The Political Economy of Enforcing Conditional Welfare Programs: Evidence from Brazil*

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Abstract

We analyze whether politicians manipulate the enforcement of the rules of social welfare programs to influence electoral outcomes, by studying Bolsa Familia (BFP) in Brazil. BFP provides a monthly stipend to poor families conditional on school attendance. Failure to comply with this requirement results in delayed benefit disbursements and even exclusion from the program. First, we exploit random variation in the timing when different beneficiaries learn about penalties for noncompliance and find that in the 2008 municipal elections the vote share of candidates aligned with the President's party is lower in zip codes where more beneficiaries received penalties before the elections. Second, using a regression discontinuity design, we find that enforcement of BFP requirements is weaker around the time of elections in municipalities where mayors can run for reelection, consistent with the argument that local electoral incentives may lead mayors to manipulate enforcement. Finally, we provide evidence on a possible mechanism for this manipulation, finding that schools with politically-connected directors tend to excuse insufficient attendance relatively more before the elections, so that beneficiaries face no penalty.

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1 Introduction

In recent years there has been an increasing use of conditionality in welfare programs, spanning different areas (e.g., unemployment and social assistance benefits, maternity grants, child support) as well as different regions (from the US to the UK and a large number of developing countries). Notably, conditional cash transfer (CCT) programs have become a widely used tool to fight poverty in developing countries.[1] Although specific program characteristics vary from country to country, CCT programs typically provide a small stipend to poor families, as long as they meet certain conditions, such as regular school attendance of children, scheduling prenatal checkups and basic preventive health care. These "conditionalities"are a salient characteristic of CCT programs, and differentiate them from unconditional cash transfer programs (UCT).[2]

This paper analyzes whether politicians manipulate targeted government programs to influence electoral outcomes, by studying the enforcement of conditionalities in the Bolsa Familia Program (BFP), a large-scale conditional cash transfer program in Brazil. If targeted government programs affect voter choices, then politicians may have incentives to strategically manipulate these programs. To test this, we (i) analyze whether voters respond to the enforcement of program rules and (ii) study whether local authorities manipulate this enforcement, particularly when they face stronger electoral incentives. Moreover, we provide suggestive evidence on a possible channel through which local authorities may manipulate program enforcement.

In theory, targeted government programs may influence voters' decisions and therefore politicians may have an incentive to manipulate these programs to sway voters.[3] Existing

[1]The first generation of CCT programs in Colombia, Mexico, and Nicaragua has been successful in addressing many of the failures in delivering social assistance, such as weak poverty targeting, disincentive effects, and limited welfare impacts. See Rawlings and Rubio (2005) for a review of the impact of CCT programs. For many countries in Latin America there is also evidence that CCT programs have been successful in increasing school enrollment rates, improving preventive health care, and raising household consumption. (Schultz 2004; de Janvry et al. 2006; Filmer and Schady 2011)

[2]Many studies provide evidence on the effectiveness of UCT programs in developing countries (Duflo 2003; Case, Hosegood, and Lund 2005; Edmonds 2006; Edmonds and Schady 2009). See Baird, McIntosh and Ozler (2011) for experimental evidence comparing CCT and UCT programs.

[3]Politicians might manipulate the allocation of targeted benefits to increase their electoral returns. See Brender and Drazen(2000), Cox and McCubbins (1998), Kenneth and Schultz (1995), and Lindbeck and Weibull (1987) for theoretical discussions of whether politicians have incentives to target benefits to certain group of voters.

research has focused on the electoral effects of the allocation of program benefits, finding evidence that targeted transfers affect individual voting behavior (see, for example, Chen, 2008a, 2008b; Elinder et al., 2008; Levitt and Snyder, 1997; and Markus, 1988). Regarding CCT programs, the empirical evidence suggests that support for incumbents increases among program beneficiaries, in particular through a higher voter turnout (see Green 2006a; De La O, 2013; Manacorda, Miguel and Vigorito, 2010; and Zucco, 2013). In contrast with this evidence on the effects of benefit allocation, there is no research on the effects of the enforcement of CCT conditionalities on electoral outcomes and no evidence on whether politicians manipulate the degree of enforcement. This is a significant limitation, since conditionality is a salient characteristic of CCT programs. The enforcement of program requirements could affect voter behavior (e.g., voters that lose their benefits because of noncompliance with program rules could retaliate by not voting for the politician they believe is responsible), and thus politicians may have incentives to manipulate enforcement. Moreover, manipulating enforcement of program conditionalities can be a direct way to reach a subset of the population without being too visible for other parts of the population (e.g. giving a family transfers despite noncompliance will not easily be widely known, at least outside of the community of beneficiaries) and thus might be less costly than directly manipulating benefits in terms of losing votes from non-beneficiaries.[4] The manipulation of the enforcement of CCT rules could reduce the effectiveness of these programs in increasing school enrollment and/or improving health outcomes.[5]

We analyze whether politicians manipulate the enforcement of conditionalities by studying the Bolsa Familia Program, which is currently the largest conditional cash transfer program in the world, reaching around 14 million Brazilian families, that is 60 million poor people (equivalent to about 30 percent of the Brazilian population). This program provides a monthly stipend that depends on family income and the number of children. Benefits are conditional on school attendance for all school-age children in a family.[6] The program is

[4]There is little evidence on whether politicians manipulate the allocation of CCT program benefits (see Fried, 2012 on Brazil; and Green (2006b) on Mexico.)

[5]Brollo et al. (2014) analyze the effects of enforcement of the Bolsa Familia program. They find that the enforcement of program requirements increases school attendance both via a direct effect of warnings on the non-compliant families and through learning about the degree of enforcement from other families who receive warnings.

[6]Benefits are conditional also on certain health checkups and vaccination. We do not focus on health conditionalities as their enforcement is soft and much less frequent (every six months).

enforced through a system of "warnings" which gradually increase in their intensity: non-compliance with program requirements initially leads only to a notification, but repeated noncompliance results in the postponement of benefit disbursements, temporary suspension of benefits and can eventually lead to exclusion from the program. These sanctions imply a cost for noncompliant families, and it is conceivable that those affected may respond by not voting for the politician (or party) they hold responsible for this. On the other hand, enforcing conditionality is the government's responsibility, and voters, particularly those not directly affected, may appreciate that.

Program requirements are enforced at different levels. Data on daily school attendance for all children are collected by teachers, and consolidated by school directors, who can "justify" nonattendance, so that it does not count towards noncompliance. The federal government agency responsible for the program then gives warnings to families who show up in the system as having failed to comply with attendance requirements. Moreover, the municipal government can intervene and erase beneficiaries' noncompliance history in some cases, so that they do not move on to subsequent warning stages. So these different institutions can affect whether people receive warnings.

In the first part of the paper we test whether the enforcement of the rules of Bolsa Familia affects electoral outcomes. From a theoretical perspective it is not clear whether and how voters would respond to a strict enforcement of the program rules. Voters that lose their benefits due to noncompliance (or receive a notification that they might lose their benefits in the future) may be disgruntled and punish politicians at the polls. On the other hand, beneficiaries who comply with program rules may appreciate the fact that the program is implemented in a serious way, and reward politicians at the polls. So whether the enforcement of the program has a positive or negative effect on electoral outcomes is an empirical question. Moreover, even if program enforcement has electoral effects, it is not clear a priori which politician or party should be affected. Voters that lose their benefits due to noncompliance (or receive a notification that they might lose their benefits in the future) may punish incumbent mayors (or their parties), if they associate local authorities with the enforcement of program rules. Alternatively, if voters associate the program with the national government, they may punish candidates belonging to the president's party (*Partido dos Trabalhadores*, PT) or the presidential coalition.

To study the effects of program enforcement on electoral outcomes, we focus on the 2008 mayoral elections, as enforcement efforts by the Brazilian government significantly increased starting in 2006. Identifying the effects of the enforcement of program rules on voting behavior is challenging, as enforcement may be correlated with other factors that might affect electoral outcomes. For instance, municipalities with better program enforcement might also differ in terms of income, institutional quality, and/or voter preferences, which are likely to affect electoral results. To address this problem, we exploit random variation in the timing when different beneficiaries learn about the penalties they may receive for noncompliance. In particular, the exact date of the month when beneficiaries receive notifications of penalties for noncompliance depends on the last digit of their 11-digit Social Identification Number (*Número de Identificação Social* - NIS). The second round of the 2008 municipal elections was held on October 26^{th} and non-compliant beneficiaries with last digit of their NIS from 1 to 5 received notifications of penalties in the week *before* the election, while those with higher last-digits received them in the week after the election. We exploit this random assignment by comparing zip codes within a given municipality where a higher fraction of those beneficiaries in noncompliance received any penalties before the elections, with zip codes where a higher fraction were penalized after the elections. [7]

Our results suggest that beneficiaries respond negatively to the enforcement of Bolsa Familia program rules and that they associate this enforcement with the national government. We find that the vote share of candidates from PT and its coalition is lower in zip codes (within a given municipality) where a higher fraction of the beneficiaries in noncompliance received penalties before the elections. In particular, a one-standard deviation increase the share of non-compliant beneficiaries who received a warning or penalty before the elections leads to a reduction in the vote share of PT of 1.8 percentage points (this is equivalent to 11 percent of the standard deviation of this variable). Penalties that imply financial losses lead to a reduction in the vote share of PT that is almost twice as large. While the impact of program enforcement on the vote share of PT is significant and robust, we find that the vote share of incumbent mayors, independent of their political party affiliation, does not seem to be affected by the enforcement of the BFP rules. This is consistent with the fact that BFP

[7] This identification strategy assumes that voters in noncompliance cannot predict with certainty whether program rules will be enforced or not, and as a result the actual enforcement of these rules has an effect. Brollo et al. (2014) present evidence in this respect.

is strongly associated with PT.

