It's not all about the money: How corruption undermines policy effectiveness.[*]

Christian Lukas Zünd

November 13, 2019

([Click here for the newest version](#))

Abstract

This paper demonstrates how corruption negatively impacts policy effectiveness in a program that is explicitly designed to be unsusceptible to bribery, clientelism, and the embezzlement of money: Brazil's Bolsa Família program. The program's centralized beneficiary selection process enables me to identify families that were randomly admitted or not admitted to Bolsa Família and to estimate the program's effectiveness in different years and municipalities. Exploiting a second natural experiment, I then show that Bolsa Família's effect on school enrollment increases by a third after a municipality has been audited at random. Using a theoretical model, administrative data, and a field experiment with 6,998 registration centers, I find that local corruption increases the probability that families successfully underreport their income when registering for Bolsa Família, making it harder to target the families that benefit most. Ruling out alternative explanations, I show that neither improved school attendance monitoring, administrative processes, infrastructure, complementary programs, tighter governance, nor increased whistleblowing can account for the increase in Bolsa Família's effectiveness after a municipality has been audited at random. Taken together, these results suggest that income underreporting can explain how, despite the program's safeguards, local corruption undermines the effectiveness of Bolsa Família.

---

[*] University of Zurich, Department of Economics, email: christian.zuend(at)econ.uzh.ch.

I. INTRODUCTION

Corruption is costly. While the direct costs of corruption—money diverted, embezzled, stolen—are substantial, they may well be outweighed by indirect effects (Kaufmann, 2005) such as underinvestment in human capital (Mo, 2001), higher child mortality (Gupta et al., 2001), decreased private investment (Mauro, 1995), lower returns to public investment (Tanzi and Davoodi, 1998), and slower economic growth (Mauro, 1995, 2004). This literature has mostly focused on two consequences of corruption: the effect of missing funds (through embezzlement, graft, inappropriate procurement) and of preferential access to public goods and services (in exchange for bribes, votes, or because of family or group membership). Leakage of funds and preferential access to public goods reduces the effectiveness of governments to provide adequate education (e.g., Reinikka and Svensson, 2005; Ferraz et al., 2012; Abdulai and Hickey, 2016), run medical systems (e.g., McPake et al., 1999; Holmberg and Rothstein, 2011; Mostert et al., 2015), enforce traffic regulation (e.g., Bertrand et al., 2007; Olken and Barron, 2009), and operate poverty-relief programs (e.g., Olken, 2006; Penfold-Becerra, 2007).However, even if effective safeguards against bribery, clientelism, and embezzlement are in place, government programs are not necessarily immune to the corrosive impact of corruption.

This paper shows how corruption negatively impacts policy effectiveness in a program that is explicitly designed to be unsusceptible to bribery, clientelism, and the embezzlement of money. Brazil's Bolsa Família promotes educational participation by paying a monthly benefit to approximately 14 million families if their children attend school regularly. These funds are transferred directly to the beneficiaries so that local officials cannot pocket the money. Not only are local officials bypassed in the payment process, they are also not involved in selecting the families that benefit from Bolsa Família; the selection of beneficiaries is anonymized and conducted through a central process to prevent local officials from controlling access to the program to extract rents or to benefit their supporters. As a result, Bolsa Família is generally considered an exception to Brazil's otherwise widespread clientelism (Sugiyama and Hunter, 2013).[1]

One possible explanation for the negative effect of corruption is that it enables families to underreport their income to gain or keep Bolsa Família; even if corrupt *officials* can not embezzle funds, local corruption can make it easier for *families* to cheat. Accusations of families underreporting their income are common both in the media and in the political discussion (e.g., Brazil, 2016; Caram, 2016; Fausto Macedo, 2016; OGlobo, 2016). However, although it is generally assumed that income underreporting decreases the effectiveness of programs like Bolsa Família, this assumption has not previously been tested. This paper provides causal evidence

---

1. The prevention of clientelism at the local level does not ensure that politicians at the federal level cannot expand the program for electoral reasons. How large these electoral gains are and whether this constitutes a form of clientelism is hotly debated (e.g., Hunter and Power, 2007; Zucco, 2009; Bohn, 2011; Daïeff, 2015) but is beyond the scope of this paper.

linking local corruption, families' ability to successfully underreport their income, and lower causal effects of Bolsa Família on school enrollment.

Estimating the impact of corruption on the effectiveness of government policy is challenging for two main reasons: First, it requires that corruption varies sufficiently much between the places or during the time where the programs are evaluated—with all the usual caveats about the endogeneity of corruption. To address this challenge, I use a well-established natural experiment, Brazil's audit lottery program. These audits of randomly selected municipalities have been shown to decrease corruption in subsequent years (Avis et al., 2018), thus providing exogenous variation in corruption between different places and years.

The main challenge is to measure the causal effect of the policy on school enrollment in different regions and times, so that changes in its effectiveness can be estimated. While it is common to use the accuracy of targeting as a proxy for the effectiveness of anti-poverty programs, this is primarily done because no appropriate measure of program effectiveness is available (De Janvry et al., 2012). Indeed, as pointed out by Ravallion (2009), better targeting does not guarantee that a program is actually more effective. Thus, even if there is plausibly exogenous variation in corruption, one also needs a second identification strategy that provides causal estimates of the effects of the policy in different regions or times.

To estimate the effectiveness of Bolsa Família in different regions and times, I develop a novel identification strategy that makes use of the program's central automated selection process. By using data from the Cadastro Único, the official database used for beneficiary selection, I can reconstruct the algorithm's priority strata to identify families on the margin of the program that were randomly included or not included in Bolsa Família. Because the beneficiary selection process uses only data in the Cadastro Único, there are no *unobservable* differences that affect who gets included in the program. This built-in conditional independence allows me to estimate the causal effect of Bolsa Família on school enrollment for different years, priority strata, and municipalities. The identification strategy passes several validation tests, and it successfully recovers the positive impact of the program documented by others (e.g., Cardoso and Souza, 2003; Glewwe and Kassouf, 2012; Schaffland, 2012; De Brauw et al., 2015).

This identification strategy has several advantages over existing approaches to estimating the effect of Bolsa Família. First, because it uses the same data as the selection algorithm, the identification strategy can identify families that were actually on the margin of the program, unlike many evaluations that rely on propensity scores to match families with similar probabilities of being included in Bolsa Família (e.g., Cardoso and Souza, 2003; Schaffland, 2012). Second, the estimated treatment effects are unbiased, conditional on self-reported income, because the identification strategy uses the official database to identify marginal families based on their income *before* they are included in Bolsa Família.[2] Third, unlike approaches that use variation in

---

2. Self-reported incomes in other data sets are likely to differ from data in the Cadastro Único, where families have stronger incentives to underreport their incomes. Moreover, relative to non-beneficiaries, beneficiary

the program's rollout or geographic coverage (e.g., Glewwe and Kassouf, 2012), it can estimate the effectiveness of the program at the municipality level and at different times. Finally, while De Janvry et al. (2012) measure policy effectiveness at the municipality level, they estimate it for Bolsa Escola, Bolsa Família's predecessor program. One crucial difference between the two programs is that municipalities selected the beneficiaries for Bolsa Escola. As a result, the critical assumption that future beneficiaries and non-beneficiaries have similar trends in educational participation, conditional on child characteristics, need not hold for Bolsa Escola,[3] but it is guaranteed by Bolsa Família's centralized beneficiary selection process.

The first main result of the paper is that Bolsa Família is less effective in municipalities with more corruption, even though local officials cannot divert money from the program and have no say in the selection of beneficiaries. Using Brazil's initiative to randomly audit municipalities as an exogenous shock to corruption, I show that Bolsa Família's effect on school enrollment increases by about a third after a random audit. This finding is highly robust. Most importantly, it persists if the sample is defined more restrictively and for different specifications of the two natural experiments.

I then test whether income underreporting can account for this result. Because municipalities are responsible for registering potential beneficiaries, local corruption can play the most prominent role at the registration stage. Using a theoretical model, administrative data, and a field experiment, I show that local corruption increases the probability that families successfully underreport their income. This makes it harder to target the families that benefit most from Bolsa Família, which decreases the effectiveness of the program.

I consider a simplified model of Bolsa Família's registration process, where families decide what income to report, and the families with the lowest self-reported income are included in the program. If a family's reported income deviates too much from its actual income, it risks being caught and the probability of detection is lower in municipalities with more corruption. The first part of the registration model predicts that after a random audit income underreporting decreases together with local corruption. As more families underreport their income, Bolsa Família can no longer target the families that benefit most, and its effectiveness decreases. Thus, the second part of the model explains how Bolsa Família becomes more effective after a random audit and makes testable predictions about the profile of families that will see the most substantial effectiveness gains.

households are more likely to underreport their incomes in other surveys in order to avoid detection, so that methods that match or weight beneficiary and non-beneficiary families based on their reported income *after* treatment assignment (e.g., Cardoso and Souza, 2003; De Brauw et al., 2015) are potentially biased. Strategic income reporting is also problematic for regression discontinuity designs (e.g., Schaffland, 2012) that rely on the assumption that families with a self-reported income around Bolsa Família's income threshold are relatively similar.

3. Indeed, as program effectiveness depends on selecting the children with the biggest expected *gains* from inclusion, municipalities' selections might well be affected by characteristics that are unobservable to the researchers.

As predicted by the registration model, fewer families claim to have zero income, and fewer families report an income that is eligible for Bolsa Família after a municipality has been audited at random. Moreover, just after a random audit the number of ineligible families that are detected and excluded from the Bolsa Família program skyrockets, but then falls to a lower level than before the audit, suggesting that the audits indeed increase the probability that ineligible families are detected and discourage new registrants from underreporting their income. As the second part of the model predicts, the treatment effect increases most for families that were not visited by a social worker during the registration process and for families that have the strongest incentives to misrepresent their incomes.

As direct evidence for the income underreporting explanation, I conducted a field experiment with 6,998 Bolsa Família registration centers to show that local administrators are indeed less likely to register ineligible families after a random audit. Fictitious applicants asked about the possibility of receiving Bolsa Família, and their characteristics were experimentally varied to make them eligible or ineligible while holding everything else constant. Consistent with the income underreporting mechanism, registration centers in audited municipalities are significantly less likely to engage with ineligible families and are more likely to point out when a family is ineligible for Bolsa Família. Taken together, these findings suggest that the effectiveness gains of Bolsa Família are the result of improvements in the way the program is targeted to the families that benefit from it the most.

Finally, I test several alternative explanations for the increased effectiveness of Bolsa Família after a random audit. To show that embezzled or otherwise diverted funds can indeed not explain the effect of local corruption on the program's effectiveness, I quantify the maximum amount of funding that local administrators can pocket through various strategies. Less than 0.0001% of funds are paid to stolen benefit cards, and the rate of income underreporting is more than ten times smaller among public servants than in the general population. Thus, the financial damages are negligible and cannot account for the effectiveness gains. As municipalities are also responsible for monitoring school attendance, I test whether improvements in monitoring contribute to the effect. Moreover, data from Bolsa Família's internal control programs show that unintentional errors and outdated information do not account for the gains in program effectiveness. Similarly, neither changes in administrative processes, nor better-equipped registration centers can explain the results. Finally, I rule out that improved oversight is behind the effectiveness gains, using data on municipalities' social governance councils and Bolsa Família's whistleblowing systems.

To my knowledge, this paper presents the first evidence that links income underreporting to lower treatment effects of Bolsa Família or a similar conditional cash transfer program. While evidence for income underreporting is not new,[4] showing that it can account for the negative

---

4. In addition to cases uncovered by the press and by government audits, there is also the alarming observation that the number of families receiving Bolsa Família exceeds the number of eligible families estimated from

impact of corruption on the program's effectiveness fills a significant gap in the literature given the attention it receives in the public discussion of Bolsa Família and similar conditional cash transfer programs. The results suggest that efforts to detect underreporting by Brazil's federal government (e.g., Brazil, 2016) probably increase the effectiveness of Bolsa Família in the long run.

The previous research that the findings of this paper are most closely related to is De Janvry et al. (2012), who study the effects of electoral incentives on the effectiveness of Bolsa Escola and show that the program is more effective at reducing school dropout if a mayor faces reelection. My paper differs from theirs in three essential aspects: First, the focus of my paper is on the causal impact of corruption, whereas De Janvry et al. (2012) study electoral incentives. Second, municipalities were in charge of selecting the beneficiaries under Bolsa Escola. Thus, my paper shows that even with Bolsa Família's additional safeguards against corruption, program effectiveness is still affected by local corruption. Finally, as mentioned earlier, Bolsa Família's centralized selection process allows me to make a much stronger case against selection and unobserved variable bias when estimating the program's effectiveness. The results are also related to a recent paper by Brollo et al. (2019), who provide evidence that mayors strategically manipulate the enforcement of school attendance conditionalities for electoral reasons. While I find no evidence that school attendance monitoring contributes substantially to the gains in program effectiveness, the results reported by Brollo et al. (2019) suggest that electoral motives might explain why some administrators are more lenient when verifying families' incomes.

This paper contributes to the discussion of the effective targeting of social programs. The discussion commonly focuses on the advantages and drawbacks of different targeting schemes (e.g., Alderman, 2002; Ravallion, 2008; Alatas et al., 2012; Stoeffler et al., 2016) or the financial consequences of mistargeting and elite capture (Alatas et al., 2019). However, better targeting does not necessarily imply that anti-poverty programs also perform better (Ravallion, 2009), and the actual implications of mistargeting for the effectiveness of these programs are rarely tested. The evidence presented here makes a strong case that improved targeting indeed increases the effectiveness of Bolsa Família. Given the importance of accurate self-reporting and the challenges of income verification for the effective targeting of Bolsa Família and similar programs, the final section of this paper suggests possible interventions to increase the likelihood of accurate reporting.

Taken together, the results of this paper suggest that income underreporting can explain how—despite Bolsa Família's safeguards against bribery, clientelism, and embezzlement—local corruption undermines the effectiveness of the program. Even though Bolsa Família is often cited as an example of how to safeguard anti-poverty programs against corruption, it is significantly

---

the census in a significant share of municipalities (Fried, 2012). Moreover, Firpo et al. (2014) describe irregularities in the distribution of reported incomes around the eligibility threshold that are consistent with income underreporting or with a temporary reduction of labor supply to qualify for the program.

more effective after a random anti-corruption audit. Thus, the results highlight the positive effect of government audits (e.g., Di Tella and Schargrodsky, 2003; Olken, 2007; Bobonis et al., 2016; Avis et al., 2018).

The paper is organized as follows: Section II describes the relevant institutional details of the Bolsa Família program. Section III discusses how the beneficiary selection process can be used to estimate the causal effects of the program. Section IV shows the main finding of the paper, that local corruption decreases the effectiveness of Bolsa Família. Section V introduces a theoretical model of how income underreporting affects the program's effectiveness, which is then tested using administrative data and a field experiment. Section VI addresses the limited ways in which corrupt local officials can benefit financially from Bolsa Família. Section VII examines to what degree other explanations can contribute to the effect. Section VIII discusses the policy implications of the paper and concludes.

II. Institutional Background: Brazil's Bolsa Família program

Brazil's Bolsa Família program bypasses local officials for both the payment and the beneficiary selection process, making it a suitable setting to study effects of corruption other than clientelism and embezzlement. This section describes four features of the Bolsa Família program that are relevant for this study: First, I explain how Bolsa Família incentivizes educational participation through conditional cash transfers, which allows me to use the program's impact on school enrollment as a measure of its effectiveness. Second, I turn to the centralized process to select the families to include in Bolsa Família, which forms the backbone of the identification strategy that allows me to identify the program's effectiveness in different municipalities. I then discuss Bolsa Famiília's safeguards against embezzlement and abuse of the program for electoral gains, before turning to its remaining vulnerabilities to other forms of corruption.

A. Bolsa Família's Incentives for Educational Participation

Bolsa Família, the world's largest conditional cash transfer program, covers approximately 14 million families and pays a monthly benefit provided that families comply with several conditionalities, including regular school attendance. It is arguably Brazil's main federal initiative to increase educational participation, both in terms of its size and its prominence in the public discourse.

In addition to guaranteeing a minimum income for extremely poor households, Bolsa Família seeks to combat the intergenerational transmission of poverty by conditioning some of the payments on school attendance, vaccination, and medical checkups (Lindert et al., 2007). To this aim, the program combines unconditional benefits to families in extreme poverty with

7

conditional transfers to poor households with pregnant women and children.[5] The program was created in early 2004 through a merger of four predecessor programs; Fome Zero and Bolsa Alimentação were focused on nutrition, Auxílio Gas subsidized cooking gas, and Bolsa Escola was a conditional cash transfer program to increase school enrollment.[6] The consolidation of Brazil's anti-poverty initiatives began with the creation of a shared centralized database,[7] the Cadastro Único, in 2001 and culminated with the establishment of the Ministério do Desenvolvimento Social (MDS) in 2004. The Cadastro Único serves as the main registry for data on potential beneficiaries of Brazil's anti-poverty programs and is maintained by the MDS and the state-owned federal savings bank, the Caixa Ecônomica Federal (Caixa).

Under the original rules, households with a per capita income of less than R$ 50 (the extreme poverty line) were considered extremely poor and received an unconditional basic transfer of R$ 50, and all families with a per capita income of less than R$ 100 (the poverty line) were eligible for variable benefits of R$ 15 for up to three pregnant women or children aged 0 to 15, provided that they comply with the educational and health conditionalities. Although most closely associated with the government of Luiz Inácio Lula da Silva, every government since has significantly expanded eligibility and benefits under the program: The eligibility thresholds have subsequently increased to R$ 85 and R$ 170, the basic benefit has been increased to R$ 89, variable benefits stand at R$ 41 are now paid for up to five children ages 0 to 15 and one pregnant female, and additional benefits for adolescents (16 to 17) were introduced in 2012 (currently paying R$ 48 for up to two adolescents). Also introduced was an additional payment individually calibrated such that beneficiaries' post-transfer income per capita (including Bolsa Família) reaches the extreme poverty threshold.

Once a family is included in Bolsa Família, it is required to adhere to a schedule of medical checkups and vaccinations for pregnant women, nursing mothers, and young children, and to ensure that children aged 6 to 15 attend school at least 85% of the time and adolescents at least 75% of the time. The exact conditionalities are decided by the ministries of health and education, that then train municipal workers to monitor and report program compliance through the federal government's online systems. Failure to comply initially results in a warning. If during the next six months the family does not fulfill the conditionalities, the payment is withheld for one month but can be withdrawn in the next if the family addresses the problem. A third or fourth month of non-compliance results in a two-month suspension of the benefit. During this time no benefit is paid and the family is on probation. Failure to comply during this period results in the cancellation of the benefit and exclusion of the family from the program (MDS and SENARC, 2018).

---

5. For the purpose Bolsa Família, Lei n° 10.836 defines a family as a "nuclear family, possibly extended by other individuals who have kinship or affinity with it, who form a domestic group, live under the same roof, and support each other." I will use the terms family and household interchangeably.

6. Lei n° 10.836 (January 9, 2004)

7. Decreto n° 3.877 (July 24, 2001)

The federal government can also impose sanctions if a family does not regularly update its data, does not withdraw money for several months, reaches a per capita income that exceeds half a minimum wage, or if it detects that a family has provided false information during the registration process (MDS and SENARC, 2015).

B. Bolsa Família's Beneficiary Selection Process

Partly as a result of Bolsa Família's expansion, coverage of the program is not always universal. Thus, for the time of the analysis (2012-2017), the official database, the Cadastro Único, contains data on both beneficiary families and eligible non-beneficiary families. The number of available places in Bolsa Família depends on the federal funding provided for the program and the municipality's official poverty rate, which is only periodically updated. The program's beneficiary selection process guarantees that only family characteristics that are observable in the Cadastro Único affect who is included in the Bolsa Família program. In Section III, I describe in detail how this process can be used to estimate the causal effects of Bolsa Família by constructing otherwise identical treatment and control groups.

The allocation of benefits is split into four phases:

1. Registration: Families with an income of less than half a minimum wage (currently R$ 499) register in the Cadastro Único, either at the Centro de Referência de Assistência Social (CRAS), the local center for social assistance, or with a social worker during a home visit. This step happens at the municipal level. A social worker then inputs the data into the Cadastro Único, which is maintained by the Caixa Ecônomica Federal (Caixa). Families have to update their data at least every other year.

2. Qualification: Once a month, Caixa generates aggregated reports for each municipality with the number of qualifying families in different vulnerability categories. It first extracts data on all qualifying families—families with an income below the current threshold for Bolsa Família that have complete and updated information and are not excluded from the program because of sanctions.[8] These families are categorized by vulnerability (e.g., indigenous families, families with suspected child labor, families that benefit from another social program)[9] and the Caixa then produces aggregated reports to determine the number of benefits allocated to each vulnerability category in each municipality.

3. Selection: Based on these aggregate reports, the Secretaria Nacional de Renda de Cidadania (SENARC) decides how many benefits it allocates to each vulnerability category and each municipality. SENARC uses an algorithm that optimizes the allocation of benefits

---

8. Families can be excluded from Bolsa Família for at least a year for severe infractions, such as underreporting their income. More on this in Section V.

9. The exact categories have changed over time.

to each category, subject to a budget constraint and additional parameters that allow the SENARC to prioritize especially vulnerable categories.[10] Once it has decided how many benefits it allocates to families in each category, SENARC distributes the benefits across municipalities and prioritizes places with a lower Bolsa Família coverage rate relative to the official municipal poverty rates from the census. SENARC then instructs the Caixa how many benefits to grant to families in each municipality and category.

4. Concession: In the final phase, the Caixa's computer system determines the actual beneficiaries in each vulnerability category and municipality based on families' per capita income and the number of children. Thus, conditional on being in the same vulnerability category and in the same municipality, families with a lower per capita income are included first. Conditional on also having the same per capita income, families with more school-aged children are prioritized. When a family is formally included in the program, the Caixa sets up an account for the family, starts the monthly payments, and issues a magnetic stripe card that allows the family to access the benefits. Importantly, once a family is part of the program, it continues to receive the benefits even if a more deserving family registers in the Cadastro Único. Even if the family's income increases above the threshold for Bolsa Família, the family stays in the program for two more years.[11]

C. Bolsa Família's Safeguards against Corruption

Bolsa Família was designed to minimize the influence of corruption—both at the federal and the local level—and several government agencies operate whistleblowing systems and have the power to investigate alleged abuse of the program.[12]

At the federal level, the main concern is the targeting of funds to municipalities and demographic groups for electoral gains. The risk of geographical quotas being set for electoral purposes is mitigated by tying the process to objective census data. Moreover, the federal government has at various points been barred from issuing reports or publications about Bolsa Família in the months leading up to an election.

At the local level, two concerns dominate the efforts: that local officials embezzle funds from the program for personal gain and that they control the allocation process to trade benefits for bribes or votes. The risk of embezzlement is greatly mitigated by transferring funds directly to beneficiaries instead of making bulk payments to state or municipal governments.

---

10. The process is not standardized, a fact that has repeatedly been criticized by the Federal Court of Accounts (e.g., TCU, 2006). Fortunately, while this introduces an undesirable human element in the allocation of benefits *between* categories, the allocation of benefits *within* categories is unaffected. The estimation strategy outlined in Section III uses only variation within categories.

11. This rule mitigates families' incentives to underreport their income when they update their data. Moreover, due to the frequent increases of the income threshold, many families would otherwise leave the program just to qualify again after the next increase.

12. For a detailed description of Bolsa Família's anti-corruption efforts see Lindert et al. (2007).

To address the second concern, the centralized beneficiary selection process was put in place. Moreover, municipalities are required to publish monthly lists of beneficiaries and the payments they received, and all transfers, including full names of the beneficiaries, are also published by the federal government on the Portal da Transparência.

The MDS employs a combination of control mechanisms and incentives for program administrators. Self-reported incomes are compared to records of the department of labor and lists of beneficiaries are cross-referenced with other administrative data such as motor-vehicle registrations. As the majority of beneficiaries is not formally employed, per capita income often cannot be readily verified and income underreporting will often only be detected if a family's lifestyle is incompatible with its reported income—for example, if the family buys a new car. In addition to its verification efforts, the MDS assesses the quality of monitoring and registrations every month and ties federal contributions to administrative costs to an index of municipal management quality, the Índice de Gestão Descentralizada do Município (IGD-M).

In addition to the MDS's internal monitoring initiatives, three other government agencies—the Office of the Comptroller General (CGU), the Federal Court of Accounts (TCU), and the independent public prosecutor's office—are responsible for oversight of the program. The CGU regularly selects municipalities at random to conduct in-depth audits of municipalities.[13] The CGU's manuals specify the specific actions to be taken concerning Bolsa Família in randomly audited municipalities: First, the auditors will verify the eligibility of a random sample of beneficiaries in the municipality. Second, auditors will cross-reference public employment records with lists of Bolsa Família beneficiaries. Third, payments and operations of the Caixa are scrutinized. Amongst other things, auditors look for proof that benefit cards were delivered to beneficiary families.[14] Fourth, auditors examine monitoring systems for school attendance and compliance with the medical conditionalities. For example, auditors cross-reference reported attendance rates in the online system with entries in class books. Finally, auditors control the existence and recent activities of local governance bodies such as the municipality's social control council. Moreover, the MDS, the CGU, and the Caixa all operate whistleblower systems to report discrepancies and abuse of the program, and auditors investigate specific complaints received through these systems.

D. Bolsa Família's Vulnerabilities to Corruption

Despite these efforts, the number of families receiving Bolsa Família exceeds the number of eligible families estimated from the census in a significant share of municipalities (Fried, 2012). While Bolsa Família's centralized beneficiary selection and payment systems are highly effective at preventing outright embezzlement of money designated for beneficiary families, they do not guarantee that the program is effectively administered on site. Indeed, as others have pointed

---

13. This is described in more detail in Section IV.

14. This has become less important, as most of the payment cards are now sent directly to beneficiaries.

out (e.g., Lindert et al., 2007), fraud, error, and political interference in Bolsa Família are most likely at the municipal level. As a result, local corruption can affect the effectiveness of the Bolsa Família program by influencing how diligently municipalities fulfill their responsibilities in implementing the program.

A quick Google search reveals numerous news reports of irregularities uncovered by the CGU audits. Common findings include families underreporting their income, schools maintaining sloppy attendance records, social control councils that haven't met for more than a year, delays in the delivery of benefit cards, and the occasional public servant being listed as a member of an unrelated beneficiary household. Note that this implies that local officials can still benefit financially from Bolsa Família, albeit to a much smaller degree than if they could get their hands on all of the payments. Section VI takes a detailed look at the remaining strategies for personal enrichment and shows that the financial damages from these practices are relatively small and cannot account for the differences in program performance.

Bolsa Família's effectiveness in promoting school enrollment depends crucially on its ability to target the families that have the worst expected outcomes in the absence of the cash transfer. In places where corruption is rampant, the program's targeting accuracy is likely to be lower: families, particularly those with an income just above the eligibility threshold, have a strong incentive to underreport their earnings, and corrupt local administrators, aware of this fact, can turn a blind eye in return for a favor. Families whose gross income can be more readily verified will occasionally misrepresent their household composition and include the children of relatives or neighbors to achieve a lower per capita income. In a newsworthy example, a family received monthly benefits for four-year-old Billy da Rosa Silva until a social worker sent to invite the family to a routine health checkup discovered that Billy was a cat (Hider, 2014). There is also evidence that some families deliberately reduce their labor supply leading up to their registration (Firpo et al., 2014).[15]

III. Estimating the Effectiveness of Bolsa Família

To measure the effectiveness of Bolsa Família in different municipalities, I reconstruct the program's priority strata to find families that were randomly admitted or not admitted, using Bolsa Família's official database and beneficiary selection algorithm.

A. Data—the Cadastro Único

The primary data used for the analysis is the Cadastro Único, the official database used to select beneficiary families for the Bolsa Família program. As the selection algorithm uses only

---

15. Not all mistargeting is the result of intentional deception. Potential beneficiaries might provide weekly instead of monthly wages and mistakes are made because an administrator's fingers miss the mark on a keyboard or sloppy handwriting on a form is incorrectly deciphered when digitizing the data. However, Section VII shows that inadequate administrative processes alone cannot explain the differences in program performance.

the Cadastro Único, there can be no unobservable characteristics that affect who receives Bolsa Família.

Families with a per capita income of less than half a minimum wage can register in the Cadastro Único and are then required to update their data at least every other year. As registration is voluntary, one might worry that self-selection into the Cadastro Único leads to non-representative estimates. However, this is hardly a concern in practice: for the period of the analysis (2012-2017), the registration rate is close to 100%. So close in fact, that the MDS stopped using the registration rate as a performance indicator in the IGD-M in July 2015 (see Figure A4 in Appendix IV).

The Cadastro Único provides two types of files on each family. The personal files contain information on each family member's demographics, as well as information on literacy, education, and employment. The family file contains information on the family income, participation in other welfare programs, as well as information on the family's living conditions, such as the material of floors and walls, and access to public services such as water, electricity, sewage systems, and garbage collection. As this additional data is not used to select beneficiaries, I can use it to test whether the identification strategy successfully deals with spurious correlations.

While some data in the Cadastro Único is self-reported by families, other variables are set by the MDS's computer system. For example, the indicator of whether a family benefits from Bolsa Família is automatically changed at some point after the family registered. Thus, a cross-section from the Cadastro Único will contain data from different time-points. I address this problem by considering a family's Bolsa Família status at the beginning of the year. This approach is conservative in that it overestimates the outcomes of supposedly unincluded families, some of which might have benefited from Bolsa Família later in the year.

In addition to the Cadastro Único, this paper uses data from several other sources to investigate how corruption affects the effectiveness of the Bolsa Família program. Data on corruption and Brazil's random audit program is from the Office of the Comptroller General (CGU) and is discussed in more detail in Section IV. The Portal da Transparência publishes expenditures by the Brazilian government, including lists of all the Bolsa Família payments made in any given month and can be used to determine whether some payments have been withheld because of a failure to comply with the program's conditionalities and whether a family has been excluded from the Bolsa Família program. To better understand the mechanism, the paper uses data from the annual census of social services (the Censo SUAS), quality control data from the index of municipal management quality (IGD-M), and data on denunciations and complaints received through the whistleblowing systems of the MDS.

B. Sample—Randomly Admitted Families

Having access to all the data that the selection algorithm uses, I can reconstruct the priority strata to find families that were randomly included or not included in Bolsa Família. This allows

me to estimate the effects of the Bolsa Família program in different years and municipalities.

As Bolsa Família explicitly targets the most impoverished families, there are significant differences between beneficiaries and non-beneficiaries. While some of these differences are observable in the Cadastro Único, others are unobservable and cannot easily be controlled for a regression. Fortunately, because the selection of beneficiaries is based solely on families' information in the Cadastro Único, it cannot be affected by unobservable variables.[16] Thus, having access to both the selection algorithm and the database used to select the beneficiaries, I can identify otherwise identical beneficiaries and non-beneficiaries who have not only the same *observable* characteristics but also the same expected *unobservable* characteristics.

To understand how the selection algorithm can be used to find families that were randomly admitted or not admitted to the Bolsa Família program, recall the four phases of the benefit allocation process: In the *registration phase*, families register in their municipality, and their data is entered in the Cadastro Único. In the *qualification phase*, the Caixa categorizes all eligible families by vulnerability (e.g., indigenous families, families with suspected child labor) and sends the aggregated numbers for each municipality to SENARC. In the *selection phase*, SENARC decides how many benefits it allocates to each vulnerability category and each municipality. In the final *concession phase*, the Caixa determines the actual beneficiaries in each category and municipality based on families' per capita income and the number of children.

