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 both a difference-in-differences approach and a regression discontinuity design, we find that enforcement of BFP requirements is weaker around the time of elections in municipalities with politically-aligned mayors that can run for reelection. 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. These conditions include, for example, scheduling prenatal checkups, regular school attendance of children, 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 (1) analyze whether voters respond to the enforcement of program rules and (2) study whether local authorities manipulate this enforcement, particularly when they face stronger electoral incentives. Moreover, we provide suggestive evidence on the possible channels through which local authorities may manipulate program enforcement.

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

[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.

with the first part of this argument, several papers find an effect of targeted transfers on 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 and De La O 2013 on Mexico; Manacorda, Miguel and Vigorito 2010 on Uruguay, and Zucco 2013 on Brazil). Despite these findings, there is little evidence on whether politicians manipulate the degree of enforcement, and in particular no research on whether the enforcement of CCT "conditionalities" affects electoral outcomes. 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 could retaliate by not voting for incumbents), and thus politicians may have incentives to manipulate enforcement. This could reduce the effectiveness of these programs in increasing school enrolment and/or improving health outcomes.[4] 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.[5]

We analyze whether politicians manipulate the enforcement of CCT program "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% 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 enforced through a system of "warnings" which gradually increase in their intensity. noncompliance with program requirements initially leads only to a notification, but

[4]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.

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

[6]Benefits are conditional 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).

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 on noncompliant families, and it is conceivable that those affected may respond by not voting for incumbents. On the other hand, enforcing conditionality is part of the government's role, 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 out 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 the Bolsa Familia Program affects electoral outcomes, focusing on mayoral elections in 2008. The choice of this election is dictated by the fact that enforcement efforts by the Brazilian government significantly increased starting in 2006. Identifying the effects of the enforcement of program rules on voting behaviour 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 penalised after the elections.[7]

In principle, 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, 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. Our results suggest that beneficiaries associate the enforcement of the Bolsa Familia program with the national government, as we find that the vote share of candidates from PT and its coalition is lower in areas where a higher fraction of the beneficiaries in noncompliance received penalties before the elections. In contrast, 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.

After showing that the enforcement of Bolsa Familia conditionalities reduces voter support for mayoral candidates from parties in the presidential coalition, we analyze whether these mayors manipulate the enforcement of program requirements around the time of elections to try to improve their electoral performance. Despite the potential electoral costs of enforcing program requirements, it is not clear whether mayors aligned with the presidential coalition would actually attempt to manipulate enforcement. These mayors may have stronger incentives to enforce the BFP rules, for instance because the success of the program is viewed as important component of the president's social policies or for other reasons (e.g., the reputational costs of low compliance with program requirements may be stronger). Even if this is the case, we would expect that, among the mayors that belong to the presidential coalition, those who face higher electoral incentives may have greater incentives to manipulate enforcement. To examine whether this is the case, we focus on the possibility of running for re-election 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 that belong to the presidential coalition.

We test whether mayors that belong to the presidential coalition manipulate the en-

[7]This identification strategy assumes that voters in noncompliance do not perfectly anticipate the enforcement of program rules, and as a result learning about the actual enforcement of these rules has an effect. Brollo et al. (2014) present evidence in this respect.

forcement of program requirements around the time of elections, using both a Difference in Difference (DID) approach and a Regression Discontinuity (RD) design. In the DID approach, we compare the enforcement of program conditionalites between municipalities with mayors that are politically aligned with the presidential coalition and municipalities with non-aligned mayors, before and after the elections. To address concerns that the political alignment of the mayor could be correlated with time-varying factors that might affect program enforcement, we use an RD design (Lee 2008), comparing municipalities where aligned candidates barely won the previous election with municipalities where aligned candidates barely lost.

We consider two different variables that might capture the enforcement of BFP rules: (i) whether beneficiaries (families) that did not meet the attendance requirement had their low attendance "justified" by the school principal (which would imply no penalties), (ii) whether beneficiaries (families) that failed to meet the attendance requirement are penalized or not.

