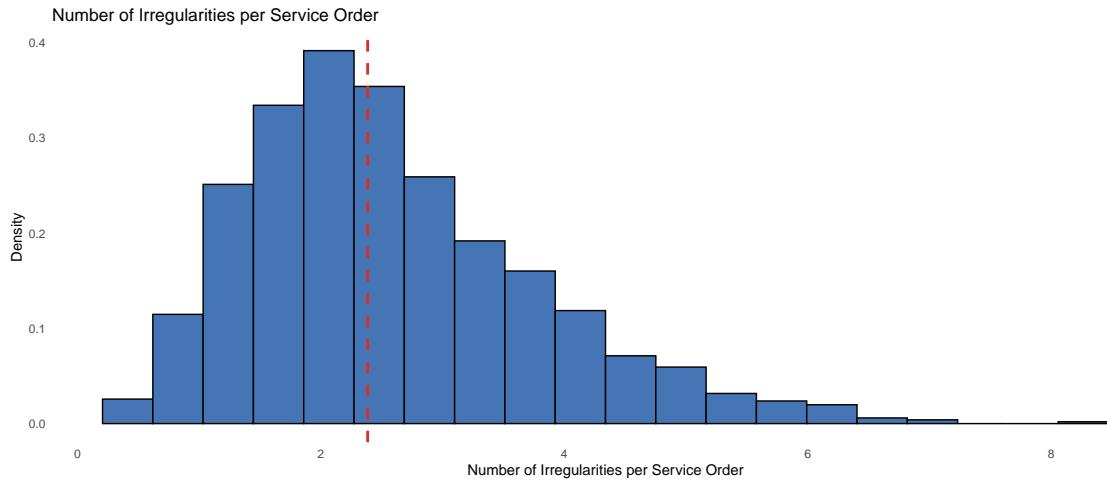
IX Figures

Figure 1: Map of Brazil - Audits



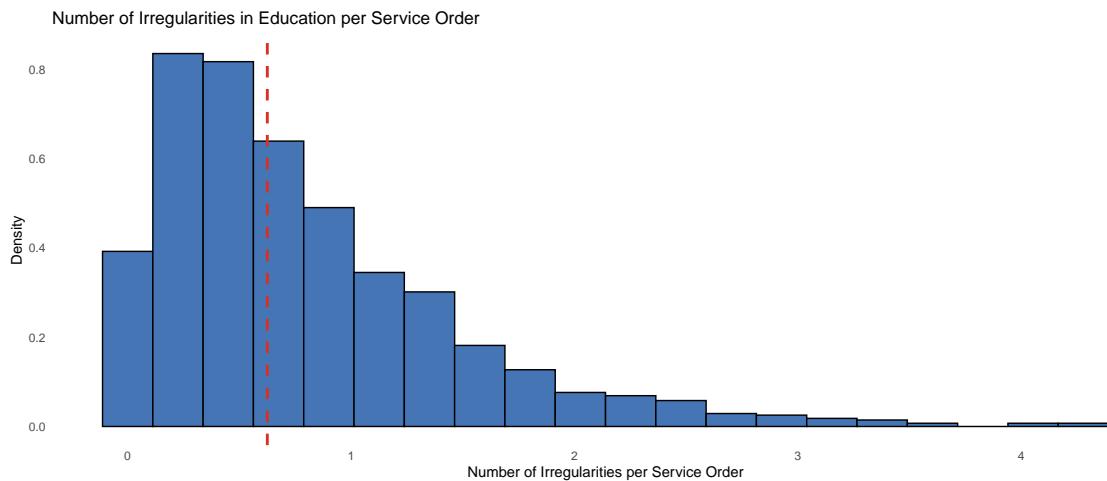
Note: This map depicts all Brazilian municipalities, and is colored according to their audit status in the sample. The audited municipalities are shaded in accordance with the number of irregularities revealed for each audit service order. In other words, the color represents the number of irregularities 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 Irregularities



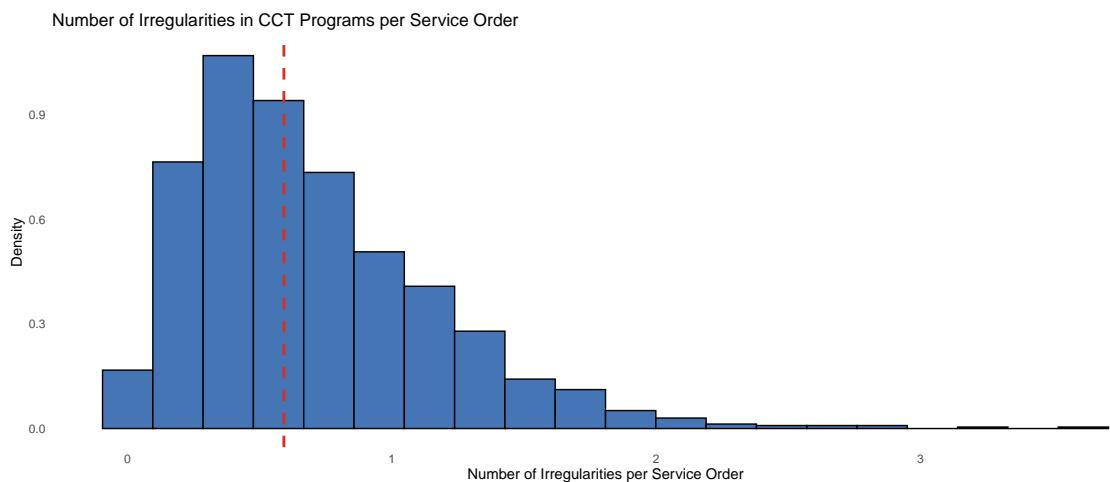
Note: This histogram plots the distribution of irregularities found across accounts that were audited. The dashed vertical line marks the median number of irregularities found.

Figure 3: Histogram of Education Irregularities



Note: This histogram plots the distribution of irregularities found when education-related accounts were audited. The dashed vertical line marks the median number of irregularities found.

Figure 4: Histogram of Social Programs (CCT) Irregularities



Note: This histogram plots the distribution of irregularities found when social program-related accounts were audited. The dashed vertical line marks the median number of irregularities found.

X Tables

Table 1: Accounts Audited

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

Note: This table shows counts of which categories of accounts which were audited as part of the randomized audits from 2006-2015. The unit of observation is a municipality-audit. The table includes only the first time a given municipality was audited in the relevant period (i.e., there were 1,132 first time audits in the period). Additionally, Bolsa Família constitutes a subset of the social programs which were audited.