The identification strategy closely mirrors the four phases of the beneficiary selection algorithm: First, in line with the *registration phase*, only families that were never part of the program are considered; these are mostly newly registered families and families that did not previously qualify for the program but that now qualify after a change in the eligibility rules of Bolsa Família. Second, mimicking the *qualification phase* and the *selection phase*, families are matched based on the municipality and vulnerability category, and fixed effects are used to exploit only variation between families of the same group. Finally, to capture the *concession phase*, families are additionally matched based on their exact income and their household composition.[17] At each point in time, only the marginal priority strata—those with both selected and unselected families—are considered, leaving only strata where the algorithm randomly included some but not all families in the Bolsa Família program.

While the selection algorithm does not per se prioritize families that have registered earlier, these families have had more opportunities to be included than more recently registered families. The most conservative approach is to include only families that register for the first time, excluding those that only updated data from an earlier registration. This, however, takes quite a toll on the number of families that can be sufficiently precisely matched—especially in smaller

---

16. Lindert et al. (2007, p.45) write: *"Application of eligibility criteria to family data is carried out automatically by the Cadastro Único software, which compares self-reported incomes to the official eligibility thresholds, prioritizing families and assigning benefits according to income and family composition."*

17. Note that this is the most conservative approach to address the preferential inclusion of poorer and larger families.

municipalities. Alternatively, families can also be matched on the exact month of the registration or the update, but this tends to reduce geographic coverage by a comparable amount. As a compromise, results are shown for all three samples: the most representative sample of all families that can be sufficiently precisely matched irrespective of whether their current information is from a new or an updated registration, the sample of families that are newly registered, and the sample of families that is matched on the exact month of the registration or data actualization.

C. Estimating the Treatment Effects

Assume for a moment that there are only families belonging to the same vulnerability category, with the exact same family income and number of children—i.e., all families are in the same priority stratum—and living in the same municipality. Families are observed for two periods:[18] At $t = 0$, families register (or update their data) and none of the families receive Bolsa Família. Over the next year, the algorithm includes some of the families in the Bolsa Família program, while it cannot accommodate others due to the number of available funds for this category and municipality. Thus, when outcomes are observed again at $t = 1$, some families have been benefiting from Bolsa Família, while others have not.

The educational outcome $Y_{i,f,\theta,m,t}$ of child i in family f of priority stratum θ living in municipality m at time t can be modeled using the following potential outcomes framework:

$$Y_{i,f,\theta,t} = \alpha + \beta \text{ Bolsa Família}_{f,t} + X'_{i,f}\gamma_1 + W'_m\gamma_2 + Z'_t\gamma_3 + u_{i,f,\theta,m,t} \tag{1}$$

where $Bolsa\ Famlia_{f,t}$ is an indicator of whether family f is included in the Bolsa Família program at time t, $X_{i,f}$ is a vector of (unobservable) child and family characteristics that are fixed over time, W_m is a vector of municipality characteristics, and Z_t are time-varying external factors such as changes in the educational system.

So far I have focused on only one priority stratum. I can estimate the causal effect of Bolsa Família across several marginal priority strata using time × priority strata fixed effects to account for the fact that different strata—and therefore families with different observable characteristics—are on the margin at different points in time. Also, as a child's family is fixed a specification with child fixed effects can be used to account for observable and unobservable child and family characteristics. This leads to the following specification:

$$Y_{i,f,\theta,m,t} = \beta \text{ Bolsa Família}_{f,t} + \alpha_i + \nu_m + \mu_{\theta,t} + \varepsilon_{i,f,\theta,m,t} \tag{2}$$

The treatment variable $Bolsa\ Famlia_{f,t}$ is an indicator that takes value 1 if a family gets included in the Bolsa Família program. The specification includes municipality fixed-effects ν_m and $Year \times Priority\ strata$ fixed effects $\mu_{\theta,t}$ to account for both the randomization within strata

---

18. Otherwise, repeated re-matching and the possibility of families leaving the program introduce unnecessary complications. However, as shown later, the results are robust if the families are followed for an additional year.

and, importantly, also for the fact that different priority strata are on the margin at different points in time. In fact, the priority strata fixed effects non-parametrically control for thousands of combinations of the month of registration, the household's exact per capita income, and the number of children. The error term $\varepsilon_{i,f,\theta,m,t}$ is allowed to cluster at the family and municipality level to account for the selection of *families* into Bolsa Família and to facilitate comparison with the results in Section IV, where the effect of changes in *municipality*-level corruption are studied. As Bolsa Família is randomly assigned for these families, the coefficient β estimates the causal change in children's educational outcomes when their families are included in the Bolsa Família program.

D. Validating the Identification Strategy

The identification strategy relies on the fact that the selection algorithm uses only data that is observable in the Cadastro Único so that there can be no unobservable family characteristics that affect which families get included in the Bolsa Família program. Moreover, as the selection algorithm uses only income, household composition, and the vulnerability category, we can think of other data in the Cadastro Único as observable to the researcher, but no to the selection algorithm. This provides a test for whether the identification strategy successfully recovers the random allocation of benefits within priority strata.

Table I shows that the identification strategy successfully deals with confounding family characteristics that do not influence the selection of beneficiary families but are strongly correlated with per capita income, such as access to utilities, or the materials of the dwelling.[19] Columns (1) and (2) show that these characteristics are relatively well-balanced between future beneficiary and non-beneficiary households in the marginal priority strata. As a more stringent test of balancedness (Pei et al., 2019), Column (3) reports the coefficient when the variable of interest is regressed on an indicator of whether a family will be included in Bolsa Família and the fixed effects, and tests whether these coefficients are jointly significant from zero. A significant test statistic suggests that these variables are jointly predictive of which families will be included in the program. This is not the case ($\chi^2(24) = 23.696$, $p = 0.479$).

E. Bolsa Família Increases School Enrollment

Before progressing to the main question—whether corruption affects the effectiveness of Bolsa Família—I show that the identification strategy can recover the positive effect of Bolsa Família on school enrollment. Consider the expected impact of a program that pays families for regular school attendance. While we would expect a positive effect, a naïve OLS approach finds a negative effect, because the program was explicitly designed to target families with a low

---

19. The results show the sample of families who have registered for the first time, to ensure that the Bolsa Família indicator is set after the last observed data update.

TABLE I

BALANCEDNESS OF FAMILY CHARACTERISTICS IN MARGINAL PRIORITY STRATA

| | No Bolsa Família | | Bolsa Família | | LHS-test |
| | (1) | | (2) | | (3) |
	Mean	Standard Deviation	Mean	Standard Deviation	Coeff.
Location: urban	0.810	0.393	0.848	0.359	-0.001
Material: brick	0.754	0.430	0.789	0.408	0.003
Material: clay	0.025	0.156	0.018	0.133	-0.002
Material: timber	0.083	0.275	0.080	0.271	0.002
Material: surplus timber	0.028	0.165	0.030	0.169	-0.001
Material: straw	0.002	0.043	0.002	0.040	0.000
Sewage: canalization	0.394	0.489	0.470	0.499	-0.007[+]
Sewage: tank	0.153	0.360	0.144	0.351	0.000
Sewage: tank (rudimentary)	0.264	0.441	0.229	0.420	0.006[+]
Sewage: open ditch	0.024	0.152	0.024	0.153	-0.001
Sewage: river	0.008	0.088	0.011	0.103	-0.000
Piped water	0.788	0.408	0.832	0.374	0.007[+]
Water source: network	0.671	0.470	0.714	0.452	-0.005
Water source: spring	0.172	0.378	0.159	0.366	0.006
Water source: cistern	0.022	0.148	0.019	0.138	0.001
Indoor bathroom	0.847	0.360	0.883	0.322	0.000
Garbage: collected	0.767	0.423	0.817	0.387	-0.003
Garbage: burned or burried	0.125	0.330	0.099	0.299	0.004
Garbage: dumped on land	0.010	0.097	0.009	0.093	0.000
Garbage: dumped in river	0.001	0.025	0.001	0.023	-0.000
Light: electric (with meter)	0.804	0.397	0.821	0.383	0.001
Light: electric (without meter)	0.060	0.237	0.066	0.248	0.003
Light: oil or gas	0.014	0.118	0.011	0.106	-0.001
Light: candles	0.008	0.088	0.006	0.079	-0.001
F-test: $\chi^2(24)$					23.696
F-test: P-value					0.479
Observations	234757		238733		473490

Notes. This table reports on the balancedness of family characteristics that are not relevant for inclusion in the Bolsa Família program. Columns (1) and (2) present the summary statistics for non-beneficiary and beneficiary families. Column (3) uses a left-hand-side test (Pei et al., 2019) to check whether these characteristics are predictive of a family's inclusion in Bolsa Família in regressions of the form $X_{f,\theta,m,t} = \alpha + \beta BF_{f,t} + \mu_{t,\theta,m} + \varepsilon_{f,\theta,m,t}$. The F-test tests whether these coefficients are jointly different from zero. Significance levels: $^+p < 0.1, ^*p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$.

baseline school enrollment. As a final validation of the identification strategy, I thus reestimate the effect of Bolsa Família on school enrollment.

Table II shows the change in children's school enrollment if a family gets included in the Bolsa Família program. Column (1) shows the estimates for all families in marginal priority strata, irrespective whether their information is from a new or updated registration. Column (2) shows the estimates if only families that register for the first time are considered, and Column (3) shows the estimates if families are matched on the month of their registration or last data update. In the first sample, 2,573,117 children are in marginal strata that contain both treated and untreated families. These children live in 5,401 municipalities, covering 97.5% of the municipalities that are eligible for the random audits. If only newly registered families are considered, only 590,630 children from 5,068 municipalities are included in the analysis, and the

TABLE II
BOLSA FAMÍLIA INCREASES SCHOOL ENROLLMENT

	(1) School enrollment (%) (All marginal families)	(2) School enrollment(%) (Newly registered families)	(3) School enrollment (%) (Same registration month)
BF	1.006***	1.498***	0.919***
	(0.053)	(0.122)	(0.067)
Control mean	87.283	86.442	86.809
Child FE	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes
R2	0.925	0.932	0.931
N(municipalities)	5,401	5,068	4,858
N(priority strata)	12,559	8,641	6,008
N(children)	2,573,117	590,630	747,786
N	5,146,234	1,181,260	1,495,572
Years	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program on children's school enrollment. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program. Column (1) presents the results for the most representative sample, where families are matched on the municipality, vulnerability category, the exact income, the number of children, and the year the families last updated their data. Column (2) uses the same definition, but only matches families who are newly registered. Column (3) requires families to have last updated their data in the same month. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. For the fixed effects, families in each priority stratum have the exact same income, the same number of children, belong to the same vulnerability category, and have last updated their data in the same month. Standard errors are clustered at both the family and the municipality level. Significance levels: $^+p < 0.1,$* $p < 0.05,$** $p < 0.01,$*** $p < 0.001$.

geographic coverage drops to 91.5%. Likewise, when families are required to have registered in the same month, only 747,786 children are included, and geographic coverage reduces to 4,832 municipalities or 87.7% (see Figure A5 in Appendix IV for maps of the geographic coverage of each sample).[20]

Inclusion in Bolsa Família increases enrollment by 1.01 percentage points in the largest sample (Column 1), by 1.50 percentage points in the sample that considers only families who registered for the first time (Columns 2), and by 0.92 percentage points in the sample where families are also matched on the month they registered (Column 3). The effects are highly significant ($p = 0.000$ for all samples) and robust to several alternative specifications (see Appendix II). However, there is considerable heterogeneity in the effectiveness of Bolsa Família across different municipalities (see Figure A6 in Appendix IV).

These estimates are lower than those of previous evaluations of the program. For example, in one of the earliest studies, Cardoso and Souza (2003) estimate that inclusion in Bolsa Escola, Bolsa Família's predecessor, increases school enrollment by 3 percentage points. Glewwe and Kassouf (2012) find an increase of approximately 5 percentage points using data from 1998 to

---

20. Unsurprisingly, the municipalities that are lost because families cannot be matched precisely enough tend to be less populous. In terms of the population living in municipalities that are eligible for the random audits the coverage is still quite high; 99.7%, 98.7%, and 97.8%, respectively, live in one of the municipalities that are covered by the regressions.

2005, Schaffland (2012) documents gains of 4 percentage points using data from 2004 to 2006, and De Brauw et al. (2015) find an effect of 8% for girls but no significant gains for boys using data from 2009.

Three factors are likely to accounts for this quantitative difference: School enrollment rates increased considerably over the last two decades, leaving less room for substantial gains. Moreover, the identification strategy uses only the marginal priority strata: as poorer families are more likely to be included with certainty, the marginal strata consist of families with somewhat higher incomes and higher baseline school enrollment. Finally, the use of child fixed effects appears to depress the estimated treatment effects further. Indeed, without controls for individual-level heterogeneity, point estimates are larger (see Table A13 in Appendix V).

IV. LOCAL CORRUPTION AFFECTS PROGRAM EFFECTIVENESS

The first main result of the paper is that local corruption decreases the effectiveness of Bolsa Família, even though local officials are bypassed in both the payment or the beneficiary selection process. Using Brazil's program to randomly audit municipalities as an exogenous shock to corruption (Avis et al., 2018), I show that Bolsa Família's effect on school enrollment increases when municipalities become less corrupt.

A. Random Audits as Exogenous Shocks to Corruption

Since 2003, Brazil's federal government operates an anti-corruption program that includes audits of randomly selected municipalities. This audit lottery constitutes a uniquely compelling natural experiment: a series of exogenous shocks, explicitly designed to combat corruption, distributed all over Brazil, and spread over more than a decade. As several municipalities have been audited more than once, it's possible to estimate the effects of a previous audit on instances of corruption detected in later rounds of the program.

In response to rampant abuse of federal transfers by municipal officials, the federal government created the position of the Comptroller General, Controladoria Geral da União (CGU) in 2003. In the same year, the Programa de Fiscalização por Sorteios Públicos started to randomly select municipalities for comprehensive audits in a draw that is held in conjunction with the national lottery. Municipalities with less than 500,000 are eligible for the lottery, whereas more populous cities are subject to other audits.[21] Once a municipality has been audited, it cannot be selected again for some time.[22]

If a municipality is randomly selected, the CGU lists all federal transfers made to this municipality in the previous years and randomly chooses a number of them for in-depth audits.

---

21. This affects 31 municipalities, mostly state capitals, home to approximately 27% of the Brazilian population.

22. The exact rules have varied over time. See Ferraz and Finan (2008) and Avis et al. (2018) for details of the audit lottery program.

The CGU then issues an inspection order for each of the selected transfers and sends a team of 10-15 auditors to the municipality, usually within less than a month after the lottery. Auditors are highly qualified and competitively paid and, consequently, less susceptible to bribery than other public employees (Avis et al., 2018). The auditors carefully examine expenditures associated with the inspection order, verify the delivery of goods and services paid for by the transfer, and check whether procurement and hiring decisions comply with the relevant laws. Auditors also engage with the local community and with municipal councils to gather additional information and register complaints (Ferraz and Finan, 2008). Once completed, the auditors share their findings with federal prosecutors, the federal police, the local judiciary, and the city council.

Since round 17 of the audit lottery, the CGU focuses on specific sectors in each draw.[23] As not all sectors are audited in every round, one might worry that some audits don't have the same corruption reducing effect on the Bolsa Família program. Fortunately, Bolsa Família was subject to every round of the program since 2011.

Moreover, while the inclusion of all audits may underestimate the true impact on the Bolsa Família program, there are several reasons why one should still expect an effect of the audits: First, although auditors are not allowed to venture into other sectors, they report additional suspicions back to the CGU, which can then take appropriate actions. Second, even though the audited sectors differ, the people implicated will often be the same, as many municipalities are dominated by a small political elite. Third, using evidence from municipalities that have been audited more than once, Avis et al. (2018) show that, if anything, the corruption reduction is somewhat weaker in the audited sector, suggesting that politicians assume that a sector is now less likely to be audited again. Finally, there are spill-over and learning effects between municipalities through local media and shared political networks (Avis et al., 2018) and it is reasonable to expect that these effects are at least as important within a municipality. Indeed, Table A14 in Appendix V shows that corruption in the Bolsa Família program and the educational system more generally are highly correlated with corruption in other sectors.

In 2016, the CGU was reconstituted as the Ministério da Transparência, Fiscalização e Controladoria-Geral da União and the random audit lotteries were superseded by the Programa de Fiscalização em Entes Federativos. While some audits remain random, the new program also conducts non-random audits. So far, only the third cycle was lottery-based,[24] while the first and fourth cycle used insights from the previous program to select those municipalities deemed to be the most vulnerable and the second cycle conducted comprehensive audits of state capitals.

---

23. In round 27, for example, the auditors looked at transfers related to social assistance, agriculture, commerce and services, and culture in municipalities with a population of more than 100,000, and at all these sectors, plus health and education-related transfers in municipalities with a population between 20,000 and 100,000 thousand. In municipalities with less than 20,000 inhabitants, all sectors were audited.

24. Eligibility rules differed somewhat from previous lotteries. As a safeguard, I include only municipalities that would have been eligible under the previous regime. Appendix II shows that the results are robust if only the original program is considered.
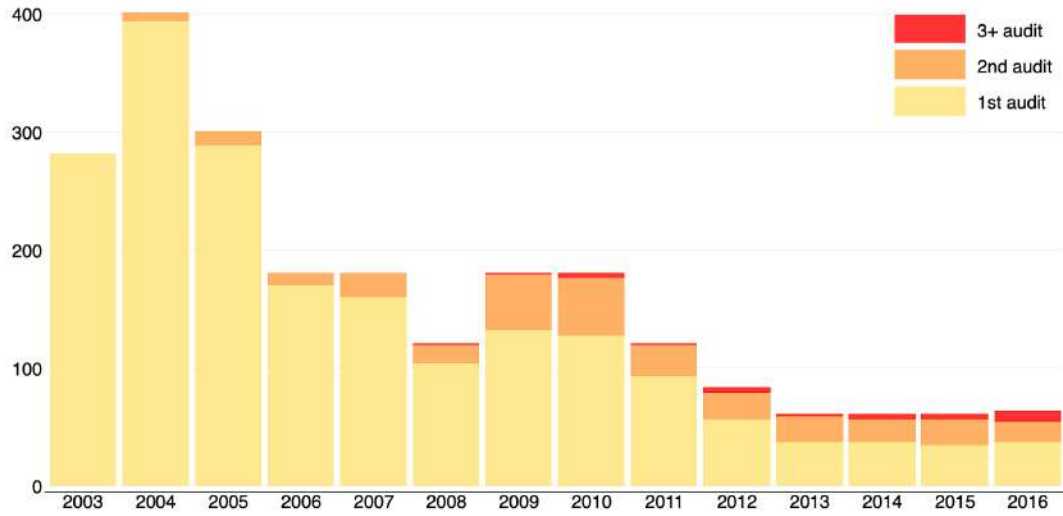
Figure I

Timeline of Random Audits

Notes. This figure displays the timeline of random audits under 40 rounds of the Programa de Fiscalização em Entes Federativos (2003-2015) and the random third cycle of its successor, the Programa de Fiscalização em Entes Federativos (2016). Colors indicate whether a municipality is being randomly audited for the first time, the second time, or at least the third time.

B. Validating the Corruption Reduction After Random Audits

As several municipalities have been audited more than once, it's possible to estimate the effects of a previous audit on instances of corruption and mismanagement detected in later rounds of the program (Avis et al., 2018). Since 2003, 1,956 municipalities with a population of less than 500,000 were randomly audited; 285 of which were audited twice, 25 three times and one municipality four times—2,267 random audits in total (Figure I).

The treatment variable is an indicator, *Past audit*, whether a municipality has been randomly audited in the past. For the construction of the treatment variable, I consider all municipalities with a population of less than 500,000 in the census of the year 2000 and I include all 40 rounds of the original Programa de Fiscalização por Sorteios Públicos as well as the random third cycle of its successor, the Programa de Fiscalização em Entes Federativos.[25] As a result, the sample differs from the one used by Avis et al., who restrict their analysis to rounds 22 to 38 of the audit lottery program to focus on the two electoral terms from 2004 to 2012. Moreover, in round 36, the audits of several selected municipalities were canceled less than three weeks later due to a strike of the auditors.[26] Avis et al. code these municipalities as having been treated.[27]

To validate the corruption-reducing effect of previous audits in the full sample, I re-estimate

---

25. See Figures A7 and A8 in Appendix IV for the geographic distribution of the audits and the *Past audit* variable over time.

26. *Portaria n° 1.713* (August 10, 2012)

27. Appendix II shows that the results are robust to alternative definitions of the treatment variable.

Equation (16) in Avis et al. (2018):

$$
\begin{aligned}
log(\text{Corruption}_{m,s,t}) = {} & \alpha + \beta \text{ Past audit}_{m,s,t} + Z'_{m,s,2000}\gamma \\
& + f(\text{Inspection orders}_{m,s,t}) + \nu_s + \mu_t + \varepsilon_{m,s,t}
\end{aligned}
\tag{3}
$$

where the logarithm of the number of detected incidents in municipality m in round t of the audits program, is regressed on the treatment variable $Past\ audit_{m,s,t}$, that takes value 1 if a municipality has been subject to a random audit in an earlier round of the program. Depending on the specification, the model controls for socioeconomic factors $Z_{m,s,2000}$: the logarithm of population, the share of population living in an urban area, income inequality, log. income per capita, and illiteracy rate—measured in 2000 before the inception of the audit program. Because the number of transfers and programs auditors inspect, *Inspection orders*, directly affects the number of uncovered incidents, it is controlled for either logarithmically or non-parametrically. State fixed effects ν_s mirror the stratification of the lottery: different locations face different probabilities of being audited depending on the number of municipalities in a state. Finally, fixed effects for the round of the audits program μ_t account for the fact that the number of municipalities that have been *previously* audited is necessarily weakly increasing over time and that the sectors chosen for the audits vary across rounds. With the appropriate fixed effects in place, the coefficient β can be interpreted as the causal effect of a previous audit on corruption.

Table III displays the effects of having previously been audited at random on the total number of irregularities, cases of mismanagement, and corruption[28] in three different specifications: using the logarithm of the number of inspection orders without controlling for sociodemographic factors (Columns 1, 4, and 7), adding the sociodemographic factors (Columns 2, 5, and 8), and including fixed effects for the number of inspection orders and control variables—the preferred specification of Avis et al. (Columns 3, 6, and 9).

As in the restricted sample used in Avis et al. (2018), there is a clear difference between mismanagement and corruption. For mismanagement, there is no effect of previous audits and, other than population, none of the control variables has any predictive power. In contrast, there are significantly fewer instances of corruption in audited municipalities, although the reduction is slightly less pronounced in the full sample.

---

28. Since round 20 of the program, the CGU has coded the severity of its findings internally as either *falha formal*, *falha média*, or *falha grave*—formal, moderate, and severe cases. As discussed by others (Avis et al., 2018; Zamboni and Litschig, 2018), the distinction between moderate and severe cases is primarily a question of the potential financial damage and says relatively little about the nature of the corrupt action. There is also considerable overlap between losses judged as moderate and severe (see Figure A9 in Appendix IV); thus I use the classification of Avis et al. (2018) and refer to formal errors as instances of *mismanagement* and to moderate and severe findings as acts of *corruption*.

TABLE III

RANDOM AUDITS SIGNIFICANTLY DECREASE CORRUPTION

	Irregularities (Log.)			Mismanagement (Log.)			Corruption (Log.)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Past audit	-0.044*	-0.049*	-0.047*	-0.070	-0.067	-0.074	-0.040$^+$	-0.046*	-0.043*
	(0.020)	(0.020)	(0.020)	(0.051)	(0.051)	(0.051)	(0.022)	(0.022)	(0.022)
Population (Log.)		0.028**	0.033**		-0.062*	-0.070**		0.040***	0.046***
		(0.010)	(0.010)		(0.026)	(0.027)		(0.011)	(0.011)
Income inequality (Gini)		0.158	0.215$^+$		-0.036	-0.186		0.188	0.277$^+$
		(0.122)	(0.123)		(0.363)	(0.371)		(0.136)	(0.139)
Income per capita (Log.)		-0.090*	-0.120***		0.083	0.104		-0.101**	-0.133***
		(0.036)	(0.036)		(0.096)	(0.095)		(0.039)	(0.039)
Illiteracy		0.002$^+$	0.002		0.002	0.002		0.003*	0.003$^+$
		(0.001)	(0.001)		(0.004)	(0.004)		(0.002)	(0.002)
Urban population		0.101*	0.112*		0.180	0.177		0.097$^+$	0.111*
		(0.050)	(0.051)		(0.129)	(0.132)		(0.056)	(0.057)
Constant	0.868***	0.949***	3.840***	-1.002***	-1.068$^+$	1.258*	0.808***	0.813***	3.657***
	(0.106)	(0.223)	(0.223)	(0.219)	(0.585)	(0.627)	(0.121)	(0.246)	(0.250)
Inspection orders	Log.	Log.	Nonpar.	Log.	Log.	Nonpar.	Log.	Log.	Nonpar.
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lottery FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.804	0.807	0.823	0.275	0.278	0.307	0.780	0.785	0.801
F	72.829	64.807	18.741	19.494	17.047	8.673	58.480	52.924	17.473
N	1432	1432	1432	1432	1432	1432	1432	1432	1432

Notes. This table replicates the effect of having been randomly audited in the past on uncovered instances of corruption and mismanagement in later rounds reported in Avis et al. (2018). The dependent variable in Columns (1) to (3) is the logarithm of the total number of irregularities uncovered by the random audit program. Columns (4) to (6) include only instances of mismanagement (*falha formal*), and Columns (7) to (9) only instances of corruption (*falha média* or *falha grave*). "Past audit" indicates that a municipality has been audited at random in a previous round of the program. "Population (Log.)", "Income inequality (Gini)", "Income per capita (Log.)", and the rates of "Illiteracy" and "Urban population" control for municipality characteristics in 2000, before the inception of the audits program. Columns (3), (6), and (9) include fixed effects for the number of inspection orders issues, while the other columns control for the logarithm or inspection orders. All models include fixed effects for the state and the round of the audit lottery. Robust standard errors are reported in brackets. Significance levels: $^+p < 0.1$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

C. Estimating the Change in Treatment Effects

To estimate the change in the effectiveness of Bolsa Família after a municipality has been randomly audited, Equation (2) can be appended with the audit indicator and an interaction term:

$$Y_{i,f,\theta,m,t} = \beta \text{ Bolsa Família}_{f,t} + \gamma \text{ Past audit}_{m,t} + \delta \text{ (BF} \times \text{Past audit)}_{f,m,t}$$
$$+ \alpha_i + \nu_m + \mu_{\theta,t} + \varepsilon_{i,f,\theta,m,t} \tag{4}$$

Both Bolsa Família and the audits are randomly assigned (once we correctly account for stratification), and we can interpret the coefficient δ as the causal change in the effectiveness of Bolsa Família after a municipality has been audited at random. All standard errors are clustered at both the family and the municipality level, to account for the selection of *families* into Bolsa Família and the selection of *municipalities* in the audit lotteries.

D. Bolsa Família is More Effective After a Random Audit

After a random audit, Bolsa Família's effect on school enrollment increases. This finding is robust to a large number of alternative specifications.

Figure II shows that the school enrollment gains from inclusion in the Bolsa Família program increase significantly after a municipality has been audited at random. In the most representative sample, Bolsa Família is estimated to increase school enrollment by 0.90 percentage points in unaudited municipalities, but by in 1.18 percentage points in audited municipalities ($p = 0.031$ for the interaction term). Thus, a random audit increases the effect of Bolsa Família by 31%. The estimates are somewhat larger if only families who registered for the first time are considered. Bolsa Família is estimated to increase school enrollment by 1.31 percentage points in unaudited municipalities, but by 1.82 percentage points in audited municipalities, an increase of 39% ($p = 0.032$ for the interaction term). In the sample that matches families on the month of registration, Bolsa Família is estimated to increase school enrollment by 0.81 percentage points before an audit. The effect increases to 1.09 percentage points after a municipality has been audited at random ($p = 0.070$ for the interaction term), a gain of about 34% relative to the pre-audit level.

The effect is robust for several alternative specifications. For comparison, Column (1) of Table IV shows the initial estimate.[29] The standard estimation follows each child for just two years to mitigate the problem of included families dropping out of Bolsa Família or unincluded families gaining access to the program. However, it is conceivable that the beneficial effect of the random audits is less pronounced after families have been included for some time. Column (2) displays the estimates of an intent-to-treat approach that follows families for an additional

---

29. The table shows the estimates for the most representative sample. However, the result is also robust for all specifications in the sample of newly registered families and for almost all specifications in the sample of families that are also matched on the month they updated their data. See Appendix II for details on the robustness checks, including the results in the other samples, and additional tests.
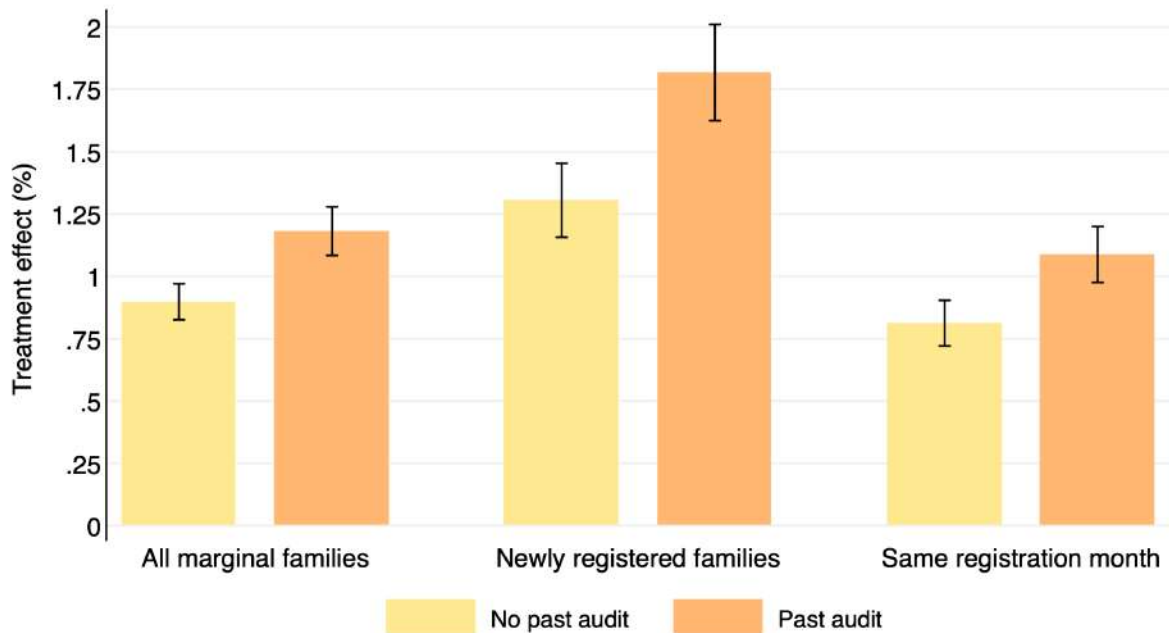
Figure II

Bolsa Família Is More Effective After A Random Audit

Notes. This figure displays the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment. Treatment effects are estimates using the specification in Equation (4). Colors indicate whether a municipality has been audited at random.

year and ignores (potentially non-random) dropout and new inclusions. As expected, ignoring later entries and exists to the program leads to lower estimates of Bolsa Família's effectiveness; 0.44 percentage points as opposed to 0.90 percentage points. However, the gains after a random audit are slightly larger and continue to be statistically significant ($p = 0.045$).