Using both the DID and RD approaches, we find no significant differences in the enforcement of the rules around the election time between municipalities with politically-aligned and non-aligned mayors. As discussed above, we also test whether local electoral incentives affect the level of enforcement by politically-aligned mayors, by exploiting differences between first and second term mayors. Using both the DID and RD approaches, we find evidence that enforcement of BFP requirements is weaker around the time of elections in municipalities with politically-aligned mayors that can run for reelection, consistent with the argument that local electoral incentives may lead mayors to manipulate enforcement.

Finally, we investigate a possible mechanism for the manipulation of program enforcement. In particular, 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 manipulate justifications in this way 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. Our results suggest that attendance justifications might be a relevant channel for the political manipulation of program enforcement, as the fraction of beneficiaries with low school attendance that receive a justification is higher in schools with politically connected principals before the elections.

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 a possible mechanism for the manipulation of the enforcement of the rules. 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, (*Número de Identificação Social* - NIS).

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 vaccination 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 heath "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 occurs as follows: the ministry of Social Development (MDS) feeds the "conditionalities" system (SISCON) 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 through "Sistema Presença". 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 at 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 ("Sistema Presença") 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 municipal administration can decide to allow a family back sooner).

Program beneficiaries are well informed about "conditionalities" and punishments in case of 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 year 2005 up to 2008.

2.3 Data

We make use of a unique dataset that we assembled combining many different sources. First,we use administrative data from the Brazilian Ministry of Social Development (MDS) on the Bolsa Familia program to construct a dataset containing the following information for each beneficiary family: monthly school attendance of each child during years 2008 and 2009 (in particular, for those below the 85 percent attendance threshold, we know the exact fraction of hours attended, and 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 (INEP) 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 from the Brazilian Electoral Commission, *Tribunal Superior Eleitoral* (TSE).

Summary statistics for our variables of interest regarding Bolsa Familia at the municipality level are reported in Table 1. We have data for 5,053 municipalities for 6 attendance monitoring periods (July, October, November 2008 and July, September, and November 2009). On average there are 1,258 families that receive benefits from Bolsa Familia per municipality in each period, which represents about 8.7 percent of the electorate. In terms of compliance with program conditions, on average about 7 percent of families that participate in the program failed to meet the attendance requirements (*Fail/BFP families*). Regarding enforcement, out of all beneficiaries that did not comply with attendance requirements, on average 28 percent had their non-attendance justified by the schools (*Fraction justified*), 53 percent received any type of warnings (*Fraction warned*), and 23 percent received warnings that imply financial losses (*Fraction with financial losses*).

[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 point of view it is not clear whether and how voters would respond to a strict enforcement of the program rules. In the first place, voters could react negatively or positively to the enforcement of the program. Voters that lose their benefits due to noncompliance (or receive a notification that they might lose their benefits in the future) may punish incumbents at the polls. In addition, beneficiaries may talk among themselves. If that is the case, there could be an effect not only on those that are directly affected by the enforcement of program rules, but also on their peers who comply with the rules. Those in compliance may like or dislike strict enforcement of the program. So whether the enforcement of the program has a positive or negative effect on electoral outcomes is an empirical question. In the second place, as discussed above, it is not clear which party or candidates would be affected by voters' reaction to the enforcement of the rules. Voters that receive any warnings, may punish incumbent mayors, 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 or the presidential coalition. To try to distinguish between these two possibilities, in our empirical analysis we consider two sets of dependents variables: the vote share of the incumbent mayor (or party) and the vote share of the candidate of the presidential party or coalition. In principle the enforcement of program rules could also affect turnout (and not just vote shares), but we find no effects for turnout.