Sources: CGU, 2006-2015

Table 2: Summary Statistics

	Mean	SD	p10	Median	p90
<i>Panel A : Eligible Municipalities</i>					
Population	24,613	46,594	3,164	10,992	51,380
GDP	338,607	1,189,191	21,655	76,433	596,892
Gini Coef.	0.17	0.09	0.063	0.16	0.29
Perc. of Pop. with HS Degree	0.52	0.072	0.43	0.52	0.61
Perc. Urban	0.59	0.22	0.29	0.6	0.87
Dropout Rate (Elem. + Middle School)	0.034	0.069	0	0.001	0.1
Dropout Rate (High School)	0.11	0.12	0	0.076	0.26
Failure Rate (Elem. + Middle School)	0.1	0.11	0	0.077	0.25
Failure Rate (High School)	0.077	0.11	0	0.05	0.18
Number of Classrooms	4.9	5.5	1	3	11
Perc. HH w. Open Trash	0.12	0.16	0.0021	0.05	0.36
Perc. HH w. Collected Trash	0.66	0.25	0.3	0.68	0.98
Perc. HH w. Public Water	0.67	0.23	0.34	0.7	0.96
Perc. HH w. Public Sewage	0.3	0.34	0.0029	0.11	0.87
Perc. HH w. Electricity	0.93	0.11	0.78	0.97	1
N. Prenatal Visits	5.1	1.7	2.8	5.5	6.9
$\mathbb{1}\{\text{Prenatal Visits}\}$	0.95	0.15	0.89	0.99	1
<i>Panel B : Audited Municipalities</i>					
Population	25,831	46,280	3,357	12,603	54,211
GDP	329,867	1,068,642	22,343	78,453	560,865
Gini Coef.	0.17	0.088	0.062	0.16	0.29
Perc. of Pop. with HS Degree	0.52	0.071	0.43	0.52	0.61
Perc. Urban	0.59	0.21	0.29	0.6	0.87
Dropout Rate (Elem. + Middle School)	0.036	0.072	0	0.004	0.11
Dropout Rate (High School)	0.088	0.12	0	0.052	0.22
Failure Rate (Elem. + Middle School)	0.11	0.11	0	0.082	0.25
Failure Rate (High School)	0.082	0.11	0	0.057	0.19
Number of Classrooms	4.6	4.8	1	3	11
Perc. HH w. Open Trash	0.13	0.16	0.0024	0.061	0.38
Perc. HH w. Collected Trash	0.65	0.25	0.3	0.67	0.97
Perc. HH w. Public Water	0.66	0.23	0.34	0.69	0.95
Perc. HH w. Public Sewage	0.27	0.33	0.0026	0.078	0.84
Perc. HH w. Electricity	0.92	0.12	0.76	0.97	1
N. Prenatal Visits	5	1.7	2.8	5.5	6.8
$\mathbb{1}\{\text{Prenatal Visits}\}$	0.95	0.15	0.88	0.99	1

Note: This table reports the summary statistics at the municipal level (and school level for educational outcomes) using data from a variety of sources from 2000-2019. The sample in Panel A includes *all* municipalities which were eligible for CGU audits, including audited municipalities. The sample in Panel B includes only the municipalities audited as part of the GGU program from 2006-2015. Variables are described in the text (sources in bold, years available in parenthesis). **IBGE:** Population estimates represent the number of residents in a municipality (2000-2019). **IPEA:** Municipal data from IPEA relates to the most recent census year (2000, 2010), and they are as follows. GDP is the municipal **GDP** in R\$1000s, **Gini Coef.** is an estimate of the municipality's Gini coefficient, and **Perc. of Pop with HS Degree** and **Perc. Urban** are calculated by dividing the corresponding numbers with the population values for the respective years. **INEP:** **Dropout Rate** and **Failure Rate** are school-level statistics by their respective names (2007-2019). **School Census:** **Number of Classrooms** counts the number of classrooms available in each school (2007-2019). **SIAB:** All variables starting with "Perc. HH" come from the SIAB survey (2004-2015) and were collected yearly at the household level. **Perc. HH w. Open Trash** is the percentage of households in the municipality who dispose of their trash via landfills. **SINASC:** **N. Prenatal Visits** is the mean number of prenatal visits for each birth that took place in a given municipality for a given year. $\mathbb{1}\{\text{Prenatal Visits}\}$ was constructed by taking the municipal mean of the indicator variable at the birth level, thus, the variable represents the approximate percentage of births for which the mother had a prenatal visit.

Table 3: Corruption Effects

Dependent Variables: Model:	Municipal			State	
	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
N. Irreg. Educ.	0.0109*** (0.0034)	0.0466*** (0.0146)	0.0434** (0.0171)	0.0031 (0.0051)	0.0020 (0.0023)
N. Irreg. Sch. Transport					
N. Irreg. CCT		0.0073* (0.0041)	0.0062 (0.0039)		
N. Irreg. Food.		0.0017 (0.0089)			
<i>Fixed-effects</i>					
State	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>					
Clusters	975	975	975	975	975
E[Y]	0.0472	0.0472	0.0472	0.0472	0.0472
Observations	22,044	22,044	22,044	22,044	22,044
R ²	0.19700	0.19580	0.19129	0.19762	0.14164
<i>Clustered (Municipality) standard-errors in parentheses</i>					

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the regression coefficients for the variables which measure the count of irregularities per service order. The specification is $Y_{int} = \gamma C_{mt} + \delta_s + \lambda_t + X_{mt} + \epsilon_{int}$ and is discussed in Section IV. The sample is a cross-section of audited municipalities in the years when they are audited. Each column represents a different regression, with the dependent variable listed at the column header. Rows represent different categories of irregularities. Note that school transportation and food irregularities are both subsets of education irregularities. Only municipal public elementary and middle 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 a high school degree, percentage urban, and Gini coefficient). Standard errors are clustered at the municipality level. Dependent variable means are reported below the coefficients.
Sources: CGU (2006-2015), INEP (2007-2015), IPEA (2000,2010)

Table 4: Stacked DiD: Audit Effects

Dependent Variables: Model:	Elementary + Middle Sch.		High Schools	
	Dropout Rate (1)	Failure Rate (2)	Dropout Rate (3)	Failure Rate (4)
<i>Variables</i>				
Audited	-0.0034** (0.0016)	-0.0067** (0.0034)	-0.0125 (0.0164)	-0.0145 (0.0192)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	4,130	4,130	456	456
E[y]	0.0325	0.1016	0.1176	0.0700
Observations	3,526,934	3,526,934	15,494	15,494
R ²	0.48997	0.49486	0.68711	0.62065

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{imth} = \beta A_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), INEP (2009-2019)