After showing that the enforcement of Bolsa Familia conditionalities decrease voter support for mayoral candidates from parties in the presidential coalition, we analyze whether mayors become more lenient on the enforcement of program requirements around the time of elections, particularly when they face stronger electoral incentives.

Despite the potential electoral costs of enforcing program requirements, it is not clear whether mayors would actually reduce enforcement before the elections. Manipulating enforcement could be costly, as it may require the mayor to invest effort. Moreover, manipulation can have financial costs, since the national government provides additional funding to schools and municipalities based on the quality of program enforcement at the local level. Thus, mayors may compare this potential costs of reducing enforcement with the potential electoral costs of enforcing program rules. Given this trade-off, we would expect that mayors who face stronger electoral incentives may have greater incentives to manipulate enforcement.

To examine whether this is the case, we focus on the possibility of running for reelection as a measure of the intensity of local electoral incentives. Mayors in Brazil are only allowed to run for a consecutive term one time, so we analyze whether the enforcement of the BFP requirements before elections differs between first and second term mayors. One concern with this type of analysis is that having a first or second term mayor may be correlated with (time-varying) municipality-level factors that may also affect program enforcement. To address this concern, we use an RD design (Lee 2008). In particular, we analyze those municipalities where the incumbent ran for re-election in the previous election and compare municipalities where incumbents won by a narrow margin with municipalities where they lost by a narrow margin. This regression discontinuity approach provides quasi-random assignment of first-term mayors (municipalities where incumbents barely lost re-election in the previous election and a new mayor was elected) and second-term mayors (municipalities where incumbents barely won re-election in the previous election).

For our empirical analyses, we consider two different variables that may capture the enforcement of BFP rules: (i) whether beneficiaries who did not meet the attendance requirement had their low attendance "justified" by the school principal (which would imply no penalties); and (ii) whether beneficiaries who failed to meet the attendance requirement receive any warnings or penalties.

We find evidence that enforcement of BFP requirements is weaker around the time of elections in municipalities where mayors can run for reelection, consistent with the argument that local electoral incentives may lead mayors to manipulate enforcement. Specifically, we find that the share of non-compliant families that receive a justification around election times is about 11 percentage points higher for first term mayors, an increase that corresponds to roughly 1/3 of the mean (and of the standard deviation) of this variable. Furthermore, fewer warnings and penalties are observed in municipalities where incumbent mayors can be reelected, and this effect is particularly strong on warnings that imply financial losses. For these warnings, the fraction of noncompliant families who are penalized is between 10 and 12 percentage points lower (about one half of the mean and of the standard deviation of this variable) in municipalities where the mayor can run for reelection.

Finally, we investigate one possible mechanism for the manipulation of program enforcement. School principals have the power to "justify " the lack of attendance, so that beneficiaries face no penalty for not meeting the attendance requirements. If school principals increase justifications due to political reasons, we might expect to see a higher level of justifications before elections, particularly in schools where the principal is politically connected. To test whether this is the case, we use a DID approach comparing, within a given municipality, schools where the director was politically appointed with schools where this is not the case, before and after the elections. We find that in politically connected schools the fraction of BFP beneficiaries who fail the attendance requirement and receive a justification is 1 percentage point higher before the elections than in non-connected schools, which corresponds to a 7 percent increase with respect to the mean of this variable for non-connected schools. We also find that beneficiaries whose children attend politically connected schools receive comparatively fewer warning before elections. These results suggest that the behavior of school principals may be one of the relevant channels for the political manipulation of program enforcement.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and data. Section 3 analyzes whether voters respond to the enforcement of the conditionalities of BFP. Section 4 examines whether politicians manipulate the enforcement of the program requirements before elections. Section 5 investigates the role of school principals as a possible mechanism for the manipulation of enforcement. Section 6 concludes.

2 Institutional Setting and Data

2.1 Background on Bolsa Familia

Coverage. The Bolsa Familia Program, launched in 2003, is currently the largest conditional cash transfer program in the world, reaching around 14 million Brazilian families, that is 60 million poor people (equivalent to about 30% of the Brazilian population). Funds invested in the program represent 0.5 percent of the Brazilian Gross Domestic Product (GDP) and 2.5 percent of government expenditure.

Benefits. The BFP provides a monthly stipend that depends on family income and the number of children. From January 2008 to July 2009, all families considered poor (income per capita below 58 reais, approximately USD 30) were eligible to receive a monthly stipend of 62 reais. In addition, families with per-capita monthly income below 120 Brazilian reais and with children under 16 years old attending school were eligible to receive 18 reais per child (20 reais after June 2008), for up to three children. The magnitude of the benefits is large for poor families in Brazil. For instance, a poor family with three children attending school would receive a monthly stipend representing 40 percent of its total family income.

BFP stipends are distributed directly to each family head through a "citizen card" which is mailed to the family. This card operates like a debit card and is issued by the Caixa Econômica Federal, a government-owned savings bank (one of the largest banks in Brazil). The money can be withdrawn every month in over 14,000 Caixa locations. Beneficiaries receive their stipend on different days of the month depending on the last digit of their 11-digit Social Identification Number.

The targeting of the program is conducted in two steps. In the first step, the federal government allocates BFP quotas to municipalities according to poverty estimates based on poverty maps. In the second step, eligibility at the household level is determined. Families need to register with the municipal administration and declare their income. This information is then transmitted to the federal government and collected in a central database known as the Cadastro Unico. Family eligibility is then determined by the Ministry of Social Development (MDS).

Monitoring conditionality. Benefits are conditional on school attendance by all school-age children in the family and vaccinations and medical checkups. Each school-aged child has to attend at least 85 percent of school hours each month (absence due to health reasons is justified and does not count towards the number of absent days). In addition, families are required to keep an up-to-date record of vaccinations and health checks for children younger than 7 years old, and pregnant and lactating women must attend regular medical checkups. We focus our analysis on school attendance conditionalities as these are strictly monitored on a monthly basis. In contrast, monitoring of health conditionalities is soft and conducted only every six months.

Since 2006 the Brazilian central government has significantly increased efforts to effectively monitor school attendance and enforce program rules. The Ministry of Education (MEC) is responsible for monitoring school attendance. The monitoring procedure is as follows: the ministry of Social Development feeds the conditionalities system with information on all beneficiaries with school-aged children that should have their attendance monitored in a given month. MEC accesses this system and makes this information available to all municipalities. Every municipal administration has a conditionality manager who is in charge of accessing and distributing this information to all schools in the municipality. Each school receives a list of the current BFP beneficiaries in the school from the municipal administration. Data on daily school attendance for all children are collected by teachers, and consolidated by school directors. Every two or three months ("monitoring period"), monthly school attendance data for BFP beneficiaries is loaded into the system and sent to MEC, which consolidates all the information before reporting this to MDS. The conditionality manager in each municipality is responsible for collecting school attendance information, consolidating the information, and checking its quality. In schools that have computers and internet access, school principals directly load daily data on the fraction of school hours attended for each BFP beneficiary into the system and this information goes directly to MEC.

Sanctions. The program is enforced through a gradual system of "warnings." The first time a family does not comply with the program requirements, the family receives a notification, without any financial repercussions. If noncompliance continues, a series of penalties is activated. In the second warning stage, benefits are blocked for 30 days; after this period the family receives the accumulated benefit for the previous and the current months. The third and fourth warning stages lead to a loss of benefits for 60 days each time. After the fifth warning stage, the benefit is canceled and the family loses eligibility (the family can return to the program after 18 months, but the municipal administration can decide to allow a family back sooner).

Program beneficiaries are well informed about conditionalities and punishments for noncompliance. In case of noncompliance, a family receives a warning message when withdrawing their monthly benefit at the bank. They are also reminded about the warning stage they are in and the punishment they may receive in case of continued noncompliance (one possible warning message can be translated as "if you fail to comply again, your money might be suspended").