Identifying the effects of the enforcement of program rules on voting behaviour 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 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 beneficiaries with last digit of their NIS from 1 to 5 that did not comply with attendance requirements received notifications or 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 any notifications or penalties before the elections, with zip codes where a higher fraction were penalised after the elections.[8]

We construct our enforcement variables considering, alternatively, all program warnings including notifications and benefits postponement or cancellations (*Fraction warned*), or only those warnings that imply some financial loss for beneficiaries (*Fraction warned with financial loss*). We scale the beneficiaries that received a warning (before and after the elections) by the number of beneficiaries that did not comply with attendance requirements, since these are the ones who should receive warnings.[9] With our strategy we ensure that our results are not driven by differences across zip codes in terms of the level of compliance with program requirements.

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

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

where Y_z is, alternatively, the vote share of the incumbent mayor (or party), or the vote

[8]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 due to random factors.

[9]Penalties for not meeting the attendance requirements are released a few months after the monitoring period. The lag between the monitoring period and the release of warnings has changed over time, as the system for monitoring attendance and enforcing "conditionalities" has become more efficient. According to the monitoring attendance calendar there were five monitoring periods during year 2008: February-March; April-May; June-July, August-September; and October-November. According to the data and "conditionalities" enforcement calendar provided by the MDS, low attendance reported during the monitoring period of February and March 2008, resulted in warnings during the July 2008 payroll; and low attendance reported during April and May 2008 resulted in warnings during October 2008.

share of the candidate of the presidential party or coalition, in zip code area z; $fraction_z$ is the number of families that get a warning in October 2008 over the number of families 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 up to 5) over the number of families that did not comply with the attendance requirements in the corresponding monitoring period; X_z is the number of families (beneficiaries) over the electorate. θ_i denotes municipality fixed effects. Standard errors are clustered at the municipality level. The coefficient of interest is β, which captures whether the vote share is different in zip codes (within a given municipality) where a higher fraction of non-compliant beneficiaries received notifications or penalties before the elections, compared to zip codes where a higher fraction were penalised after the elections.[10] Tables 2 and 3 report the results of these estimations.

[Insert table 2]

Table 2 presents results analyzing the vote share of mayoral candidates belonging to the president's party or coalition. In Panel A the numerator of the variable *fraction*, in equation 1, is the number of beneficiaries (families) that received any warnings (regardless of their warning stage) in October 2008. In Panel B the numerator of the variable *fraction* is the number of beneficiaries (families) that get warnings that imply financial losses (warning stages from 2 to 5) in October 2008. We use these two alternative variables to measure the enforcement of the program since it is possible that voters only respond to the enforcement of program rules when they suffer a loss of transfers. Regressions in columns 2 and 4 include municipality fixed effects. The dependent variable of regressions displayed in columns 1 and 2 is the vote share of the mayoral candidate affiliated with the party of the president (PT, *share PT*) and the dependent variable in columns 3 and 4 is the vote share of the mayoral candidate affiliated with the coalition of the President (*share coalition*).

Results suggest that there are political costs of the enforcement of program "conditionalities" for candidates belonging to the president's party and the presidential coalition. The vote share for these candidates is significantly lower in zip code areas where a higher fraction

[10]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.

of those beneficiaries that did not meet attendance requirements received a warning in the days before the elections. These results hold both when analyzing all warnings and only those warning stages that imply financial losses, and also when including municipality fixed effects. These findings are consistent with arguments suggesting that beneficiaries that get a warning may punish authorities at the polls and also suggests that voters associate the enforcement of BFP rules with the national government. In terms of economic magnitude, the estimates in column 6 imply that if we increase the share of non-compliant beneficiaries who received a warning before the elections ("fraction treated") by one standard deviation (0.29), this will reduce the share of votes received by the PT by 1.4 percentage points (that is about 10 percent of the standard deviation).

[Insert table 3]

Table 3 reports results analyzing the vote share of incumbent mayors (and parties). In particular, the dependent variable in columns 1 and 2 is the vote share of the incumbent mayor (in those municipalities where he/she runs for e-election); in columns 3 and 4, the vote share of the municipal incumbent party (irrespective of whether that party's candidate is the current mayor or not); and in columns 5 and 6 the vote share of the municipal incumbent mayor, when he/she is affiliated with the PT.[11] Columns 2, 4, and 6 include municipality fixed effects. Panel A shows results using the fraction of beneficiaries that did not meet the attendance requirements that received any warnings as a measure of enforcement. Panel B considers only warnings that imply financial losses (stages 2 to 5). These results suggest that voters do not associate the enforcement of the program with the incumbent mayor.[12] These results, together with those in Table 2, suggest that voters tend to associate the enforcement of the program with the president's party and coalition, but not with incumbent mayors.