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

Dependent Variables: Model:	Elementary + Middle Sch.		High Sch.	
	Dropout Rate (1)	Failure Rate (2)	Dropout Rate (3)	Failure Rate (4)
<i>Variables</i>				
Audited	-0.0034** (0.0014)	-0.0072** (0.0030)	-0.0056 (0.0034)	0.0012 (0.0031)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	4,138	4,138	4,134	4,134
E[y]	0.0330	0.1010	0.0919	0.1024
Observations	4,380,915	4,380,915	638,139	638,139
R ²	0.51042	0.51307	0.60368	0.55835

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{imth} = \beta A_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only state schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), INEP (2009-2019)

Table 6: Mechanism: Reliance on School Programs

Dependent Variable: Model:	School FE		Microregion FE	
	Dropout Rate		(3)	(4)
	(1)	(2)		
<i>Variables</i>				
Audited	-0.0024 (0.0022)	0.0097*** (0.0012)	-0.0051*** (0.0018)	0.0045*** (0.0010)
Above Median Perc. Students in School Bus	0.0035*** (0.0006)		0.0053*** (0.0004)	
Audited \times Above Median Perc. Students in School Bus	-0.0018 (0.0020)		0.0025 (0.0016)	
Above Median Per Capita CCT Value		-0.0119** (0.0060)		0.0026*** (0.0009)
Audited \times Above Median Per Capita CCT Value		-0.0161*** (0.0021)		-0.0106*** (0.0019)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes		
Year x Stack	Yes	Yes	Yes	Yes
Microregion x Stack			Yes	Yes
<i>Fit statistics</i>				
Clusters	4,130	4,130	4,130	4,130
E[y]	0.0325	0.0325	0.0325	0.0325
Observations	3,526,934	3,526,934	3,526,934	3,526,934
R ²	0.49015	0.49003	0.19933	0.19829

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates indicating a municipality's relative reliance on social programs. The specification is $Y_{imth} = \beta A_{mth} \times \mathbb{1}\{Program\}_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. $\mathbb{1}\{Program\}$ relates to the School Bus program, or Bolsa Família. *Above Median Perc. Students in School Bus* is an indicator for whether a school's percentage of students using school buses to get to school is greater than the median school's. Similarly, *Above Median Per Capita CCT Value* is an indicator for whether the total amount of Bolsa Família (CCT) transfers per capita in a municipality was greater than the median municipality in the sample of eligible municipalities. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. School-stack and year-stack fixed effects are used in columns (1) and (2), and microregion-stack fixed effects are used in columns (3) and (4) to address the multicollinearity between the fixed effects and the interacted terms. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019), School Census (2009-2019), IPEA (2009-2019)

Table 7: Mechanism: Number of Irregularities

	Educ. Irreg	School Transport Irreg.	CCT Irreg.
Dependent Variable:	Dropout Rate		
Model:	(1)	(2)	(3)
<i>Variables</i>			
Audited	0.0058 (0.0047)	0.0002 (0.0048)	0.0037 (0.0050)
Audited \times N. Irreg. Educ.	-0.0069** (0.0028)		
Audited \times N. Irreg. Sch. Transport		0.0009 (0.0203)	
Audited \times N. Irreg. CCT			-0.0047* (0.0028)
<i>Fixed-effects</i>			
School x Stack	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes
<i>Fit statistics</i>			
Clusters	332	332	332
E[y]	0.0435	0.0435	0.0435
Observations	52,209	52,209	52,209
R ²	0.49201	0.49122	0.49148

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates which count the number of irregularities per service order across several of the accounts which were audited. The specification is $Y_{imth} = \beta A_{mth} \times N.Irreg.mth + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. *N. Irreg. Educ* is the number of education irregularities found per audit service order related to education (*School Transport* irregularities are a subset of these education irregularities). *N. Irreg. CCT* is the number of irregularities per service order related to any of the social programs that were audited as a result of the audits. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. The sample consists only of municipalities which were audited, and for which the number of irregularities in the relevant categories is available. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. School-stack and year-stack fixed effects are used in columns (1) and (2), and microregion-stack fixed effects are used in columns (3) and (4) to address the multicollinearity between the fixed effects and the interacted terms. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019)

Table 8: Mechanism:
Disaggregated Number of Irregularities

	Standard	Incl. 2008
Dependent Variable:	Dropout Rate	
Model:	(1)	(2)
<i>Variables</i>		
Audited	0.0032 (0.0045)	0.0053 (0.0040)
Audited \times 1+ Irreg.	-0.0072 (0.0055)	-0.0074 (0.0048)
Audited \times 2+ Irreg.	-0.0124*** (0.0045)	-0.0116** (0.0048)
Audited \times 3+ Irreg.	-0.0036 (0.0030)	-0.0134** (0.0064)
<i>Fixed-effects</i>		
School x Stack	Yes	Yes
Year x Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	332	473
E[y]	0.0435	0.0459
Observations	52,209	78,619
R ²	0.49179	0.52056

Clustered (Municipality) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with the number of irregularities found in Education, separated by how many irregularities were found. *1+ Irreg.* an indicator for whether a municipality had between 1.0 and ~ 1.99 irregularities in education, and so on. The sample includes only municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. The sample consists only of municipalities which were audited, and for which the number of irregularities in the relevant categories is available. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. School-stack and year-stack fixed effects are used in columns (1) and (2), and microregion-stack fixed effects are used in columns (3) and (4) to address the multicollinearity between the fixed effects and the interacted terms. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019)

Table 9: Mechanism: Mayor Audit Effects

Dependent Variables: Model:	Elem. + Middle Sch.		High Shcool	
	Dropout Rate (1)	Failure Rate (2)	Dropout Rate (3)	Failure Rate (4)
<i>Variables</i>				
Mayor Audited	-0.0037** (0.0017)	-0.0055 (0.0037)	-0.0132 (0.0143)	-0.0339* (0.0183)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	4,130	4,130	450	450
E[y]	0.0316	0.0995	0.1169	0.0701
Observations	3,260,517	3,260,517	13,941	13,941
R ²	0.49726	0.50394	0.69241	0.62086

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (mayor audited in the last four years). The specification is $Y_{imth} = \beta MA_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is similar to the one discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Only municipal schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), INEP (2009-2019)