2.2 Background on Brazilian Political Institutions

The layers of political and administrative organization in Brazil are the federal government, the states, the federal district and the municipalities. Municipalities are minor federative units with an autonomous local government, ruled by a mayor, directly elected by citizens to a four-year mandate, and a legislative body, also directly elected by voters. Mayors of municipalities above 200,000 voters are directly elected by a majority runoff rule (around 80 municipalities), while mayors of municipalities below 200,000 voters are directly elected with plurality rule (around 5,490 municipalities). The elections of the President, governors, and members of Congress all take place at the same time every four years, while municipal elections are staggered by two years and also take place every four years. Before 1998 Brazilian mayors could not run for re-election, but after 1998, mayors were allowed to run for a second term. In our study we are considering the municipal administration mandate from 2005 to 2008.

2.3 Data

We make use of a unique dataset assembled combining many different sources. First, we use administrative data from the Brazilian Ministry of Social Development on the Bolsa Familia program to construct a dataset containing the following information for each beneficiary family: monthly school attendance of each child in 2008 and 2009 (in particular, for those below the 85 percent attendance threshold, we know the exact fraction of hours attended; for those above the threshold we only know that this condition was met), monthly information on warnings and monthly information on benefits that the family received (or should have received) and whether the benefit was blocked or suspended in a given month.

Second, we combine this dataset with data from the household registry data (Cadastro Unico), which contains extensive information on family background characteristics and on each individual member of the household, such as age, gender, race, marital status, education, employment status and occupation of each adult household member, per capita expenditures, ownership of durable goods, schooling history of each child and so forth.

We complement these administrative records with data from *Prova Brasil* , a nationwide proficiency test conducted by the Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira to evaluate the quality of education across Brazilian schools. This dataset includes data on several school characteristics, including how school principals were appointed.

Finally, to analyze electoral outcomes, we use data for municipal elections in October 2004 and October 2008 from the Brazilian Electoral Commission, *Tribunal Superior Eleitoral*.

Table 1 shows summary statistics for our main variables of interest. We have data for 5,342 municipalities for 6 attendance monitoring periods (Feb/Mar,April/May, and Jun/July) in 2008 and 2009. On average there are 1,169 families that receive benefits from Bolsa Familia per municipality in each period, which represents about 8 percent of the electorate.[8] In terms of compliance with program conditions, on average about 5 percent of families that participate in the program failed to meet the attendance requirements (*Fail/BFB*). Regarding enforcement, out of all beneficiaries that did not comply with attendance requirements, on average 32 percent had their non-attendance justified by the schools (*Fraction justified*) and 55 percent received any type of warnings (*Fraction warned*), with 23 percent receiving warnings that imply financial losses (*Fraction with financial loss*).

[Insert Table 1]

3 Do Voters Respond to the Enforcement of Program Conditionalities?

This section analyzes whether the enforcement of the rules of the Bolsa Familia Program affects local electoral outcomes, focusing on mayoral elections in 2008. From a theoretical

[8]Note that the data on beneficiaries refers to number of families, while the data on electorate is the number of registered voters. So this ratio underestimates the share of voters that are part of the program.

perspective it is not clear whether and how voters would respond to a strict enforcement of the program rules. The first question is whether voters react negatively or positively to the enforcement of the program. Beneficiaries that lose their benefits due to noncompliance (or receive a notification that they might lose their benefits in the future) may be disgruntled and punish politicians at the polls. On the other hand, beneficiaries who comply with program rules may appreciate the fact that the program is implemented in a serious way, and reward politicians at the polls. Moreover, taxpayers in general may also value strict enforcement of program rules. [9] So whether the enforcement of the program has a positive or negative effect on electoral outcomes is an empirical question.

Even if we knew that the net effect of strict enforcement is a loss of votes, a second question is which parties or candidates should be affected by voters' reaction. One possibility is that voters associate local authorities with the enforcement of program rules, so when they receive warnings or penalties they punish incumbent mayors or their parties. Another possibility is that voters associate the program with the national government, in which case they would punish candidates belonging to the president's party or the presidential coalition. To distinguish between these two possibilities, in our empirical analysis we consider two different dependent variables: the vote share of the party of the incumbent mayor and the vote share of the mayoral candidate of the presidential party.[10]

3.1 Methodology

Identifying the effect of the enforcement of program rules on voting behavior is challenging, as enforcement may be correlated with other factors that could affect electoral outcomes. For instance, municipalities with better program enforcement may also differ in terms of income, institutional quality and/or voter preferences, which are likely to affect electoral outcomes. To address this problem, we exploit random variation in the timing when different beneficiaries learn about any penalties they may receive for noncompliance. In particular, the exact date of the month when beneficiaries receive any notifications of penalties depends on the last digit

[9] In Brollo et al. (2014) we show that BFP recipient families learn from the warnings received by others, i.e. there is communication about the extent of enforcement of the program so that the information reaches families that have not received warnings themselves.

[10] The enforcement of program rules could also affect turnout (and not just vote shares), but we find no effects for turnout.

of their 11-digit Social Identification Number (NIS), which is random. The second round of the 2008 municipal elections was held on October 26^{th} and beneficiaries with last digit of their NIS from 1 to 5 that did not comply with attendance requirements received notifications of penalties in the week before the elections, while those with higher last-digits received them in the following week. We exploit this random assignment by comparing zip codes within a given municipality where a higher fraction of those beneficiaries in noncompliance received notifications before the elections, with zip codes where a higher fraction were penalized after the elections. This identification strategy implies that our results are not driven by differences across zip codes in terms of the level of compliance with program requirements.[11]

To conduct this analysis, we must restrict the sample to those municipalities that held second round municipal elections in 2008. We have data for 30 municipalities. Appendix Table A show summary statistics for our variables of interest at the zip level for this sample.

We construct different enforcement variables considering, alternatively, all program warnings including notifications and benefits postponement or cancellations, or only those warnings that imply some financial loss for beneficiaries. We scale the number of beneficiaries that received a warning in October by the number of beneficiaries that did not comply with attendance requirements in the relevant monitoring periods, since these are the ones who should receive warnings.[12] According to the monitoring calendar there were five monitoring periods during year 2008: February-March; April-May; June-July, August-September; and October-November. Low attendance reported during April-May 2008 resulted in warnings during October 2008.

We estimate the following regression, using observations at the zip code level:

$$Y_z = \alpha + \beta fraction\_treated_z + \delta fraction_z + \varphi X_z + \theta_i + \epsilon_z, \quad (1)$$

where Y_z is, alternatively, the vote share of the party of the incumbent mayor or the vote share of the mayoral candidate of the presidential party, in zip code area z; $fraction_z$ is the number of families that get a warning in October 2008 divided by the number of families

[11]This identification strategy requires that variation across zip codes in a given municipality in the fraction of beneficiaries with last digit of their NIS below and above 5 is orthogonal to the error term in our regression of interest. This is indeed the case due to the random nature of the last digit of the NIS.

[12]Penalties for not meeting the attendance requirements are released a few months after the monitoring period in which attendance was recorded.

that did not comply with the attendance requirements in the corresponding monitoring period; $fraction\_treated_z$ is the number of families that received a warning in the week before the elections (last-digit NIS from 1 to 5) divided by the number of families that did not comply with the attendance requirements in the corresponding monitoring period; X_z are additional control variables, including the ratio of the number of BFP beneficiary families to the electorate (*BFB/electorate*), the ratio of the number of BFP beneficiary families that did not comply to the number of beneficiary families (*Fail/BFB*), and the ratio of the number of BFP beneficiary families that did not comply and should receive the benefit before the elections (last-digit NIS from 1 to 5) over the number of families that did not comply (*Fail NIS 1 to 5/Fail*); θ_i denotes municipality fixed effects. Standard errors are clustered at the municipality level. The coefficient of interest is β, which captures whether, within a given municipality, the vote share is different in zip codes where a higher fraction of non-compliant beneficiaries received warnings before the elections, compared to zip codes where a higher fraction received warnings after the elections.[13]

[Insert Table 2]

3.2 Results

Table 2 presents results analyzing effect of program enforcement on electoral outcomes. The dependent variable in columns 1 and 2 is the vote share of the mayoral candidate affiliated with the party of the president (PT, *share PT*), the dependent variable in columns 3 and 4 is the vote share of the municipal incumbent party (*share incumbent party*) and the dependent variable in columns 5 and 6 is the vote share of the incumbent mayor (*share incumbent mayor*). In Panel A we study the effect of any warnings, regardless of whether they carry an immediate financial penalty. For these regressions, the variable *Fraction warned* is the number of beneficiary families that received any warning or penalties in October 2008, divided by the number of families that failed to comply with school attendance in the corresponding monitoring period. In Panel B we focus instead on warnings that imply financial losses. For these regressions, the numerator of the variable *Fraction warned* is the number of beneficiary