Educational participation varies by age and gender, and there are several well-documented patterns such as the delayed enrollment of younger children and the increased dropout rate for older boys (De Brauw et al., 2015). Column (3) shows that the result is robust if Age $\times$ Sex fixed effects are included. The interaction term continues to be significant ($p = 0.044$), and the estimates of Bolsa Família's impact are similar to the initial result—both before and after a municipality is audited at random.

Bolsa Família has a special provision for children above the age of 15. These children have a lower attendance requirement (75% instead of the usual 85%) and are legally able to work as part of an apprenticeship. Column (4) shows that although the point estimates are somewhat smaller if older children are included, Bolsa Família continues to be significantly more effective after a random audit ($p = 0.040$).

Although families within a priority stratum are randomly admitted to Bolsa Família, families in some priority strata have considerably higher probabilities of being included (see Figure A1 in Appendix II). To address this, Column (5) tests whether the result is robust if stabilized

25

TABLE IV

ROBUSTNESS TO ALTERNATIVE SPECIFICATIONS

	(1) Original specification	(2) Including third year	(3) Demographic fixed effects	(4) Including teenagers	School enrollment (%) (5) Inverse prob. weights	(6) Propensity score 10–90%	(7) Only original audit lottery	(8) Including current audit	(9) Data from current year	(10) Excluding 2017
BF	0.898***	0.442***	0.992***	0.750***	0.922***	0.876***	0.897***	0.900***	0.509***	0.915***
	(0.072)	(0.084)	(0.071)	(0.063)	(0.067)	(0.072)	(0.072)	(0.073)	(0.062)	(0.076)
Past audit	-0.157	-0.588*	-0.114	-0.095	-0.104	-0.096	-0.043	-0.648**	-0.119	-0.044
	(0.281)	(0.245)	(0.265)	(0.255)	(0.292)	(0.291)	(0.352)	(0.240)	(0.293)	(0.352)
BF × Past audit	0.283*	0.366*	0.258*	0.231*	0.274*	0.263*	0.287*	0.275*	0.252*	0.292*
	(0.131)	(0.182)	(0.128)	(0.113)	(0.128)	(0.132)	(0.131)	(0.130)	(0.119)	(0.140)
Control mean	87.283	87.920	87.283	87.214	87.283	87.211	87.283	87.277	88.747	87.152
Child FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age × Sex FE	No	No	Yes	No	No	No	No	No	No	No
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.925	0.858	0.928	0.929	0.931	0.926	0.925	0.925	0.925	0.923
N(municipalities)	5,401	5,401	5,401	5,416	5,401	5,401	5,401	5,401	5,395	5,381
N(priority strata)	12,559	12,752	12,559	15,250	12,559	12,547	12,559	12,559	7,233	11,696
N(children)	2,573,117	2,585,404	2,573,117	3,058,499	2,573,117	2,127,141	2,573,117	2,573,117	2,266,681	2,384,393
N	5,146,234	7,171,463	5,146,234	6,116,998	5,146,234	4,254,282	5,146,234	5,146,234	4,533,362	4,768,786
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2016

Notes. This table reports on the robustness of the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Column (1) presents the results from the initial specification for comparison. Column (2) presents the results if children are followed for up to three years. Column (3) presents the results if "Age × Sex" fixed effects are included to control for different patterns of educational participation. Column (4) presents the results if teenagers up to age 17 are included. Column (5) presents the results if families with a treatment propensity of less than 10% or more than 90% are excluded. Column (7) presents the results if families in the treatment propensity. Column (6) presents the results if teenagers up to age 17 are included. Column (5) presents the results if families with a treatment propensity of less than 10% or more than 90% are excluded. Column (7) presents the results if only the 40 rounds of the original Programa de Fiscalização por Sorteios Públicos are considered. Column (8) presents the results if municipalities are considered to be previously audited if the audit occurs in the same year. Column (9) presents the results if only families are included that have updated their data in the current year. Column (10) presents the results when the year 2017, where information is observed mid-year, is excluded. All results are for the most representative sample, where families are matched on the municipality, vulnerability category, the exact income, the number of children, and the year the families last updated their data. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. For the fixed effects, families in each priority stratum have the exact same income, the same number of children, belong to the same vulnerability category, and have last updated their data in the same month. Standard errors are clustered at both the family and the municipality level. Significance levels: +p < 0.1, *p < 0.05, **p < 0.01, ***p < 0.001.

inverse probability weights are applied to correct for the higher treatment propensities in some strata.[30] The estimates are of similar magnitude, and the interaction term continues to be significant ($p = 0.032$). Column (6) tests whether the effect persists if families with a treatment propensity of less than 10% or more than 90% are excluded from the analysis. The estimates are again of similar magnitude, and the interaction term continues to be significant ($p = 0.046$).

In 2016, the Programa de Fiscalização por Sorteios Públicos was superseded by the Programa de Fiscalização em Entes Federativos. As the third cycle of the new program consisted of a random audit lottery, it is included in the definition of the *Past audit* indicator. Column (6) shows that the result is robust if only the 40 rounds of the original audit lottery are considered. The estimates are again of similar magnitude, and the interaction term continues to be significant ($p = 0.029$).

Because the audit reports are often only published late in the calendar year or even at the beginning of the next one, the *Past audit* indicator is defined to take value 1 if a municipality has randomly been audited in a *previous* year. Column (7) relaxes this and considers municipalities as having been audited in the past, even if the audit takes place in the current year. Despite reclassifying 198 municipalities, the result does not change significantly: Bolsa Família is again roughly 30% more effective after a municipality has been audited at random and the interaction continues to be significant ($p = 0.035$).

Families are required to update their Cadastro Único registration at least every other year. Thus, in a given year, the data of some families actually reflects information from previous years. So far, this has been addressed by constructing priority strata so that families that potentially have outdated information are in separate priority strata. However, Column (9) shows that the result is robust if these priority strata are excluded ($p = 0.034$).

Finally, the data for this paper were obtained in late 2017, so the last year of the Cadastro Único data represents the state in June 2017. As a result, families that registered towards the end of the sample had less time to realize their gains, although the year fixed effects mitigate this problem to some degree. Column (10) shows that the result persists if data from 2017 is excluded: Bolsa Família is again roughly 30% more effective after a municipality has been audited at random and the interaction continues to be significant ($p = 0.037$).

V.   Understanding the Mechanism

Because municipalities are responsible for registering potential beneficiaries, local corruption is most likely to play a role at the registration stage. This section shows how local corruption increases the chance that ineligible families benefit from Bolsa Família—using a theoretical

---

30. The weights take the form $w_{1,f} = \frac{Prob(\text{BF})}{Prob(\text{BF}|\theta,m)}$ for families that get included and $w_{0,f} = \frac{1-Prob(\text{BF})}{1-Prob(\text{BF}|\theta,m)}$ for families that don't get included, where $Prob(\text{BF} \mid \theta, m)$ denotes the conditional probability of being included in Bolsa Família for a family in priority stratum θ and municipality m.

model, administrative data, and a field experiment—and how this mistargeting decreases the effectiveness of the program.

Throughout the following sections, statements from the audit of Sete Quedas (MS) in March 2015 are used to illustrate the typical ways in which municipalities fall short of their responsibilities, before testing whether these factors are likely to explain the observed performance gains. With 10,780 inhabitants at the time of the last census, Sete Quedas ranks 2,821 out of 5,570 municipalities—very close to the median. However, the municipality is chosen not so much for its demographic representativeness, but rather because the audit report is a relatively comprehensive summary of findings that are frequently encountered in the audits of other municipalities. It is worth noting that the municipality punches way below its weight in terms of educational achievement; it ranks 5,290 out of 5,570 in school enrollment despite average salaries being in the top quintile of the country.

A. Income Underreporting and Mistargeting

"We found beneficiaries of the Bolsa Família program with a per capita income higher than that established in the program's legislation." (CGU, 2015)

Bolsa Família targets those families that are most likely to underinvest in human capital: low-income families, families with many children, and families in marginalized groups. The program's impact depends crucially on its ability to reach these families. High levels of corruption make the program vulnerable to exploitation by families that don't qualify under the rules of the program. As the audit report puts it, "underreporting of income during the registration in the Cadastro Único [...] may lead to undue receipt of benefits by families outside of the target audience of government social programs and non-treatment of families in the target audience" (CGU, 2015).

This suggests a straight forward explanation why Bolsa Família should be more effective after a random audit: families that underreported their income are excluded from the program and, going forward, the municipality pays closer attention to families' incomes during the registration process. As a result, Bolsa Família is more likely to reach the families that benefit the most.

B. A Model of Income Underreporting

To better understand how income underreporting affects Bolsa Família's effectiveness, consider a simplified model of the registration process, where families decide what income to report, and the families with the lowest self-reported income are included in the program. If a family's reported income deviates too much from its true income, it risks being caught. A key assumption of the model is that the risk of detection is lower in high corruption municipalities. As more

families underreport their income, Bolsa Família can no longer target the families that benefit most, and its effectiveness decreases.

The model's structure is as follows: First, SENARC decides how many families to include in Bolsa Família, based on income data from the census. After that, families register in the Cadastro Único, potentially underreporting their incomes. The Caixa then includes the families with the lowest incomes until all places are filled. Finally, a family that is included but has underreported its income may be detected and face the consequences.

At the beginning, SENARC decides how many families to include in Bolsa Família. Let there be N families of the same vulnerability category living in the same municipality. Because SENARC sets separate numbers of beneficiaries for each category and municipality, it's reasonable to focus on just one such group when considering the strategic motives. It's assumed that the number of places M allocated by SENARC is such that not all families will be covered, $M < N$. There are two possible interpretations of this assumption: there could be insufficient funds to cover all families or the census implies that fewer than N eligible families live in the municipality.

In the next step, families register in the Cadastro Único and decide what income y they report. Families with an income below the eligibility threshold $\bar{y}$ qualify for Bolsa Família. Each family knows its own true per capita income x and the true distribution of income per capita, modeled by the cumulative distribution function $F(x)$ over the closed interval $[0, \bar{x}]$. Thus, no family has a negative income, but some families may have zero income.

The Caixa then selects the M families that report the lowest incomes below the eligibility threshold $\bar{y}$ from the Cadastro Único and pays them a fixed benefit of $b > 0$.

Once included in Bolsa Família, there is a risk of detection if a family underreported its income, which is lower in high corruption municipalities. Let's assume that underreporting one's income comes at an expected cost $c_m \cdot (x - y)$ that is increasing in the difference between the true income x and the reported income y. This captures several possible mechanisms, for example, that families are more likely to be found out if their true incomes—and their lifestyles—differ more from their reported incomes or that the punishment for underreporting is proportional to the deviation from the true income. The key assumption is that c_m is lower in municipalities where corruption is prevalent, either because the probability of being found out is smaller or because the expected punishment is less severe for a given degree of underreporting.[31]

Thus, when family i with income x_i registers in municipality m, it decides what income y_i to report to maximize the expected utility:

$$\max_{y_i \in [0, x_i]} U(y_i | x_i) = Prob(\text{receiving BF} | y_i) \left(b - c_m \cdot (x_i - y_i) \right) \tag{5}$$

---

31. Because only one municipality is considered in solving the model, the subscript is omitted in the proofs to simplify notation: $c = c_m$.

Proposition 1 *The optimal reporting function for a family with income x_i is given by*

$$y(x_i) = \begin{cases} 0, & x_i \leq x^* \\ x_i - \frac{b}{c_m} + \frac{1}{\mathfrak{B}(M-1;N-1,F(x_i))} \int_{x_i}^{x^r} \mathfrak{B}(M-1;N-1,F(\alpha))\,d\alpha, & x^* < x_i \leq x^r \\ x_i, & x^r < x_i \end{cases} \quad (6)$$

where $\mathfrak{B}(m;n,p)$ denotes the cumulative binomial distribution that a binary event with probability p occurs at most m out of n times and $x^r = \bar{y} + \frac{b}{c_m}$ denotes the highest income that allows families to benefit from underreporting their income. The function exhibits a discontinuity at $x^* \in \left[0, \frac{b}{c_m}\right]$ that is defined by the following equation:

$$\frac{b}{c_m} = x^* + \frac{\int_{x^*}^{x^r} \mathfrak{B}(m-1;N-1,F(\alpha))\,d\alpha}{\mathfrak{B}(M-1;N-1,F(x^*)) + \frac{M}{NF(x^*)}\left(1 - \mathfrak{B}(M;N,F(x^*))\right)} \quad (7)$$

Proof.   See Appendix I.[32]

Figure III illustrates the optimal reporting function, which has several intuitive properties. The amount of underreporting is stronger if the potential benefits b are higher, and it is lower if the expected costs c_m of being found out increase when a municipality becomes less corrupt. The model also predicts that families at the lower end of the income distribution and families with an income close to the eligibility threshold have the strongest incentives to underreport their income. Specifically, the model predicts that a disproportionate number of families report an income of zero—in line with the observed distribution of income in the Cadastro Único—and that more (fewer) families report an income of zero or an income that makes them eligible for Bolsa Família if the benefits b (costs c_m) increase (see Appendix I).

The model predicts that the true income of supposedly eligible families is higher than the income reported in the Cadastro Único and that the difference is more pronounced in places with more corruption, where families are less likely to be detected. Thus, even if the poorest M families are included in each municipality, a family with a reported income of y in a place with little underreporting is likely to be poorer than a family with the same reported income in a high corruption municipality where underreporting is more prevalent.

If Bolsa Família has a stronger effect on families with a lower *true* income, the model predicts that, conditional on the *reported* income, the treatment effects are smaller if there is

---

32. The model resembles an auction where families underbid each other to win one of the places in the Bolsa Família program. As a result, the proof is related to work in the theory of procurement auctions (e.g., Calveras et al., 2004; Compte et al., 2005; Li and Zheng, 2009) and auctions with capped bids (e.g., Che and Gale, 1998; Zheng, 2001; Gavious et al., 2002; Chen and Chiu, 2011). For convenience, it is assumed that families with an income above $x^r = \bar{y} + \frac{b}{c}$ report truthfully. However, none of the model's predictions depend on this assumption. Any reporting behavior such that the family never qualifies for Bolsa Família, i.e., $y_i > \bar{y}$, is an equilibrium. Thus, the second discontinuity at x^r in Figure III is not necessarily there, as families could in principle report an income arbitrarily close to $\hat{y}$. See Claim 1 in Appendix I.
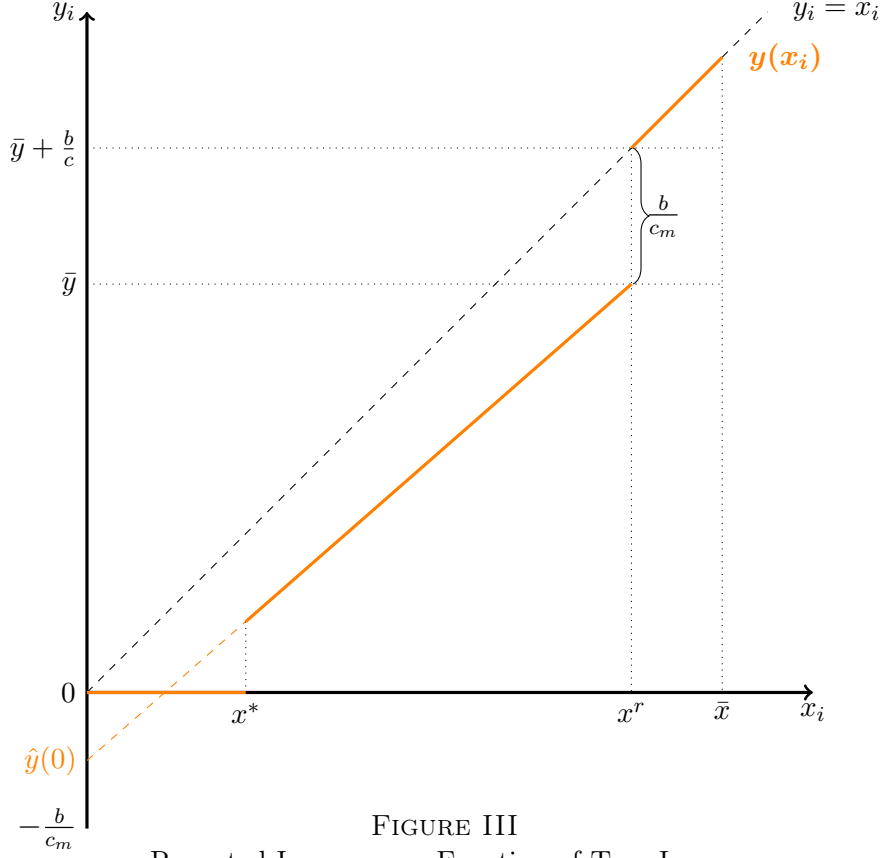
FIGURE III

Reported Income as a Function of True Income

Notes. This figure illustrates the relationship between a family's true income x_i and the income it reports y_i. The solid orange line is the optimal reporting function $y(x_i)$ if families are constrained to report non-negative incomes. The dashed orange line is the unconstrained optimal reporting function $\hat{y}(x_i)$. The dashed black line describes truthful reporting, $y_i = x_i$.

more underreporting. Suppose that school enrollment is increasing with diminishing returns in the true family income[33] including potential Bolsa Família benefits then:

Proposition 2 *Let x_{post} be the true income after the treatment assignment, i.e., $x_{post,i} = x_i + b$ if family i receives Bolsa Família and $x_{post,i} = x_i$ otherwise. Suppose that the function $g(x_{post})$ describes the relationship between school enrollment and income and that it is twice continuously differentiable with $g' > 0$ and $g'' < 0$. For $y_i > y(x^*)$:*

1. *The baseline school enrollment, $g(y^{-1}(y_i))$, is increasing in c.*

2. *The treatment effect on school enrollment, $g(y^{-1}(y_i) + b) - g(y^{-1}(y_i))$, is decreasing in c.*

Proof. The first claim follows from the fact that $y(x_i)$ is decreasing in c. The second claim additionally uses the concavity of $g(x_{post})$. $\square$

---

33. This captures the empirical relationship between income and educational participation reasonably well, see Figure A11 in Appendix IV.
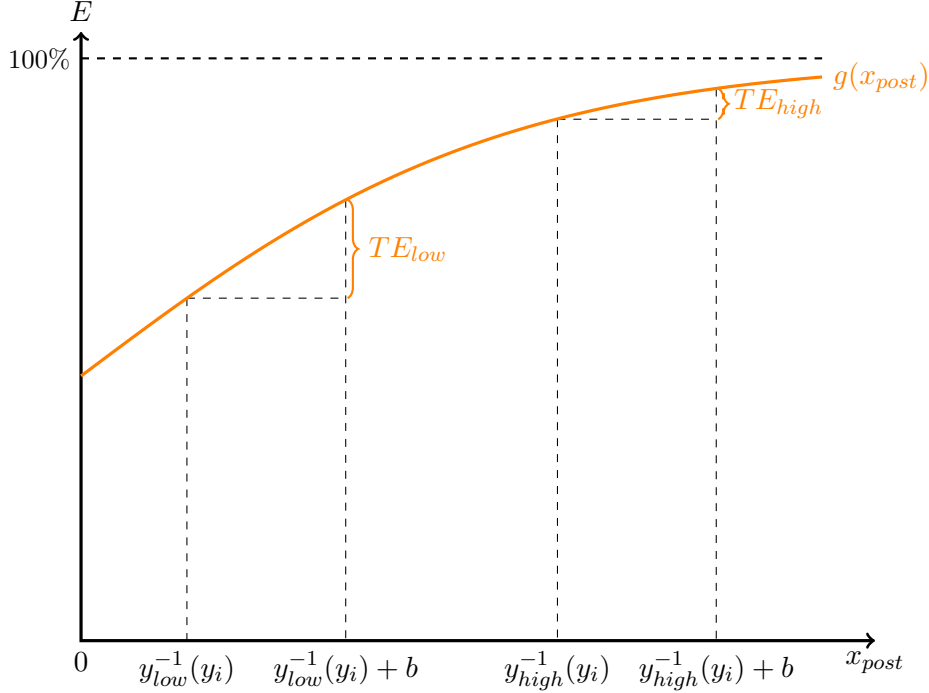
FIGURE IV

Predicted Treatment Effects For Low and High Rates of Underreporting

Notes. This figure displays the relationship between the extent of income underreporting and the predicted treatment effects of Bolsa Família. x_{post} denotes the true income after the treatment assignment, i.e., $x_{post,i} = x_i + b$ if family i receives Bolsa Família and $x_{post,i} = x_i$ otherwise. $y_{high}^{-1}(y_i)$ is the true income of a family that reports an income of y_i in a municipality with a high degree of income underreporting. $y_{low}^{-1}(y_i)$ is the true income of a family that reports an income of y_i in a municipality with a low degree of income underreporting. The function $g(x_{post})$ describes the relationship between school enrollment and income.

Figure IV illustrates the effect of underreporting on the expected treatment effects, conditional on the reported income y.

Previously, the assumption was that all families underreport their incomes equally. If some families are less able or willing to underreport, it needs no longer be the case that the M families that end up benefiting from the program are indeed the poorest M families.[34] If this is the case, the true income of some included families will exceed the true income of some families that did not underreport to the same degree. As a result, the expected true income of the included families will be even higher.

There are many situations where this is likely to be the case. For example, it is easier to verify the income of a household with a member who is formally employed or receives additional social assistance payments. Similarly, if a home visit is conducted to register some but not all of the families, social workers might be able to judge the true income of the visited families

---

34. Note that this is already not guaranteed if M is such that the highest reported income of an included family is less than $y(x^*)$.

more accurately. Local corruption can compound the problem if some families have friends or relatives with some influence on the income verification process. Moreover, understanding and gaming the system requires information about its rules and is cognitively demanding, so some families might not be aware of the strategic component.[35] Finally, families and communities might have different social norms about claiming government benefits despite not being entitled to them.[36]

C. *Testing the Model in the Administrative Data*

The registration model makes an easily testable prediction about the distribution of reported incomes as a municipality becomes less corrupt. In the aftermath of a random audit, fewer families should report an income that qualifies them for Bolsa Família, and fewer families should report an income of zero (see Claims 10 and 12 in Appendix I).

Table V shows that this is indeed the case. The share of families in the Cadastro Único[37] reporting an income below the eligibility threshold at the time of their registration falls by 1.09 percentage points (Column 1) and the share of families claiming to have an income of zero decreases by 1.12 percentage points (Column 4). Consistent with the assumption that it is harder to misrepresent one's income if the registration happens during a home visit, neither the share of families with a reported income below the eligibility threshold (Column 2) nor the share of families reporting an income of zero change significantly for families who registered during a home visit (Column 5). Although a home visit allows administrators to judge a family's true income more accurately, it is conceivable that it also makes it easier to bribe the responsible social worker. Under this scenario, the absence of a significant change would not so much indicate a low level of underreporting, but rather a continued high level of underreporting even after the audits. This interpretation, however, is unlikely as reporting an income of zero and reporting a qualifying income are both significantly less common for families registered at home ($p = 0.000$, two-sided t-tests).

In the case of Sete Quedas, CGU auditors cross-referenced data on formal employment and pensions with data from the Cadastro Único and inspected the homes of 31 families with inconsistent records. The per capita income of seven of these families made them ineligible for Bolsa Família. Three of these families reported an income of zero, while the others reported incomes that are close to the extreme poverty or the eligibility threshold at the time of their first registration. Further investigations revealed that most families deliberately "forgot" to mention

---

35. Recent research suggests that this presents a bigger challenge for poorer families (e.g., Shankar et al., 2011; Mani et al., 2013), which might exacerbate the distortion.

36. The sixth wave of the World Value Survey included a question whether this is justifiable behavior. If Brazilian states were countries, the relatively rich Espírito Santo would have the strongest norms of any country against illegally claiming benefits (before the Netherlands), whereas the state of Alagoas would have the weakest norms of any country (after Mexico).

37. The entire Cadastro Único, not just a sample of families on the margin of the program.

TABLE V
UNDERREPORTING DECREASES AFTER RANDOM AUDITS

	Eligible income (%)			Income R$0.00 (%)		
	(1) Total	(2) Home	(3) CRAS	(4) Total	(5) Home	(6) CRAS
Past audit	-1.090*	-0.219	-1.309*	-1.115*	-0.762	-0.997^{+}
	(0.506)	(1.282)	(0.543)	(0.530)	(0.904)	(0.516)
Control mean	60.505	55.952	60.512	10.823	9.779	10.981
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.946	0.742	0.940	0.844	0.673	0.844
N(municipalities)	5539	5504	5539	5539	5504	5539
N	33226	31544	33226	33226	31544	33226
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of the random audits on the distribution of self-reported income in the Cadastro Único. The dependent variable in Columns (1) to (3) is the percentage of families in a municipality who report an income that made them eligible at the time of registration. The dependent variable in Columns (4) to (6) is the percentage of families in a municipality who report having zero income. Columns (1) and (4) present the results for all families in the municipality, Columns (2) and (5) for families that were registered during a home visit, and Columns (3) and (6) for families that registered at the CRAS. "Past audit" indicates that a municipality has been audited at random. All models include municipality and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^{+}p < 0.1$,$^{*}p < 0.05$,$^{**}p < 0.01$,$^{***}p < 0.001$.

a source of income or to register a family member who receives a pension. The families have subsequently been excluded from the program.

This anecdote suggests an additional test of whether income underreporting and mistargeting decrease after a municipality has been audited at random. Immediately after the audit, exclusions from Bolsa Família should increase, before dropping to a lower level than before the audit, as municipalities inspect self-reported income more closely and are less likely to admit ineligible families. As predicted, Figure A12 in the Appendix shows that exclusions from the program increase immediately after a municipality has been audited at random, before dropping to a lower rate than before the audit ($p = 0.106$ and $p = 0.047$, respectively; see Table A16 in Appendix V). After a random audit, municipalities are also somewhat more likely to conduct home visits as part of the registration process, although the increase is not statistically significant ($p = 0.156$, two-sided t-test).

The second part of the model illustrates how income underreporting translates into lower expected treatment effects. If the effectiveness gains are driven by improvements in the targeting of low-income families and if it is easier for families to underreport their income if no home visit is conducted as part of the registration process, the effectiveness gains should be concentrated among families that registered at the registration center (CRAS), where income verification is more effortful. In families that registered at home, Bolsa Família is expected to be equally effective at incentivizing school enrollment irrespective of the audits. Table A15 in Appendix V shows that this is indeed the case: while the treatment effect increases by at least 25% for families registered at the registration center ($p = 0.025$, $p = 0.037$, and $p = 0.065$, for the three
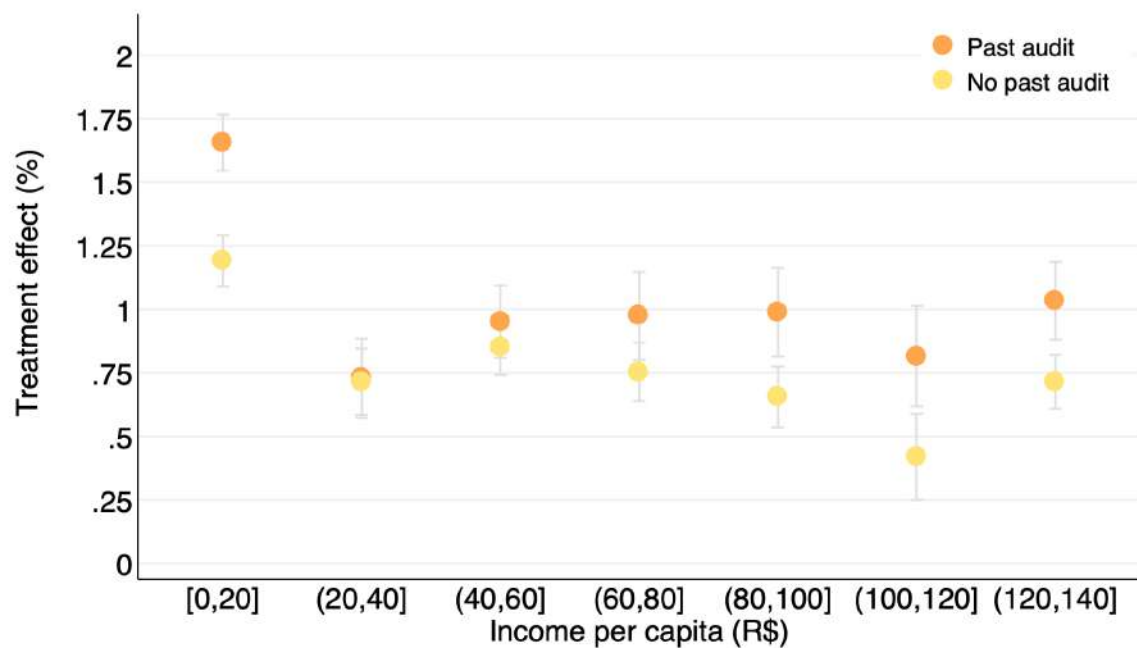
FIGURE V

Program Effectiveness Increases Most for the Lowest and Highest Self-Reported Incomes

Notes. This figure displays the estimated effect of inclusion in the Bolsa Família program on children's school enrollment by income brackets. Treatment effects were jointly estimated in the most representative sample by interacting the treatment indicators in Equation (4) with an indicator for the family's income bracket. Error bars indicate standard errors of the estimated treatment effect and are clustered at both the family and municipality level.

samples), there is no change for families who registered during a home visit—point estimates for the interaction are much smaller and not statistically significant.

The families that underreported their income in Sete Quedas fell in two categories: families that report an income of zero and families that report close to the eligibility threshold. The theoretical model indeed predicts that families with incomes close to zero and families with relatively high incomes have the strongest incentives to underreport. Figure V shows that, in line with these predictions, the effectiveness gains after a random audit are concentrated at the lower and the upper end of the income distribution.[38] In unaudited municipalities, families in the lowest income bracket are only 1.19 percentage points more likely to send their children to school once they are included in Bolsa Família. In contrast, after a municipality has been audited at random, Bolsa Família increases school enrollment by 1.66 percentage points, a relative gain of almost 40% ($p = 0.004$). While there are no significant gains for families in the next three income brackets, treatment effects increase by 50% ($p = 0.095$), 95% ($p = 0.082$), and 45% ($p = 0.084$), respectively, for the three highest income brackets. Note that this is compatible

38. Note also that while Bolsa Família increases school enrollment at all income levels, the poorest families gain most from the program, in line with the assumptions of the model.

with increased income underreporting close to the eligibility cut-off, as the income threshold was less than R$ 100 when the earliest families in the sample registered and has since been increased several times.

Thus, the income underreporting model is consistent with the observed patterns in the administrative data: immediately after a random audit ineligible families are excluded from the program and, going forward, fewer families report an eligible income or an income of zero. As a result, the Bolsa Família can be more precisely targeted, and its effectiveness increases, especially for the income levels with the highest predicted misreporting.

D. Testing the Model in a Field Experiment

To see if local administrators are indeed less likely to register ineligible families that have been audited at random, I conducted a field experiment with 6,998 Bolsa Família registration centers (CRAS).[39] Registration centers were contacted asking about the possibility of receiving Bolsa Família and the information provided in the message was experimentally varied to make the sender eligible or ineligible while holding other characteristics constant. Consistent with the income underreporting explanation, centers in audited municipalities differentiate more between eligible and ineligible families: they reply less to ineligible families and are more likely to point out if a sender's income is incompatible with Bolsa Família. For a more detailed description and additional results of the field experiment, see Appendix III.