Table 10: Mechanism: Mayor Effects \times Reliance on School Programs

	School FE		Microregion FE	
Dependent Variable:	Dropout Rate			
Model:	(1)	(2)	(3)	(4)
<i>Variables</i>				
Mayor Audited	-0.0023 (0.0017)	0.0052*** (0.0010)	-0.0054*** (0.0019)	0.0032*** (0.0009)
Above Median Perc. Students in School Bus	0.0032*** (0.0006)		0.0051*** (0.0004)	
Mayor Audited \times Above Median Perc. Students in School Bus	-0.0011 (0.0018)		0.0029* (0.0017)	
Above Median Per Capita CCT Value		-0.0162* (0.0083)		0.0027*** (0.0009)
Mayor Audited \times Above Median Per Capita CCT Value		-0.0100*** (0.0017)		-0.0093*** (0.0020)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
microreg_cod_stack		Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	4,130	4,130	4,130	4,130
E[y]	0.0316	0.0316	0.0316	0.0316
Observations	3,275,743	3,275,743	3,275,743	3,275,743
R ²	0.49640	0.49626	0.19799	0.19691

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates indicating a municipality's relative reliance on social programs. The specification is $Y_{imth} = \beta MA_{mth} \times \mathbb{1}\{Program\}_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is similar to the one discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Only municipal schools are included in the sample. $\mathbb{1}\{Program\}$ relates to the School Bus program, or Bolsa Família. *Above Median Perc. Students in School Bus* is an indicator for whether a school's percentage of students using school buses to get to school is greater than the median school's. Similarly, *Above Median Per Capita CCT Value* is an indicator for whether the total amount of Bolsa Família (CCT) transfers per capita in a municipality was greater than the median municipality in the sample of eligible municipalities. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. School-stack and year-stack fixed effects are used in columns (1) and (2), and microregion-stack fixed effects are used in columns (3) and (4) to address the multicollinearity between the fixed effects and the interacted terms. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019), School Census (2009-2019), IPEA (2009-2019)

Table 11: Mechanism: Mayor Effects \times Number of Irregularities

	Educ. Irreg.	School Transport Irreg.	CCT Irreg.
Dependent Variable:	Dropout Rate		
Model:	(1)	(2)	(3)
<i>Variables</i>			
Mayor Audited	-0.0006 (0.0037)	-0.0022 (0.0038)	-0.0017 (0.0036)
Mayor Audited \times N. Irreg. Educ.	-0.0026* (0.0014)		
Mayor Audited \times N. Irreg. Sch. Transport		-0.0043 (0.0065)	
Mayor Audited \times N. Irreg. CCT			-0.0012 (0.0012)
<i>Fixed-effects</i>			
School x Stack	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes
<i>Fit statistics</i>			
Clusters	259	259	259
E[y]	0.0438	0.0438	0.0438
Observations	40,202	40,202	40,202
R ²	0.48552	0.48524	0.48527

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (mayor audited in the last four years) interacted with covariates which count the number of irregularities per service order across several of the accounts which were audited. The specification is $Y_{imth} = \beta MA_{mth} \times N.Irreg.mth + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section VII. *N. Irreg. Educ* is the number of education irregularities found per audit service order related to education (*School Transport* irregularities are a subset of these education irregularities). *N. Irreg. CCT* is the number of irregularities per service order related to any of the social programs that were audited as a result of the audits. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. The sample consists only of municipalities which were audited, and for which the number of irregularities in the relevant categories is available. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. School-stack and year-stack fixed effects are used in columns (1) and (2), and microregion-stack fixed effects are used in columns (3) and (4) to address the multicollinearity between the fixed effects and the interacted terms. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019)

Table 12: Mechanism: Mayor Effects by Term-Status

	Educ.	Irreg.	CCT	Irreg.
Dependent Variable:			Dropout Rate	
Model:	(1)		(2)	
<i>Variables</i>				
Mayor Audited	0.0086*		0.0083*	
	(0.0045)		(0.0049)	
Second Term Mayor Audited	-0.0093		-0.0095	
	(0.0057)		(0.0059)	
Second-Term	0.0003		-0.0002	
	(0.0020)		(0.0020)	
Mayor Audited × N. Irreg. Educ.	-0.0091***			
	(0.0029)			
Second Term Mayor Audited × N. Irreg. Educ.	0.0074			
	(0.0047)			
Mayor Audited × N. Irreg. CCT			-0.0085***	
			(0.0027)	
Second Term Mayor Audited × N. Irreg. CCT			0.0083	
			(0.0060)	
<i>Fixed-effects</i>				
School x Stack	Yes		Yes	
Year x Stack	Yes		Yes	
<i>Fit statistics</i>				
Clusters	332		332	
E[y]	0.0446		0.0446	
Observations	46,353		46,353	
R ²	0.51203		0.51183	

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (mayor audited in the last four years) interacted with covariates which count the number of irregularities per service order across several of the accounts which were audited. The specification is discussed in Section VI. *N. Irreg. Educ* is the number of education irregularities found per audit service order related to education (*School Transport* irregularities are a subset of these education irregularities). *N. Irreg. CCT* is the number of irregularities per service order related to any of the social programs that were audited as a result of the audits. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. The sample consists only of municipalities which were audited, and for which the number of irregularities in the relevant categories is available. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019)

Table 13: Mechanism:
Bolsa Família vs. Education Irregularities

	Standard	Mayor
Dependent Variable:	Dropout Rate	
Model:	(1)	(2)
<i>Variables</i>		
Audited	0.0111*** (0.0014)	
Above Median Per Capita CCT Value	-0.0132** (0.0058)	-0.0135** (0.0057)
Above Median Irreg. (Educ)	-0.0396** (0.0199)	-0.0369** (0.0186)
Audited × Above Median Per Capita CCT Value	-0.0136*** (0.0029)	
Audited × Above Median Irreg. (Educ)	-0.0035 (0.0023)	
Above Median Per Capita CCT Value × Above Median Irreg. (Educ)	-0.0024 (0.0239)	-0.0075 (0.0225)
Audited × Above Median Per Capita CCT Value × Above Median Irreg. (Educ)	-0.0020 (0.0041)	
Mayor Audited		0.0027*** (0.0007)
Mayor Audited × Above Median Per Capita CCT Value		-0.0043*** (0.0014)
Mayor Audited × Above Median Irreg. (Educ)		-0.0005 (0.0011)
Mayor Audited × Above Median Per Capita CCT Value × Above Median Irreg. (Educ)		-0.0014 (0.0020)
<i>Fixed-effects</i>		
School x Stack	Yes	Yes
Year x Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	4,130	4,130
E[y]	0.0325	0.0325
Observations	3,526,934	3,526,934
R ²	0.49004	0.48998

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates indicating a municipality's relative reliance on social programs. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Only municipal schools are included in the sample. *Above Median Per Capita CCT Value* is an indicator for whether the total amount of Bolsa Família (CCT) transfers per capita in a municipality was greater than the median municipality in the sample of eligible municipalities. *Above Median Irreg.* is an indicator for whether the municipality had more irregularities than the median audited municipality. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. Column (1) shows the results for the standard treatment definition (municipality was audited), and column (2) shows the results for the mayor treatment definition. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019), School Census (2009-2019), IPEA (2009-2019)