[13] We do not have data on where program beneficiaries vote, therefore we assume that people vote in the zip code area where they live. For this analysis, our sample excludes individuals who live in zip code areas without polling stations.

families that received warnings in stages 2 to 5 (i.e., warnings that entail losing the transfer) in October 2008. We are interested in testing whether voters respond differently to the enforcement of program rules when they suffer a loss of transfers. In both panels"Fraction treated" corresponds to the same definition of "Fraction warned", but only captures the share of households that got the warnings before the election. Regressions in columns 2, 4 and 6 include municipality fixed effects.[14]

The results suggest that there are political costs of enforcing conditionalities for candidates belonging to the president's party. The vote share for these candidates is significantly lower in zip code areas where a higher fraction of non-compliant beneficiaries received a warning or penalty in the days before the elections. These results hold both when analyzing all warnings and only those warning stages that imply financial losses. The inclusion of municipality fixed effects does not significantly affect the magnitude of our coefficient of interest, consistent with the fact that, after we control for the fraction of beneficiaries warned, the share warned before the election is random. In terms of economic magnitude, the estimates in column 2 imply that an increase the share of noncompliant beneficiaries who received a warning before the elections ("fraction treated") by one standard deviation (0.286), will reduce the share of votes received by the PT by 1.8 percentage points (that is 11 percent of a standard deviation of this variable). In the case of warnings with financial losses, a one-standard deviation increase would reduce the vote share of PT by about 3 percentage points, or 20 percent of the standard deviation of this variable. Although not reported, we do not find significant results for warnings that do not carry financial penalties (warning stage one). Moreover, the results reported in Columns 3, 4, 5 and 6 show no evidence that voters associate the enforcement of the program with the local mayor. However, it should be noted that the sample size for these regressions is much smaller, which might account for the lack of statistical significance.

These findings suggest that the negative political effects of enforcement (i.e., beneficiaries that get a warning punishing authorities at the polls) more than compensate any potential political benefits of enforcement (e.g., compliant families appreciating strict implementation).

[14]Note that the sample of municipalities included in the regressions varies across columns, depending on the sample of parties that ran in the second round elections. For instance, in columns 1 and 2 when we analyze the vote share of the mayoral candidate affiliated with PT, we can only consider municipalities where this candidate ran in the second round. Similarly, in columns 3 and 4 we can only include municipalities where the municipal incumbent party made it to the second round and in columns 5 and 6 municipalities where the incumbent mayor ran in the second round elections.

They also indicate that voters associate the enforcement of BFP rules with the national government.

[Insert Table 3]

To further strengthen the credibility of our identification strategy, we conduct a placebo test considering as explanatory variables warnings received in November 2008, *after* the elections were conducted. In this case, warnings received by beneficiaries with last-digit NIS from 1 to 5 should have no effect on vote shares, because the elections were held before these warnings were issued in November. The results of this exercise are reported in Table 3. As expected, we find no significant difference in electoral outcomes between zip codes with a higher-fraction of non-compliant beneficiaries that received warnings with last-digit NIS from 1 to 5, and those with last-digit NIS above 5. This increases our confidence that our previous results are not due to some spurious association between the last digit of the NIS and vote shares.

4 Do Politicians Manipulate the Enforcement of the Program Rules Close to Elections?

This sections analyses whether incumbent mayors change the degree of program enforcement before the elections in response to electoral incentives.

The results in Section 3 show that program enforcement can affect voting, which could create incentives for mayors to reduce enforcement in the run-up to the elections. However, despite the potential electoral returns of reducing enforcement, it is not obvious that all mayors would engage in this type of manipulation. Manipulating enforcement could be costly, as it may require the mayor to invest effort. Moreover, manipulation can have financial costs, since the national government provides additional funding to schools and municipalities based on the quality of program enforcement at the local level.
footnoteThe central government developed an index of the quality of the enforcement of the conditionalities (*Indice de Gestao Descentralizada*), which is used by the central government to allocate funds to municipal administration. Given these potential costs, we would expect that only those mayors that are more likely to benefit from less strict program enforcement

will engage in manipulation. In particular, we hypothesize that those mayors who face higher electoral incentives (e.g., those who can run for re-election) should be more likely to manipulate enforcement.

Identifying the effects of electoral incentives on enforcement is not a trivial task. A comparison between municipalities with first-term mayors and those with second-term mayors will probably generate biased estimates due to endogeneity issues.For instance, program enforcement might be correlated with municipality-specific characteristics such as voter preferences or demographic characteristics, all of which could also influence whether the mayor is on her first or second mandate.

To examine whether mayors who face stronger electoral incentives become soft in the enforcement of the program conditionalities before the elections, addressing the presence of both time-invariant and time-varying confounding factors, we implement a regression discontinuity design in the spirit of Lee (2008).[15] Exploiting the fact that in Brazil mayors can run only for one consecutive term, we analyze those municipalities where the incumbent ran for re-election in the previous election and compare municipalities where incumbents won by a narrow margin with municipalities where they lost by a narrow margin. This regression discontinuity approach provides quasi-random assignment of first-term mayors (municipalities where incumbents barely lost re-election in the previous election and a new mayor was elected) and second-term mayors (municipalities where incumbents barely won re-election in the previous election).[16]

Specifically, we consider the mayoral elections held in 2004 to calculate the margin of victory of the non-incumbent mayoral candidate in each municipality i (MV_i). At the threshold $MV_i = 0$ there is a sharp change in whether the mayor in power for the mandate 2005-2008 is first or second term: for $MV_i < 0$ the incumbent is reelected in 2004, so she is a second term mayor in the mandate 2005-2008 and cannot run for re-election again; for $MV_i > 0$ the incumbent is not reelected in 2004 and a new mayor is in power in 2005-2008, having the possibility to run for re-election in 2008.

MV_i is viewed as a random variable depending on both observable and unobservable

[15]See also Lee, Moretti, and Butler (2004), and Ferraz and Finan (2011). Other applications of the close-race RD design include Hainmueller and Kern (2006), Pettersson-Lidbom (2008), Eggers and Hainmueller (2009), Brollo and Nannicini (2012) and Brollo and Troiano (2014).

[16]Appendix Table B reports the summary statistics of our variables of interest for the sample of municipalities included in this analysis.

factors, as well as on random events on election day. The ATE of having a first term mayor (who can run for reelection) in close elections is given by:

$$E[\tau_i(1) - \tau_i(0)|MV_i = 0] = \lim_{\epsilon \downarrow 0} E[\tau_i|MV_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[\tau_i|MV_i = \epsilon]. \tag{2}$$

τ is defined as a local effect, because it captures the impact of being a first term mayor on the outcome only for municipalities around the threshold $MVP = 0$ (i.e. for elections that were decided for a margin that is tiny enough).

We estimate the ATE expressed in equation (2) fitting a third order polynomial in MV_i on either side of the threshold $MV_i = 0$:

$$\tau_i = \sum_{k=0}^{3}(\rho_k MV_i^k) + F_i \sum_{k=0}^{3}(\pi_k MV_i^k) + \varepsilon_i, \tag{3}$$

where MV_i is the margin of victory of the non-incumbent candidate in the 2004 mayoral elections in municipality i and F_i is a dummy variable that equals one if the mayor in the 2005-2008 mandate is a first-term mayor (eligible to run for re-election); standard errors are clustered at the city level. The estimated coefficient $\hat{\pi}_0$ identifies the ATE at the threshold $MV_i = 0$. We follow the standard procedure of fitting a third order polynomial. We also show results for local linear regressions with optimal bandwidth computed using, alternatively, the algorithms by Calonico, Cattaneo and Titiunik (2012) and Imbens and Kalyanaraman (2011).

The standard RDD assumption is that potential outcomes must be a continuous function of the running variable at the threshold.[17] To formally test this assumption, we test the continuity of the density of the margin of victory, following McCrary (2008). The results are reported in Appendix Figure A. We find no evidence of discontinuities in the margin of victory of the non-incumbent mayoral candidate. Our estimation strategy controls for municipality-specific characteristics. Therefore we should not expect any difference in municipal characteristics between treatment and control groups around the cutoff $MV_i = 0$. To

[17]Several recent papers argue that the assumptions of RDD may be violated in recent U.S. House of Representatives elections. In particular, Caughey and Sekhon (2011) show that close U.S. House elections during 1944-2008 are prone to manipulation, as bare winners and bare losers are significantly different from each other. See also Snyder (2005), Grimmer et al. (2011), and Vogl (2011). However, Eggers et al. (2013) suggest that in no other case around the world elections exhibit a pattern that violates the RDD assumptions. Recent work on incumbency advantage in Brazilian municipalities (De Magalhães, 2012) shows that the problem of manipulative sorting identified for the U.S. does not apply to Brazil. Eggers et al. (2013) also find no evidence of sorting in Brazil.

test this, we checked whether a vast array of observable municipal pre-treatment characteristics are balanced around the cut-off. The results are reported in Appendix Table D and show that pre-treatment municipal characteristics are balanced around the cut-off. Appendix Table D also reports the results of balance checks for several mayoral characteristics (party affiliation, education, etc) showing that there is no discontinuity around the cut-off. This is important, as differences in mayoral characteristics could affect the interpretation of the results.