TABLE VI
EMAIL TEXT BY TREATMENT

Eligible I	Eligible II	Ineligible
Greetings,	Greetings,	Greetings,
My family recently moved here, and I would like to register for Bolsa Família. I make around R$ 450 a month and I live alone with my two children. Can you help me to register?	My family recently moved here, and I would like to register for Bolsa Família. I make around R$ 300 a month and I live alone with my child. Can you help me to register?	My family recently moved here, and I would like to register for Bolsa Família. I make around R$ 450 a month and I live alone with my child. Can you help me to register?
	Thank you in advance.	Thank you in advance.
Thank you in advance.		

Notes. This table displays the three experimental conditions in the field experiment.

Over several months, three emails were sent to registration centers that provided an email address as part of their official contact details. The emails asked about registering for the Bolsa Família program and provided information that makes the sender either eligible—per capita

---

39. The experiment was approved by the Human Subjects Committee of the Faculty of Economics, Business Administration, and Information Technology at the University of Zurich (*OEC IRB # 2019-010*) and was preregistered at the AEA RCT registry under the number *AEARCTR-0004151*.
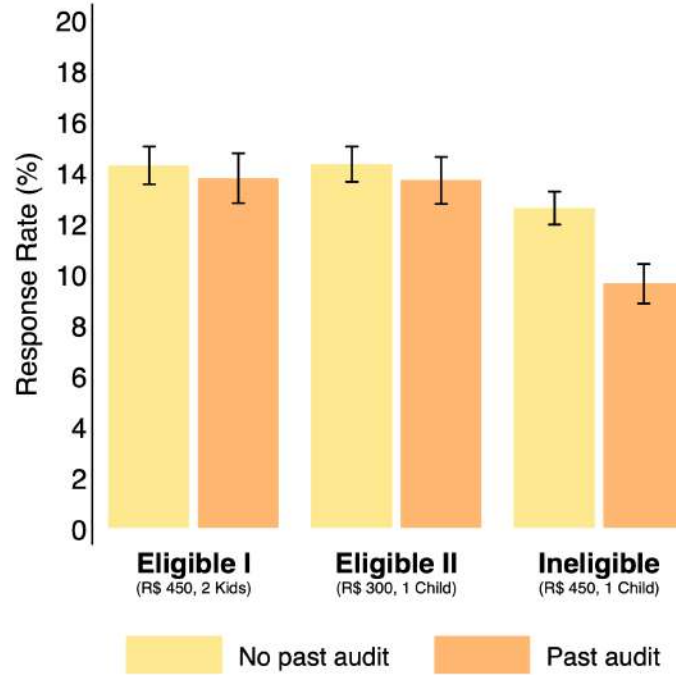
FIGURE VI

Response Rate to Requests from Eligible and Ineligible Families

Notes. This figure displays the difference in response rates in the three experimental conditions of the field experiment. Error bars indicate standard errors and are clustered at the municipality level.

income $<$ R\$ 170—or ineligible (see Table VI). Relative to the "Ineligible" treatment, "Eligible I" varied the number of children and "Eligible II" reduced the reported income.[40]

Figure VI shows that registration centers were significantly more likely to reply to requests from eligible families. Consistent with the hypothesis that local corruption makes it easier for ineligible families to gain access to the program, the effect is significantly larger for centers in municipalities that have previously been audited. In unaudited municipalities, requests from ineligible families were less likely to receive a response than requests from eligible families—1.68 percentage points relative to the Eligible I treatment ($p = 0.012$) families and 1.73 percentage points relative to the Eligible II treatment ($p = 0.010$). The differences increase to 4.15 percentage points ($p = 0.000$) and 4.07 percentage points ($p = 0.000$), respectively, if a munic-

---

40. Emails were sent in three waves at the beginning of May, June, and July 2019. Within waves, emails were sent at a random time on a workday between 9:00 and 17:00 in the centers time zone. The order of the emails and their timing was randomized at the municipality level and block-randomized with respect to states and whether a municipality has been audited at random. Roughly a quarter of emails could not be delivered and returned an error message from the host. This failure rate is independent of the wave of the experiment and whether a municipality has previously been audited or not ($\chi^2(5) = 3.248$, $p = 0.662$; see also Figure A2 in Appendix III). For most of the analysis, these messages are excluded. However, Table A10 in Appendix III shows that treating delivery errors as non-responses does not alter the results.

TABLE VII

RESPONSE RATES TO REQUESTS FROM ELIGIBLE AND INELIGIBLE
FAMILIES

	(1)	(2)	(3)	(4)	(5)
Ineligible	-1.708**	-1.526*	-1.500*	-1.672**	-1.686**
	(0.589)	(0.597)	(0.602)	(0.589)	(0.591)
Past audit	0.455			0.250	0.330
	(1.000)			(0.932)	(0.932)
Ineligible × Past audit	-2.396*	-2.703**	-2.694**	-2.435**	-2.512**
	(0.932)	(0.938)	(0.944)	(0.932)	(0.942)
Population (Log.)				3.510***	3.509***
				(0.488)	(0.489)
Income inequality (Gini)				-11.855$^+$	-12.170*
				(6.139)	(6.167)
Income per capita (Log.)				5.632**	5.618**
				(1.764)	(1.766)
Illiteracy				0.015	0.020
				(0.075)	(0.075)
Urban population				-0.707	-0.532
				(2.234)	(2.244)
Control mean	14.303	14.371	14.371	14.303	14.303
State FE	Yes	Yes	Yes	Yes	Yes
Center FE	No	Yes	Yes	No	No
Order FE	No	No	Yes	No	Yes
Subject line FE	No	No	Yes	No	Yes
Day FE	No	No	Yes	No	Yes
Time FE	No	No	Yes	No	Yes
R2	0.039	0.599	0.606	0.068	0.078
N	15891	15736	15736	15891	15891

Notes. This table reports the difference in response rates to requests from eligible and ineligible families in the field experiment. The dependent variable is an indicator that takes value 100 if the registration center replied to a request and 0 otherwise. "Ineligible" indicates that the details in the request made a family ineligible for Bolsa Família, "Past audit" indicates that a municipality has been audited at random, and "Ineligible × Past audit" is the interaction of the two treatments. "Population (Log.)", "Income inequality (Gini)", "Income per capita (Log.)", and the rates of "Illiteracy" and "Urban population" control for municipality characteristics in 2000, before the inception of the audits program. Columns (2) and (3) include registration center fixed effects. Columns (3) and (5) include fixed effects for the order of emails, the different subject lines, the day of the week and the exact time of day the email was sent. All models include state fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^+p < 0.1,^* p < 0.05,^{**} p < 0.01,^{***} p < 0.001$.

ipality has been audited at random. The treatment effects are significantly stronger in audited municipalities ($p = 0.024$ and $p = 0.031$ for the comparisons with Eligible I and II conditions, respectively). Because the response rates are relatively low, these numbers imply that requests about registration with ineligible details were approximately 12% less likely to receive a response in unaudited municipalities, but roughly 30% less likely to receive a response in municipalities that have previously been audited.[41]

Because municipalities are randomly selected for audits within states, I pre-registered that

---

41. For a discussion of why the response rates are relatively low, see Appendix III.

I would run the regression with state fixed effects to account for the stratification. Table VII shows that the effect persists if state fixed effects are used (Column 1), if only within registration center variance is exploited (Columns 2 and 3), and if the control variables from Avis et al. (2018) are used, as specified in the pre-analysis plan (Columns 4 and 5). In addition, Columns (3) and (5) control for extensive design fixed effects: the order of emails, the different subject lines, the day of the week and the exact time of day the emails were sent. Table A12 in Appendix III shows that the effects persist if the two control treatments are included separately in the regressions.

As each center receives three similar emails, the response to emails in later waves might be affected by the emails in earlier waves: the email might look familiar to social workers or be more likely to end up in a spam filter. To address these concerns, I pre-registered a robustness check to show that the effect persists if only the first wave of emails is used. The effect is even more pronounced in the first wave of the experiment. Request from ineligible families are significantly less likely to receive a response in municipalities that have been randomly audited in the past— 7.35 percentage points compared to the Eligible I treatment ($p = 0.000$) and 6.36 percentage points compared to the Eligible II treatment ($p = 0.001$). In unaudited municipalities, the effects are much weaker—2.78 percentage points compared to the Eligible I treatment ($p = 0.061$) and 0.91 percentage points compared to the Eligible II treatment ($p = 0.524$). The treatment effects are significantly stronger in audited municipalities ($p = 0.049$ and $p = 0.016$ for the comparisons with Eligible I and II conditions, respectively). See also Table A11 in Appendix III.

Although responding to emailed requests is an imperfect proxy for the likelihood of including a family in the Cadastro Único, the results of the field experiment are consistent with the income underreporting mechanism: randomly audited municipalities pay closer attention to families' eligibility.

VI. Financial Gains From Bolsa Família

In this paper, I argue that Bolsa Família has reasonably effective safe guards against clientelism and embezzlement. This claim is based primarily on the program's design—payments directly to beneficiaries' cards instead of bulk payments to the local administration and an anonymized central process to select beneficiaries. While this prevents outright embezzlement of funds and the trading of program access for bribes or votes, it does not completely rule out the possibility of financial gains from the program.

If local officials want to benefit financially from the program, they are left with three options: retaining benefit cards, registering themselves in the Cadastro Único, and diverting other funds connected to local registration centers or the complementary programs municipalities are required to offer for Bolsa Família recipients. In this section, I show that while each of these practices has been known to happen, they cannot account for the effectiveness gains of Bolsa Família after a municipality has been audited at random.

A. Retaining Benefit Cards

Retaining benefit cards allows a corrupt official to withdraw the payments from the account of legitimate beneficiary families. This was a major concern in the early years of the program but has since become very uncommon.

Once a family is admitted to the Bolsa Família program, the Caixa provides it with a magnetic stripe card that can be used to withdraw the benefits at branches of the Caixa, special ATMs, lottery points, postal offices, and certain shops. Most cards are delivered through the postal system and beneficiaries need to sign a receipt. If this is not possible, the cards are returned to the Caixa that then attempts to deliver them through other channels, e.g., by inviting families to pick them up at the nearest Caixa branch or by sending them to another location specified by the beneficiary. Once families have received their card, they have to register their PIN number at the nearest Caixa branch before they can access any benefits.

There are several ways a corrupt official can take possession of a families card. Prior to 2006, local branches of the Caixa were responsible for distributing the cards to beneficiary families and there were several incidents where cards were not immediately delivered to beneficiaries or where there was no proof of receipt (Lindert et al., 2007). Now that most cards are delivered through the postal system, fewer cards are vulnerable to this kind of fraud. Alternatively, a corrupt administrator can seize the card under a pretext or a store owner can refuse to return the card to a customer to force her to spend the benefits at his store. Finally, in remote areas, some people will offer to go to town and collect the payments for multiple families for a cut of the benefits.

The MDS receives 10-20 reports of retained cards per year—relatively few considering the almost 14 million benefit cards in circulation. Sete Quedas is representative in that the MDS has never received a complaint about retained cards since they started to collect this information in 2010; in an average year, the MDS receives not a single complaint from more than 99.7% of municipalities. As the financial damages incurred through this form of corruption are roughly proportional to the number of retained cards, the total losses are relatively minor. Even if we assume that only one in a hundred affected cards is being reported, 99.99% of payments would still be unaffected. Thus, funds stolen by retaining benefit cards cannot account for the effectiveness gains of Bolsa Família.

B. Self-registration in the Cadastro Único

> "Beneficiary families with a member employed by the city hall underreported their income."

Like any other citizen, corrupt officials can benefit by registering themselves or a family member in the Cadastro Único and providing inaccurate information that increases the probability of being included. Unlike most families, however, local officials and program administrators

can potentially exert control over the registration and income verification process. Because of this, the MDS and the CGU periodically cross-check beneficiaries with databases on elected officials and public employees. Billy the cat, who made a brief appearance in Section II, actually belonged to a local program administrator (Hider, 2014).

The financial impact of this strategy, however, is relatively small. The fraction of payments syphoned to public employees can be quantified based on audits where CGU auditors cross-referenced public employment records with data from the Cadastro Único. In the case of Sete Quedas, for example, CGU auditors discovered one Bolsa Família beneficiary who had been employed by the municipality for over a decade and earned a monthly wage of more than R\$ 2,100. Despite some news-worthy cases, less than 0.75% of payments are affected in any given year.[42]

To put this number into perspective, we can compare it to the average share of families uncovered to have underreported their income in the random samples of families investigates by the CGU (see Figure A10 in Appendix IV). This share varies from 8.5% (in 2014) to 23.9% (in 2010). This is in line with the findings from similar programs in Indonesia (Alatas et al., 2019) where local elites had significantly more control over the allocation of benefits; while local elites capture some of the benefits, the effect is not economically large and tends to pale in comparison to the costs of other targeting errors.

C. Embezzling Complementary Funds

Diverting other funds from local registration centers can potentially be quite lucrative compared to the other two strategies. If corrupt officials embezzle funds that are designated for the local registration centers, the resulting infrastructure and staff shortages might affect the performance of the Bolsa Família program. Moreover, municipalities are required to offer complementary programs designed to help Bolsa Família recipients to comply with the program's conditionalities and to realize lasting improvements in the standard of living. Here too, significant amounts can go missing. Section VII discusses these points in more detail and shows that changes in infrastructure, registration center employment, and the availability of complementary programs cannot account for the gains in program effectiveness.

VII.   Alternative Explanations

Until now, the analysis has focused primarily on income underreporting and the resulting mistargeting as an explanation for the effectiveness gains of Bolsa Família after a random audit. In this section, I show that while common findings from the CGU's audit reports provide

---

42. The number of cases was manually extracted from the CGU reports by a Brazilian research assistant and it covers municipal, state, and federal employees. The estimate is conservative, as it also includes cases where public employees were entitles to some, but not all of the benefits they received.

anecdotal evidence for several additional mechanisms how random audits might affect the effectiveness of the Bolsa Família program, none of these candidates—closer school attendance monitoring, higher data quality, changes in administrative processes, better infrastructure and funding, complementary social programs, tighter governance, or increased whistleblowing—can explain the increase in Bolsa Família's effectiveness.

A. School Monitoring

> "School attendance records of students benefiting from the Bolsa Família program entered in the Projeto Presença system by the municipality's program manager are in disagreement with those found in class books." (CGU, 2015)

Municipalities' other major responsibility, besides registering families, is collecting the data to monitor compliance with Bolsa Família's conditionalities—most importantly children's school attendance. However, there is no evidence that school attendance records become more accurate after a random audit.

Bolsa Família depends on municipal administrators to report if children of beneficiaries fail to attend school regularly enough to comply with the program's conditionalities. Teachers record students' absences in the class book, which is in turn used by the school administrators to report attendance in the ministry of education's Projeto Presença system. As the auditors explain, insufficient monitoring risks that Bolsa Família provides only short-term relief but no sustainable progress in the fight against poverty and social marginalization (CGU, 2015). Thus, Bolsa Família will be less successful if corrupt administrators don't fulfill their responsibility or collude with families to overstate compliance with conditionalities.

In the case of Sete Quedas, several students were given 99% attendance scores in the monitoring system despite having insufficient attendance rates for the two months scrutinized during the audit. Sete Quedas' negligence to adequately monitor school attendance is relatively benign compared to the failures uncovered in some other municipalities, where "class books are not traceable", "students who benefit from Bolsa Família cannot be located", and school officials "lack knowledge of their responsibilities", "report on the attendance of students enrolled in other schools", or "report 100% attendance for all students without any documentation."

Unfortunately, the Cadastro Único does not contain information on students' school attendance rates. However, the audit reports can be used to see whether school monitoring improves in municipalities that have been audited at random. Using the specification in Equation (3), there is no significant change in the number of irregularities related to attendance monitoring in municipalities that have previously been audited ($p = 0.960$). Additionally, temporary blockage of benefits due to non-compliance with the conditionalities of Bolsa Família, most importantly school attendance, can be used as a proxy for how closely families are monitored. Table A16 shows that even though the number of families with temporarily blocked benefits increases sig-

nificantly immediately after a random audit ($p = 0.032$), this is not a lasting change and there is no significant change in the long run ($p = 0.811$). Thus, improvements in school monitoring are unlikely to explain the lasting effectiveness gains of Bolsa Família after a municipality has been audited at random.

B. *Data Inconsistencies*

> "[...] the local manager should update the registry entries of the beneficiaries indicated in the inspection report, to adjust the data recorded in the CadÚnico with the real family structure." (CGU, 2015)

Mistargeting can originate not only from deliberately misleading statements during the registration process but also from unintentional errors and outdated information in the database. Because the MDS relies on local administrators for the registration and monitoring of beneficiaries, it reimburses municipalities with a payment per household per month if the municipality fulfills its responsibilities sufficiently well.

To assess municipalities' implementation of Bolsa Família, the MDS constructs a monthly index of municipal management quality (IGD-M) based on the consistency of data in the Cadastro Único and the school and health monitoring systems. The index is a weighted sum of the rates of school monitoring (the fraction of children in beneficiary families with updated entries in the Projeto Presença system), health monitoring (the fraction of families subject to medical conditionalities that is covered in the health monitoring system), and the fraction of up-to-date entries in the Cadastro Único. Until July 2015, the estimated coverage rate of the Cadastro Único also contributed to the index.[43] If the IGD-M and its subindices exceed a certain threshold and some additional administrative requirements are satisfied, municipalities receive IGD-M × R$ 3.25 per valid registration, plus additional incentive payments, for example, to follow up on families with suspended benefits.

If Bolsa Família becomes more effective because municipalities improve their data management, this will be reflected in the IGD-M and its components. Table A17 in Appendix V shows that this not the case. The index is unchanged, as are the subindices for health, data updating, and the coverage rate of the registry. If anything, school monitoring worsens slightly. The absence of changes in these indicators, however, conceals significant changes in both the nominators and the denominators that make up these fractions: There are fewer children whose school attendance is monitored, but also fewer children in beneficiary families. There are fewer families whose medical checkups are monitored, but also fewer families required to do the checkups (see Table A18 in Appendix V). In short, significantly fewer families are registered as being eligible, again suggesting that municipalities exclude ineligible families in the aftermath of an

---

43. Later, the coverage rate was so close to 100% that the MDS stopped using this information. See Figure A4 in Appendix IV.

audit and are more careful going forward.

C.   CRAS Processes

> "[...] the identified problems are caused by the lack of pre-established and properly
> formalized routines for verifying and monitoring compliance with the legislation that
> governs the program." (CGU, 2015)

When asked to explain irregularities in the income of registered families, municipal admin-istrators in Sete Quedas resorted to case-by-case explanations and tried to place the blame solely on the families. According to the auditors, they did not sufficiently appreciate that these cases are symptomatic of inadequate processes at the local registration center (CGU, 2015).

The IGD-M is a rather crude tool to monitor the registration centers and it is mostly used to incentivize consistency between the interlinking computer systems. More informative about the actual processes and practices is the annual census of social assistance, Censo SUAS, that is completed by the social assistance centers (CRAS). The survey collects basic information on employees and detailed information on physical infrastructure, technical equipment, and administrative processes—including how the Cadastro Único is updated. Questions include whether the CRAS updates the registry,[44] which employees work with the database, and more recently also in what format data is initially collected.

The results in Table A19 in Appendix V suggest that the auditors' well-meaning advice is largely ignored: centers continue to update the Cadastro Único in pretty much the same way as before the audit. Centers are equally likely to update the Cadastro Único, just as likely to have a special team to work with the registry, and individual employees are equally likely to handle the database. When collecting information on families, CRAS centers that do update the Cadastro Único stick to the method they have always used and are just as likely to rely on paper or to enter the data digitally than before the audit.

Table A20 in Appendix V fails to find any significant change in the composition of the 80,000-strong workforce at the CRAS. Employees have the same age and gender profile, the same experience, similar working hours, and the same educational level: the shares of employees with at most completed primary education, secondary education, some college, a college degree, or a post-graduate qualification all remain unchanged. There is also no change in the legal aspects of the employment relationships: employees are just as likely to be hired under the rigid Consolidation of Labor Laws (CLT) after an audit as before, and the same holds for civil servant appointed by the mayor under the discretionary rules of Art. 37.

Finally, CRAS centers in audited municipalities are also not more likely to seek citizen participation to ensure that their activities suit the needs of their clients. Table A21 in Appendix

---

44. Not all centers input data in the Cadastro Único because some lack the necessary infrastructure and training or focus on other forms of social assistance.

V shows that if anything, centers are less likely to invite citizen participation to improve the center's services: the share of centers that report not soliciting any input from the population—formal or otherwise—increases by 7.59 percentage points, albeit not significantly ($p = 0.163$). Moreover, centers are 2.51 percentage points less likely to have elected citizen representatives ($p = 0.076$) and 5.44 percentage points less likely to have a citizen committee after a random audit ($p = 0.095$), making it unlikely, that increased citizen participation is behind the observed performance gains.

Overall, changes in practices at the CRAS are insufficient to account for the improved performance of Bolsa Família after a municipality has been audited at random.

D. CRAS Infrastructure and Funding

"Used and broken toys were delivered to the Tia Solíria Day Care Center." (CGU, 2015)

One way by which local corruption can hinder the effectiveness of government policy is through its effect on municipal infrastructure: even if the money from Bolsa Família payments reaches beneficiaries, the program could be less effective if the CRAS centers lack the infrastructure to properly serve families in the municipality.

In the previous rounds of the audit program, the infrastructure at the CRAS centers in several municipalities was inspected and found lacking.[45] Unfortunately, the auditors did not inspect the infrastructure at the CRAS in Sete Quedas, but the mechanism is well-illustrated by a curious incident involving a woman the auditors identify as "the first-lady of Sete Quedas" (CGU, 2015): About half a year before the audit, the municipality spend R$ 4,614.78 it had received from the National Fund for Education Development to buy educational toys and sporting equipment for the Tia Solíria Day Care Center. At the time of the audit, the toys were in the nursery, but they were in two garbage bags—unpacked, dirty, broken, and incomplete. Asked about the garbage bags, the head of the nursery said that they had been delivered in the previous week by the first lady of the municipality. Having initially given the toys to other unidentified families, she scrambled to retrieve them when she learned of the impending audit.

However, as shown in Figure A13 in Appendix IV, major changes in center infrastructure are as conspicuously absent as changes in the workforce and practices of registration centers: For the 37 analyzed survey items in the Censo SUAS—covering everything from the physical infrastructure (number and type of rooms), whether they comply with the norms for accessibility in public buildings (ABNT NBR9095), the ownership of the premises, IT and other technological infrastructure, vehicles available to the CRAS staff, to whether the center has a toys and sporting equipment at its disposal—, there is not a single significant change after correcting for multiple

---

45. Common findings include lack of computers (e.g., CGU, 2014a), problems with of accessibility (e.g., CGU, 2014b), and absence of sufficiently large rooms for communal activities (e.g., CGU, 2014c).

hypothesis testing (FDR; Benjamini and Hochberg, 1995).[46]

Similarly, one might wonder whether municipalities allocate more funds for the proper administration of their social programs. However, Tables A22 and A23 in Appendix V show that neither the amount municipalities spend on social programs and their administration nor the amount they spend on education increases significantly after a municipality has been audited at random. This result holds irrespective whether expenditure is analyzed in absolute numbers, per capita, or as a share of the total municipal budget. If anything, municipalities decrease per capita social expenditure by about 4.1% ($p = 0.061$). Thus, neither changes in the infrastructure nor the funding for social programs can account for the increased effectiveness after a random audit.

E. *Complementary Actions and Programs*

"[...] it was verified that the Municipality did not offer complementary programs to Bolsa Família." (CGU, 2015)

Complementary programs for beneficiary families—literacy classes, occupational training, microcredits, and guidance in accessing government services—are an important ingredient of Bolsa Família's strategy to overcome poverty in a sustainable way (CGU, 2015). Municipalities play a key role in the development of these services and are especially called upon to assist families that are in breach of their Bolsa Família conditionalities. It is straightforward to see how improvements in these programs after a random audit might account for the effectiveness gains that we observe. This explanation, however, is not supported by the data; there is no significant change in complementary programs after a random audit.

While the Census SUAS does not explicitly ask about complementary programs for Bolsa Família, it does ask about activities and programs aimed at vulnerable families in general through the Serviço de Proteção e Atendimento Integral à Família (PAIF). Although PAIF does not exclusively serve families in Bolsa Família, a large number of its activities focus on low-income families (Afonso et al., 2013) so that PAIF activities are a good proxy for the existence of complementary programs.

Figure A14 in Appendix IV shows the effect of a random audit on 23 programs and activities targeted at vulnerable families. None of the changes are significant at the 5% level, even without correction for multiple hypothesis testing (Benjamini and Hochberg, 1995). Centers don't change their outreach programs to welcome new families, don't offer different programs and activities, don't provide more specialized coaching for families in various life situations, and are just as likely to refer families to other social and public services than before the municipality has been audited. Most relevantly for Bolsa Família, there is no evidence that CRAS centers are more

---

46. Without multiple hypothesis correction, only one item changes significantly at the 5% level: after an audit, centers are more likely to occupy a building owned by the municipality ($p = 0.035$).

likely to coach families that are in breach of their conditionalities ($p = 0.909$), nor that they are more likely to help families to register or update their data in the Cadastro Único ($p = 0.950$).

F. Governance and Oversight

"The Municipal Council of Social Assistance does not fulfill its obligations to monitor and inspect the programs and services." (CGU, 2015)

Brazilian municipalities are required to establish a Council for Social Assistance (CMAS) to monitor the local provision of social services. More conscientious governance could account for the effectiveness gains of Bolsa Família if social councils monitor programs more closely after a random audit. This explanation, however, is not consistent with the data from the Censo SUAS.

Although the social council's powers and responsibilities vary from place to place, they usually involve approving plans and budgets for local social services, establishing the rules to grant special relieve to families hit by certain life-events, and monitoring and overseeing the various social and welfare programs. Social councils should be composed of representatives of the municipal administration and other organizations providing social assistance, as well an equal number of members from civic organizations, program beneficiaries, and other representatives of the public. In many cases, the regulations specify that the presidency rotates between representatives of the administration and the public.

In the case of Sete Quedas, the social council is formally responsible for monitoring all aspects of Bolsa Família—the registration and data management in the Cadastro Único, the monitoring of compliance, the temporary blockages of benefits, and the development of complementary programs.[47] When asked by the auditors, however, the members of the social council in Sete Quedas confirmed not only that they had not carried out any inspections of the municipality's social programs in the previous year but that they had not even met during this time (CGU, 2015).

Figure A15 in Appendix IV shows that social councils are unlikely to account for the increased effectiveness of Bolsa Família after a random audit: social councils are equally likely to be responsible for the program and its monitoring, to discuss results from inspections in meetings, to include a beneficiary representative, to have a commission specifically for Bolsa Família, or to receive and discuss alleged misconduct and abuse of the program. More generally, councils discuss the same topics, engage in the same activities, and are governed by the same rules. Among 53 items in the annual census of social councils, there is not a single one that changes significantly after correcting for multiple hypothesis testing (Benjamini and Hochberg, 1995).[48]

---

47. This is increasingly the case: 73.3% of social councils had this responsibility in 2011, and the number has increased to 91.8% in 2017.

48. Without the correction, only one item is significant at the 5% level: social councils may be somewhat

"Civic participation in the control of the Bolsa Família Program is restricted due to non-disclosure of the list of beneficiaries of the program by the municipal administration." (CGU, 2015)

Municipalities are required to publicly post lists of all Bolsa Família beneficiaries with their names and social security numbers, similar to the disclosure made by the federal government through the Portal da Transparência. Publication of this lists, it is assumed, enables members of the public to denounce families that illegitimately claim benefits: "it should be emphasized that the disclosure of the list of Bolsa Família beneficiaries is important to make the program transparent, to identify irregularities and to allow possible denunciations by citizens" (CGU, 2015). These complaints against families and officials received through whistleblower systems are investigated as part of the audits.

In the case of Sete Quedas, the MDS did not record any denunciations of illegitimate payments from citizens or program administrators, nor any complaints against employees of the CRAS. Subsequent inspections of the public areas at the city hall and the CRAS uncovered that the municipality had failed to publish the list of beneficiaries.

There is, however, no evidence that more citizens blow the whistle after a municipality has been audited at random. Table A24 in Appendix V shows that there is no change in the number of denunciations the MDS receives from beneficiaries, non-beneficiaries, or program administrators, nor is there a change in the number of complaints about the CRAS or its employees. Naturally, not every complaint will be made through the MDS's system and complaints with local authorities are not collected centrally. Although it cannot directly be observed whether the number of complaints made to local authorities increases significantly after a random audit, municipalities report whether they have a special ombudsman to deal with denunciations, and whether their social council received and discussed any denunciations over the last year. None of these items change significantly after a municipality has been audited at random.[49] Thus, increased social control is unlikely to explain the improved performance of Bolsa Família after an audit.

VIII. Conclusion

This paper shows how corruption negatively impacts the effectiveness of government policy in a setting where bribery, clientelism, and embezzlement play at most a negligible role. Despite Bolsa Família's strong safeguards against corruption, local corruption significantly reduced its

more likely to organize town-hall meetings ($p = 0.047$).

49. See Figure A15 in Appendix IV for the result on social councils. The result on special ombudsmen is based on a regression with state fixed effects, as the question was only included in the Censo SUAS 2017, precluding the use of the more rigorous specification with municipality and time fixed effects.

effectiveness. After a municipality has been audited at random, Bolsa Família becomes roughly 30% more effective at increasing school enrollment. Thus, even though Bolsa Família bypasses local governments for the allocation and payment of benefits to minimize the potential for corruption (Lindert et al., 2007), local corruption can still affect the effectiveness of the program if it leads to more income underreporting.

These effectiveness gains are largely driven by better targeting of the program to families that benefit the most. The mechanism is illustrated using a theoretical model of the registration process where families decide how to report their income: underreporting one's income increases the probability of being included in Bolsa Família and detection is less likely in high corruption municipalities. The model is consistent with patterns in the distribution of reported incomes and administrative data on the effects of home-visits during the registration process and the number of families excluded from the program due to income underreporting. A field experiment with registration centers provides additional evidence that income underreporting is easier in municipalities that have not been audited. Other explanations such as tampering with benefit cards, closer monitoring of school attendance, better administrative processes, improvements in infrastructure and funding for social assistance, complementary programs for beneficiary families, and tighter governance and social control cannot explain the results.

Although using Bolsa Família's official database offers many advantages over secondary data sets, it limits which policy outcomes can be studied. The Cadastro Único is not a particularly rich dataset when it comes to educational outcomes and it includes little that can be used to study the program's health priorities. For example, it does not include information from the school attendance and health systems used to monitor compliance with the program's conditionalities. Using data from the ministries of health and education, future research could show whether local corruption also reduces Bolsa Família's effectiveness in promoting its health and nutritional goals and whether the school enrollment gains translate into improved test scores and better labor market outcomes.

The results of this paper speak to the positive effects of government audits (e.g., Di Tella and Schargrodsky, 2003; Olken, 2007; Bobonis et al., 2016; Avis et al., 2018): while the focus of government audits is to reduce the embezzlement of funds, they have second-order effects such as stimulating economic activity (Bologna et al., 2015; Colonnelli and Prem, 2017; Giannetti et al., 2017), improving educational attainment through better school funding (Ferraz et al., 2012) or, in this case, improved targeting of social programs. However, it is worth noting that the random audits not exclusively investigate whether municipalities can account for their use of federal funds and whether the goods and services they pay for have been delivered. As the audits look specifically for evidence of ineligible families in the Bolsa Família program, it is an open question whether the same gains would be observed if the auditors focused solely on municipalities' use of funds.