[Insert Table 4]

[11] Note that the sample of incumbent mayors affiliated with PT and coalition of the president are the same.

[12] We also tested whether the results reported in Table 2 are stronger for incumbent candidates from the PT/coalition. We run regressions where the dependent variable is the vote share of the PT/coalition and we include on equation (1) an interaction term between "fraction treated" and a dummy variable that is equal to one if the incumbent is affiliated with PT/coalition, The results show that, while voters punish candidates from the PT/coalition, there is no significant difference if they are incumbents or not (the interaction is not significant).

Finally, we conduct a placebo test considering warnings received in November 2008, after the elections were conducted. Warnings received by beneficiaries with last-digit NIS from 1 to 5 after the elections should have no effect on electoral outcomes. The results of these estimations are reported in Table 4. We find no significant difference in electoral outcomes between zip codes with a higher-fraction of non-compliant beneficiaries that received warnings in November with last-digit NIS from 1 to 5, and those with higher fractions of non-compliant beneficiaries with last-digit NIS above 5.

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

This section analyzes whether politically-aligned mayors take into account the negative response by voters to the enforcement of program rules that we document in Section 3, and manipulate enforcement before elections to try to improve their electoral performance. As we mention above, it is not clear whether these mayors would manipulate enforcement. These mayors may actually have stronger incentives to enforce the BFP rules, for instance, because the success of the program is viewed as important component of the president's social policies or for other reasons (e.g., the central government might monitor more closely municipalities where the mayor is affiliated with the presidential coalition). Even if this is the case, we would expect that, among the mayors that belong to the presidential coalition, those who face higher electoral incentives may have greater incentives to manipulate enforcement.

Identifying the effects of politically aligned mayors on enforcement is not a trivial task. A comparison between municipalities with an aligned mayor and those with a non-aligned mayor 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 the political affiliation of the mayor. To examine whether politically-aligned mayors became soft in the enforcement of the program "conditionalities" before the elections, we start the analysis by estimating these effects using a difference in difference approach, which controls for unobservable municipal characteristics that are constant over time. We estimate the difference in difference model as follows:

$$
\begin{aligned}
Y_{im} = \alpha + \beta_1 before_m + \beta_2 before_m * coalition_i \\
+ \gamma coalition_i + \theta_i + \varsigma_m + \delta_y + \varepsilon_{im},
\end{aligned} \tag{2}
$$

where Y_{im} is our outcome of interest that captures the enforcement of the program "conditionalities" in municipality i and month m. $before_m$ is a dummy variable that equals to one for months before the election and zero otherwise; $coalition_i$ equals one if the party of the mayor is part of the presidential coalition; θ_i denotes municipality fixed effects; ς_m is a linear time trend; and δ_y year fixed effects. Robust standard errors ε_{im} are clustered at municipality level. We estimate this regression considering all municipalities with available data for the period July 2008 to November 2009. Note that warnings are not released every month, but rather every few months. So we only consider observations for those months when penalties were released. During our sample period, warnings were released on July 2008, October 2008, November 2008, July 2009, September 2009, and November 2009. The before dummy in equation above equals one for July and October 2008, and zero otherwise.

We consider three different measures of the enforcement of program rules: (i) whether beneficiaries (families) that failed to attend school have their low attendance justified by the school principal (which would exempt them from getting a warning), (ii) whether beneficiaries (families) that failed to attend school actually receive any warnings, (iii) whether beneficiaries (families) that failed to attend school actually receive a warning that implies in financial loss (warning stages 2-5).