Table 14: Mechanism:
Bolsa Família vs. CCT Irregularities

	Standard	Mayor
Dependent Variable:	Dropout Rate	
Model:	(1)	(2)
<i>Variables</i>		
Audited	0.0089*** (0.0014)	
Above Median Per Capita CCT Value	-0.0122** (0.0060)	-0.0129** (0.0058)
Above Median Irreg. (CCT)	-0.0199*** (0.0009)	-0.0182*** (0.0008)
Audited × Above Median Per Capita CCT Value	-0.0144*** (0.0028)	
Audited × Above Median Irreg. (CCT)	0.0020 (0.0022)	
Above Median Per Capita CCT Value × Above Median Irreg. (CCT)	-0.0227* (0.0132)	-0.0265** (0.0128)
Audited × Above Median Per Capita CCT Value × Above Median Irreg. (CCT)	-0.0034 (0.0042)	
Mayor Audited		0.0022*** (0.0005)
Mayor Audited × Above Median Per Capita CCT Value		-0.0051*** (0.0012)
Mayor Audited × Above Median Irreg. (CCT)		0.0009 (0.0011)
Mayor Audited × Above Median Per Capita CCT Value × Above Median Irreg. (CCT)		-0.0009 (0.0020)
<i>Fixed-effects</i>		
School x Stack	Yes	Yes
Year x Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	4,130	4,130
E[y]	0.0325	0.0325
Observations	3,526,934	3,526,934
R ²	0.49004	0.48998

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates indicating a municipality's relative reliance on social programs. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Only municipal schools are included in the sample. *Above Median Per Capita CCT Value* is an indicator for whether the total amount of Bolsa Família (CCT) transfers per capita in a municipality was greater than the median municipality in the sample of eligible municipalities. *Above Median Irreg.* is an indicator for whether the municipality had more irregularities than the median audited municipality. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. Column (1) shows the results for the standard treatment definition (municipality was audited), and column (2) shows the results for the mayor treatment definition. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019), School Census (2009-2019), IPEA (2009-2019)

XI Appendix

Table A1: Corruption Effects: Standardized Exam Scores

Dependent Variables: Model:	Portuguese (5th) (1)	Portuguese (9th) (2)	Math (5th) (3)	Math (9th) (4)
<i>Panel A</i>				
N. Irreg. Educ.	-0.0893* (0.0538)	-0.0245 (0.0660)	-0.0968* (0.0566)	-0.0241 (0.0652)
<i>Panel B</i>				
N. Irreg. CCT	-0.0665 (0.0569)	-0.1155* (0.0595)	-0.0868 (0.0550)	-0.1955*** (0.0666)
<i>Fixed-effects</i>				
State	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	385	276	385	276
E[y]	-0.0878	-0.2103	-0.0884	-0.1939
Observations	2,278	1,010	2,278	1,010
R ²	0.50454	0.38107	0.50446	0.40877

Clustered (Municipality) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table shows the regression coefficients for the variables which measure the count of irregularities per service order and is an approximate replication of [Ferraz et al. \(2012\)](#). The specification is $Y_{it} = \gamma C_{it} + \delta_s + \lambda_t + X_{it} + \epsilon_{it}$ and is discussed in Section IV. The sample is a cross-section of audited municipalities in the years when they are audited. Each column represents a different regression, with the dependent variable listed at the column header. Rows represent different categories of irregularities. Note that school transportation and food irregularities are both subsets of education irregularities. Only municipal public elementary and middle 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 a high school degree, percentage urban, and Gini coefficient). Standard errors are clustered at the municipality level. Dependent variable means are reported below the coefficients.

Sources: CGU (2006-2015), Prova Brasil (2009, 2011, 2013), IPEA (2000,2010)

Table A2: Audit Effects: Standardized Exam Scores

Dependent Variables: Model:	Math Scores (5th Grade) (1)	Math Scores (9th Grade) (2)	Port. Scores (5th Grade) (3)	Port. Scores (9th Grade) (4)
<i>Variables</i>				
Audited	-0.0341 (0.0407)	-0.0183 (0.0376)	-0.0279 (0.0423)	-0.0284 (0.0447)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	3,958	2,676	3,958	2,676
E[y]	0.0171	-0.1658	0.0252	-0.1466
Observations	356,526	157,393	356,526	157,393
R ²	0.87168	0.87328	0.87353	0.85322

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{imth} = \beta A_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), Prova Brasil (2009, 2011, 2013)

Table A3: Corruption Effects: Schools

Dependent Variables: Model:	Overall		Elementary School		Middle School		High School	
	N. Classrooms (1)	N. Enrolled (2)	N. Teachers (3)	N. Enrolled (4)	N. Teachers (5)	N. Enrolled (6)	N. Teachers (7)	
<i>Variables</i>								
N. Irreg. Educ.	-0.1379 (0.0911)	-1.610* (0.9692)	-0.0145 (0.0493)	0.9045 (5.403)	-0.0221 (0.1827)	0.0542 (0.2115)	0.0339 (0.0931)	
<i>Fixed-effects</i>								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>								
Clusters	778	778	775	778	775	778	775	775
E[y]	3.975	19.01	1.330	128.3	6.465	0.8236	1.515	
Observations	16,881	16,885	16,845	16,885	16,845	16,885	16,845	
R ²	0.21929	0.08987	0.07894	0.12206	0.08032	0.01260	0.03656	

Clustered (Municipality) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table shows the regression coefficients for the variables which measure the count of irregularities per service order. The specification is $Y_{it} = \gamma C_{it} + \delta_s + \lambda_t + X_{it} + \epsilon_{it}$ and is discussed in Section IV. The sample is a cross-section of audited municipalities in the years when they are audited. Each column represents a different regression, with the dependent variable listed at the column header. Rows represent different categories of irregularities. Note that school transportation and food irregularities are both subsets of education irregularities. Only municipal public elementary and middle 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 a high school degree, percentage urban, and Gini coefficient). Standard errors are clustered at the municipality level. Dependent variable means are reported below the coefficients.