The results of the paper also have implications for the effective targeting of social programs.

Bolsa Família's increased effectiveness after a municipality has been audited appear to be driven solely by the fact that the program is more likely to reach the families that respond most to the program. Borrowing from the field of personalized medicine (Kent et al., 2018), one promising approach to poverty alleviation focuses on predicting families' heterogeneous treatment responses to maximize the effectiveness of anti-poverty programs (e.g., McBride and Nichols, 2018).

Given the importance of accurate self-reporting and the challenges of income verification for the effective targeting of Bolsa Família and similar programs, future research should explore interventions that increase the likelihood of accurate reporting. Studies in other settings provide encouraging evidence of significant improvements in the honesty of self-reports from remedies as simple as having people sign at the beginning rather than at the end of a self-report (Shu et al., 2012), including a moral appeal or a reminder about the possibility of detection (Bott et al., 2017), and priming participants' religious (Randolph-Seng and Nielsen, 2007), professional (Cohn et al., 2014), and social identify (Cohn et al., 2015). Approaches could also focus on the administrator's duty to verify families' incomes, possibly inspired by interventions to curb physicians' overprescription of antibiotics (e.g., Meeker et al., 2014, 2016). This research should focus on those design aspects of Bolsa Família that might inadvertently increase underreporting: As underreporting only increases the probability of being included, there is a moral wiggle room to think that underreporting not necessarily leads to illegitimate benefits (Dana et al., 2007). Similarly, some families who would not underreport if the social worker made the inclusion decision directly might do so if an impersonal process in the capital decides (Mazar et al., 2008).

References

ABDULAI, A.-G. AND S. HICKEY (2016): "The politics of development under competitive clientelism: Insights from Ghana's education sector," *African Affairs*, 115, 44–72.

AFONSO, M. L. M., C. B. HENNON, T. L. CARICO, AND G. W. PETERSON (2013): "A methodological approach for working with families in SUAS: a critical reading through the lens of citizenship," *Psicologia & Sociedade*, 25, 80–90.

ALATAS, V., A. BANERJEE, R. HANNA, B. A. OLKEN, R. PURNAMASARI, AND M. WAI-POI (2019): "Does elite capture matter? Local elites and targeted welfare programs in Indonesia," in *AEA Papers and Proceedings*, vol. 109, 334–39.

ALATAS, V., A. BANERJEE, R. HANNA, B. A. OLKEN, AND J. TOBIAS (2012): "Targeting the poor: evidence from a field experiment in Indonesia," *American Economic Review*, 102, 1206–40.

ALDERMAN, H. (2002): "Do local officials know something we don't? Decentralization of targeted transfers in Albania," *Journal of public Economics*, 83, 375–404.

AVIS, E., C. FERRAZ, AND F. FINAN (2018): "Do Government Audits Reduce Corruption? Estimating the Impacts of Exposing Corrupt Politicians," *Journal of Political Economics*, 126, 1912–1964.

BENJAMINI, Y. AND Y. HOCHBERG (1995): "Controlling the false discovery rate: A practical and powerful approach to multiple testing," *Journal of the Royal Statistical Society. Series B*, 57, 289–300.

BENJAMINI, Y. AND D. YEKUTIELI (2005): "False discovery rate–adjusted multiple confidence intervals for selected parameters," *Journal of the American Statistical Association*, 100, 71–81.

BERTRAND, M., S. DJANKOV, R. HANNA, AND S. MULLAINATHAN (2007): "Obtaining a driver's license in India: an experimental approach to studying corruption," *The Quarterly Journal of Economics*, 122, 1639–1676.

BOBONIS, G. J., L. R. CÁMARA FUERTES, AND R. SCHWABE (2016): "Monitoring corruptible politicians," *American Economic Review*, 106, 2371–2405.

BOHN, S. R. (2011): "Social policy and vote in Brazil: Bolsa Família and the shifts in Lula's electoral base," *Latin American Research Review*, 54–79.

BOLOGNA, J., A. ROSS, ET AL. (2015): "Corruption and entrepreneurship: Evidence from a random audit program," *Department of Economics Working Paper Series*, No. 15-05.

BOTT, K. M., A. W. CAPPELEN, E. Ø. SØRENSEN, AND B. TUNGODDEN (2017): "You've got mail: A randomised field experiment on tax evasion," *NHH Working Paper*.

BRAZIL (2016): "Governo coloca em prática ação para barrar fraudes no Bolsa Família," *Portal Brasil*.

BROLLO, F., K. KAUFMANN, AND E. LA FERRARA (2019): "The political economy of program enforcement: Evidence from Brazil," *Journal of the European Economic Association*.

CALVERAS, A., J.-J. GANUZA, AND E. HAUK (2004): "Wild bids. Gambling for resurrection in procurement contracts," *Journal of Regulatory Economics*, 26, 41–68.

CARAM, B. (2016): "Ministério aponta 1,1 milhão de irregularidades no Bolsa Família," *G1 Globo*.

CARDOSO, E. AND A. P. SOUZA (2003): "The impact of cash transfers on child labor and school attendance in Brazil," *Working Paper*.

CGU (2014a): "39a Etapa do Programa de Fiscalização a partir de Sorteios Públicos - Baía da Traição/PB," Tech. rep., Controladoria-Geral da União.

——— (2014b): "39a Etapa do Programa de Fiscalização a partir de Sorteios Públicos - Camapuã/MS," Tech. rep., Controladoria-Geral da União.

——— (2014c): "39a Etapa do Programa de Fiscalização a partir de Sorteios Públicos - Xavantina/SC," Tech. rep., Controladoria-Geral da União.

——— (2015): "40a Etapa do Programa de Fiscalização a partir de Sorteios Públicos - Sete Quedas/MS," Tech. rep., Controladoria-Geral da União.

CHE, Y.-K. AND I. L. GALE (1998): "Caps on political lobbying," *The American Economic Review*, 88, 643–651.

CHEN, B. R. AND Y. S. CHIU (2011): "Competitive bidding with a bid floor," *International Journal of Economic Theory*, 7, 351–371.

COHN, A., E. FEHR, AND M. A. MARÉCHAL (2014): "Business culture and dishonesty in the banking industry," *Nature*, 516, 86.

COHN, A., M. A. MARÉCHAL, AND T. NOLL (2015): "Bad boys: How criminal identity salience affects rule violation," *The Review of Economic Studies*, 82, 1289–1308.

COLONNELLI, E. AND M. PREM (2017): "Corruption and firms: evidence from randomized audits in Brazil," *SSRN 2931602*.

COMPTE, O., A. LAMBERT-MOGILIANSKY, AND T. VERDIER (2005): "Corruption and competition in procurement auctions," *Rand Journal of Economics*, 1–15.

DAÏEFF, L. (2015): "Why the Bolsa Família is not clientelistic (and what it might be instead)," *Chroniques des Amériques*, 15.

DANA, J., R. A. WEBER, AND J. X. KUANG (2007): "Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness," *Economic Theory*, 33, 67–80.

DE BRAUW, A., D. O. GILLIGAN, J. HODDINOTT, AND S. ROY (2015): "The impact of Bolsa Familia on schooling," *World Development*, 70, 303–316.

DE JANVRY, A., F. FINAN, AND E. SADOULET (2012): "Local electoral incentives and decentralized program performance," *Review of Economics and Statistics*, 94, 672–685.

DI TELLA, R. AND E. SCHARGRODSKY (2003): "The role of wages and auditing during a crackdown on corruption in the city of Buenos Aires," *The Journal of Law and Economics*, 46, 269–292.

FAUSTO MACEDO, JULIA AFFONSO, M. C. (2016): "Procuradoria aponta fraude de R$ 2,5 bi no Bolsa Família," *Estadão*.

FERRAZ, C. AND F. FINAN (2008): "Exposing corrupt politicians: the effects of Brazil's publicly released audits on electoral outcomes," *The Quarterly Journal of Economics*, 123, 703–745.

FERRAZ, C., F. FINAN, AND D. B. MOREIRA (2012): "Corrupting learning: Evidence from missing federal education funds in Brazil," *Journal of Public Economics*, 96, 712–726.

FIRPO, S., R. PIERI, E. PEDROSO JR, AND A. P. SOUZA (2014): "Evidence of eligibility manipulation for conditional cash transfer programs," *EconomiA*, 15, 243–260.

FRIED, B. J. (2012): "Distributive politics and conditional cash transfers: the case of Brazil's Bolsa Família," *World Development*, 40, 1042–1053.

GAVIOUS, A., B. MOLDOVANU, AND A. SELA (2002): "Bid costs and endogenous bid caps," *RAND Journal of Economics*, 709–722.

GIANNETTI, M., G. LIAO, J. YOU, AND X. YU (2017): "The externalities of corruption: Evidence from entrepreneurial activity in China," *CEPR Discussion Paper No. DP12345*.

GLEWWE, P. AND A. L. KASSOUF (2012): "The impact of the Bolsa Escola/Familia conditional cash transfer program on enrollment, dropout rates and grade promotion in Brazil," *Journal of Development Economics*, 97, 505–517.

GUPTA, S., H. DAVOODI, AND E. TIONGSON (2001): "Corruption and the provision of health care and education services," in *The political economy of corruption*, Routledge, 123–153.

HIDER, J. (2014): "Cat is out of the bad in welfare scam in Brazil," *The Times*.

HOLMBERG, S. AND B. ROTHSTEIN (2011): "Dying of corruption," *Health Economics, Policy and Law*, 6, 529–547.

HUNTER, W. AND T. J. POWER (2007): "Rewarding Lula: Executive power, social policy, and the Brazilian elections of 2006," *Latin American Politics and Society*, 49, 1–30.

KAUFMANN, D. (2005): "Six Questions on the Cost of Corruption with World Bank Institute," Tech. rep., The World Bank.

KENT, D. M., E. STEYERBERG, AND D. VAN KLAVEREN (2018): "Personalized evidence based medicine: predictive approaches to heterogeneous treatment effects," *Bmj*, 363, k4245.

LI, T. AND X. ZHENG (2009): "Entry and competition effects in first-price auctions: theory and evidence from procurement auctions," *The Review of Economic Studies*, 76, 1397–1429.

LINDERT, K., A. LINDER, J. HOBBS, AND B. DE LA BRIÈRE (2007): "The nuts and bolts of Brazil's Bolsa Família Program: implementing conditional cash transfers in a decentralized context," *Social Protection Discussion Paper*.

MANI, A., S. MULLAINATHAN, E. SHAFIR, AND J. ZHAO (2013): "Poverty impedes cognitive function," *science*, 341, 976–980.

MAURO, P. (1995): "Corruption and growth," *The quarterly journal of economics*, 110, 681–712.

——— (2004): "The persistence of corruption and slow economic growth," *IMF staff papers*, 51, 1–18.

MAZAR, N., O. AMIR, AND D. ARIELY (2008): "The dishonesty of honest people: A theory of self-concept maintenance," *Journal of marketing research*, 45, 633–644.

MCBRIDE, L. AND A. NICHOLS (2018): "Retooling poverty targeting using out-of-sample validation and machine learning," *The World Bank Economic Review*, 32, 531–550.

MCPAKE, B., D. ASIIMWE, F. MWESIGYE, M. OFUMBI, L. ORTENBLAD, P. STREEFLAND, AND A. TURINDE (1999): "Informal economic activities of public health workers in Uganda: implications for quality and accessibility of care," *Social science & medicine*, 49, 849–865.

MDS AND SENARC (2015): "Manual de Gestão do Programa Bolsa Família (2a Edição atualizada)," Tech. rep., Governo Federal, Ministério do Desenvolvimento Social e Combate à Fome e Secretaria Nacional de Renda de Cidadania.

——— (2018): "Manual de Gestão do Programa Bolsa Família (3a Edição atualizada)," Tech. rep., Governo Federal, Ministério do Desenvolvimento Social e Secretaria Nacional de Renda de Cidadania.

MEEKER, D., T. K. KNIGHT, M. W. FRIEDBERG, J. A. LINDER, N. J. GOLDSTEIN, C. R. FOX, A. ROTHFELD, G. DIAZ, AND J. N. DOCTOR (2014): "Nudging guideline-concordant antibiotic prescribing: a randomized clinical trial," *JAMA internal medicine*, 174, 425–431.

MEEKER, D., J. A. LINDER, C. R. FOX, M. W. FRIEDBERG, S. D. PERSELL, N. J. GOLDSTEIN, T. K. KNIGHT, J. W. HAY, AND J. N. DOCTOR (2016): "Effect of behavioral interventions on inappropriate antibiotic prescribing among primary care practices: a randomized clinical trial," *Jama*, 315, 562–570.

MO, P. H. (2001): "Corruption and economic growth," *Journal of comparative economics*, 29, 66–79.

MOSTERT, S., F. NJUGUNA, G. OLBARA, S. SINDANO, M. N. SITARESMI, E. SUPRIYADI, AND G. KASPERS (2015): "Corruption in health-care systems and its effect on cancer care in Africa," *The Lancet Oncology*, 16, e394–e404.

OGLOBO (2016): "Pente-fino do governo descobre fraude no Bolsa Família," *O Globo*.

OLKEN, B. A. (2006): "Corruption and the costs of redistribution: Micro evidence from Indonesia," *Journal of public economics*, 90, 853–870.

——— (2007): "Monitoring corruption: evidence from a field experiment in Indonesia," *Journal of political Economy*, 115, 200–249.

OLKEN, B. A. AND P. BARRON (2009): "The simple economics of extortion: evidence from trucking in Aceh," *Journal of Political Economy*, 117, 417–452.

PEI, Z., J.-S. PISCHKE, AND H. SCHWANDT (2019): "Poorly measured confounders are more useful on the left than on the right," *Journal of Business & Economic Statistics*, 37, 205–216.

PENFOLD-BECERRA, M. (2007): "Clientelism and social funds: Evidence from Chávez's Misiones," *Latin American Politics and Society*, 49, 63–84.

RANDOLPH-SENG, B. AND M. E. NIELSEN (2007): "Honesty: One effect of primed religious representations," *The International Journal for the Psychology of Religion*, 17, 303–315.

RAVALLION, M. (2008): "Miss-targeted or Miss-measured?" *Economics Letters*, 100, 9–12.

——— (2009): "How relevant is targeting to the success of an antipoverty program?" *The World Bank Research Observer*, 24, 205–231.

REINIKKA, R. AND J. SVENSSON (2005): "Fighting corruption to improve schooling: Evidence from a newspaper campaign in Uganda," *Journal of the European economic association*, 3, 259–267.

SCHAFFLAND, E. (2012): "Conditional Cash Transfers in Brazil: Treatment Evaluation of the Bolsa Família Program on Education," Tech. rep., Courant Research Centre: Poverty, Equity and Growth-Discussion Papers.

SHANKAR, S., R. GAIHA, AND R. JHA (2011): "Information, access and targeting: The national rural employment guarantee scheme in India," *Oxford Development Studies*, 39, 69–95.

SHU, L. L., N. MAZAR, F. GINO, D. ARIELY, AND M. H. BAZERMAN (2012): "Signing at the beginning makes ethics salient and decreases dishonest self-reports in comparison to signing at the end," *Proceedings of the National Academy of Sciences*, 109, 15197–15200.

Stoeffler, Q., B. Mills, and C. Del Ninno (2016): "Reaching the Poor: cash transfer program targeting in Cameroon," *World Development*, 83, 244–263.

Sugiyama, N. B. and W. Hunter (2013): "Whither clientelism? Good governance and Brazil's Bolsa Família program," *Comparative Politics*, 46, 43–62.

Tanzi, V. and H. Davoodi (1998): "Corruption, public investment, and growth," in *The welfare state, public investment, and growth*, Springer, 41–60.

TCU (2006): "Relatório de Acompanhamento do Programa Bolsa Família," Tech. Rep. TC n° 022.093.2006-5, Governo Federal, Tribunal de Contas da União.

Zamboni, Y. and S. Litschig (2018): "Audit risk and rent extraction: Evidence from a randomized evaluation in Brazil," *Journal of Development Economics*, 134, 133–149.

Zheng, C. Z. (2001): "High bids and broke winners," *Journal of Economic theory*, 100, 129–171.

Zucco, C. (2009): "Cash-Transfers and Voting Behavior: An Empirical Assessment of the Political Impacts of the Bolsa Fam lia Program," in *APSA 2009 Toronto Meeting Paper*.

Appendix I.

Derivation of the Registration Model

A.   Properties of the Cumulative Binomial Distribution

Let $\mathfrak{B}(m; n, p)$ denote the cumulative binomial distribution that a binary event with probability p occurs at most $m \in \mathbb{N}_0$ out of $n \in \mathbb{N}$ times: $\mathfrak{B}(m; n, p) = \sum_{k=0}^{m} \binom{n}{k} p^k (1-p)^{n-k}$

Lemma 1 *For $m < n$ and $p \in (0, 1)$, $\mathfrak{B}(m; n, p)$ is strictly increasing in m.*

Proof.   Trivial.

Lemma 2 *For $m < n$ and $p \in (0, 1)$, $\mathfrak{B}(m; n, p)$ is strictly decreasing in n.*

Proof.   Let $B(a, b)$ denote the beta function and $I_x(a, b)$ the regularized incomplete beta function.

$$
\begin{aligned}
\mathfrak{B}(m; n+1, p) - \mathfrak{B}(m; n, p) &= I_{1-p}(n-m+1, m+1) - I_{1-p}(n-m, m+1) \\
&= I_{1-p}(n-m, m+1) - \frac{(1-p)^n p^{m+1}}{(n-m)B(n-m, m+1)} - I_{1-p}(n-m, m+1) \\
&= -\frac{(1-p)^n p^{m+1}}{\frac{(n-m)!(m!)}{n!}} = -\binom{n}{m}(1-p)^n p^{m+1} < 0 \quad \square
\end{aligned}
\tag{8}
$$

Lemma 3 *For $m < n$, $\mathfrak{B}(m; n, p)$ is strictly decreasing in p.*

Proof.

$$
\begin{aligned}
\mathfrak{B}_p(m; n, p) &= \sum_{k=0}^{m} \binom{n}{k} \left(kp^{k-1}(1-p)^{n-k} - (n-k)p^k(1-p)^{n-k-1} \right) \\
&= \frac{n!}{0!(n-0)!} \left(0 - n(1-p)^{n-1} \right) \\
&\quad + \frac{n!}{1!(n-1)!} \left((1-p)^{n-1} - (n-1)p(1-p)^{n-2} \right) \\
&\quad + \frac{n!}{2!(n-2)!} \left(2p(1-p)^{n-2} - (n-2)p^2(1-p)^{n-3} \right) \\
&\quad + \dots \\
&\quad + \frac{n!}{m!(n-m)!} \left(mp^{m-1}(1-p)^{n-m} - (n-m)p^m(1-p)^{n-m-1} \right) \\
&= -\frac{n!}{m!(n-m-1)!} p^m(1-p)^{n-m-1} < 0 \quad \square
\end{aligned}
\tag{9}
$$

56

B.   Deriving the Reporting Function

If a family i with income x_i registers, it will decide what income y_i to report to maximize the expected utility:

$$\max_{y_i \in [0, x_i]} U(y_i | x_i) = Prob(\text{receiving BF}|y_i)\,(b - c \cdot (x_i - y_i)) \tag{10}$$

where $b > 0$ is the benefit a family receives from being part of the Bolsa Família program and $c \cdot (x_i - y_i)$ are the expected costs if it is later detected that the family underreported its income.

Claim 1 *If $\bar{x} > \bar{y} + \frac{b}{c} = x^r$, families with an income $x_i \in (x^r, \bar{x}]$ will never report an income $y_i \leq \bar{y}$. Thus, they will never be included in Bolsa Família.*

Proof.   The expected utility from reporting y_i given a true income x_i is $Prob(\text{receiving BF}|y_i)$ $(b - c \cdot (x_i - y_i))$. For $x_i > \bar{y} + \frac{b}{c}$, the expected costs of detection exceed the gains from being included in Bolsa Família. As $Prob(\text{receiving BF}|y_i) > 0$ for $y_i \leq \bar{y}$, a family with $x_i > \bar{y} + \frac{b}{c}$ has strictly negative expected utility from reporting an eligible income. $\square$

For convenience, these families are assumed to report their income truthfully. However, none of the results are affected by this assumption.

Claim 2 *Suppose that families are not constraint to reporting positive incomes. Then, families with an income of $x_i \leq x^r = \bar{y} + \frac{b}{c}$ report according to the unconstrained reporting function $\hat{y}(x_i)$:*

$$\hat{y}(x_i) = x_i - \frac{b}{c} + \frac{1}{\mathfrak{B}(M-1; N-1, F(x_i))} \int_{x_i}^{x^r} \mathfrak{B}(M-1; N-1, F(\alpha))\, d\alpha \tag{11}$$

where $\mathfrak{B}(m; n, p)$ denotes the cumulative binomial distribution that a binary event with probability p occurs at most m out of n times.

Proof. The expected utility from reporting y_i given a true income x_i is:

$$U(y_i|x_i) = Prob(\text{receiving BF}|y_i)\,(b - c \cdot (x_i - y_i))$$

$$= \sum_{k=0}^{M-1} \binom{N-1}{k} F(\hat{y}^{-1}(y_i))^k \left(1 - F(\hat{y}^{-1}(y_i))\right)^{N-k-1} (b - c \cdot (x_i - y_i)) \tag{12}$$

$$= \mathfrak{B}\left(M-1; N-1, F(\hat{y}^{-1}(y_i))\right)(b - c \cdot (x_i - y_i))$$

The first order condition with respect to y_i is given by:

$$0 = \frac{d}{dy_i}\left[\mathfrak{B}\left(M-1; N-1, F(\hat{y}^{-1}(y_i))\right)(b - c \cdot (x_i - y_i))\right]$$

$$= \mathfrak{B}_p\left(M-1; N-1, F(\hat{y}^{-1}(y_i))\right) f(\hat{y}^{-1}(y_i))\frac{1}{\hat{y}'(\hat{y}^{-1}(y_i))}(b - c \cdot (x_i - y_i)) \tag{13}$$

$$+ c \cdot \mathfrak{B}\left(M-1; N-1, F(\hat{y}^{-1}(y_i))\right)$$

Using $y_i = \hat{y}(x_i)$ and rearranging:

$$\frac{d}{dx}\left[\mathfrak{B}(M-1; N-1, F(x_i))\,\hat{y}(x_i)\right] = \frac{d}{dx}\left[\mathfrak{B}(M-1; N-1, F(x_i))\,x_i\right]$$

$$- \frac{d}{dx}\left[\mathfrak{B}(M-1; N-1, F(x_i))\right]\frac{b}{c} - \mathfrak{B}(M-1; N-1, F(x_i)) \tag{14}$$

By integrating from x_i to x^r and using the boundary condition $\hat{y}(x^r) = \bar{y}$, the optimal reporting function is recovered:

$$\hat{y}(x_i) = x_i - \frac{b}{c} + \frac{1}{\mathfrak{B}(M-1; N-1, F(x_i))}\int_{x_i}^{x^r} \mathfrak{B}(M-1; N-1, F(\alpha))\,d\alpha \tag{15}$$

Next it is verified that no family can increase its expected utility by deviating from the reporting function. Consider the expected utility of a family with a true income x_i that reports as if its income were x', and suppose all other families report according to $\hat{y}(x)$:

$$U(\hat{y}(x')|x_i) = \mathfrak{B}(M-1; N-1, F(x'))\,(b - c \cdot (x_i - \hat{y}(x')))$$

$$= \mathfrak{B}(M-1; N-1, F(x'))\,(b - c \cdot (x' - \hat{y}(x')))$$

$$- c \cdot \mathfrak{B}(M-1; N-1, F(x'))\,(x_i - x')$$

$$= c \cdot \int_{x'}^{x^r} \mathfrak{B}(M-1; N-1, F(\alpha))\,d\alpha - c \cdot \int_{x'}^{x_i} \mathfrak{B}(M-1; N-1, F(x'))\,d\alpha$$

$$= c \cdot \int_{x_i}^{x^r} \mathfrak{B}(M-1; N-1, F(\alpha)) \, d\alpha$$

$$+ c \cdot \int_{x'}^{x_i} \left(\mathfrak{B}(M-1; N-1, F(\alpha)) - \mathfrak{B}(M-1; N-1, F(x')) \right) \, d\alpha \tag{16}$$

$$= \mathfrak{B}(M-1; N-1, F(x_i)) \left(b - c \cdot (x_i - \hat{y}(x_i)) \right)$$

$$+ c \cdot \int_{x'}^{x_i} \left(\mathfrak{B}(M-1; N-1, F(\alpha)) - \mathfrak{B}(M-1; N-1, F(x')) \right) \, d\alpha$$

Because the cumulative binomial distribution function is strictly decreasing in the probability (Lemma 3), and $F(x)$ is strictly increasing, $\mathfrak{B}(M-1; N-1, F(x'))$ is decreasing in x. As a result, the final integral is positive for both $x' < x_i$ and $x' > x_i$ and the expected utility is lower than the expected utility from reporting $y(x_i)$. $\square$

Claim 3 *If $\hat{y}(0) < 0$ and $\frac{b}{c} < \bar{y}$, there exists a critical value $x^* \in (0, \frac{b}{c}]$ such that a family with $x_i = x^*$ is indifferent between reporting $\hat{y}(x^*)$ and reporting $y = 0$.*

Proof. If all families report according to the constraint reporting function $y(x_i)$, the probability of being included in Bolsa Família with a reported income of $y = 0$ depends on the number of other families that also report $y = 0$. If at most $M - 1$ other families report $y = 0$, reporting $y = 0$ guarantees a place in Bolsa Família. If $k \geq M$ other families report $y = 0$, a family is included with probability $\frac{M}{k+1} < 1$ if it reports $y = 0$. For $x^* = 0$, $Prob(\text{receiving BF}|0) = 1$. For $x^* > 0$, the probability of inclusion if a family reports $y = 0$ is:

$$Prob(\text{receiving BF}|0) = \mathfrak{B}(M-1; N-1, F(x^*))$$

$$+ \underbrace{\sum_{k=M}^{N-1} \binom{N-1}{k} F(x^*)^k (1 - F(x^*))^{N-k-1} \frac{M}{k+1}}_{\underbrace{\sum_{k=M+1}^{N} \binom{N-1}{k-1} F(x^*)^{k-1} (1 - F(x^*))^{N-k} \frac{M}{k}}_{\frac{M}{NF(x^*)} \sum_{k=M+1}^{N} \binom{N}{k} F(x^*)^k (1 - F(x^*))^{N-k}}} \tag{17}$$

$$= \mathfrak{B}(M-1; N-1, F(x^*)) + \frac{M}{NF(x^*)} \left(1 - \mathfrak{B}(M; N, F(x^*)) \right)$$

A family with income x^* is indifferent between reporting $\hat{y}(x^*)$ and $y = 0$ if and only if:

$$\mathfrak{B}(M-1; N-1, F(x^*)) \left(b - c \cdot (x^* - \hat{y}(x^*)) \right)$$

$$= \left(\mathfrak{B}(M-1; N-1, F(x^*)) + \frac{M}{NF(x^*)} \left(1 - \mathfrak{B}(M; N, F(x^*)) \right) \right) (b - c \cdot x^*) \tag{18}$$

For x^* arbitrarily close to 0, the expected utility from reporting $\hat{y}(x^*)$ is strictly smaller than the expected utility from reporting $y = 0$, because $\hat{y}(x^*) < 0$ and the probability of receiving Bolsa Família from reporting $\hat{y}(x^*)$ is smaller than the probability from reporting $y = 0$. At $x^* = \frac{b}{c}$, the expected utility from reporting $\hat{y}(x^*)$ exceeds the expected utility from reporting $y = 0$:

$$
\begin{aligned}
\mathfrak{B}(M-1; N-1, F(x^*))\,(c \cdot \hat{y}(x^*)) &= c \cdot \int_{\frac{b}{c}}^{x^r} \mathfrak{B}(M-1; N-1, F(\alpha))\; d\alpha \\
&\geq \left(\mathfrak{B}(M-1; N-1, F(x^*)) + \frac{M}{NF(x^*)}\left(1 - \mathfrak{B}(M; N, F(x^*))\right)\right) \cdot 0 = 0
\end{aligned}
\tag{19}
$$

By the intermediate value theorem, there exists $x^* \in (0, \frac{b}{c}]$ such that a family with income x^* is indifferent between reporting $\hat{y}(x^*)$ and reporting 0. $\square$

C. Optimality of the Reporting Function

To show that the constrained reporting function $y(x)$ is a Bayesian Nash Equilibrium, it is shown that if all other families report according to $y(x)$, a family with an income of $x_i < x^*$ has a higher expected utility from reporting 0 than from reporting any $y > 0$ (Claims 4 and 5), and a family with an income of $x^* \leq x_i < x^r$ has a higher utility from reporting according to $\hat{y}(x_i)$ than from reporting any other $y \geq 0$ (Claim 6).

Claim 4 *Suppose all other families report according to $y(x)$. Reporting $y = 0$ yields a higher expected utility than reporting $\hat{y}(x^*)$ if and only if $x_i < x^*$.*

Proof. The expected utility from reporting $\hat{y}(x^*)$ is linearly decreasing in x_i at a rate of $-\mathfrak{B}(M-1; N-1, F(x^*)) \cdot c$, while the expected utility from reporting $y = 0$ is linearly decreasing in x_i at the steeper rate of $-(\mathfrak{B}(M-1; N-1, F(x^*)) + \frac{M}{NF(x^*)}(1 - \mathfrak{B}(M; N, F(x^*)))) \cdot c$. By definition, the expected utilities are equal at $x_i = x^*$. Thus, for all $x_i < x^*$, reporting $y = 0$ leads to a strictly higher expected utility. $\square$

Claim 5 *Suppose all other families report according to $y(x)$. For $x_i < x^*$, reporting $y = 0$ is better than reporting $y > 0$.*

Proof. For $x_i < x^*$, reporting $y > \hat{y}(x^*)$ leads to lower expected utility than reporting $\hat{y}(x^*)$:

$$
\begin{aligned}
\mathfrak{B}\big(M-1; N-1, &F(\hat{y}^{-1}(y))\big)\,(b - c \cdot (x_i - y)) \\
&= \mathfrak{B}\big(M-1; N-1, F(\hat{y}^{-1}(y))\big)\,(b - c \cdot (x^* - y)) \\
&\quad + \mathfrak{B}\big(M-1; N-1, F(\hat{y}^{-1}(y))\big)\,(c \cdot (x^* - x_i)) \\
&\leq \mathfrak{B}(M-1; N-1, F(x^*))\,(b - c \cdot (x^* - y(x^*))) \\
&\quad + \mathfrak{B}(M-1; N-1, F(x^*))\,(c \cdot (x^* - x_i)) \\
&= \mathfrak{B}(M-1; N-1, F(x^*))\,(b - c \cdot (x_i - y(x^*)))
\end{aligned}
\tag{20}
$$

The inequality holds because $\hat{y}(x^*)$ is the best response for x^* and the cumulative binomial distribution is decreasing in the probability (Lemma 3). As shown earlier, reporting $y = 0$ gives higher expected utility than reporting $\hat{y}(x^*)$ for $x_i < x^*$, thus reporting $y = 0$ is better than reporting any $y > 0$ for $x_i < x^*$. $\square$

Claim 6 *Suppose all other families report according to $y(x)$. For $x^* < x_i < x^r$, reporting $\hat{y}(x_i)$ is better than reporting any other $y \geq 0$.*

Proof. Consider the following cases:

1. If $\hat{y}(0) \geq 0$, the reporting function is unconstrained and the claim follows from the optimality of $\hat{y}(x)$.