Therefore, we use three alternative dependent variables for this analysis: "*fraction justified*" is the number of families with low attendance justified over the number of families that did not comply with the attendance requirements; "*fraction warned*" is the number of families that get a warning over the number of families that did not comply with the attendance requirements; and "*fraction warned with financial loss*" is the number of families that get a warning that implies in financial loss over the number of families that did not comply with the attendance requirements.

[Insert Table 5]

Results from these estimations are reported in Table 5. In particular, the dependent variable in columns 1 and 2 is the "*fraction justified*"; in columns 3 and 4 the "*fraction* *warned*"; and in columns 5 and 6 "*fraction warned with financial loss*". All regressions include year fixed effects and a linear time trend. Regressions in columns 2, 4, and 6 include municipality fixed effects. Panel A reports results of regressions of our dependent variables on the before dummy, the linear time trends, and the different sets of fixed effects, to analyze the correlation between enforcement and the timing of the municipal elections in October 2008. For all specifications we find that enforcement is less stringent in the period before the municipal elections. Panel B report the results difference in difference estimates as in equation 3 to analyze whether mayors that are politically aligned with the president manipulate enforcement before the elections. We find no evidence that this is the case, as the coefficient on "*CoalitionXBefore*" is not statistically significant in any specification.

However, as discussed above, among the mayors that belong to the presidential coalition, those who face higher electoral incentives may have greater incentives to manipulate enforcement. To examine whether this is the case, we focus on the possibility of running for re-election 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 that belong to the presidential coalition. Following this idea, we estimate the following equation:

$$\begin{aligned} Y_{im} = \alpha + \beta_1 before_m + \beta_2 before_m * coalition_i + \beta_3 before_m * coalition_i * first_i \\ + \zeta before_m * first_i + \gamma coalition_i + \theta_i + \varsigma_m + \delta_y + \varepsilon_{im}, \end{aligned} \tag{3}$$

where Y_{im} is our outcome of interested that captures the enforcement of the "conditionalities" in municipality i and month m. $before_m$ is a dummy variable that equals to one for months before the election and zero otherwise; $coalition_i$ equals one if the party of the mayor is part of the presidential coalition; $first_i$ equals one if the mayor is eligible to run for a consecutive municipal mandate (first-term mayor); θ_i denotes municipality fixed effects; ς_m is a linear time trend; and δ_y year fixed effects. Robust standard errors ε_{im} are clustered at municipality level.

Our coefficient of interest is β_3, which will capture whether enforcement before the elections is different for mayors affiliated with the coalition that can run for re-election.

The results are reported in Panel C of Table 5. The coefficient of the triple interaction term is positive and significant in columns 1 and 2, where the dependent variable is "*frac-*

tion justified". These results suggest that municipalities with politically aligned mayors that face re-election incentives, have higher justifications before the elections. First-term mayors aligned with the coalition justify around 4 percentage points more than politically-aligned second term mayors before the elections (this is about 14 percent of the mean fraction justified of second term mayors; 32 percent). Note that the coefficient for the interaction term "*Coalition*Before*" is negative and significant, which suggest that in those municipalities with politically-aligned mayors that do not have re-election incentives "*fraction justified*" is lower. The coefficient of the triple interaction is not significant for the other measures of manipulation ("*fraction warned*" and "*fraction warned with financial loss*").

The difference-in-difference estimations reported above control for unobservable municipal characteristics that are constant over time. However, this approach does not control for time varying unobservable factors, which might be correlated both with enforcement and with the political alignment of the mayor. To address the presence of both time-invariant and time-varying confounding factors, we implement a regression discontinuity design in the spirit of Lee (2008) and compare municipalities where a politically-aligned candidate barely won the mayoral elections with municipalities where the aligned candidate barely lost. Our estimation strategy follows Brollo and Nannicini (2012) who use RD design in close races to analyze the effects of political alignment of mayors on intergovernmental transfers in Brazil.[13] Specifically, we calculate the margin of victory of the mayoral candidate aligned with the President in each municipality i (MVP_i): at the threshold $MVP_i = 0$, political alignment P_{it} sharply changes from zero to one.[14]