Sources: CGU (2006-2015), School Census (2007-2015), IPEA (2000,2010)

Table A4: Stacked DiD: Audit Effects - Schools

Dependent Variables: Model:	Overall		Elementary School		Middle School		High School	
	N. Classrooms (1)	N. Enrolled (2)	N. Teachers (3)	N. Enrolled (4)	N. Teachers (5)	N. Enrolled (6)	N. Teachers (7)	
<i>Variables</i>								
Audited	-0.0286 (0.0326)	-0.3657 (0.4582)	-0.0711 (0.0509)	1.978* (1.197)	-0.1782 (0.1976)	0.1335 (0.1363)	-0.0693 (0.1006)	
<i>Fixed-effects</i>								
School x Stack	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Year x Stack	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
<i>Fit statistics</i>								
Clusters	4,136	4,136	4,133	4,136	4,133	4,136	4,133	
E[y]	4.958	20.34	1.832	151.4	7.783	0.7040	2.101	
Observations	3,275,042	3,320,390	3,316,111	3,320,390	3,316,111	3,320,390	3,316,111	
R ²	0.73695	0.88888	0.32172	0.96588	0.29230	0.85675	0.23072	

Clustered (Municipality) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{imth} = \beta A_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), School Census (2009-2019)

Table A5: Corruption Effects: Public Services

Dependent Variables:	Perc. HH w. Open Trash	Perc. HH w. Collected Trash	Perc. HH w. Public Water	Perc. HH w. Public Sewage	Perc. HH w. Electricity
Model:	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
N. Irreg.	0.0061* (0.0036)	0.0016 (0.0041)	0.0131** (0.0051)	0.0064 (0.0061)	-0.00004 (0.0029)
<i>Fixed-effects</i>					
State	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>					
Clusters	1,030	1,077	1,077	1,039	1,078
E[y]	0.1490	0.6313	0.6505	0.2604	0.9087
Observations	1,114	1,162	1,161	1,116	1,163
R ²	0.65269	0.75927	0.56401	0.67283	0.55389

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the regression coefficients for the variables which measure the count of irregularities per service order. The specification is $Y_{mt} = \gamma C_{mt} + \delta_s + \lambda_t + X_{mt} + \epsilon_{mt}$ and is discussed in Section IV. The sample is a cross-section of audited municipalities in the years when they are audited. Each column represents a different regression, with the dependent variable listed at the column header. Rows represent different categories of irregularities. Note that school transportation and food irregularities are both subsets of education irregularities. All regressions use state and year fixed effects, as well as various municipal characteristics (log GDP, log population, percentage of residents with a high school degree, percentage urban, and Gini coefficient). Standard errors are clustered at the municipality level. Dependent variable means are reported below the coefficients.

Sources: CGU (2006-2015), SIAB (2006-2015), IPEA (2000,2010)

Table A6: Stacked DiD: Audit Effects Public Services

Dependent Variables:	Perc. HH w. Open Trash	Perc. HH w. Collected Trash	Perc. HH w. Public Water	Perc. HH w. Public Sewage	Perc. HH w. Electricity
Model:	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
Audited	-0.0083*** (0.0020)	0.0048** (0.0024)	0.0010 (0.0023)	-0.0033 (0.0023)	0.0081*** (0.0022)
<i>Fixed-effects</i>					
Munic. x Stack	Yes	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>					
Clusters	4,400	4,493	4,490	4,475	4,493
E[y]	0.1190	0.6583	0.6732	0.3086	0.9308
Observations	222,776	237,333	236,846	228,354	237,426
R ²	0.96347	0.97064	0.96283	0.98460	0.90415

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{mth} = \beta A_{mth} + \mu_{mh} + \lambda_{th} + \epsilon_{mth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2006 and 2015, and covers the window of [-2,+4] years in relation to the audit year. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), SIAB (2006-2015)

Table A7: Stacked DiD: Mayor Audit Effects Public Services

Dependent Variables:	Perc. HH w. Open Trash	Perc. HH w. Collected Trash	Perc. HH w. Public Water	Perc. HH w. Public Sewage	Perc. HH w. Electricity
Model:	(1)	(2)	(3)	(4)	(5)
<i>Variables</i>					
Mayor Audited	-0.0058*** (0.0019)	0.0026 (0.0025)	-0.00007 (0.0022)	-0.0050** (0.0021)	0.0054*** (0.0020)
<i>Fixed-effects</i>					
Munic. x Stack	Yes	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>					
Clusters	4,397	4,490	4,487	4,469	4,490
E[y]	0.1190	0.6582	0.6732	0.3087	0.9308
Observations	221,494	235,975	235,493	227,061	236,068
R ²	0.96376	0.97075	0.96295	0.98468	0.90504

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (mayor audited in the last four years). The specification is $Y_{mth} = \beta MA_{mth} + \mu_{mh} + \lambda_{th} + \epsilon_{mth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2006 and 2015, and covers the window of [-2,+4] years in relation to the audit year. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), SIAB (2006-2015)

Table A8: Stacked DiD: Bolsa Família

	Full Sample	School Sample
Dependent Variable:	N. Families per 1000 residents	
Model:	(1)	(2)
<i>Variables</i>		
Audited	-0.0005 (0.0059)	-0.0013 (0.0068)
<i>Fixed-effects</i>		
Munic. x Stack	Yes	Yes
Year x Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	4,567	4,303
E[y]	0.9019	0.9062
Observations	250,385	190,994
R ²	0.94519	0.94879

Clustered (Municipality) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{mth} = \beta A_{mth} + \mu_{mh} + \lambda_{th} + \epsilon_{mth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2006 and 2015, and covers the window of [-2,+4] years in relation to the audit year. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), IPEA (2004-2019)

Table A9: Stacked DiD: Audit Effects by Grades

	<i>1st – 5th Grades</i>	<i>6th – 9th Grades</i>	<i>Elem. + Middle Sch. (Overall)</i>
Dependent Variables:	Dropout Rate	Dropout Rate	Dropout Rate
Model:	(1)	(2)	(3)
<i>Variables</i>			
Audited	-0.0034* (0.0019)	-0.0029 (0.0025)	-0.0034** (0.0016)
<i>Fixed-effects</i>			
School x Stack	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes
<i>Fit statistics</i>			
Clusters	4,129	4,033	4,130
E[y]	0.0289	0.0567	0.0325
Observations	3,424,908	1,081,892	3,526,934
R ²	0.43827	0.60792	0.48997

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) disaggregated by the level of education of the students (listed on the column headers). The specification is $Y_{imth} = \beta A_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). 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 school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), INEP (2009-2019)

Table A10: Audit Effects: Prenatal Visits

	Full Sample	School Sample
Dependent Variable:	N. Prenatal Visits	
Model:	(1)	(2)
<i>Variables</i>		
Audited	-0.0034 (0.0614)	0.0060 (0.0701)
<i>Fixed-effects</i>		
Munic. x Stack	Yes	Yes
Year x Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	3,872	3,673
E[y]	5.034	5.056
Observations	126,923	106,021
R ²	0.62318	0.60956

Clustered (Municipality) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (mayor audited in the last four years). The specification is $Y_{mth} = \beta A_{mth} + \mu_{mh} + \lambda_{th} + \epsilon_{mth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2006 and 2015, and covers the window of [-2,+4] years in relation to the audit year. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Column (1) uses the full sample (2006-2019), whereas column (2) uses only the sample of years for which school data was available (2009-2019).