2. If $\hat{y}(0) < 0$ and $y = 0$, the claim follows from Claim 4.

3. If $\hat{y}(0) < 0$ and $0 < y < \hat{y}(x^*)$, the probability of being included is the same whether the family reports y or $\hat{y}(x^*)$, but the expected utility is strictly smaller if it reports y:
$$
\mathfrak{B}(M-1; N-1, F(x^*))\,(b - c \cdot (x_i - y)) < \mathfrak{B}(M-1; N-1, F(x^*))\,(b - c \cdot (x_i - y(x^*)))
$$

4. If $\hat{y}(0) < 0$ and $\hat{y}(x^*) \leq y$, the claim follows directly from the optimality of $\hat{y}(x)$.

   This concludes the proof. $\square$

D. *Implications of the Reporting Function*

Claim 7 *For $\frac{b}{c} < \min(\bar{y}, \bar{x})$, at least some families report a strictly positive income $0 < y \leq \bar{y}$.*

Proof. The claim follows immediately from Claim 3 because the critical value $x^* \in [0, \frac{b}{c}]$.

Claim 8 *For $\frac{b}{c} > \int_0^{\bar{y}+\frac{b}{c}} \mathfrak{B}(M-1; N-1, F(x^*)) \, d\alpha$, $y(x)$ is discontinuous at x^*.*

Proof. Proof by contradiction: Note that if $\frac{b}{c} > \int_0^{\bar{y}+\frac{b}{c}} \mathfrak{B}(M-1; N-1, F(x^*)) \, d\alpha$ then $\hat{y}(0) < 0$. If the reporting function was continuous at x^* it would need to be the case that $\hat{y}(x^*) = 0$. From Equation (18) it is immediately clear that this also requires that the probabilities of inclusion are the same in the constrained and the unconstrained model. This is only true for $x^* = 0$, contradicting the initial assumption. $\square$

Claim 9 *If the benefit b increases, more families will report $y = 0$.*

Proof. Consider a family with income x^*. If b increases, the expected utility from reporting $y = 0$ increases more than the expected utility from reporting $\hat{y}(x^*)$. The family now strictly prefers to report $y = 0$. As only families with an income below the critical value report $y = 0$ (Claim 4), the new solution to Equation (18) must be higher than the initial x^*. $\square$

Claim 10 *If the expected cost c increases, fewer families will report $y = 0$.*

Proof. Consider a family with income x^*. If c increases, the expected utility from reporting $y = 0$ decreases more than the expected utility from reporting $\hat{y}(x^*)$. The family now strictly prefers to report $\hat{y}(x^*)$. As only families with an income above the critical value report according to $\hat{y}(x)$ (Claim 6), the new solution to Equation (18) must be lower than the initial x^*. $\square$

Claim 11 *For $\bar{x} > x^r$, more families will report $y \leq \bar{y}$ if the expected benefit b increases.*

Proof. The claim follows immediately from the fact that families report an income below the eligibility threshold whenever $x_i < x^r$. $\square$

Claim 12 *For $\bar{x} > x^r$, fewer families will report $y \leq \bar{y}$ if the expected cost c increases.*

Proof. The claim follows immediately from the fact that families report an income below the eligibility threshold whenever $x_i < x^r$. $\square$

Appendix II.

Robustness

The first main result of the paper—that Bolsa Família is more effective after a municipality has been audited at random—is robust for a large number of alternative specifications.

A. *Following Children for More Than Two Years*

The standard estimation follows each child for just two years to mitigate the problem of included families dropping out of Bolsa Família or unincluded families gaining access to the program. This might overestimate the impact of the random audits if the beneficial effect decreases once families have been included for some time.

TABLE A1

BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT

(FOLLOWING CHILDREN FOR UP TO THREE YEARS)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	0.583***	0.442***	0.784***	0.521**	0.377***	0.239*
	(0.045)	(0.084)	(0.115)	(0.174)	(0.060)	(0.101)
Past audit		-0.588*		-0.913**		-0.284
		(0.245)		(0.340)		(0.422)
BF × Past audit		0.366*		0.692*		0.355^{+}
		(0.182)		(0.332)		(0.203)
Control mean	87.920	87.920	87.539	87.539	87.631	87.631
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.858	0.858	0.861	0.861	0.863	0.863
N(municipalities)	5,401	5,401	5,071	5,071	4,860	4,860
N(priority strata)	12,752	12,752	8,803	8,803	6,055	6,055
N(children)	2,585,404	2,585,404	594,973	594,973	751,123	751,123
N	7,171,463	7,171,463	1,657,313	1,657,313	2,095,969	2,095,969
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if children are followed for up to three years. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1,^{*}p < 0.05,^{**}p < 0.01,^{***}p < 0.001$.

Table A1 displays the estimates of an intent-to-treat approach where families are followed for an additional year and (potentially non-random) dropout and inclusions are ignored. As expected, this leads to lower estimates of Bolsa Família's effectiveness. However, the gains after a municipality has been audited at random are comparable, if anything, the interaction effects are somewhat larger.

B. Non-parametric Controls for Age and Gender

Educational participation varies by age and gender. Table A2 shows that the result is robust if, in addition to the individual fixed effects, non-parametric controls for age and gender are included and Equation (4) is appended as follows: $Y_{i,f,\theta,m,t} = \beta$ Bolsa Família$_{f,t} + \gamma$ Past audit$_{m,t} + \delta$ (BF $\times$ Past audit)$_{f,m,t} + \alpha_i + \text{age}_{i,t} \times \text{sex}_i + \nu_m + \mu_{\theta,t} + \varepsilon_{i,f,\theta,m,t}$

TABLE A2

BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT
(NON-PARAMETRIC CONTROLS FOR AGE AND GENDER)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	1.090***	0.992***	1.524***	1.353***	1.120***	1.024***
	(0.052)	(0.071)	(0.119)	(0.143)	(0.068)	(0.092)
Past audit		-0.114		-0.375		0.375
		(0.265)		(0.366)		(0.541)
BF × Past audit		0.258*		0.454*		0.251+
		(0.128)		(0.229)		(0.148)
Control mean	87.283	87.283	86.442	86.442	86.809	86.809
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Age × Sex FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.928	0.928	0.934	0.934	0.934	0.934
N(municipalities)	5,401	5,401	5,068	5,068	4,858	4,858
N(priority strata)	12,559	12,559	8,641	8,641	6,008	6,008
N(children)	2,573,117	2,573,117	590,630	590,630	747,786	747,786
N	5,146,234	5,146,234	1,181,260	1,181,260	1,495,572	1,495,572
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if non-parametric controls for age and gender are included. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects, "Age × Sex" fixed effects, and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^+p < 0.1,^* p < 0.05,^{**} p < 0.01,^{***} p < 0.001$.

C. Including Teenagers

Youths above the age of 15 were not originally covered by Bolsa Família. While there have been benefits for those aged 16 and 17 during all the years of the analysis, they are subject to different conditionalities, including lower attendance requirements (75% instead of the usual 85%). Moreover, while regular employment is only legal from age 17, apprenticeship contracts are possible from age 15, after the end of compulsory education.

Table A3 shows that although the effect of Bolsa Família is somewhat smaller (Columns 1, 3, and 5), the program is still estimated to be significantly more effective after a municipality has been audited at random in the first two samples (Columns 2 and 4), but not in the third one ($p = 0.103$).

TABLE A3
BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT
(INCLUDING TEENAGERS)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	0.839***	0.750***	1.325***	1.152***	0.765***	0.682***
	(0.047)	(0.063)	(0.109)	(0.133)	(0.059)	(0.079)
Past audit		-0.095		-0.364		0.324
		(0.255)		(0.341)		(0.540)
BF × Past audit		0.231*		0.458*		0.213
		(0.113)		(0.214)		(0.131)
Control mean	87.214	87.214	84.884	84.884	86.519	86.519
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.929	0.929	0.941	0.941	0.936	0.936
N(municipalities)	5,416	5,416	5,146	5,146	4,944	4,944
N(priority strata)	15,250	15,250	9,944	9,944	7,205	7,205
N(children)	3,058,499	3,058,499	682,427	682,427	900,544	900,544
N	6,116,998	6,116,998	1,364,854	1,364,854	1,801,088	1,801,088
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if teenagers up to the age of 17 are included. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1, ^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$.

In 2016, the CGU was formally reconstituted as the Ministério da Transparência, Fiscaliza-ção e Controladoria-Geral da União and the original random audit program, the Programa de Fiscalização por Sorteios Públicos, was superseded by the Programa de Fiscalização em Entes Federativos, that includes both random and non-random audits.

Table A4 shows that the results are robust to if only the 40 rounds of the Programa de Fiscalização por Sorteios Públicos are considered, and the random third cycle of the Programa de Fiscalização em Entes Federativos is excluded. This is unsurprising given that only one round of random audits is excluded, affecting the classification of only 35 municipalities in 2017 in the most representative sample, and only 34 and 32 municipalities, respectively, in the smaller samples.

TABLE A4
BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT
(ONLY ORIGINAL RANDOM AUDIT PROGRAM)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	1.006***	0.897***	1.498***	1.304***	0.919***	0.811***
	(0.053)	(0.072)	(0.122)	(0.148)	(0.067)	(0.091)
Past audit		-0.043		-0.343		0.468
		(0.352)		(0.544)		(0.698)
BF × Past audit		0.287*		0.517*		0.279+
		(0.131)		(0.239)		(0.152)
Control mean	87.283	87.283	86.441	86.441	86.810	86.810
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.925	0.925	0.932	0.932	0.931	0.931
N(municipalities)	5,401	5,401	5,068	5,068	4,858	4,858
N(priority strata)	12,559	12,559	8,641	8,641	6,008	6,008
N(children)	2,573,117	2,573,117	590,630	590,630	747,786	747,786
N	5,146,234	5,146,234	1,181,260	1,181,260	1,495,572	1,495,572
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if only audits under the original Programa de Fiscalização por Sorteios Públicos are considered. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1,^{*}p < 0.05,^{**}p < 0.01,^{***}p < 0.001$.

E. Including Audits in the Same Year

Because the results of the random audits are often only published at the end of the calendar year or even at the beginning of the next year, the *Past audit* indicator has been defined to take value 1 if a municipality has been audited at random in a *previous* year. Moreover, if the audits increase the effectiveness of Bolsa Família because they make it harder for families to misrepresent their income, including audits that occurred later in the year of registration might underestimate their effectiveness.

If municipalities are considered as having been audited in the past even if the audit takes place in the current year, 198 municipalities are reclassified as having been audited at random earlier than in the original sample. For the smaller samples, 182 and 172 municipalities are affected, respectively. However, Table A5 shows that the results don't change. This is also true

TABLE A5
BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT
(INCLUDING AUDITS IN THE SAME YEAR)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	1.006***	0.900***	1.498***	1.318***	0.919***	0.807***
	(0.053)	(0.073)	(0.122)	(0.150)	(0.067)	(0.092)
Past audit		-0.648**		-1.088**		-0.469
		(0.240)		(0.347)		(0.298)
BF × Past audit		0.275*		0.480*		0.285+
		(0.130)		(0.237)		(0.151)
Control mean	87.277	87.277	86.426	86.426	86.795	86.795
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.925	0.925	0.932	0.932	0.931	0.931
N(municipalities)	5,401	5,401	5,068	5,068	4,858	4,858
N(priority strata)	12,559	12,559	8,641	8,641	6,008	6,008
N(children)	2,573,117	2,573,117	590,630	590,630	747,786	747,786
N	5,146,234	5,146,234	1,181,260	1,181,260	1,495,572	1,495,572
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if audits that happen in the same year are considered. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random in a previous year or is being audited at random in the same year, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1,^{*}p < 0.05,^{**}p < 0.01,^{***}p < 0.001$.

if only audits from the original Programa de Fiscalização por Sorteios Públicos are considered.

F. Updating the Cadastro Único

Once a family is included in the Cadastro Único, it is required to update its data at least every other year. Thus, in a given year the data of some families actually reflects information from previous years. The identification strategy addressed this challenge by constructing priority strata so that these families would only be matched with other families who also have potentially outdated information. Table A6 shows that the results are robust if families in these priority strata are excluded.

TABLE A6
BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT
(EXCLUDING FAMILIES WITH OLD DATA)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	0.605***	0.509***	0.908***	0.738***	0.588***	0.493***
	(0.045)	(0.062)	(0.118)	(0.135)	(0.060)	(0.077)
Past audit		-0.119		-0.229		0.401
		(0.293)		(0.378)		(0.600)
BF × Past audit		0.252*		0.456*		0.247$^+$
		(0.119)		(0.208)		(0.138)
Control mean	88.747	88.747	88.188	88.188	87.627	87.627
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.925	0.925	0.939	0.939	0.933	0.933
N(municipalities)	5,395	5,395	5,045	5,045	4,829	4,829
N(priority strata)	7,233	7,233	5,649	5,649	4,244	4,244
N(children)	2,266,681	2,266,681	496,929	496,929	688,148	688,148
N	4,533,362	4,533,362	993,858	993,858	1,376,296	1,376,296
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if families that didn't update their data in the year of the matching are excluded. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

G. Including only complete years.

The data for this paper was obtained in late 2017, so the last year of the Cadastro Único data represents the state in June 2017. As a result, families who registered towards the end of the sample had less time to realize their gains, although the year fixed effects mitigate this problem to some degree. Table A7 shows that the results are robust if data from 2017 is excluded and only data from complete years are used to estimate the treatment effects.

TABLE A7

BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT
(EXCLUDING 2017)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	1.026***	0.915***	1.556***	1.349***	0.954***	0.847***
	(0.054)	(0.076)	(0.127)	(0.160)	(0.069)	(0.095)
Past audit		-0.044		-0.347		0.468
		(0.352)		(0.546)		(0.698)
BF × Past audit		0.292*		0.551*		0.279+
		(0.140)		(0.280)		(0.160)
Control mean	87.152	87.152	86.325	86.325	86.740	86.740
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.923	0.923	0.928	0.928	0.929	0.929
N(municipalities)	5,381	5,381	5,013	5,013	4,821	4,821
N(priority strata)	11,696	11,696	8,171	8,171	5,734	5,734
N(children)	2,384,393	2,384,393	532,812	532,812	706,958	706,958
N	4,768,786	4,768,786	1,065,624	1,065,624	1,413,916	1,413,916
Years	2012-2016	2012-2016	2012-2016	2012-2016	2012-2016	2012-2016

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment excluding data from 2017 where the data is observed in June instead of December. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1,^{*}p < 0.05,^{**}p < 0.01,^{***}p < 0.001$.

H. Treatment Propensity

The left panel of Figure A1 makes clear that although the overlap assumption is satisfied, families in some priority strata have considerably higher probabilities of being included. Two additional robustness tests suggest themselves from the graph: testing whether treatment

effects are robust when inverse probability weights are applied to correct for the higher treatment propensity of some families and when the families with the lowest and highest treatment propensity are excluded.
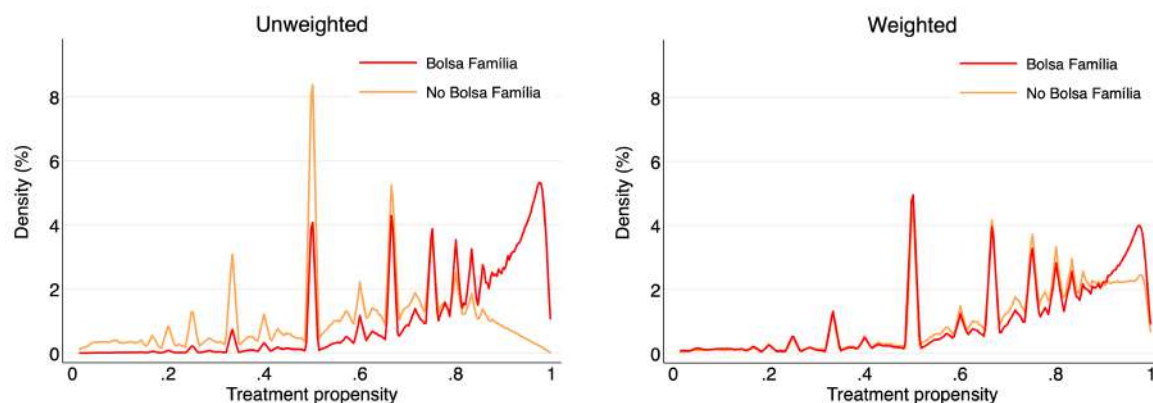
FIGURE A1

Overlap of Treatment Propensities

Notes. This figure displays the density functions for the treatment propensities of newly registered families in marginal priority strata. The left panel shows the unweighted density function for Bolsa Família beneficiaries and non-beneficiaries. The right panel shows the density functions after stabilized inverse-probability weights are applied.

The right panel of Figure A1 shows that the use of stabilized inverse probability weights does indeed improve the overlap. The weights take the form $w_{1,f} = \frac{Prob(\text{BF})}{Prob(\text{BF}|\theta,m)}$ for families that get included and $w_{0,f} = \frac{1-Prob(\text{BF})}{1-Prob(\text{BF}|\theta,m)}$ for families that don't get included, where $Prob(\text{BF} \mid \theta, m)$ denotes the conditional probability of being included in Bolsa Família for a family in priority stratum θ and municipality m. Table A8 shows that the results are robust: the beneficial effect of Bolsa Família on school enrollment persists if stabilized inverse probability weights are applied (Columns 1, 3, and 5) and, for the most part, continues to be stronger after a municipality has been audited at random (Columns 2 and 4). Only in the sample where families are also required to have registered in the same month is the interaction effect no longer significant ($p = 0.187$).

Table A9 shows that the results are relatively robust if only marginal priority strata with a treatment probability of more than 10% and less than 90% are included, although this reduces the sample size considerably. The interaction remains significant in the most representative sample ($p = 0.046$) and the sample of newly registered families ($p = 0.047$), but not in the sample of families who registered in the same month ($p = 0.143$).

BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT
(INVERSE PROBABILITY WEIGHTS)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	1.026***	0.922***	1.270***	1.083***	0.928***	0.851***
	(0.045)	(0.067)	(0.120)	(0.147)	(0.070)	(0.094)
Past audit		-0.104		-0.277		0.418
		(0.292)		(0.396)		(0.661)
BF × Past audit		0.274*		0.494*		0.200
		(0.128)		(0.241)		(0.151)
Control mean	87.283	87.283	86.442	86.442	86.809	86.809
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.931	0.931	0.937	0.937	0.935	0.935
N(municipalities)	5,401	5,401	5,068	5,068	4,858	4,858
N(priority strata)	12,559	12,559	8,641	8,641	6,008	6,008
N(children)	2,573,117	2,573,117	590,630	590,630	747,786	747,786
N	5,146,234	5,146,234	1,181,260	1,181,260	1,495,572	1,495,572
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if stabilized inverse probability weights are applied to correct for differences in the treatment propensity across priority strata. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1,$ $^{*}p < 0.05,$ $^{**}p < 0.01,$ $^{***}p < 0.001.$

TABLE A9

BOLSA FAMÍLIA IS MORE EFFECTIVE AFTER A RANDOM AUDIT

(FAMILIES WITH TREATMENT PROPENSITY 10–90%)

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1)	(2)	(3)	(4)	(5)	(6)
BF	0.975***	0.876***	1.483***	1.288***	0.866***	0.781***
	(0.051)	(0.072)	(0.112)	(0.149)	(0.068)	(0.092)
Past audit		-0.096		-0.223		0.216
		(0.291)		(0.509)		(0.434)
BF × Past audit		0.263*		0.526*		0.223
		(0.132)		(0.264)		(0.152)
Control mean	87.211	87.211	86.494	86.494	86.852	86.852
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.926	0.926	0.933	0.933	0.932	0.932
N(municipalities)	5,401	5,401	5,060	5,060	4,858	4,858
N(priority strata)	12,547	12,547	8,601	8,601	6,005	6,005
N(children)	2,127,141	2,127,141	470,867	470,867	691,381	691,381
N	4,254,282	4,254,282	941,734	941,734	1,382,762	1,382,762
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment if families in priority strata with less than 10% of more than 90% treatment propensity are excluded. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1, ^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$.

Appendix III.

Details and Results of the Field Experiment

To test if local administrators are indeed less likely to register ineligible families after a random audit, I conducted a field experiment with 6,998 Bolsa Família registration centers (CRAS).[50] Registration centers were contacted asking about the possibility of receiving Bolsa Família, and the information provided in the message was experimentally varied to make the sender eligible or ineligible while holding other characteristics constant.

A. Sample

The sample consisted of 6,998 registration centers that could be contacted individually using email.[51] There are a total of 8,176 CRAS centers in Brazil, distributed across 5,526 municipalities.[52] While the largest municipality, São Paulo, has more than 50 centers, most municipalities (82%) have just one center. The field experiment included only municipalities that were eligible for the random audits. This excluded 557 registration centers located in the 31 most populous municipalities. Of the remaining 7,619 registration centers, 74 were excluded because the official contact list does not include an email address. An additional 547 centers were excluded because they share an email address, making it impossible to contact them individually. This left a final sample of 6,998 registration centers.

B. Treatments

Over several weeks, three emails were sent to registration centers. The emails asked about registering for the Bolsa Família program and provided information that makes the sender either eligible or ineligible (see Table VI in Section V.D. for the email texts). The "Ineligible" treatment

---

50. The experiment was approved by the Human Subjects Committee of the Faculty of Economics, Business Administration, and Information Technology at the University of Zurich (*OEC IRB # 2019-010*) and was preregistered at the AEA RCT registry under the number *AEARCTR-0004151*.

51. Email addresses are from the official contact details the centers listed on the MDS website: https://aplicacoes.mds.gov.br/sagi/mops/serv-cras.php (Accessed on March 21, 2019)

52. Some municipalities do not operate their own center but rely on a neighboring municipality or mobile state-operated services.

mentioned a monthly income of R\$ 450 and for a mother with one child. The resulting per capita income (R\$ 225) exceeds the eligibility threshold of R\$ 170. The "Eligible I" treatment held the gross monthly income constant but mentioned two children, leading to a per capita income of R\$ 150, which makes the household eligible. Meanwhile, the "Eligible II" treatment held the household composition constant, but reduced the monthly income to R\$ 300, again leading to a per capita income of R\$ 150.

C. Sending the Emails

I set up a private email server to send the emails and collect responses. The use of a custom email server allowed me to ensure that none of the incoming emails are filtered or blocked and that even incorrectly addressed emails would be registered, provided that the domain name was correct. Each outgoing email was sent from a unique email address mentioning only the first name of the sender, Maria, and a random five-digit number.

Emails were sent in three waves at the beginning of May, June, and July 2019. Within waves, emails were sent at a random time on a workday between 9:00 and 17:00 in the centers time zone. The order of the emails was randomized at the municipality level and block-randomized with respect to states and whether a municipality had been audited at random. Three different subject lines were used and block-randomly assigned with respect to the treatment, the order of emails, states, and past audit status. Finally, the day of the week and the time of day were block-randomly assigned with respect to all the other design parameters.

Roughly a quarter of emails could not be delivered and returned an error message from the host. Common error messages included not being able to find the user on the host, email memory being full, and timeout errors. This failure rate is independent of the wave of the experiment and whether a municipality had previously been audited or not ($\chi^2(5) = 3.248$, $p = 0.662$; see Figure A2), as well as the treatment ($\chi^2(2) = 0.228$, $p = 0.892$).[53]

---

53. The timing of delivery error messages is also independent of both the treatment and the audit status; a Kruskal-Wallis rank test does not reject the hypothesis that the response times are drawn from the same distribution ($\chi^2(5) = 8.148$, $p = 0.148$), suggesting that these are indeed the same automatic server responses.
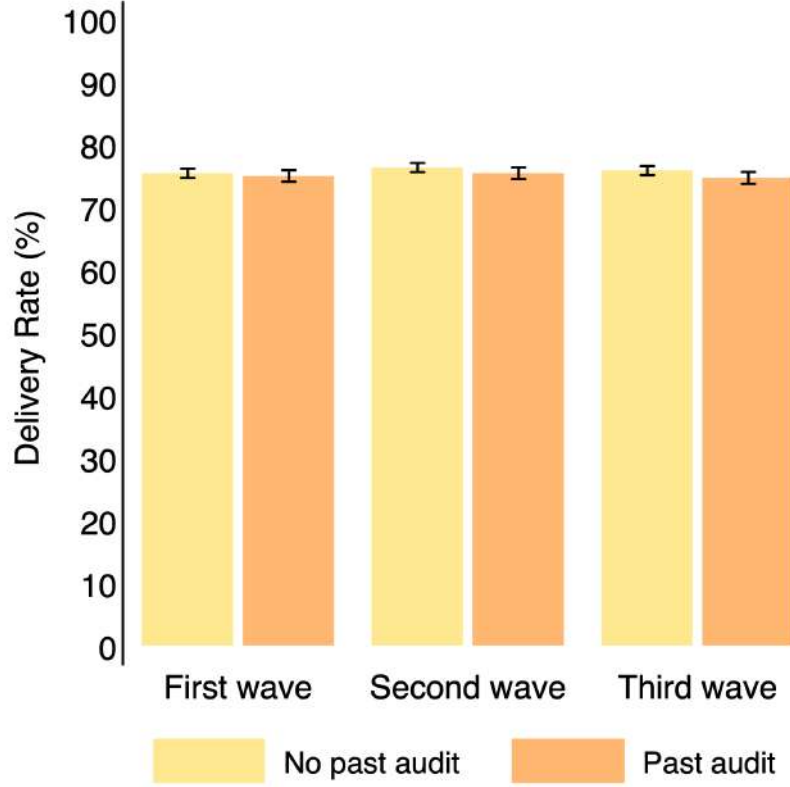
FIGURE A2

Email Delivery Rates by Wave and Previous Audit

Notes. This figure displays the delivery rate of the email requests, split by the wave of the experiment and previous audits. Error bars indicate standard errors and are clustered at the municipality level.

D.   Additional Results

Figure A3 displays the cumulative distribution function of the response times. The response times to delivered emails are independent of the treatment and whether a municipality has preciously been audited or not; a Kruskal-Wallis rank test does not reject the hypothesis that the response times are drawn from the same distribution ($\chi^2(5) = 1.504$, $p = 0.913$).[54] I pre-registered that I would use a Heckman selection model to analyze whether the treatments or the interactions of the treatments and the random audits significantly affect the response times. Unsurprisingly, given the Kruskal-Wallis test, they do not.

---

54. Delivery error messages arrive significantly faster than responses to delivered emails ($\chi^2(1) = 3675.972$, $p = 0.000$).
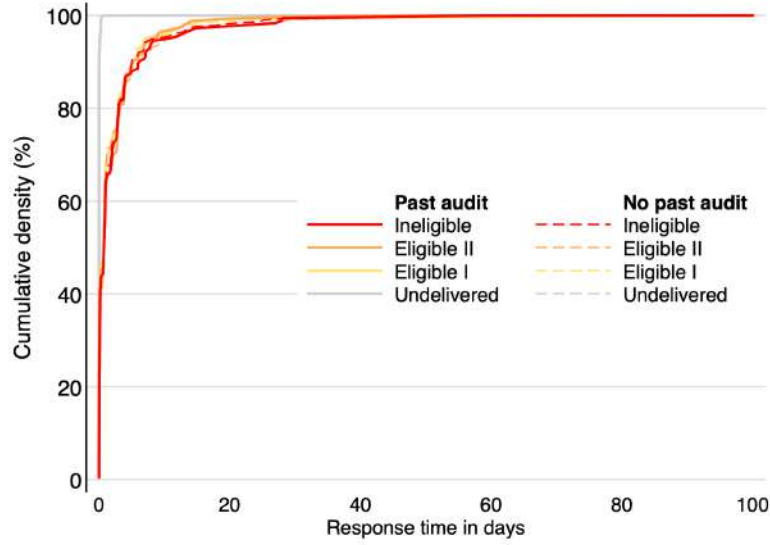
FIGURE A3

Response Times by Treatment and Previous Audit

Notes. This figure displays the cumulative distribution function for the time elapsed between the sending of the request and the responses from the registration centers, split by experimental condition and previous audits. The cumulative distribution functions for delivery errors are shown in gray.

E. Low Response Rate

The response rate was below 20% for all treatments in all waves of the experiment. There are several possible explanations for this unexpectedly low response rates with different implications: CRAS centers could not have received the emails, they could not be used to correspond via email, or they might have been suspicious.

The relatively high number of emails that returned an error message casts doubt on the reliability of the centers' IT systems and it is conceivable that there are additional emails that did not reach the centers. In the absence of an error message, it is unfortunately not possible to know whether this is the case. However, given that audited and unaudited municipalities were equally likely to return an error message and had similar response rates in the control treatments, it is unlikely that these additional undelivered emails (should they exist) bias the result about the treatment differences.

Not all CRAS centers list an email address as part of their contact details and it is possible that even some that do check their email only infrequently. Especially in rural areas, potential

Bolsa Família recipients are arguably more likely to visit the center or to call rather than sending an email.[55] This factor is very likely to have depressed the response rate. Of the email addresses listed, quite a few appear to be private email addresses of employees (some Gmail or Hotmail address) and a small number of responses mentioned that the request had been forwarded to the CRAS by an employee who no longer worked there. Apart from the shift in levels, unfamiliarity with email per se is again unlikely to bias the results, for the same reason outlined above.

The most serious concern is that the low response rate is indicative of suspicion. Several aspects of the design might have contributed to it: the emails stated the sender's approximate income, they provided relatively little other information and no full name, and the email addresses included only the first name. Mentioning the income was unavoidable to make sure that the sender is objectively eligible or ineligible[56] and providing as little demographic information as possible was an ethical necessity to avoid confusion with real families that register during the time of the study.[57] It is possible that CRAS employees were more suspicious of emails in the Ineligible treatment and even more so in audited municipalities. If this is the case, it might explain the observed differences in the response rates. Note that it is entirely consistent with the income underreporting mechanism if having previously been audited makes CRAS employees more careful not to register ineligible families because they suspect some audit or test.

F.   Robustness

The results of the field experiment are robust for different specifications of the regression, if undelivered emails are coded as non-responses, if only the first wave of emails is considered, and if the two control treatments—Eligible I and Eligible II—are treated separately.

Table A10 shows that treating delivery errors as non-responses does not affect the results.

55. The use of email was guided in equal parts by financial and practical considerations: telephone numbers in Brazil are geographically coded and might have revealed that the caller does not reside in the municipality.

56. Other approaches such as mentioning an occupation would have required CRAS employees to guess whether the household may or may not be eligible and would have depended on local factors such as the median wage.

57. The household composition—a single mother with one or two children—is very common among registrants, there is no information on the children's names, age or gender, and Maria is by far the most common female name in Brazil. Almost 15% of women in the Cadastro Único list Maria as one of their first names. Even if an administrator thought that the email matches someone he knows, it would be very easy to argue that it is just a coincident.