MVP_i is viewed as a random variable depending on both observable and unobservable political factors, as well as on random events on election day. The standard RDD assumption is that potential outcomes must be a continuous function of the running variable at the threshold. Electoral outcomes depend on both predictable elements and random chance, but the latter is crucial only when the race is close. To test this assumption, we formally test the continuity of the density of the margin of victory, following McCrary (2008). The results are reported in Appendix Figures A and B. Figure A reports results for all mayors in our sample

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

[14]We consider the mayoral elections held in 2004 to calculate the margin of victory.

and Figure B reports results for first-term mayors only. In both cases, we find no evidence of discontinuities in the margin of victory of candidates aligned with the President's party.

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 $MVP_i = 0$. To test this, we checked whether a vast array of observable municipal pre-treatment characteristics, including geographic location of the city, income, and population, are balanced around the cut-off. These balance tests show that all these characteristics are balanced around the cut-off.

The ATE in close elections is thus:

$$E[\tau_i(1) - \tau_i(0)|MVP_i = 0] = \lim_{\epsilon \downarrow 0} E[\tau_i|MVP_i = \epsilon] - \lim_{\epsilon \uparrow 0} E[\tau_i|MVP_i = \epsilon]. \tag{4}$$

τ is defined as a local effect, because it captures the impact of political alignment on the outcome only for municipalities around the threshold $MVP = 0$ (i.e. for the elections that were decided for a margin that is tiny enough).[15]

We estimate the ATE expressed in equation (4) fitting a *third* order polynomial in MV_i on either side of the threshold $MV_i = 0$:

$$\tau_i = \sum_{k=0}^{3}(\rho_k MV_i^k) + P_{it}\sum_{k=0}^{3}(\pi_k MV_i^k) + \varepsilon_i, \tag{5}$$

where MV_i is the margin of victory in municipality i and standard errors are clustered at the city level. The estimated coefficient $\hat{\pi}_0$ identifies the ATE at the threshold $MV_i = 0$.[16]

[15] 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. Furthermore, this criticism of the close-race RD design does not directly apply to our setup. The validity of our identification strategy is not affected by the possibility that incumbents are more successful in close races, as long as incumbents aligned with the coalition of President have the same probability of running for reelection as incumbents from the opposition coalition. The richness of our dataset allows to show that a wide array of pre-treatment covariates are similar between municipalities where a candidate politically aligned with coalition of the President barely won an election against a non-aligned candidate with municipalities where the opposite occurred, supporting the validity of our research design in the Brazilian setting. Furthermore, 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.

[16] We follow the standard procedure of fitting a third order polynomial. We computed our results with lower or higher order polynomials (i.e. identifying the effect on observations respectively farther and closer to the threshold) and our results are robust to different specifications. Results are available upon request.

[Insert Table 6]

Table 6 reports the results for regressions in equation (5). Panel A consider races with only two-candidates where one is aligned with the party/coalition of the president and the other is not. Regressions in Panel B consider a broader sample, including also races with more than two candidates. The dependent variable in columns 1 and 4 is the "*fraction justified*"; in columns 2 and 5 the "*fraction warned*" and in columns 3 and 6 "*fraction warned with financial loss*".[17] Consistent with the results in Table 5, we find no significant effects of political alignment on enforcement.

As we mention above, politically-aligned mayors may have more incentives to manipulate the enforcement of the program when they are allowed to run for reelection.To test this hypothesis, we analyze whether the treatment effect differs between mayors that are allowed to run for reelections and those that cannot run. In particular, we estimate the following regression:

$$\begin{aligned}\tau_i = &\sum_{k=0}^{p}(\rho_k MVP_i^k) + P_i\sum_{k=0}^{p}(\pi_k MVP_i^k) + \\ &+D_i \cdot \Big[\sum_{k=0}^{p}(\alpha_k MVP_i^k) + P_i\sum_{k=0}^{p}(\beta_k MVP_i^k)\Big] + \xi_i.\end{aligned} \tag{6}$$