One potential limitation of our empirical approach is that we cannot disentangle the effects of electoral incentives from those of experience in office, since second term mayors by definition have spent more time in office compared to first term mayors that are eligible to run for reelection. If there is a learning process in terms of how to enforce program conditionalites, we may find that municipalities with second term mayors have more stringent enforcement, as these mayors have had more time in office to acquire the required knowledge. However, if this is the case one might expect to find differences in terms of enforcement for all types of warnings and penalties. In contrast, if electoral incentives are leading first term mayors to be less stringent with enforcement, one might expect this manipulation to focus only on those types of warnings or penalties that have larger electoral affects. As discussed below, our results seem to be more consistent with this second argument, as we only find evidence that first term mayors enforce less when analyzing warning stages that imply financial losses and find no significant differences between first and second term mayors in terms of school attendance or in terms of simple warnings for noncompliance. We are currently conducting additional analyses to provide further evidence to disentangle the effects of electoral incentives from those of experience in office.

BFP program requirements are enforced both at the national and local levels. As described above, schools and the local administration are responsible for reporting attendance to the national government, which then is in charge of handing out warnings or penalties for noncompliance. Local authorities could reduce the level of enforcement in at least three ways. First, schools and/or municipal authorities could misreport attendance figures to reduce the number of beneficiaries that fail to meet the attendance requirement. Second, according to program rules, absence due to health reasons can be excused by school principals and does not count towards the number of absent days. Thus, politically-motivated school principals could increase justifications before the elections. Finally, the municipal administration can directly remove beneficiaries from the warning list in response to complaints.

Given these different ways in which local authorities could reduce the level of enforcement, we consider three main measures of enforcement for our empirical analysis: (i) the fraction of beneficiaries (families) that failed to meet the school attendance requirement that had their low attendance justified by the school principal (which would exempt them from getting a warning) (*fraction justified*); (ii) the fraction of beneficiaries (families) that failed to attend school that received any warnings or penalties (*fraction warned*); and (iii) the fraction of beneficiaries (families) that failed to attend school that received warnings that imply a financial loss (warning stages 2-5) (*fraction warned with financial loss*).[18] We also analyze the fraction of beneficiaries families who did not comply with school attendance.

[Insert Table 4]

Table 4 reports our estimates of the effects of electoral incentives on program enforcement. We report results for OLS regressions without controlling for the margin of victory, the regressions described in equation (3) using a third order spline polynomial specification, and local linear regressions with optimal bandwidth. The dependent variable in column 1 is the *Fraction justified*; in column 2 it is the *Fraction warned*; and in column 3 it is the *Fraction warned with financial loss*. All variables are measured before the 2008 municipal elections.

We find significant effects of electoral incentives on enforcement. The results in column 1 show that first term mayors justify 10 to 11 percentage points more than second term mayors (this is about 1/3 of the mean as well as of the standard deviation of the fraction justified). The results in column 2 indicate that the fraction of beneficiaries who did not comply with attendance requirements and actually got a warning is lower in municipalities with mayors that face reelection incentives, consistent with the idea that these mayors have more incentives to manipulate program enforcement to try to reduce any potential electoral costs of program enforcement. The effects are stronger for *fraction warned with financial loss*. For these warnings, the decrease in the fraction warned is between 10 and 12 percentage

[18] We also estimated our regressions constructing these different measures of enforcement considering the total number of beneficiaries as the denominator (instead of the number of beneficiaries that failed to meet the school attendance requirement) and obtained similar results.

points, i.e., about one half of the mean and of the standard deviation of the fraction warned with financial loss.

In columns (4) and (5) we test whether the above results, which we interpret as evidence of program manipulation, may instead be due to changes in attendance patterns or in the number of beneficiaries, that happened to occur differentially before the 2008 elections in municipalities with mayors that could run for reelection. To do this, we consider two alternative dependent variables: *BFB/electorate* is the ratio of the number of BFP beneficiary families to the electorate and *fail/BFB* is the ratio of the number of BFP beneficiary families who did not comply with school attendance to the number of beneficiary families. If our results on justifications and warnings are driven by the manipulation of program enforcement, and not by these other factors, we should find no effects for these dependent variables. Consistent with this hypothesis, we do not find any significant results for these two variables.[19] The lack of significant results for *fail/BFB* also suggests that local authorities are not manipulating program enforcement by underreporting attendance due to political reasons.[20]

Visual inspection of the outcomes in Figure 1 confirms the results described above, as there are visible discontinuities around the cut-off for *Fraction justified*, *Fraction warned*, and *Fraction warned loss*. As expected, we do not find any discontinuity for the fraction of beneficiary families who fail attendance *Fail/BFB*, nor for the fraction of beneficiary families in the electorate *BFB/electorate*.[21] The graphical and non-graphical evidence is robust to the presence of specific outliers.

5 How Do Politicians Manipulate Enforcement?

We finally explore a possible mechanism through which the enforcement of the program rules may be manipulated. As discussed above, if school principals justify low attendance, beneficiaries do not receive any warnings for noncompliance. Therefore, politically-motivated

[19] As an additional robustness check, we also analyzed warnings and justifications after the elections, which should not be subject to manipulation, and found no significant effects.

[20] Note that this does not imply that attendance is accurately reported. Teachers may be sympathetic to beneficiaries and may over report attendance to help them avoid any penalties. However, our results suggest that any such misreporting is not politically motivated.

[21] In this figure, outcomes are averaged into bins of intervals of the margin of victory. Bins closer to the cut-off contain many more observations, given that the density of our running variable (margin of victory of the female candidate) is concentrated around zero: there are comparatively more data in the bins closer to the threshold than in the bins farther from the threshold.

school principals may increase justifications before the elections. To test whether this is the case, we exploit information on whether school directors were politically appointed and use a diff-in-diff approach at the school level to compare politically connected schools before and after the elections.

We have data for 37,807 schools (with 9,457 schools having a politically appointed director) for six attendance monitoring periods (Feb/Mar, April/May, and Jun/July in 2008 and 2009). Appendix Table C reports the summary statistics of our variables of interest for politically connected and non-politically connected schools. Using these data we estimate the following regression:

$$Y_{smt} = \alpha + \beta_1 Before_t * Pol_{sm} + \theta_{sm} + \gamma_t + \varepsilon_{smt}, \tag{4}$$

where Y_{smt} is one of our measures of enforcement of program conditionalities in school s located in municipality m in monitoring period t. $Before_t$ is a dummy variable that equals one for the monitoring periods before the elections and zero otherwise; Pol_{sm} equals one if the school principal was politically appointed –our proxy for political connection between the mayor and the school's director; θ_{sm} denotes school fixed effects and γ_t are monitoring period fixed effects. Standard errors are clustered at the school level.

[Insert Table 5]

Table 5 reports regression results analysing whether politically connected schools contribute to less strict enforcement of conditionalities before the elections. For each outcome variable, we report results only with monitoring period fixed effects (columns 1, 4, 7 and 10), with municipality fixed effects (columns 2, 5, 8 and 11) and with school fixed effects (columns 3, 6, 9 and 12). When our dependent variable is the fraction of non-compliant families who receive a justification (columns 1 to 3) we find consistently positive and significant coefficients on the interaction between the dummy for monitoring periods before the election and the dummy for politically connected schools. According to our most conservative specification –with school fixed effects– politically connected schools justify 1 percentage point more before the elections than non-connected schools, which corresponds to 7 percent of the mean fraction justified (14 percent). We also find consistently negative and significant coefficients on the interaction term when the dependent variable is the fraction of non-compliant families who receive a warning (columns 4 to 9), suggesting that relatively fewer students in these schools receive warnings before elections.

As a robustness check, in columns 10 to 12 we report similar regressions using as dependent variable the ratio of the number of BFP beneficiary families who did not comply with school attendance to the number of beneficiary families in a given school. If our results on justifications and warnings are driven by the manipulation of program enforcement, and not by changes in attendance patterns that happened to occur differentially before the 2008 elections in politically connected schools, then we should find no effects for this dependent variable. Consistent with this hypothesis, we find that, once we control for either municipality or school fixed effects, there is no significant difference between politically and non-politically connected school before the elections.

Overall, these findings suggest that one potential channel through which the enforcement of the BFP rules may be manipulated is through increases in justifications before the election by politically-motivated school principals.

6 Conclusions

This paper analyzes whether politicians manipulate targeted government programs to influence electoral outcomes, by studying the enforcement of conditionalities in the Bolsa Familia Program (BFP), a large-scale conditional cash transfer program in Brazil. Our working hypothesis is that, if targeted government programs affect voter choices, then politicians may have incentives to strategically manipulate these programs.