RESPONSE RATE TO REQUESTS FROM ELIGIBLE AND INELIGIBLE
FAMILIES
(INCLUDING UNDELIVERED EMAILS)

	(1)	(2)	(3)	(4)	(5)
Ineligible	-1.284**	-1.284**	-1.304**	-1.284**	-1.349**
	(0.450)	(0.450)	(0.452)	(0.450)	(0.450)
Past audit	0.392			0.168	0.171
	(0.785)			(0.747)	(0.745)
Ineligible × Past audit	-1.752*	-1.752*	-1.670*	-1.752*	-1.684*
	(0.709)	(0.709)	(0.709)	(0.709)	(0.714)
Population (Log.)				2.554***	2.576***
				(0.401)	(0.401)
Income inequality (Gini)				-8.170$^+$	-8.179$^+$
				(4.941)	(4.965)
Income per capita (Log.)				4.993***	4.875***
				(1.425)	(1.428)
Illiteracy				0.015	0.018
				(0.058)	(0.058)
Urban population				-0.647	-0.574
				(1.794)	(1.800)
Control mean	10.864	10.864	10.864	10.864	10.864
State FE	Yes	Yes	Yes	Yes	Yes
Center FE	No	Yes	Yes	No	No
Order FE	No	No	Yes	No	Yes
Subject line FE	No	No	Yes	No	Yes
Day FE	No	No	Yes	No	Yes
Time FE	No	No	Yes	No	Yes
R2	0.037	0.600	0.606	0.058	0.067
N	20994	20994	20994	20994	20994

Notes. This table reports the difference in response rates to requests from eligible and ineligible families in the field experiment if undelivered emails are coded as non-responses. The dependent variable is an indicator that takes value 100 if the registration center replied to a request and 0 otherwise. "Ineligible" indicates that the details in the request made a family ineligible for Bolsa Família, "Past audit" indicates that a municipality has been audited at random, and "Ineligible × Past audit" is the interaction of the two treatments. "Population (Log.)", "Income inequality (Gini)", "Income per capita (Log.)", and the rates of "Illiteracy" and "Urban population" control for municipality characteristics in 2000, before the inception of the audits program. Columns (2) and (3) include registration center fixed effects. Columns (3) and (5) include fixed effects for the order of emails, the different subject lines, the day of the week and the exact time of day the email was sent. All models include state fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^+p < 0.1,^* p < 0.05,^{**} p < 0.01,^{***} p < 0.001$.

As in Table VII in Section V.D., Column (1) shows the results if state fixed effects are included to account for the stratification of the audit lottery, Columns (2) and (3) show the results if only within CRAS center variance is exploited, and Columns (4) and (5) if the control variables from Avis et al. (2018) are used. Columns (3) and (5) again add fixed effects for the different subject lines, the order, and the timing of the emails. Although the coefficients are somewhat

Response Rate to Requests from Eligible
and Ineligible Families
(First Wave Only)

	(1)	(2)	(3)
Ineligible	-2.174+	-1.904	-1.802
	(1.245)	(1.232)	(1.247)
Past audit	1.651	1.536	1.386
	(1.402)	(1.342)	(1.371)
Ineligible × Past audit	-4.422*	-4.692*	-4.743*
	(1.902)	(1.890)	(1.947)
Population (Log.)		2.868***	2.954***
		(0.608)	(0.612)
Income inequality (Gini)		-11.944	-13.252
		(8.903)	(8.987)
Income per capita (Log.)		5.686*	5.336*
		(2.319)	(2.363)
Illiteracy		-0.001	-0.002
		(0.093)	(0.096)
Urban population		1.060	1.183
		(3.047)	(3.093)
Control mean	15.064	15.064	15.064
State FE	Yes	Yes	Yes
Subject line FE	No	No	Yes
Day FE	No	No	Yes
Time FE	No	No	Yes
R2	0.043	0.066	0.098
N	5276	5276	5276

Notes. This table reports the difference in response rates to requests from eligible and ineligible families in the first round of the field experiment. The dependent variable is an indicator that takes value 100 if the registration center replied to a request and 0 otherwise. "Ineligible" indicates that the details in the request made a family ineligible for Bolsa Família, "Past audit" indicates that a municipality has been audited at random, and "Ineligible × Past audit" is the interaction of the two treatments. "Population (Log.)", "Income inequality (Gini)", "Income per capita (Log.)", and the rates of "Illiteracy" and "Urban population" control for municipality characteristics in 2000, before the inception of the audits program. Column (3) includes fixed effects for the order of emails, the different subject lines, the day of the week and the exact time of day the email was sent. All models include state fixed effects. Robust standard errors are reported in brackets. Significance levels: $^{+}p < 0.1,^{*} p < 0.05,^{**} p < 0.01,^{***} p < 0.001$.

smaller when undelivered emails are included, they remain statistically significant, as is the case for the interaction terms ($p = 0.001$ for the coefficients and $p \leq 0.020$ for the interactions in all specifications).

As each center receives three similar emails, the response to emails in later waves might be affected by the emails in earlier waves: the email might look familiar to social workers or be more

likely to end up in a spam filter. To address these concerns, I pre-registered a robustness check to show that the effect persists if only the first wave of emails is used. Table A11 shows that the effect persists. Column (1) shows the model if only state fixed effects are included. Column (2) adds the control variables from Avis et al. (2018). Column (3) further add fixed effects for the different subject lines and the timing of the emails. In all three specifications, emails from the Ineligible treatment were approximately two percentage points less likely to receive a response in unaudited municipalities, and 6.5 percentage points less likely to receive a response in audited municipalities. The treatment effects are significantly bigger in municipalities that have been audited at random ($p < 0.020$ in all specifications).

Finally, while both control treatments imply the same per capita income and qualify the household to receive Bolsa Família, they could nevertheless be perceived differently by administrators at the CRAS. I thus test whether the results persist if the Eligible I and Eligible II treatments are included separately in the regression. Table A12 shows that this is indeed the case. While there is no significant difference between either the two control treatments or their interactions with previous audits ($p > 0.700$ for all comparisons), the coefficients for the Ineligible treatment and its interaction are quantitatively similar to the values in Table VII in Section V where they are compared to both control treatments simultaneously. Both the coefficients and the interaction terms differ significantly from those of the Eligible I treatment ($p < 0.020$ for all comparisons) and the Eligible II treatment ($p < 0.040$ for all comparisons).

Table A12
Response Rate to Requests from Eligible and Ineligible Families
(Separate Treatments)

	(1)	(2)	(3)	(4)	(5)
Eligible II	0.042	-0.060	-0.234	0.051	-0.091
	(0.645)	(0.648)	(0.651)	(0.645)	(0.649)
Ineligible	-1.687*	-1.556*	-1.617*	-1.646*	-1.732**
	(0.672)	(0.682)	(0.686)	(0.671)	(0.672)
Past audit	0.513			0.307	0.363
	(1.180)			(1.111)	(1.112)
Eligible II × Past audit	-0.118	-0.029	-0.095	-0.113	-0.064
	(1.126)	(1.127)	(1.123)	(1.124)	(1.126)
Ineligible × Past audit	-2.455*	-2.717*	-2.740*	-2.492*	-2.545*
	(1.094)	(1.105)	(1.107)	(1.093)	(1.103)
Population (Log.)				3.510***	3.509***
				(0.488)	(0.489)
Income inequality (Gini)				-11.855+	-12.169*
				(6.140)	(6.168)
Income per capita (Log.)				5.632**	5.618**
				(1.765)	(1.766)
Illiteracy				0.015	0.020
				(0.075)	(0.075)
Urban population				-0.707	-0.533
				(2.235)	(2.244)
Eligible II - Ineligible	0.010	0.027	0.043	0.012	0.016
Diff. Interactions	0.031	0.013	0.015	0.028	0.023
Control mean	14.277	14.345	14.345	14.277	14.277
State FE	Yes	Yes	Yes	Yes	Yes
Center FE	No	Yes	Yes	No	No
Order FE	No	No	Yes	No	Yes
Subject line FE	No	No	Yes	No	Yes
Day FE	No	No	Yes	No	Yes
Time FE	No	No	Yes	No	Yes
R2	0.039	0.599	0.606	0.068	0.078
N	15891	15736	15736	15891	15891

Notes. This table reports the difference in response rates to requests from eligible and ineligible families in the field experiment if the two control conditions are treated separately. The dependent variable is an indicator that takes value 100 if the registration center replied to a request and 0 otherwise. "Eligible II" indicates that the request is part of the Eligible II condition, "Ineligible" indicates that the details in the request made a family ineligible for Bolsa Família, "Past audit" indicates that a municipality has been audited at random, and "Eligible II × Past audit" and "Ineligible × Past audit" are the interactions of the two conditions with the random audits. "Population (Log.)", "Income inequality (Gini)", "Income per capita (Log.)", and the rates of "Illiteracy" and "Urban population" control for municipality characteristics in 2000, before the inception of the audits program. Columns (2) and (3) include registration center fixed effects. Columns (3) and (5) include fixed effects for the order of emails, the different subject lines, the day of the week and the exact time of day the email was sent. All models include state fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^{+}p < 0.1,^{*}p < 0.05,^{**}p < 0.01,^{***}p < 0.001$.

ADDITIONAL FIGURES
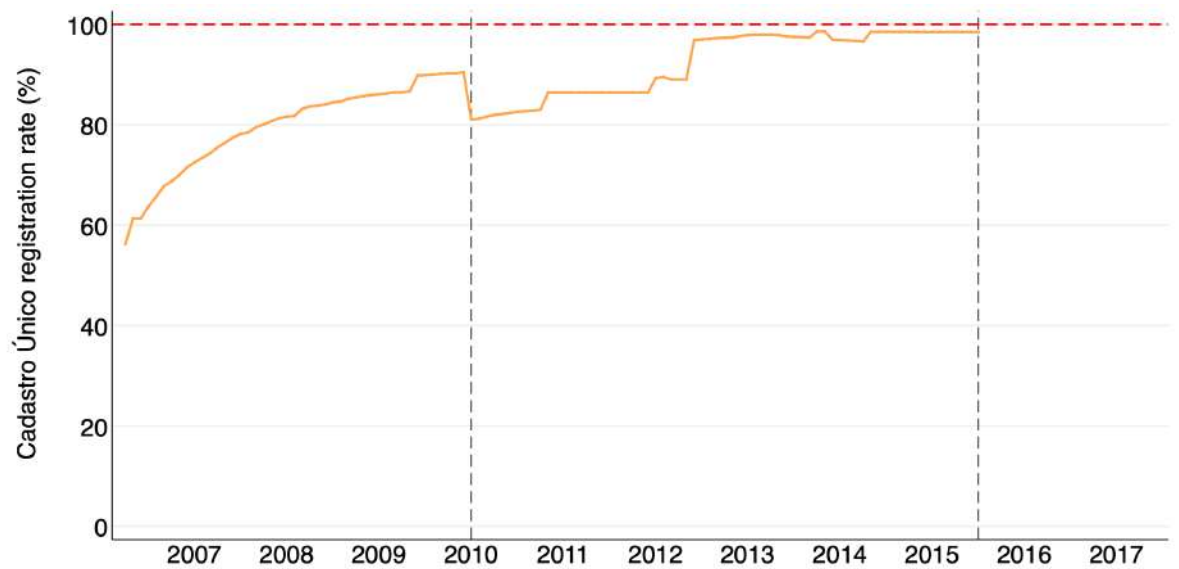
FIGURE A4

Cadastro Único Registration Rate

Notes. This figure displays the average Cadastro Único registration rate across time. The index of municipal management quality (IGD-M) defines the registration rate as the number of registered families divided by the number of eligible families in the municipality, estimated based on the last census. The indicator was dropped from the IGD-M in July 2015.
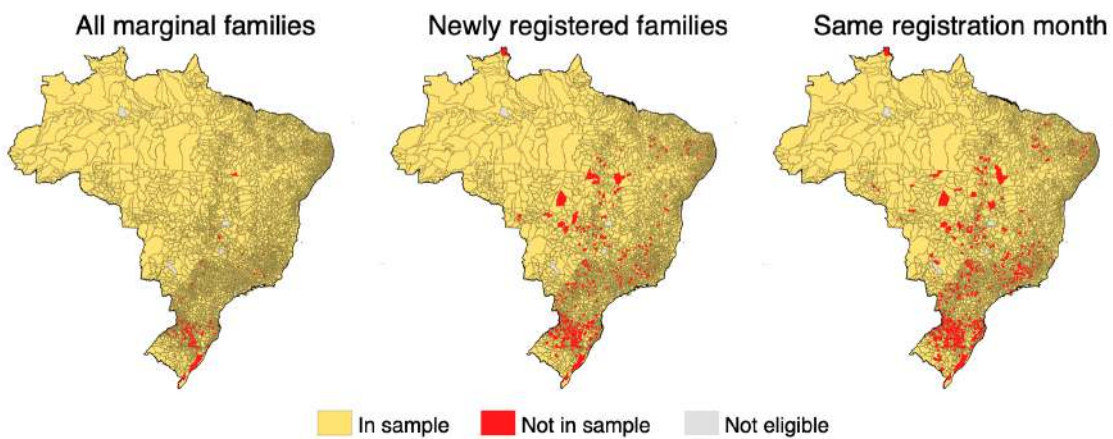
FIGURE A5

Geographic Coverage of Marginal Priority Strata

Notes. This figure displays the geographic coverage of marginal priority strata in the most representative sample (left), the sample of newly registered families (middle), and the sample of families that registered in the same month (right). Gray municipalities are not included because they are not eligible for the random audits.
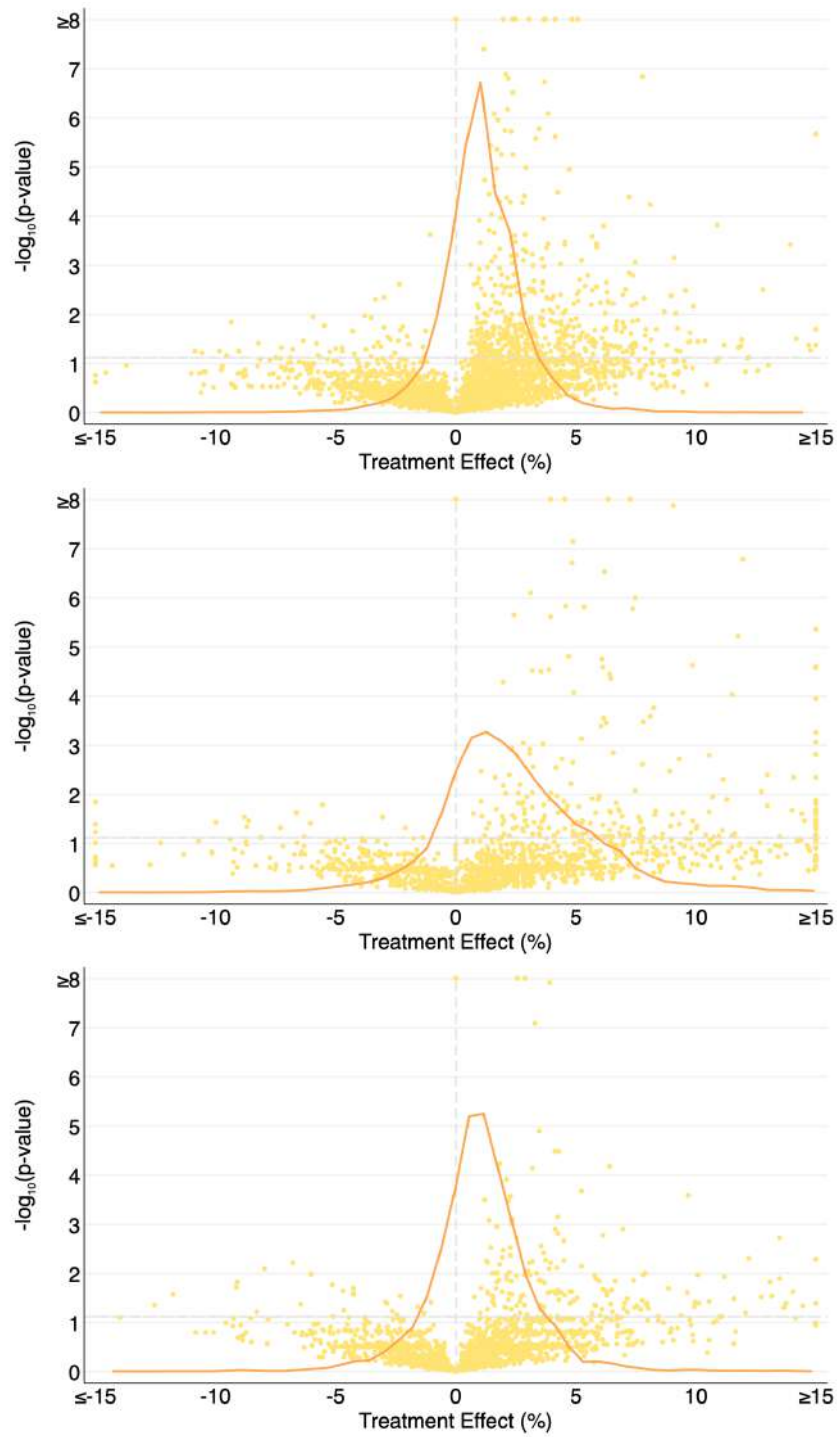
FIGURE A6

Distribution of Bolsa Família's Effectiveness in Different Municipalities

Notes. This figure displays the distribution of estimated treatment effects for different municipalities in the most representative sample (top), the sample of newly registered families (middle), and the sample of families that registered in the same month (bottom). Each dot represents the effect size and p-value for a municipality and is estimated based on at least 50 children. The density is weighted by the number of observations in each group. The dashed vertical gray line represents significance at the 5% level.

83
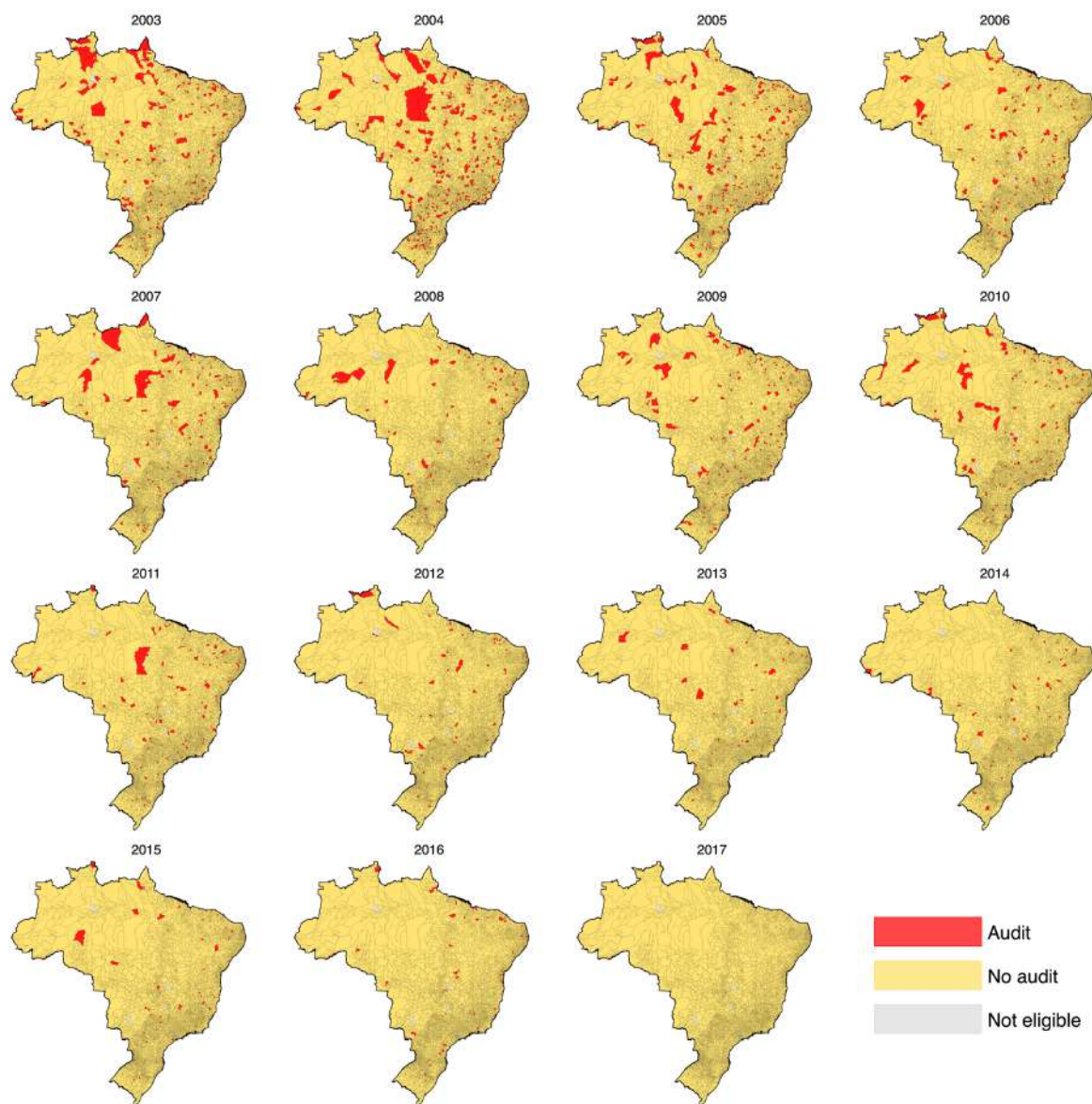
Figure A7

Distribution of Random Audits

Notes. This figure displays the geographic distribution of the random audits in each year under the Programa de Fiscalização em Entes Federativos (2003-2015) and the random third cycle of its successor, the Programa de Fiscalização em Entes Federativos (2016).
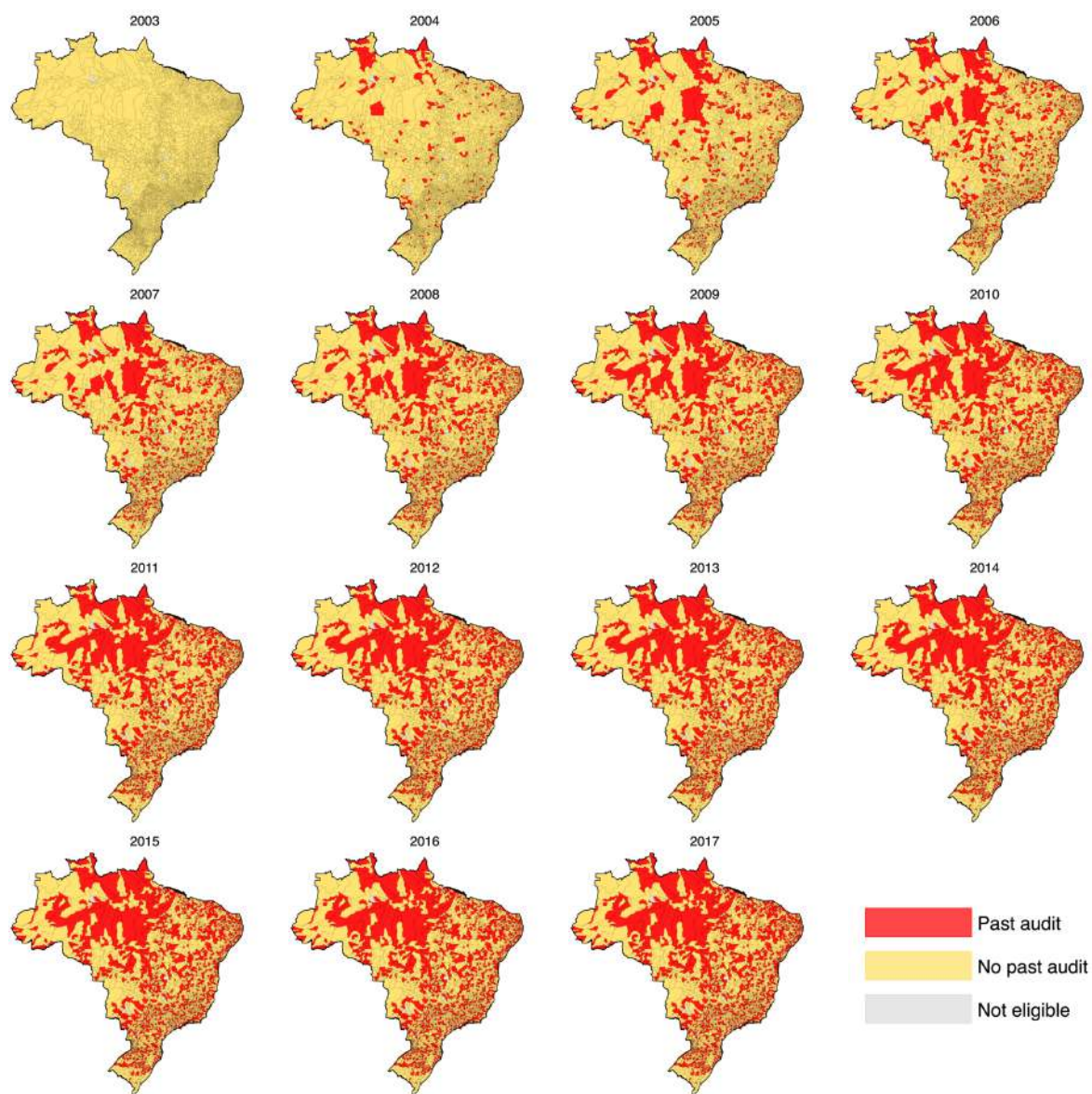
FIGURE A8

Distribution of the "Past Audit" Indicator

Notes. This figure displays the geographic distribution of the *Past audit* indicator for each year. The indicator takes value 1 if a municipality has been audited at random in a previous year and 0 otherwise.
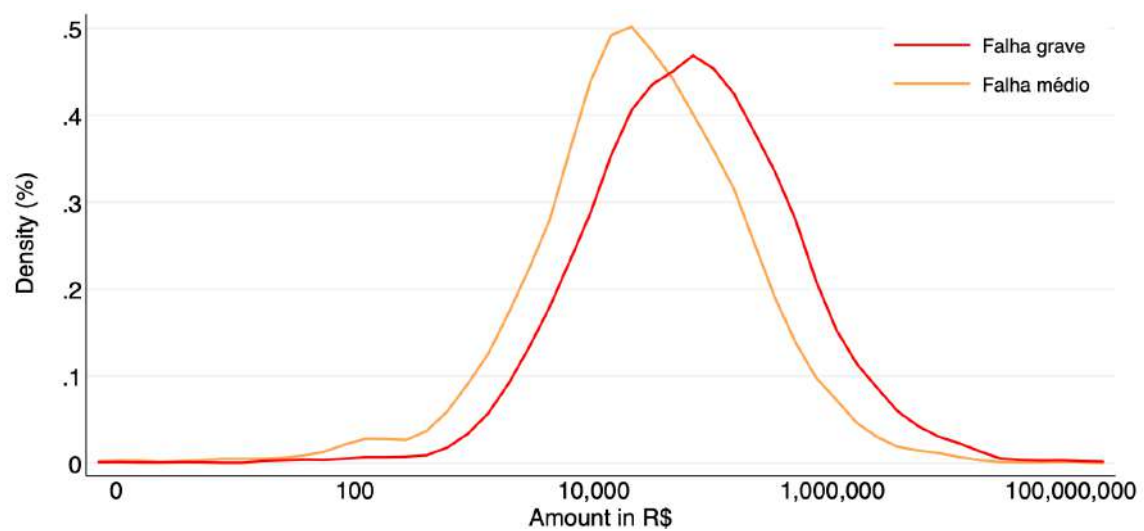
FIGURE A9

Financial Loss Uncovered by the Random Audits

Notes. This figure displays the considerable overlap between the financial losses judged as *falha média* or *falha grave*.
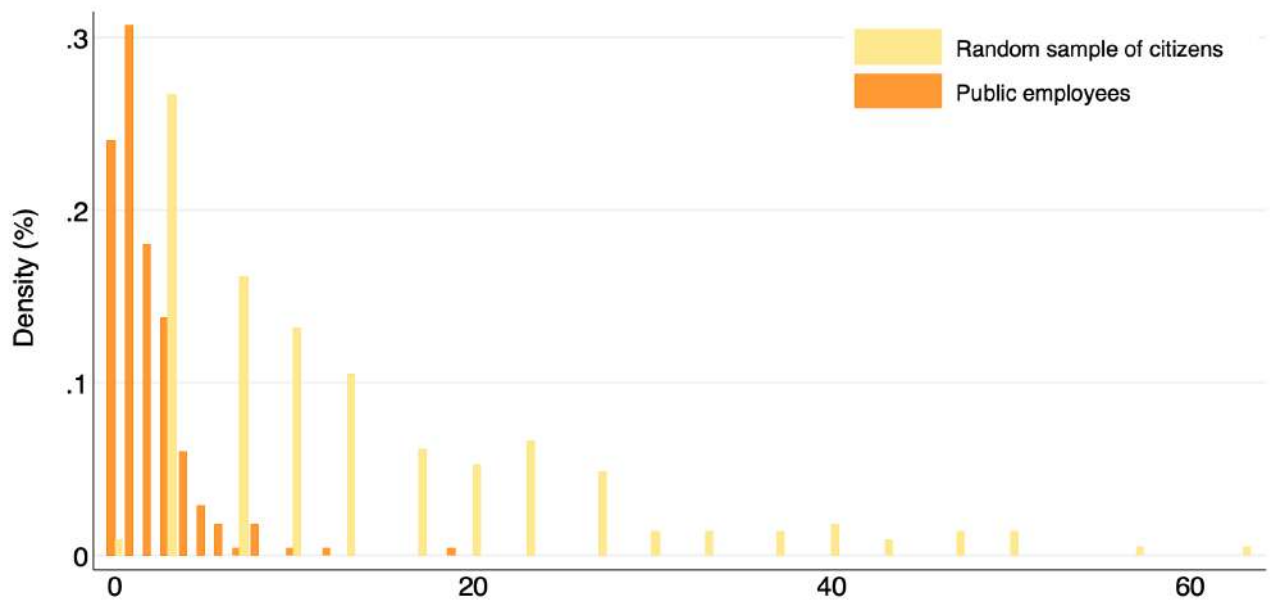
FIGURE A10

Rates of illegitimate payments for citizens and public servants

Notes. This figure displays the distribution of the rate of illegitimate payments in municipalities uncovered by the random audits. The rate for citizens is estimated based on families that were randomly sampled (N = 30). The rate for public servants is estimated based on the auditor's cross-referencing of public employment records with Bolsa Família payments.

FIGURE A12

Change in Exclusions After a Random Audit

Notes. This figure displays the percentage of families that are sanctioned and excluded from Bolsa Família in the years after a municipality has been audited at random. Numbers are corrected for municipality and year fixed effects. Error bars indicate standard errors.

FIGURE A11

School Enrollment and Family Income

Notes. This figure displays the relationship between families' per capita income and children's school enrollment. The curve shows the fractional polynomial fit and 95% confidence interval for newly registered families who don't benefit from Bolsa Família. The dots represent the mean income and school enrollment for 30 equally sized bins.

FIGURE A13

CRAS Infrastructure Doesn't Change After a Random Audit

Notes. This figure displays the change in the available infrastructure at Bolsa Família registration centers after a municipality has been audited at random. The white diamonds show the estimated treatment effect from 37 regressions where infrastructure variables in the Censo SUAS are regressed on the "Past audit" indicator, registration center and year fixed effects. Colors indicate false coverage rate adjusted 90%, 95%, 99%, and 99.9% confidence intervals (Benjamini and Yekutieli, 2005).