Sources: CGU (2006-2015), SINASC (2006-2019)

Table A11: Stacked DiD: Prenatal Visits \times Bolsa Família

	Full Sample	School Sample
Dependent Variable:	N. Prenatal Visits	
Model:	(1)	(2)
<i>Variables</i>		
Audited	-0.2007* (0.1034)	-0.2207* (0.1132)
Audited \times Above Median Per Capita CCT Value	0.3115** (0.1246)	0.3213** (0.1350)
<i>Fixed-effects</i>		
Munic. \times Stack	Yes	Yes
Year \times Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	3,872	3,758
E[y]	5.034	5.044
Observations	126,923	117,340
R ²	0.62321	0.61634

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates indicating a municipality's relative reliance on Bolsa Família. The specification is $Y_{mth} = \beta A_{mth} \times \mathbb{1}\{\text{AboveMedianPerCapitaCCTValue}\}_{mth} + \mu_{mh} + \lambda_{th} + \epsilon_{mth}$. The sample includes all municipalities whose first audit took place between 2007 and 2015, and covers the window of [-2,+4] years in relation to the audit year. $\mathbb{1}\{\text{Above Median Per Capita CCT Value}\}$ is an indicator for whether the total amount of Bolsa Família (CCT) transfers per capita in a municipality was greater than the median municipality in the sample of eligible municipalities. Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Column (1) uses the full sample (2006-2019), whereas column (2) uses only the sample of years for which school data was available (2009-2019).

Sources: CGU (2006-2015), IPEA (2006-2019) , SINASC (2006-2019)

Table A12: Mechanism: Audit Effects \times Number of Irregularities

	Educ. Irreg.	CCT Irreg.
Dependent Variable:	N. Prenatal Visits	
Model:	(1)	(2)
<i>Variables</i>		
Audited	0.1249 (0.1821)	0.0323 (0.1709)
Audited \times N. Irreg. Educ.	0.2397*** (0.0849)	
Audited \times N. Irreg. CCT		0.2392* (0.1343)
<i>Fixed-effects</i>		
Munic. x Stack	Yes	Yes
Year x Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	557	755
E[y]	4.889	4.834
Observations	3,335	4,785
R ²	0.65499	0.66690

Clustered (Municipality) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates which count the number of irregularities per service order across several of the accounts which were audited. The specification is $Y_{mth} = \beta A_{mth} \times N. Irreg.{}_{mth} + \mu_{mh} + \lambda_{th} + \epsilon_{mth}$ and is discussed in Section VII. $N. Irreg. Educ$ is the number of education irregularities found per audit service order related to education. $N. Irreg. CCT$ is the number of irregularities per service order related to any of the social programs that were audited as a result of the audits. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. The sample consists only of municipalities which were audited, and for which the number of irregularities in the relevant categories is available. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), SINASC (2006-2019)

Table A13: Mechanisms: GDP vs. Bolsa Família

Dependent Variable: Model:	Dropout Rate	
	(1)	(2)
<i>Variables</i>		
Audited	0.0078*** (0.0018)	-0.0222* (0.0127)
GDP pc	0.3570*** (0.0821)	
Above Median Per Capita CCT Value	-0.0635** (0.0323)	0.1233*** (0.0352)
Audited × GDP pc	0.0671 (0.0810)	
Audited × Above Median Per Capita CCT Value	-0.0156*** (0.0029)	-0.0604** (0.0289)
GDP pc × Above Median Per Capita CCT Value	-0.5171*** (0.1214)	
Audited × GDP pc × Above Median Per Capita CCT Value	0.2157 (0.2028)	
log(GDP)		0.0126*** (0.0026)
Audited × log(GDP)		0.0030** (0.0013)
log(GDP) × Above Median Per Capita CCT Value		-0.0228*** (0.0016)
Audited × log(GDP) × Above Median Per Capita CCT Value		0.0061* (0.0032)
<i>Fixed-effects</i>		
School x Stack	Yes	Yes
Year x Stack	Yes	Yes
<i>Fit statistics</i>		
Clusters	4,124	4,124
E[y]	0.0316	0.0316
Observations	3,270,386	3,270,386
R ²	0.49645	0.49712

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years) interacted with covariates indicating a municipality's relative reliance on social programs as well as a measure of GDP (per capita, or log(GDP)). The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. $\mathbb{1}\{\text{AboveMedianPerCapitaCCTValue}\}$ is an indicator for whether the total amount of Bolsa Família (CCT) transfers per capita in a municipality was greater than the median municipality in the sample of eligible municipalities. $GDPpc$ is GDP per capita, and $\log(GDP)$ is the natural log of the total GDP for the municipality. Only municipal schools are included in the sample. Standard errors are clustered at the municipality level. School-stack and year-stack fixed effects are used across all columns. Each column represents a different regression, with the dependent variables listed on the column headers.

Sources: CGU (2006-2015), INEP (2009-2019), IPEA (2009-2019)

XII Robustness Tests

I perform a series of robustness tests to the main results discussed in Section VI to lend further credence to the validity of the results discussed therein. The first set of tests are displayed on [Table R1](#). Column 1 shows that the coefficients of the main regressions are unaffected by restricting the sample to a balanced sample of schools, and Column 2 shows that the results are also robust to the use of municipal fixed effects (interacted with the stack fixed effects).

Tables [R2](#) and [R3](#), on the other hand, show the results from using various methodologies to estimate the difference-in-differences coefficient; [Table R2](#) uses school fixed effects, and [Table R3](#) uses municipal fixed effects. In both tables, Column 1 corresponds to the [Sun and Abraham \(2021\)](#) coefficient, Column 2 corresponds to [Wing et al. \(2024\)](#), which specifically corrects for potential issues with the stacked difference-in-differences methodology, and Column 3 corresponds to the estimator from [Callaway and Sant'Anna \(2021\)](#). As seen from both tables, the magnitude of the estimates is largely unaffected, and the coefficients remain statistically significant. It should be noted that differences in the sample sizes are driven by how the various methods deal with unbalanced samples and always-treated observations.

Moreover, [Table R4](#) presents the results from [Table 4](#), but the robustness table clusters the standard errors at the municipality-year level. This is done to address the possibility that errors are correlated not only within municipalities over time, but also within municipalities in any given year. As seen from [Table R4](#), the statistical significance of the results robust to the choice of clustering level.