To test this, we first analyze whether voters respond to the enforcement of program rules. Exploiting random variation in the timing when different beneficiaries learn about penalties for noncompliance, we find that in the 2008 municipal elections the vote share of candidates aligned with the President's party is lower in zip codes where more beneficiaries received penalties before the elections. Second, we study whether local authorities manipulate this enforcement, particularly when they face stronger electoral incentives. Using a regression discontinuity design, we find that enforcement of BFP requirements is weaker around the time of elections in municipalities where mayors can run for reelection. Moreover, we provide evidence on a possible mechanism for this manipulation, finding that schools with politically-connected principals tend to excuse insufficient attendance relatively more before the elections, so that beneficiaries face no penalty.

Our results have important policy implications, as the manipulation of the enforcement of programme rules has the potential to reduce the effectiveness of conditional cash transfer programmes. Research shows that programmes that impose more stringent conditions tend to have larger effects on schooling (higher enrollment and attendance and lower drop-out rates), health (more vaccinations and medical check-ups) and child nutrition. Careful attention to the design and implementation of conditional welfare programmes could reduce their vulnerability to political manipulation. For example, having more formal criteria to assess whether non-attendance at schools is justified would help. And, to the extent possible, giving decision-making power for justifications to less politically dependent authorities would also improve the system.

Another interesting implication regards the ability of the relevant stakeholders to detect manipulations in the system. The classic form of manipulation studied in the case of transfer programs is mis-targeting, in particular giving benefits to ineligible people. This form of manipulation is relatively easier to detect than the form we study in this paper: in fact such targeting errors may be very visible to neighbors who know the family, and could be uncovered by an audit scheme (of the type used for Brazilian municipalities). On the other hand, detecting "excess" justifications or missed warnings would require a more sophisticated analysis, one that keeps track of the entire attendance and payment history of the family. A possible lesson from our study is that, as the structure of transfer schemes becomes more complex (e.g., with an articulated system of conditionalities), so does the sophistication of political actors who wish to manipulate such schemes for electoral purposes.

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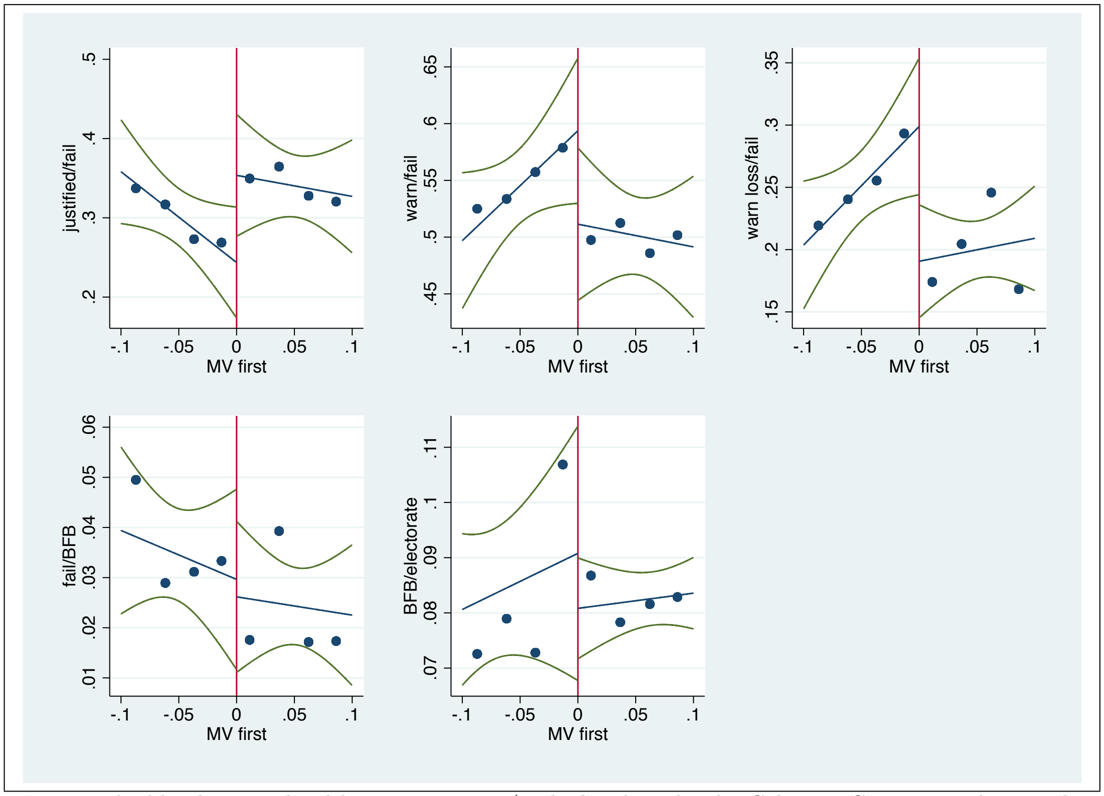
Figure 1: The Effects of Re-Election Incentives on Enforcement



Notes. The blue line is a local linear regression (with the algorithm by Calonico, Cattaneo and Titiunik, 2012) in the margin of victory of the non-incumbent mayoral candidate in the 2004 elections (MV first) in municipality i, fitted separately on each side of the margin of victory thresholds at zero. $MV_i > 0$ when the winner in municipality i is the non-incumbent mayoral candidate, $MV_i < 0$ when the winner in municipality i is the incumbent mayor. The green lines are the 95 percent confidence interval of the polynomial. Scatter points are averaged over 2 percent intervals. The sample only includes municipalities where the incumbent mayor ran for reelection in 2004.

Table 1: Summary Statistics at the Municipality Level

	mean	Std. Dev.	Obs
	(1)	(2)	(3)
Number of BFP families	1,169	2,486	5,342
Families that failed attendance requirement	51	201	5,342
Fail/BFB	0.052	0.063	5,342
BFB/electorate	0.084	0.056	5,342
Fraction justified	0.322	0.265	5,342
Fraction warned	0.546	0.241	5,342
Fraction warned with financial loss	0.225	0.189	5,342

Notes. Data at the municipality level for attendance monitoring periods Feb/Mar, Apr/May, and June/July in 2008 and 2009. *Fail/BFB* is the fraction of beneficiaries (families) that did not comply with the attendance requirement. *BFB/electorate* is the number of BFP beneficiary families over electorate.*Fraction justified* is the fraction of beneficiaries (families) that did not meet the attendance requirement that had their low attendance justified by the school. *Fraction warned* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning of any type. *Fraction warned with financial loss* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning that implies a financial loss (warning stages from 2 to 5).

Table 2: Do Voters Respond to the Enforcement of Program Conditionalities?

	(1)	(2)	(3)	(4)	(5)	(6)
	share PT	share PT	share incumbent party	share incumbent party	share incumbent mayor	share incumbent mayor
Panel A: Effects of warnings						
Fraction treated	-0.065***	-0.062***	-0.021	0.016	-0.009	0.016
	(0.024)	(0.022)	(0.039)	(0.030)	(0.048)	(0.042)
Fraction warned	0.009	-0.005	0.004	-0.021	-0.019	-0.040
	(0.016)	(0.015)	(0.030)	(0.023)	(0.037)	(0.033)
BFB/electorate	0.041	-0.001	-0.346***	-0.218***	-0.358***	-0.246***
	(0.111)	(0.064)	(0.072)	(0.056)	(0.070)	(0.056)
Fail NIS 1 to 5/fail	0.015	0.013	-0.008	-0.022	-0.023	-0.026
	(0.015)	(0.014)	(0.028)	(0.021)	(0.037)	(0.032)
Fail/BFB	-0.077***	-0.071***	0.014	-0.020	-0.014	0.021
	(0.014)	(0.019)	(0.031)	(0.026)	(0.035)	(0.046)
Observations	1,150	1,150	254	254	151	151
R-squared	0.034	0.162	0.046	0.474	0.079	0.363
Panel B: Effects of warnings with financial loss						
Fraction treated	-0.109***	-0.103***	-0.087*	0.019	-0.050	0.082
	(0.030)	(0.027)	(0.051)	(0.040)	(0.051)	(0.053)
Fraction warned	0.025	-0.001	0.029	-0.009	0.006	-0.057
	(0.021)	(0.020)	(0.040)	(0.035)	(0.048)	(0.048)
BFB/electorate	0.042	0.004	-0.344***	-0.215***	-0.356***	-0.231***
	(0.110)	(0.064)	(0.072)	(0.057)	(0.070)	(0.056)
Fail NIS 1 to 5/fail	0.011	0.010	-0.004	-0.017	-0.019	-0.032
	(0.013)	(0.012)	(0.021)	(0.017)	(0.027)	(0.024)
Fail/BFB	-0.075***	-0.075***	0.018	-0.022	-0.009	0.032
	(0.014)	(0.018)	(0.032)	(0.027)	(0.041)	(0.047)
Observations	1,150	1,150	254	254	151	151
R-squared	0.038	0.167	0.055	0.472	0.077	0.364
Municipality FE	no	yes	no	yes	no	yes