88
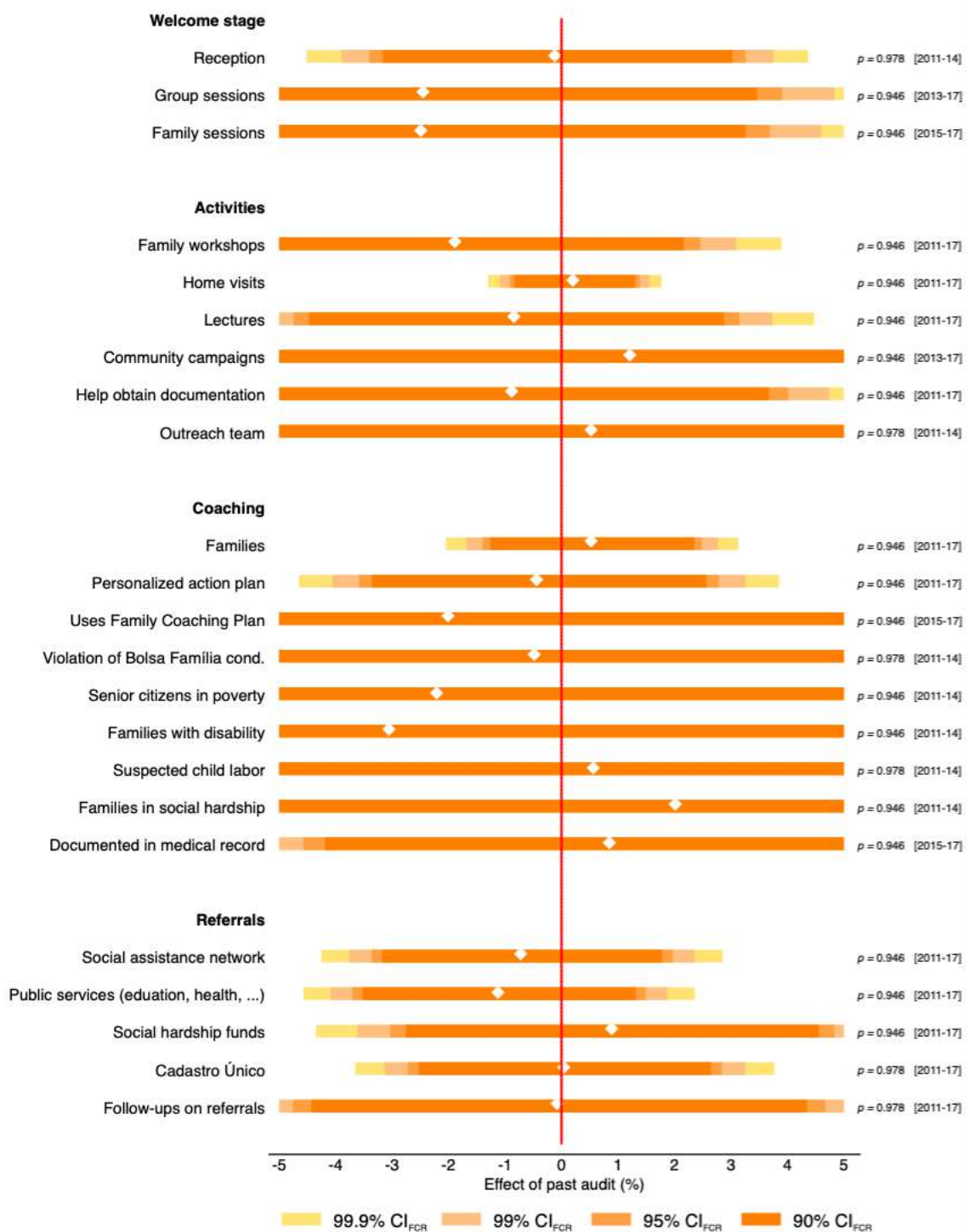
FIGURE A14

Complementary Actions Don't Change After a Random Audit

Notes. This figure displays the change in activities and programs aimed at vulnerable families through the Serviço de Proteção e Atendimento Integral à Família (PAIF) after a municipality has been audited at random. The white diamonds show the estimated treatment effect from 23 regressions where variables related to complementary programs in the Censo SUAS are regressed on the "Past audit" indicator, registration center and year fixed effects. Colors indicate false coverage rate adjusted 90%, 95%, 99%, and 99.9% confidence intervals (Benjamini and Yekutieli, 2005).

89

Bolsa Família

Label	p-value	Years
Responsible for program	p = 0.786	[2011-17]
Monitors program	p = 0.786	[2011-17]
Discussed monitoring in meeting	p = 0.786	[2014-17]
Beneficiary representative	p = 0.843	[2014-17]
Special commission	p = 0.843	[2011-17]
Receives denuncions	p = 0.843	[2011-17]
Discusses denuncions	p = 0.843	[2014-17]

Discussions

Label	p-value	Years
Plan of action	p = 0.843	[2014-17]
Financial performance statement	p = 0.786	[2014-17]
Social assistance plan	p = 0.912	[2014-17]
Budgetary law and guidelines	p = 0.843	[2014-17]
Social assistance conference	p = 0.843	[2014-17]
Planing council activities	p = 0.847	[2014-17]
Monitoring of continuous benefits	p = 0.897	[2014-17]
Occational benefits	p = 0.843	[2014-17]
Basic social protection (PSB)	p = 0.843	[2014-17]
Special social protection (PSE)	p = 0.843	[2014-17]
Monitoring of social service providers	p = 0.843	[2014-17]
Procurement of equipment	p = 0.847	[2016-17]
Procurement of services	p = 0.927	[2014-17]
Social assistance policy	p = 0.963	[2014-17]
Participation in community actions	p = 0.843	[2014-17]
Hiring and training of social workers	p = 0.897	[2014-17]
Use of IDG-M funds	p = 0.897	[2014-17]
Development of SUAS	p = 0.786	[2014-17]
Ten-year plan	p = 0.897	[2015-17]
Monitoring of projects	p = 0.897	[2015-17]
Labor force participation	p = 0.912	[2014-17]
Traditional and indigenous groups	p = 0.922	[2016-17]
Students with disabilities	p = 0.975	[2016-17]

Activities

Label	p-value	Years
Seeks feedback from citizens	p = 0.843	[2011-17]
Organize townhall meetings	p = 0.786	[2011-17]
Visit the CRAS	p = 0.927	[2011-16]
Organize communicty actions	p = 0.843	[2011-17]
Engage with legislative branch	p = 0.897	[2013-17]

Rules

Label	p-value	Years
Election of (vize-)president	p = 0.786	[2011-17]
Election of citizen representatives	p = 0.786	[2011-17]
Alternating presidency	p = 0.897	[2014-17]
Replacement of council members	p = 0.843	[2011-17]
Role of president	p = 0.786	[2011-17]
Role of vize-president	p = 0.786	[2011-17]
Role of secretary	p = 0.975	[2011-17]
Seats for beneficiary representatives	p = 0.897	[2014-17]
Seats for union representatives	p = 0.912	[2014-17]
Seats for social workers	p = 0.843	[2014-17]
Plenary meetings	p = 0.834	[2011-17]
Permanent commissions	p = 0.843	[2014-17]
Setting up commissions	p = 0.897	[2011-15]
Announcing the agenda	p = 0.843	[2014-17]
Publication of meeing notes	p = 0.843	[2011-17]
Publication of resolutions	p = 0.843	[2014-16]
Change statutes	p = 0.995	[2011-16]
Disolution on council	p = 0.786	[2011-15]

Effect of past audit (%)

99.9% CI_{FCR}   99% CI_{FCR}   95% CI_{FCR}   90% CI_{FCR}

FIGURE A15

Governance of Social Programs Doesn't Change After a Random Audit

Notes. This figure displays the change in the governance of social programs after a municipality has been audited at random. The white diamonds show the estimated treatment effect from 53 regressions where variables related to municipalities' Councils for Social Assistance (CMAS) in the Censo SUAS are regressed on a "Past audit" indicator, municipality and year fixed effects. Colors indicate false coverage rate adjusted 90%, 95%, 99%, and 99.9% confidence intervals (Benjamini and Yekutieli, 2005).

90

Appendix V.

Additional Tables

Table A13
Bolsa Família Increases School Enrollment
(No Child Fixed Effects)

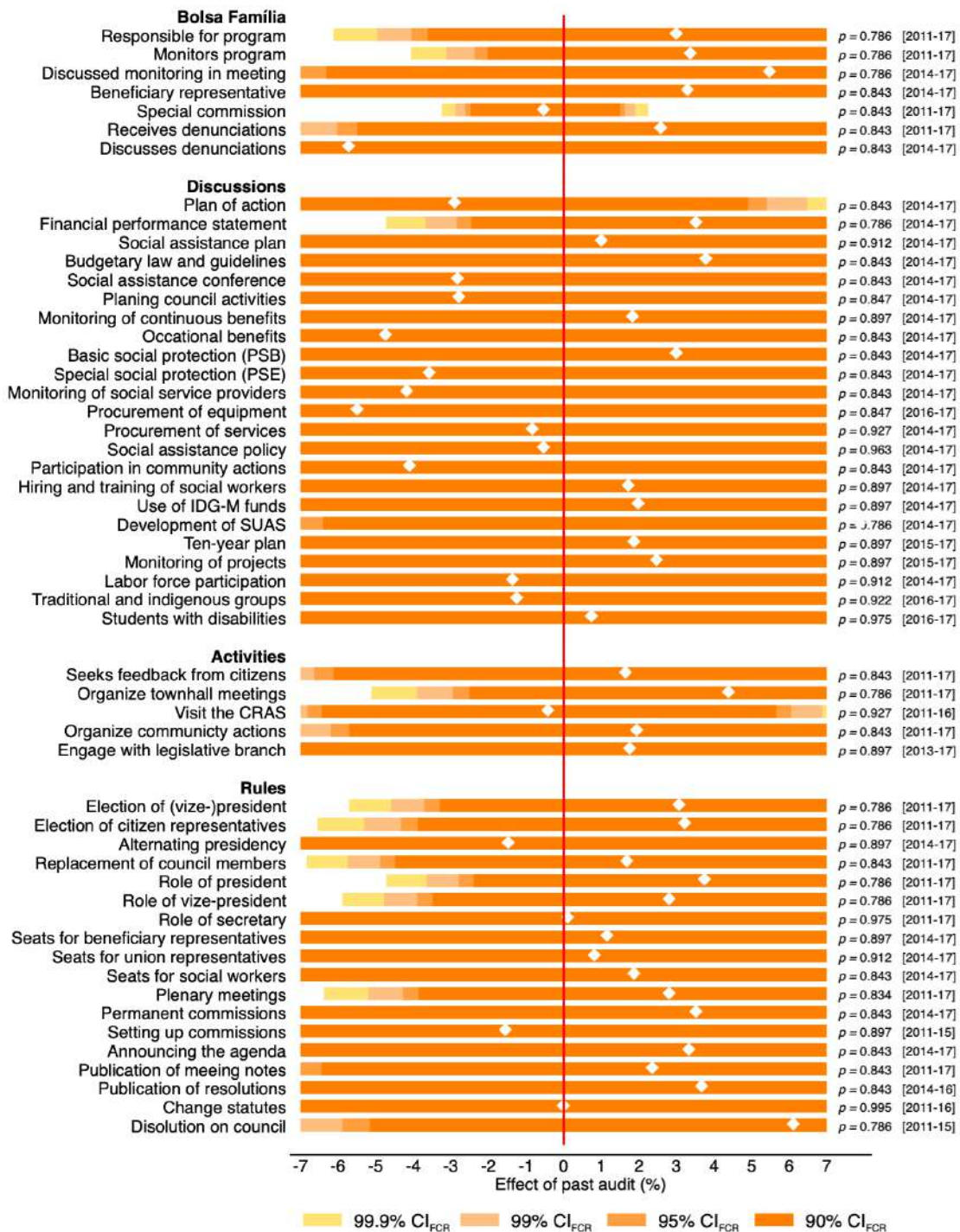|	(1) School enrollment (%) (All marginal families)	(2) School enrollment(%) (Newly registered families)	(3) School enrollment (%) (Same registration month)
BF	4.432*** (0.093)	3.104*** (0.145)	5.163*** (0.125)
Control mean	87.283	86.442	86.809
Child FE	No	No	No
Municipality FE	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes
R2	0.075	0.107	0.082
N(municipalities)	5,401	5,068	4,858
N(priority strata)	12,559	8,641	6,008
N(children)	2,573,117	590,630	747,786
N	5,146,234	1,181,260	1,495,572
Years	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment in a model without child fixed effects. The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include municipality and "Year × Priority strata" fixed effects, but no individual-level child fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^{+}p < 0.1$,$^{*}p < 0.05$,$^{**}p < 0.01$,$^{***}p < 0.001$.

Table A14
Corruption in Bolsa Família and the Educational System is Correlated with Corruption in Other Sectors

	Bolsa Família			Education		
	(1) Irregularities (Log.)	(2) Mismanagement (Log.)	(3) Corruption (Log.)	(4) Irregularities (Log.)	(5) Mismanagement (Log.)	(6) Corruption (Log.)
Other corruption (Log.)	0.149***	-0.012	0.159***	0.805***	-0.006	0.909***
	(0.031)	(0.025)	(0.032)	(0.041)	(0.037)	(0.040)
Other mismanagement (Log.)	-0.009	0.105***	-0.040*	0.081***	0.553***	-0.031
	(0.017)	(0.015)	(0.018)	(0.018)	(0.021)	(0.019)
Constant	1.712***	0.428	1.577***	-1.071***	-0.447*	-1.388***
	(0.165)	(0.308)	(0.172)	(0.241)	(0.226)	(0.237)
Inspection orders	Nonpar.	Nonpar.	Nonpar.	Nonpar.	Nonpar.	Nonpar.
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Lottery FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.314	0.166	0.306	0.683	0.591	0.698
N	1134	1134	1134	1102	1102	1102

Notes. This table reports the relationship between corruption in the Bolsa Família program and the educational system with corruption in other parts of municipal government. The dependent variable in Columns (1) and (4) is the logarithm of the total number of irregularities uncovered by the random audit program. Columns (2) and (5) include only instances of mismanagement (*falha formal*), and Columns (4) and (6) only instances of corruption (*falha média* or *falha grave*). All models include the regressors of interest—the logarithms of the number of instances of corruption and mismanagement in other sectors—, fixed effects for the number of inspection orders, the state, and the round of the audit lottery. Robust standard errors are reported in brackets. Significance levels: $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

TABLE A15

BOLSA FAMÍLIA IS EQUALLY EFFECTIVE AFTER A RANDOM AUDIT FOR FAMILIES
REGISTERED AT HOME

	School enrollment (%) (All marginal families)		School enrollment(%) (Newly registered families)		School enrollment (%) (Same registration month)	
	(1) Home	(2) CRAS	(3) Home	(4) CRAS	(5) Home	(6) CRAS
BF	0.679***	0.900***	0.065	1.389***	0.834***	0.816***
	(0.155)	(0.072)	(0.342)	(0.154)	(0.192)	(0.092)
Past audit	2.482$^+$	-0.315$^+$	3.555$^+$	-0.482$^+$	5.348***	-0.237
	(1.488)	(0.178)	(1.937)	(0.292)	(1.118)	(0.322)
BF × Past audit	-0.078	0.302*	0.278	0.518*	-0.118	0.283$^+$
	(0.275)	(0.135)	(0.421)	(0.248)	(0.367)	(0.153)
Control mean	88.213	87.163	86.727	86.388	86.877	86.748
Child FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × strata FE	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.936	0.927	0.952	0.933	0.946	0.933
N(municipalities)	3,601	5,382	2,194	4,979	2,284	4,802
N(priority strata)	5,157	12,350	3,066	8,376	1,967	5,881
N(children)	174,384	2,350,705	48,316	531,521	49,350	684,731
N	348,768	4,701,410	96,632	1,063,042	98,700	1,369,462
Years	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017	2012-2017

Notes. This table reports the effect of inclusion into the Bolsa Família program and random audits on children's school enrollment separately for families that registered during a home visit (Columns 1, 3, and 5) and those that registered at the CRAS (Columns 2, 4, and 6). The dependent variable in all models takes on value 100 if a child is enrolled in school and 0 otherwise. "BF" indicates if a child's family is included in the Bolsa Família program, "Past audit" indicates that a municipality has been audited at random, and "BF × Past audit" is the interaction of the two treatments. Columns (1) and (2) present the results for the most representative sample. Columns (3) and (4) consider only newly registered families. Columns (5) and (6) require families to have last updated their data in the same month. All models include individual-level child fixed effects, municipality fixed effects and "Year × Priority strata" fixed effects. Standard errors are clustered at both the family and the municipality level. Significance levels: $^+p < 0.1,^* p < 0.05,^{**} p < 0.01,^{***} p < 0.001$.

TABLE A16

CHANGE IN SANCTIONS AFTER A RANDOM AUDIT

	Log. excluded families		Log. withheld benefits	
	(1)	(2)	(3)	(4)
Past audit	-0.017		0.021	
	(0.042)		(0.021)	
Immediately after		0.080		0.049*
		(0.049)		(0.023)
Long run		-0.089*		0.005
		(0.045)		(0.023)
Control mean	3.311	3.311	5.085	5.085
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R2	0.855	0.855	0.933	0.933
N(municipalities)	5539	5539	5539	5539
N	33226	33226	38760	38760
Years	2012-2017	2012-2017	2011-2017	2011-2017

Notes. This table reports the effect of a random audit on the number of beneficiary families that are sanctioned. The dependent variable in Columns (1) and (2) is the logarithm of the number of families that are excluded from Bolsa Família. The dependent variable in Columns (3) and (4) is the logarithm of the number of families whose benefits are withheld for at least a month. "Past audit" indicates that a municipality has been audited at random. "Immediately after" takes value 1 if the municipality has been randomly audited in the previous year. "Long run" takes value 1 if the municipality has been randomly audited in an earlier year. All models include municipality and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^{+}p < 0.1,^{*} p < 0.05,^{**} p < 0.01,^{***} p < 0.001$.

Table A17

The Bolsa Família Management Index Doesn't Change...

	(1) IGD-M	(2) TAFE (Attendance monitoring)	(3) TAAS (Medical check-ups)	(4) TAC (Data updating)	(5) TCQC (CadU coverage)
Past audit	-0.002 (0.008)	-0.004$^+$ (0.003)	-0.004 (0.006)	0.002 (0.003)	-0.001 (0.005)
Control mean	.782	.877	.716	.759	.866
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes
R2	0.275	0.315	0.501	0.564	0.654
N	542630	841405	841456	841466	619906
Years	2010-2018	2006-2018	2006-2018	2006-2018	2006-2015

Notes. This table reports the effect of a random audit on the IGD-M, the index of municipal management quality, (Column 1) and its subindices for school monitoring (Column 2), health monitoring (Column 3), the data actualization (Column 4), and Cadastro Único coverage (Column 5). "Past audit" indicates that a municipality has been audited at random. All models include municipality and Year × Month fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^+ p < 0.1, ^* p < 0.05, ^{**} p < 0.01, ^{***} p < 0.001$.

Table A18

... But the Components Do.

	TAFE		TAAS		TAC		TCQC	
	(1) Children monitored (Log.)	(2) Children of beneficiaries (Log.)	(3) Families monitored (Log.)	(4) Families with check-ups (Log.)	(5) Updated entries (Log.)	(6) Registred families (Log.)	(7) Registred families (Log.)	(8) Eligible families (Log.)
Past audit	-0.023$^+$ (0.014)	-0.025* (0.012)	-0.033$^+$ (0.019)	-0.023$^+$ (0.014)	-0.011 (0.013)	-0.012 (0.011)	-0.022** (0.008)	0.003 (0.007)
Control mean	6.80	6.91	6.34	6.57	7.03	7.34	7.37	7.22
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.970	0.992	0.955	0.987	0.987	0.991	0.998	0.995
N	437471	437471	437471	437471	437471	437471	215911	215911
Years	2012-2018	2012-2018	2012-2018	2012-2018	2012-2018	2012-2018	2012-2015	2012-2015

Notes. This table reports the effect of a random audit on the individual components of the IGD-M subindices: the logarithms of the number of children whose school attendance is monitored (Column 1) and the number of beneficiary children (Column 2), the number of families in the health monitoring system (Column 3) and the number of families subject to health conditionalities (Column 4), the number of entries in the Cadastro Único that are up to date (Column 5) and the number of families that are registered (Column 6), and the number of registered families (Column 7) and the estimated number of eligible families (Column 8). "Past audit" indicates that a municipality has been audited at random. All models include municipality and Year × Month fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^+ p < 0.1, ^* p < 0.05, ^{**} p < 0.01, ^{***} p < 0.001$.

TABLE A19

CRAS Centers Don't Update The Cadastro Único Differently After A Random Audit

| | Center | | | | | | Employees |
	(1) Updates CadU (%)	(2) Special team (%)	(3) Only paper (%)	(4) Mostly paper (%)	(5) Mostly digital (%)	(6) Only digital (%)	(7) Updates CadU (%)
Past audit	0.125 (2.277)	-2.065 (2.426)	-23.543 (16.507)	9.334 (10.738)	7.313 (10.983)	6.896 (12.531)	1.333 (1.544)
Control mean	63.596	41.373	29.351	1.812	11.464	57.374	11.757
Center FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.573	0.525	0.697	0.511	0.566	0.675	0.157
N(centers)	7768	7761	4332	4332	4332	4332	7817
N	51957	44597	8664	8664	8664	8664	324993
Years	2011-17	2011-17	2016-17	2016-17	2016-17	2016-17	2014-17

Notes. This table reports the effect of a random audit on the way that Bolsa Família registration centers (CRAS) update the Cadastro Único. The dependent variable in Column (1) is an indicator of whether the center updates the Cadastro Único. Column (2) reports whether the center has a special team to do so. Columns (3) to (6) indicate whether the center initially collects information on paper that is later digitized or whether information is collected digitally. The dependent variable in Column (7) is an indicator of whether a given employee updates the Cadastro Único. The dependent variables in all models are scaled to take value 100 if the condition is met and 0 otherwise. "Past audit" indicates that a municipality has been audited at random. All models include registration center and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^{+}p < 0.1, ^{*}p < 0.05, ^{**}p < 0.01, ^{***}p < 0.001$.

Table A20

CRAS Centers Don't Employ Different People After a Random Audit

	Demographics		Education					Experience		Position		
	(1) Male (%)	(2) Age (Years)	(3) Primary or less (%)	(4) High school (%)	(5) Some college (%)	(6) College (%)	(7) Postgrad (%)	(8) Tenure (Years)	(9) Hours (Weekly)	(10) CLT (%)	(11) Temporary Art. 37 (%)	(12) Commission Art. 37 (%)
Past audit	0.058	-0.063	-0.342	0.196	0.840	-1.179	0.485	0.147	-0.138	0.072	0.090	-0.850
	(0.688)	(0.213)	(0.564)	(0.994)	(0.760)	(1.102)	(0.868)	(0.159)	(0.364)	(0.908)	(2.165)	(0.762)
Control mean	16.736	36.812	7.831	35.007	9.779	41.283	6.100	2.864	34.651	7.027	33.892	9.917
Center FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.069	0.121	0.073	0.092	0.066	0.080	0.109	0.204	0.217	0.423	0.381	0.200
N(centers)	7868	7790	7868	7868	7868	7868	7868	7793	7868	7868	7868	7868
N	465883	326605	465883	465883	465883	465883	465883	251436	465883	465883	465883	465883
Years	2012-17	2012-16	2012-17	2012-17	2012-17	2012-17	2012-17	2015-17	2012-17	2012-17	2012-17	2012-17

Notes. This table reports the effect of a random audit on the workforce at Bolsa Família registration centers (CRAS). The dependent variable in Column (1) takes value 100 if an employee is male. The dependent variable in Column (2) is the employee's age in years. Columns (3) to (7) are binary indicators for an employee's educational level. The dependent variable in Column (8) is an employee's tenure in years and Column (9) the weekly working hours, based on a categorical variable. The dependent variables in Columns (10), (11), and (12) indicate whether an employee is hired under the relatively strict Consolidação das Leis do Trabalho, or temporarily appointed or commissioned under the discretionary Art. 37. All binary indicators are scaled to take value 100 if the condition is met and 0 otherwise. "Past audit" indicates that a municipality has been audited at random. All models include registration center and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^{+}p < 0.1,$ $^{*}p < 0.05,$ $^{**}p < 0.01,$ $^{***}p < 0.001$.

TABLE A21

BENEFICIARY INVOLVEMENT AT CRAS DOESN'T CHANGE MUCH AFTER A RANDOM AUDIT

	Access	Beneficiary involvement					Beneficiary representation			
	(1) Open (Hours)	(2) None (%)	(3) Informal, occat. (%)	(4) Informal, regular (%)	(5) Formal (%)	(6) Invites public (%)	(7) Financial support (%)	(8) Citizen rep. (%)	(9) Rep. is elected (%)	(10) Committees (%)
Past audit	-0.142	7.590	-4.454	-2.454	-0.683	0.216	-1.200	0.288	-2.506^{+}	-5.435^{+}
	(0.117)	(4.689)	(5.793)	(3.833)	(1.850)	(3.228)	(0.795)	(2.522)	(1.411)	(3.255)
Control mean	45.526	31.552	45.572	16.316	6.560	23.719	1.319	9.863	2.311	5.977
Center FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.464	0.504	0.424	0.443	0.450	0.466	0.351	0.427	0.378	0.390
N(centers)	7768	7708	7708	7708	7708	7708	7708	7708	7708	7708
N	51956	30431	30431	30431	30431	30431	30431	30431	30431	30431
Years	2011-17	2014-17	2014-17	2014-17	2014-17	2014-17	2014-17	2014-17	2014-17	2014-17

Notes. This table reports the effect of a random audit on efforts for beneficiary participation at Bolsa Família registration centers (CRAS). The dependent variable in Column (1) is the number of hours the CRAS is open each week. Columns (2) to (4) indicate how often and in what form the CRAS involves beneficiaries in planning activities: never, occasionally and informally, informally but regularly, or through a formal and established process. The dependent variables in Columns (6) to (10) indicate what forms of beneficiary participation exist at the registration center: whether the center invites beneficiaries to planning meetings (Column 6) and whether it offers financial support for such meetings (Column 7), whether there is a beneficiary representative (Column 8) and whether she is elected (9), and whether the CRAS encourages the formation of beneficiary committees or collectives (Column 10). All binary indicators are scaled to take value 100 if the condition is met and 0 otherwise. "Past audit" indicates that a municipality has been audited at random. All models include registration center and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^{+}p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001$.

TABLE A22
MUNICIPALITIES DON'T SPEND MORE ON SOCIAL ASSISTANCE

	Total social assistance			Administration			Children			Senior citizens			Disability		
	(1) Per capita	(2) Absolute	(3) % total	(4) Per capita	(5) Absolute	(6) % total	(7) Per capita	(8) Absolute	(9) % total	(10) Per capita	(11) Absolute	(12) % total	(13) Per capita	(14) Absolute	(15) % total
Past audit	-0.041$^+$ (0.022)	-0.038$^+$ (0.022)	-0.002 (0.002)	-0.029 (0.192)	-0.022 (0.212)	-0.006 (0.007)	-0.072 (0.057)	-0.086 (0.088)	-0.000 (0.001)	0.021 (0.107)	-0.074 (0.200)	-0.000 (0.000)	-0.073 (0.120)	-0.203 (0.282)	-0.001 (0.000)
Control mean	4.547	13.917	0.046	3.359	12.891	0.018	2.559	11.783	0.008	1.382	10.119	0.002	1.163	10.077	0.002
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.887	0.942	0.756	0.944	0.950	0.906	0.779	0.754	0.269	0.776	0.772	0.976	0.876	0.859	0.850
N	26545	26545	26530	3946	3946	3946	23286	23286	23260	12673	12673	12661	5550	5550	5546
Years	2013-17	2013-17	2013-17	2016-17	2016-17	2016-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17

Notes. This table reports the effect of a random audit on municipalities' social assistance expenditure. Columns (1) to (3) present the effect on total social assistance expenditure—in per capita terms, on the absolute amount, and as a percentage of total municipal expenditure. Columns (4) to (6) present the same metrics for expenditure on the administration of social programs, Columns (7) to (9) for expenditure on social assistance programs focused on children, Columns (10) to (12) for expenditure on social assistance for the elderly, and Columns (13) to (15) for expenditure on disability-related social assistance. "Past audit" indicates that a municipality has been audited at random. All models include municipality and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

TABLE A23
MUNICIPALITIES DON'T SPEND MORE ON EDUCATION

	Total educ. expenditure			Elementary schools			Middle school			Vocational schools			Higher education		
	(1) Per capita	(2) Absolute	(3) % total	(4) Per capita	(5) Absolute	(6) % total	(7) Per capita	(8) Absolute	(9) % total	(10) Per capita	(11) Absolute	(12) % total	(13) Per capita	(14) Absolute	(15) % total
Past audit	-0.007 (0.013)	-0.003 (0.012)	-0.004 (0.007)	-0.012 (0.018)	-0.008 (0.018)	0.027 (0.031)	0.120 (0.124)	0.119 (0.232)	0.004 (0.004)	0.048 (0.161)	0.000 (0.282)	0.000 (0.001)	-0.085 (0.091)	-0.184 (0.154)	0.000 (0.001)
Control mean	6.652	16.035	0.360	6.356	15.735	0.280	2.153	11.135	0.008	1.460	10.581	0.004	2.361	11.375	0.008
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.893	0.987	0.782	0.813	0.950	0.256	0.867	0.859	0.810	0.876	0.884	0.854	0.886	0.863	0.840
N	26584	26584	26569	26023	26023	25991	6587	6587	6581	2433	2433	2429	10270	10270	10262
Years	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17	2013-17

Notes. This table reports the effect of a random audit on municipalities' educational expenditure. Columns (1) to (3) present the effect on total educational expenditure—in per capita terms, on the absolute amount, and as a percentage of total municipal expenditure. Columns (4) to (6) present the same metrics for expenditure on elementary schools, Columns (7) to (9) for expenditure for middle schools, Columns (10) to (12) for expenditure on vocational training schools, and Columns (13) to (15) for expenditure for higher education. "Past audit" indicates that a municipality has been audited at random. All models include municipality and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^+ p < 0.1$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

TABLE A24

WHISTLEBLOWING DOESN'T CHANGE AFTER A RANDOM AUDIT

	All sources	Beneficiaries			Non-beneficiaries			Administrators		
	(1) Complaints about CRAS (Log.)	(2) Total denunciations (Log.)	(3) Illegitimate benefit (Log.)	(4) Retained benefit card (Log.)	(5) Total denunciations (Log.)	(6) Illegitimate benefit (Log.)	(7) Retained benefit card (Log.)	(8) Total denunciations (Log.)	(9) Illegitimate benefit (Log.)	(10) Retained benefit card (Log.)
Past audit	-0.005	-0.004	-0.005	0.001	0.016	0.019	0.000	-0.000	0.001	-0.000
	(0.003)	(0.006)	(0.006)	(0.001)	(0.016)	(0.016)	(0.002)	(0.002)	(0.002)	(0.000)
Control mean	0.009	0.015	0.014	0.000	0.157	0.153	0.001	0.004	0.003	0.000
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.173	0.206	0.209	0.125	0.449	0.449	0.126	0.141	0.139	0.125
N	44328	44328	44328	44328	44328	44328	44328	44328	44328	44328
Years	2010-17	2010-17	2010-17	2010-17	2010-17	2010-17	2010-17	2010-17	2010-17	2010-17

Notes. This table reports the effect of a random audit on the number of denunciations and complaints the MDS receives from the municipality. The dependent variable in Column (1) is the logarithm of the number of complaints about the CRAS or its workforce. Columns (2) to (4) present the effect on denunciations received from Bolsa Família beneficiaries—on the logarithm of the total number of denunciations, the logarithm of the number of denunciations about illegitimate receipts of Bolsa Família payments, and the logarithm of denunciations concerning retained benefit cards. Columns (5) to (7) present the same metrics for denunciations received from non-beneficiaries and Columns (8) to (10) for denunciations received from administrative staff. "Past audit" indicates that a municipality has been audited at random. All models include municipality and year fixed effects. Standard errors are clustered at the municipality level. Significance levels: $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.