Finally, Tables [R5](#) and [R6](#) show that the results from the main specification are robust to the choice of disaggregating the analysis between municipal and state schools. Both tables consider all public schools in the municipalities, irrespective of funding source, and we can observe that the results are also robust to this choice.

Table R1: Stacked DiD: Robustness

	Elem. + Middle Sch.	
Dependent Variable:	Dropout Rate	
Model:	(1)	(2)
<i>Variables</i>		
Audited	-0.0035** (0.0017)	-0.0036*** (0.0008)
<i>Fixed-effects</i>		
School x Stack	Yes	
Year x Stack	Yes	Yes
Munic. x Stack		Yes
<i>Fit statistics</i>		
Clusters	4,027	4,129
E[y]	0.0305	0.0216
Observations	2,282,607	200,422
R ²	0.51716	0.84026

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{mth} = \beta A_{mth} + \mu_{mh} + \lambda_{th} + \epsilon_{mth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level. Column (1) presents the results from the main regressions at the school level, but restricting the sample to a balanced panel of schools (i.e. only schools for which dropout data is available for each period). Column (2) presents data aggregated to the municipality level, and thus uses municipal fixed effects.

Sources: CGU (2006-2015), INEP (2009-2019)

Table R2: Differences-in-Differences Robustness

	Sun & Abraham (2021)	Wing et al. (2024)	Callaway and Sant'Anna (2021)
Dependent Variable:	Dropout Rate	Dropout Rate	Dropout Rate
Model:	(1)	(2)	(3)
<i>Variables</i>			
ATT	-0.0039** (0.0019)	-0.0049*** (0.0017)	-0.0021*** (0.0006)
Observations	911,972	2,638,055	1,283,394

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows various difference-in-differences coefficients on the treatment variable (audited in the last four years) for different estimators ([Sun and Abraham \(2021\)](#), [Wing et al. \(2024\)](#), [Callaway and Sant'Anna \(2021\)](#)). The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. Treatment assignment is consistent with the rest of the paper. Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. The dependent variable listed on the column header.

Sources: CGU (2006-2015), INEP (2009-2019)

Table R3: Differences-in-Differences Robustness (Municipal Level)

	Sun & Abraham (2021)	Wing et al. (2024)	Callaway and Sant'Anna (2021)
Dependent Variable:	Dropout Rate	Dropout Rate	Dropout Rate
Model:	(1)	(2)	(3)
<i>Variables</i>			
ATT	-0.0038*** (0.0010)	-0.0041*** (0.0009)	-0.0028*** (0.0008)
Observations	59,072	179,500	280,473

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows various difference-in-differences coefficients on the treatment variable (audited in the last four years) for different estimators ([Sun and Abraham \(2021\)](#), [Wing et al. \(2024\)](#), [Callaway and Sant'Anna \(2021\)](#)). The data in the sample is aggregated to the municipality level and the sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. Treatment assignment is consistent with the rest of the paper. Standard errors are clustered at the municipality level, and municipality-stack and year-stack fixed effects are used in all specifications. The dependent variable listed on the column header.

Sources: CGU (2006-2015), INEP (2009-2019)

Table R4: Stacked DiD: Audit Effects - Robustness - Clustering

Dependent Variables: Model:	Elem. + Middle Sch.		High Shc.	
	Dropout Rate (1)	Failure Rate (2)	Dropout Rate (3)	Failure Rate (4)
<i>Variables</i>				
Audited	-0.0034* (0.0016)	-0.0067* (0.0036)	-0.0125 (0.0164)	-0.0145 (0.0191)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	13	13	13	13
E[y]	0.0325	0.1016	0.1176	0.0700
Observations	3,526,934	3,526,934	15,494	15,494
R ²	0.48997	0.49486	0.68711	0.62065

Clustered (Municipality & Year) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{imth} = \beta A_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Only municipal schools are included in the sample. A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality-year level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), INEP (2009-2019)

Table R5: Audit Effects: Municipal + State Schools

Dependent Variables: Model:	Elem. + Middle Sch.		High Shc.	
	Dropout Rate (1)	Failure Rate (2)	Dropout Rate (3)	Failure Rate (4)
<i>Variables</i>				
Audited	-0.0034** (0.0014)	-0.0072** (0.0030)	-0.0056 (0.0034)	0.0012 (0.0031)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	4,138	4,138	4,134	4,134
E[y]	0.0330	0.1010	0.0919	0.1024
Observations	4,380,915	4,380,915	638,139	638,139
R ²	0.51042	0.51307	0.60368	0.55835

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (audited in the last four years). The specification is $Y_{imth} = \beta A_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. *This table combines municipal and state schools.* A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), INEP (2009-2019)

Table R6: Mayor Audit Effects: Municipal + State Schools

Dependent Variables: Model:	Elem. + Middle Sch.		High Shc.	
	Dropout Rate (1)	Failure Rate (2)	Dropout Rate (3)	Failure Rate (4)
<i>Variables</i>				
Mayor Audited	-0.0028** (0.0011)	-0.0035 (0.0029)	-0.0043 (0.0032)	-0.0038* (0.0023)
<i>Fixed-effects</i>				
School x Stack	Yes	Yes	Yes	Yes
Year x Stack	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
Clusters	4,138	4,138	4,134	4,134
E[y]	0.0321	0.0990	0.0904	0.1024
Observations	4,070,108	4,070,108	598,738	598,738
R ²	0.51697	0.52181	0.61135	0.56891

Clustered (Municipality) standard-errors in parentheses

Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Note: This table shows the stacked difference-in-differences coefficients on the treatment variable (mayor audited in the last four years). The specification is $Y_{imth} = \beta MA_{mth} + \mu_{ih} + \lambda_{th} + \epsilon_{imth}$ and is similar to the one discussed in Section V. The sample includes all municipalities whose first audit took place between 2009 and 2015, and covers the window of [-2,+4] years in relation to the audit year. Once audited mayors leave office, the municipality is removed from the sample to avoid compromising the control group. *This table combines municipal and state schools.* A_{mth} is an indicator variable taking the value 1 for all years starting on the audit year, and 0 otherwise (it is always 0 for non-audited municipalities). Standard errors are clustered at the municipality level, and school-stack and year-stack fixed effects are used in all specifications. Each column represents a different regression, with the dependent variables listed on the column headers. Columns (1) and (2) show the audit effects for elementary and middle schools, whereas columns (3) and (4) show the same audit effects for high schools.

Sources: CGU (2006-2015), INEP (2009-2019)