Notes. Results are displayed for regressions as in equation 1. Data correspond to October 2008 and include only municipalities that held second round mayor elections. *Fraction warned* denotes the fraction of beneficiaries that did not meet the attendance requirement and received a warning. *Fraction treated* denotes the fraction of beneficiaries that did not meet the attendance requirement and were warned in the week days before the elections. *BFB/electorate* is the number of BFP beneficiary families over electorate. *Fail NIS 1 to 5/fail* is the number of BFP beneficiary families who did not comply with school attendance and should receive the benefit before the elections (last-digit NIS from 1 to 5) over the number of families that did not comply with the attendance requirement. *Fail/BFB* is the fraction of beneficiaries that did not comply with the attendance requirement. The dependent variable in columns (1) and (2) is the vote share of the incumbent mayor that is affiliated with the PT; in columns (3) and (4) the dependent variable is the vote share of the municipal incumbent party, and in columns (3) and (4) the dependent variable is the vote share of the municipal incumbent mayor. Panel A displays regressions where the *Fraction warned* and *Fraction treated* variables are constructed considering all types of warnings. Panel B displays regressions where the *Fraction warned* and *Fraction treated* variables are constructed considering only warnings that imply a financial loss (warning stage from 2 to 5). Regressions in columns (2), (4), and (6) include municipality fixed effects. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 3: Placebo (November): Do Voters Respond to the Enforcement of Program Conditionalities?

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Effects of warnings						
	share PT	share PT	share incumbent party	share incumbent party	share incumbent mayor	share incumbent mayor
Fraction treated 0.006	0.001	0.035	-0.034	-0.019	-0.065	
	(0.032)	(0.024)	(0.040)	(0.032)	(0.054)	(0.048)
Fraction warned	-0.005	-0.000	0.045	0.038	0.021	0.046
	(0.021)	(0.018)	(0.028)	(0.024)	(0.038)	(0.034)
BFB/electorate	0.046	-0.018	-0.293***	-0.210***	-0.289***	-0.232***
	(0.102)	(0.056)	(0.071)	(0.056)	(0.072)	(0.054)
Fail before/fail	0.017	0.005	0.011	0.033	0.032	0.052
	(0.028)	(0.021)	(0.031)	(0.026)	(0.043)	(0.041)
Fail/BFB	0.013	-0.083***	0.012	-0.026	-0.006	0.014
	(0.020)	(0.017)	(0.033)	(0.027)	(0.044)	(0.036)
Observations	504	504	259	259	148	148
R-squared	0.006	0.435	0.077	0.453	0.057	0.347
Panel B: Effects of warnings with financial loss						
Fraction treated -0.002	-0.008	0.021	-0.004	0.058	0.019	
	(0.025)	(0.019)	(0.036)	(0.030)	(0.047)	(0.040)
Fraction warned	0.006	-0.003	-0.009	0.005	-0.031	-0.004
	(0.018)	(0.014)	(0.026)	(0.023)	(0.036)	(0.032)
BFB/electorate	0.045	-0.015	-0.272***	-0.207***	-0.285***	-0.230***
	(0.102)	(0.056)	(0.072)	(0.055)	(0.071)	(0.053)
Fail before/fail	0.022	0.009	0.026	0.012	0.001	0.003
	(0.016)	(0.013)	(0.022)	(0.018)	(0.025)	(0.023)
Fail/BFB	0.013	-0.084***	0.013	-0.021	0.002	0.027
	(0.020)	(0.017)	(0.033)	(0.028)	(0.044)	(0.038)
Observations	504	504	259	259	148	148
R-squared	0.006	0.435	0.040	0.448	0.063	0.339
Municipality FE	no	yes	no	yes	no	yes

Notes. Results are displayed for regressions as in equation 1. Data correspond to November 2008 and include only municipalities that held second round mayor elections. *Fraction warned* denotes the fraction of beneficiaries that did not meet the attendance requirement and received a warning. *Fraction treated* denotes the fraction of beneficiaries that did not meet the attendance requirement and were warned in the week days before the elections. *BFB/electorate* is the number of BFP beneficiary families over electorate. *Fail NIS 1 to 5/fail* is the number of BFP beneficiary families who did not comply with school attendance and should receive the benefit before the elections (last-digit NIS from 1 to 5) over the number of families that did not comply with the attendance requirement. *Fail/BFB* is the fraction of beneficiaries that did not comply with the attendance requirement. The dependent variable in columns (1) and (2) is the vote share of the incumbent mayor that is affiliated with the PT; in columns (3) and (4) the dependent variable is the vote share of the municipal incumbent party, and in columns (3) and (4) the dependent variable is the vote share of the municipal incumbent mayor. Panel A displays regressions where the *Fraction warned* and *Fraction treated* variables are constructed considering all types of warnings. Panel B displays regressions where the *Fraction warned* and *Fraction treated* variables are constructed considering only warnings that imply a financial loss (warning stage from 2 to 5). Regressions in columns (2), (4), and (6) include municipality fixed effects. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 4: The Effects of Electoral Incentives on Enforcement, RD estimates

	(1)	(2)	(3)	(4)	(5)
	Results			Robustness checks	
	fraction justified	*fraction warned*	*fraction warned financial loss*	*fail/ BFB*	*BFB/ electorate*
OLS (First Term)	0.019	-0.031*	-0.021	-0.003	0.000
	(0.019)	(0.017)	(0.013)	(0.004)	(0.004)
Observations	979	979	979	979	979
POLY (3^{rd} (First Term)	0.099**	-0.075*	-0.117***	-0.016*	-0.009
	(0.044)	(0.040)	(0.032)	(0.010)	(0.019)
Observations	979	979	979	979	979
LLR (First Term)	0.110**	-0.075*	-0.115***	0.005	0.013
	(0.050)	(0.044)	(0.035)	(0.010)	(0.009)
H (CCT)	11	12	12	12	15
Observations	482	515	520	232	287
LLR (First Term)	0.096**	-0.081*	-0.094***	-0.012	0.005
	(0.048)	(0.041)	(0.030)	(0.009)	(0.010)
H (IK)	12	15	18	18	12
Observations	523	605	689	329	232

Notes. Results are displayed for OLS, RDD 3^{rd} order spline polynomial, and local linear regression using optimal bandwidth with the algorithm by Calonico, Cattaneo and Titiunik (2012) (CCT), and local linear regression with optimal bandwidth with the algorithm by Imbens and Kalyanaraman (2011) (IK). The sample only includes municipalities where the incumbent mayor ran for reelection in 2004. Dependent variables: In column (1) the dependent variable is the fraction of beneficiaries (families) that did not meet the attendance requirement that had their low attendance justified by the school. In column (2) the dependent variable is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning of any type. In column (3) the dependent variable is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning that implies a financial loss (warning stages from 2 to 5). In column (4) the dependent variable is the fraction of beneficiaries (families) that did not comply with the attendance requirement. In column (5) the dependent variable is the number of BFP beneficiary families over electorate. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

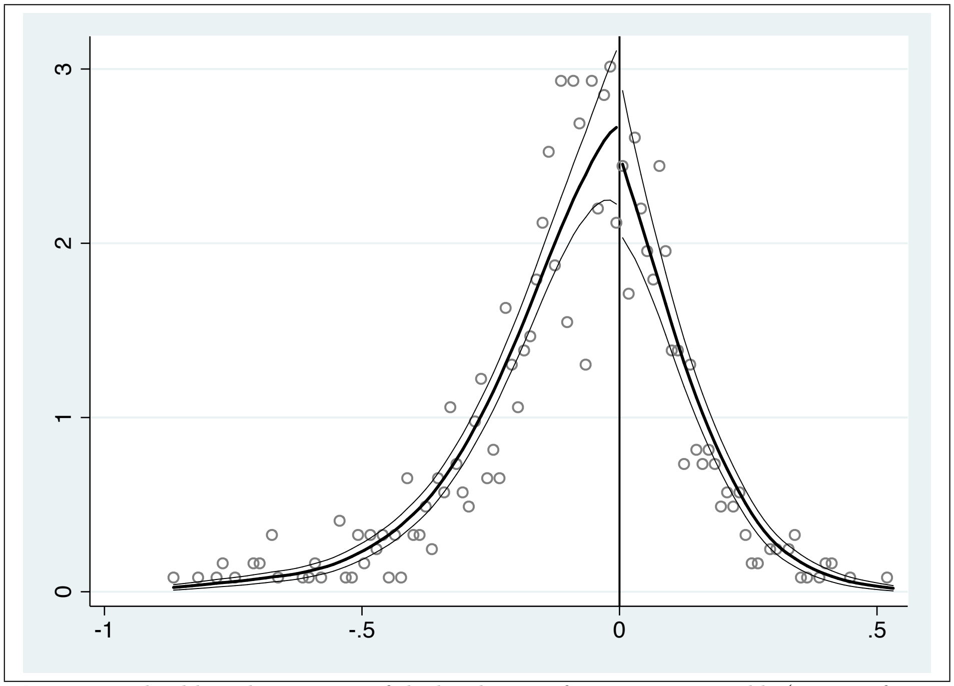
Table 5: Politically Connected Schools and Enforcement