where D_i equals one if the incumbent mayor is eligible to run for re-election, and zero otherwise. $\hat{\beta}_0$ is the difference in the treatment effect between mayors who are eligible for reelection and those that are not. Thus, $\hat{\pi}_0$ identifies the treatment effect for mayors that cannot run for re-election ($D_i = 0$) and $\hat{\pi}_0 + \hat{\beta}_0$ for those that can ($D_i = 1$). Even if the difference between the two sub-samples should not be interpreted as causal, stronger treatment effects for mayors that are eligible for reelection would suggest that politically-aligned mayors are more likely to manipulate program enforcement when they face local electoral incentives to do so.

[Insert Table 7]

[17] We take the mean of the dependent variable for two months before the elections (July and October 2008).

The results from this estimation are reported in Table 7. Panel A consider the races with only two-candidates where one is aligned with the coalition of the president and the other is not. Regressions in Panel B consider a broader sample, including also races with more than two candidates. The dependent variable in column 1 is the "*fraction justified*"; in column 2 "*fraction warned*" and in column 3 "*fraction warned with financial loss*". In municipalities with politically-aligned mayors that face re-election incentives, the fraction of beneficiaries that missed attendance and were justified is higher before the elections. First term mayors aligned with the presidential coalition justify around 20 percentage points more than politically aligned second term mayors (this is about 59 percent of the mean fraction justified of second term mayors; 34 percent). We also find that the fraction of beneficiaries that do not comply with attendance requirements and actually get a warnings is lower in municipalities with politically-aligned mayors that face re-election incentives, consistent with the ideas that these mayors have more incentives to manipulate program enforcement to try to reduce any potential electoral costs of program enforcement.

5 How do politicians manipulate enforcement?

This section explores 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 noncompliances. Therefore, politically-motivated 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 estimate the following equation:

$$Y_{sm} = \alpha + \beta_1 before_m + \beta_2 before_m * pol_s + \theta_s + \nu_m + \delta_t + \varepsilon_{sm}, \quad (7)$$

where Y_{sm} is our outcome of interested that captures the enforcement of the "conditionalities" in school s and month m. $before_m$ is a dummy variable that equals to one for two months before the election and zero otherwise; pol_s equals one if the school principal was politically appointed, our proxy for political connection between the mayor and the school's director; θ_s denotes school fixed effects; ν_m is month fixed effects; and δ_t year fixed effects. Robust standard errors ε_{sm} are clustered at the school level.

[Insert Table 8]

Table 8 report the results. The results suggest that politically-connected school directors tend to manipulate justifications before the elections. Politically-connected schools justify *0.9* percentage point*s* more before the elections, which is six percent of the mean fraction justified (15 percent). This finding suggests that one potential channel *through* which the enforcement of the BFP rules may be manipulated is through increases in justifications before the election, particularly by politically-motivated school principals.

6 Conclusion

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 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 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, we study whether local authorities manipulate this enforcement, particularly when they face stronger electoral incentives. Using both a difference-in-differences approach and a regression discontinuity design, we find that enforcement of BFP requirements is weaker around the time of elections in municipalities with politically-aligned mayors that can run for reelection. Moreover, 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.

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 enrolment 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.

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Table 1: Summary Statistics at the Municipality Level

	mean	Std. Dev.	Obs
	(1)	(2)	(3)
Number of BFP families	1,258	2,738	30,318
Families that failed attendance requirement (fail)	64	243	30,318
Fraction justified (justified/fail)	0.280	0.285	30,318
Fraction warned (warned/fail)	0.534	0.295	30,318
Fraction warned with financial loss (warned loss/fail)	0.225	0.226	30,318
Fail/BFP families	0.067	0.080	30,318
Fail/electorate	0.004	0.007	30,318
BFP families/electorate	0.087	0.058	30,318