	Results									Robustness check		
	fraction justified			fraction warned			fraction warned with financial loss			fail/ BFB		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pol	0.030***	0.035***		-0.015***	0.012***		-0.057***	0.005		-0.018***	0.004***	
	(0.003)	(0.003)		(0.004)	(0.004)		(0.003)	(0.003)		(0.001)	(0.001)	
Pol*Before	0.014***	0.008**	0.010**	-0.020***	-0.015***	-0.016***	-0.010**	-0.011***	-0.011**	-0.003***	-0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.001)	(0.001)	(0.001)
Observations	158,517	158,517	158,517	158,517	158,517	158,517	158,517	158,517	158,517	158,517	158,517	158,517
R-squared	0.008	0.222	0.604	0.036	0.222	0.522	0.027	0.176	0.399	0.020	0.275	0.566
Monitoring period FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Municipality FE	no	yes	no	no	yes	no	no	yes	no	no	yes	no
School FE	no	no	yes	no	no	yes	no	no	yes	no	no	yes

Notes. Data at the school level for attendance monitoring periods Feb/Mar, Apr/May, and June/July in 2008 and 2009. Results are displayed for regressions as in equation 4. The dummy variable $before$ equals one for monitoring periods before the election and zero otherwise. The dummy variable Pol equals one if the school principal was politically appointed by the mayor. Dependent variables: In columns (1), (2) and (3) the dependent variable is the fraction of beneficiaries (families) that did not meet the attendance requirement that had their low attendance justified by the school. In columns (4), (5) and (6) the dependent variable is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning of any type. In columns (7), (8) and (9) the dependent variable is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning that implies a financial loss (warning stages from 2 to 5). In columns (10), (11) and (12) the dependent variable is the fraction of beneficiaries (families) that did not comply with the attendance requirement. All regressions include monitoring period fixed effects. Regressions displayed in columns (2), (5), (8) and (11) include municipality fixed effects. Regressions displayed in columns (3), (6), (9), and (12) include school fixed effects. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

7 Appendix

Figure A: McCrary Density Test



Notes. Weighted kernel estimation of the log density of our running variable (margin of victory of non-incumbent mayoral candidate in the 2004 elections in municipality i), performed separately on each side of the margin of victory threshold at zero. $MV_i > 0$ when the winner in municipality i is the non-incumbent mayoral candidate, $MV_i < 0$ when the winner in municipality i is the incumbent mayor. Discontinuity estimate: point estimate -0.066 and standard error 0.128. The sample only includes municipalities where the incumbent mayor ran for reelection in 2004.

Table A: Summary Statistics at the Zip-code Level

	mean	Std. Dev.	Obs
	(1)	(2)	(3)
Number of BFP families	27	125	1,150
Families that failed attendance requirement (fail)	5	15	1,150
Fail/BFB	0.484	0.333	1,150
Fail NIS 1 to 5/fail	0.488	0.388	1,150
BFB/electorate	0.011	0.046	1,150
Fraction warned	0.328	0.382	1,150
Fraction warned with financial loss	0.161	0.289	1,150
Fraction treated	0.155	0.286	1,150
Fraction warned treated with financial loss treated	0.079	0.286	1,150
Share PT	0.493	0.157	1,150
Share incumbent (party)	0.508	0.113	653

Notes. Data at the zip code level. Data correspond to October 2008 and include only municipalities that held second round mayor elections. *Fail/BFB* is the fraction of beneficiaries (families) that did not comply with the attendance requirement. *BFB/electorate* is the number of BFP beneficiary families over electorate. *Fail NIS 1 to 5/fail* is the number of BFP beneficiary families who did not comply with school attendance and should receive the benefit before the elections (last-digit NIS from 1 to 5) over the number of families that did not comply with the attendance requirement. *Fraction warned* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning of any type. *Fraction warned with financial loss* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning that implies a financial loss (warning stages from 2 to 5). *Fraction warned treated* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning of any type in the week days before the elections. *Fraction warned with financial loss treated* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning that implies a financial loss (warning stages from 2 to 5) in the week days before the elections. *Share PT* is the vote share of the mayoral candidate of the PT. *Share incumbent (party)* is the vote share of the mayoral candidate of the incumbent party.

Table B: Summary Statistics RDD Sample

	mean	Std. Dev.	Obs
	(1)	(2)	(3)
Number of BFP families	661	802	979
Families that failed attendance requirement (fail)	28	51	979
Fail/BFP families	0.053	0.066	979
BFB/electorate	0.083	0.080	979
Fraction justified	0.318	0.282	979
Fraction warned	0.539	0.260	979
Fraction warned with financial loss	0.227	0.208	979
First-term mayor	0.353	0.478	979

Notes. Data at the municipality level. The sample only includes municipalities where the incumbent mayor ran for reelection in 2004. *Fail/BFB* is the fraction of beneficiaries (families) that did not comply with the attendance requirement. *BFB/electorate* is the number of BFP beneficiary families over electorate.*Fraction justified* is the fraction of beneficiaries (families) that did not meet the attendance requirement that had their low attendance justified by the school. *Fraction warned* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning of any type. *Fraction warned with financial loss* is the fraction of beneficiaries (families) that did not meet the attendance requirement that received a warning that implies a financial loss (warning stages from 2 to 5). *First term mayor* is a dummy variable that equals one for municiaplities that had a first-term mayor in the mandate 2005-2008.

Table C: Summary Statistics at the School Level

	Politically connected schools		Non politically connected schools	
	mean	Std. Dev.	mean	Std. Dev.
	(1)	(2)	(3)	(4)
Number of BFP families	148	121	142	116
Families that failed attendance requirement	10	16	10	16
Fraction justified	0.153	0.291	0.144	0.283
Fraction warned	0.730	0.343	0.736	0.337
Fraction warned with financial loss	0.321	0.327	0.335	0.327
Number of schools	9,457		28,350	

Notes. Data at the school level for attendance monitoring periods Feb/Mar, Apr/May, and June/July in 2008 and 2009. Politcally connected schools are those whose principal was politically appointed. *Fraction justified* is the fraction of beneficiaries (families) in the school that did not meet the attendance requirement that had their low attendance justified by the school. *Fraction warned* is the fraction of beneficiaries (families) in the school that did not meet the attendance requirement that received a warning of any type. *Fraction warned with financial loss* is the fraction of beneficiaries (families) in the school that did not meet the attendance requirement that received a warning that implies a financial loss (warning stages from 2 to 5).

Table D: Balance tests on municipal pre-treatment characteristics and discontinuity checks on mayoral characteristics

	Panel A: Pre-treatment municipal characteristics							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Population	Income	Urbanization	Radio	Literacy rate	Electricity	Sewer	Water
first	628.278	0.747	-0.016	-0.013	-0.027	-0.007	0.001	-0.001
	-4,115	(13.746)	(0.034)	(0.037)	(0.042)	(0.025)	(0.020)	(0.052)
Observations	979	979	979	979	979	979	979	979
R-squared	0.006	0.005	0.011	0.013	0.020	0.004	0.007	0.004
	Panel B: Mayoral characteristics							
	high school	college	blue	president party	coalition party			
first	-0.037	0.045	0.002	0.014	0.045			
	(0.073)	(0.076)	(0.003)	(0.044)	(0.071)			
Observations	979	979	979	979	979			
R-squared	0.006	0.003	0.001	0.035	0.018			

Notes. Results are displayed for regressions as in equation (2). *Population* denotes the number of inhabitants. *Literacy rate* denotes the fraction of people above age 20 who are literate. *Income* denotes per-capita income in 2000 in Brazilian *reais*. *Urbanization* denotes the fraction of people living in urban areas. *Electricity* denotes the fraction of houses with access to electricity. *Sewer* denotes the fraction of houses linked to the sewerage system. *Water* denotes the fraction of houses linked to the water system. *Radio* equals one if there is at least one local radio station in the municipality. *Higher Education* equals to one if the mayor has at least high school degree. *College* equals to one if the mayor has at least college degree. *Blue* equals to one if the mayor has previous experience in blue collar jobs. *President's party* equal sto one if the mayor is affiliated with PT. *President's coalition* equals to one if the mayor is affiliated with a party belonging to the president's coalition.