Notes. Data at the municipality level for attendance monitoring periods July, October, and November 2008 and July, September, and November 2009. *Fraction justified* is the fraction of beneficiaries (families) that did not meet the attendance requirement during the monitoring month that had their low attendance justified by the school. *Fraction warned* is the fraction of beneficiaries (families) that did not meet the attendance requirement during the monitoring month 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 during the monitoring month that received a warning that implies a financial loss according to their wanting stage (warning stages from 2 to 5). *Fail/BFP* is the fraction of beneficiaries (families) that did not meet the attendance requirement during the monitoring month. *Fail/electorate* is the number of beneficiaries (families) that did not meet the attendance requirement during the monitoring month divided by the electorate. *BFP families/electorate* is the number of beneficiaries (families) divided by the electorate.

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

	(1)	(2)	(3)	(4)
		Panel A: Fraction warn/fail		
	share	share	share	share
	PT	PT	coalition	coalition
	(1)	(2)	(3)	(4)
Fraction treated	-0.054***	-0.052***	-0.034**	-0.026*
	(0.019)	(0.017)	(0.016)	(0.014)
Fraction	0.014	-0.010	-0.003	-0.023**
	(0.014)	(0.014)	(0.012)	(0.011)
BFP/electorate	0.170*	0.034	0.123	-0.006
	(0.102)	(0.067)	(0.100)	(0.061)
hline Observations	1,150	1,150	1,415	1,415
R-squared	0.009	0.149	0.006	0.145
		Panel B: Fraction warn loss/fail		
Fraction treated	-0.097***	-0.092***	-0.064***	-0.049**
	(0.028)	(0.024)	(0.023)	(0.021)
Fraction 0.022	-0.004	0.010	-0.015	
	(0.021)	(0.020)	(0.018)	(0.016)
BFP/electorate	0.171*	0.041	0.116	-0.003
	(0.102)	(0.067)	(0.100)	(0.060)
Observations	1,150	1,150	1,415	1,415
R-squared	0.014	0.154	0.008	0.145
Municipality FE	no	yes	no	yes

Notes. Results are displayed for regressions as in equation 1. Panel A display the results for regressions where the fraction of beneficiaries (families) that get a warning divided by those with low attendance during the monitoring period, regardless their warning stage. Panel B display the results for regressions where the fraction of beneficiaries (families) that get a warn divided by those with low attendance during the monitoring period and suffer financial loss according to their warning stage (warning stage from 2 to 5). *Fraction* denotes the fraction of beneficiaries with missed attendance. *Fraction treated* denotes the fraction of beneficiaries warned in the week days before the elections with missed attendance. *BFP/electorate* is the number of families (beneficiaries) over electorate. The dependent variable in regressions reported in columns 1 and 2 is the vote share of the PT candidate and the dependent variable in columns 3 and 4 is the vote share of the candidate affiliated with the coalition of the President. Regressions in columns 2 and 4 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: Do Voters Respond to the Enforcement of Program Conditionalities? (Incumbents)

	(1)	(2)	(3)	(4)	(5)	(6)
	share inc	share inc	share inc party	share inc party	share inc PT	share inc PT
Panel A: Fraction warned						
Fraction treated	-0.009	0.004	-0.010	0.001	-0.046	-0.044
	(0.019)	(0.016)	(0.015)	(0.012)	(0.034)	(0.031)
Fraction	-0.013	-0.027*	-0.006	-0.010	-0.024	-0.012
	(0.017)	(0.016)	(0.012)	(0.010)	(0.025)	(0.025)
BFP/electorate	-0.138	-0.061	-0.109	-0.031	0.965**	0.791**
	(0.114)	(0.079)	(0.100)	(0.069)	(0.402)	(0.379)
Observations	320	320	653	653	128	128
R-squared	0.015	0.376	0.006	0.483	0.058	0.126
Panel B: Fraction warned with financial loss						
Fraction treated	-0.025	0.036	-0.034	0.007	-0.099*	-0.081
	(0.036)	(0.034)	(0.028)	(0.021)	(0.059)	(0.050)
Fraction	-0.020	-0.037	-0.024	-0.018	-0.010	0.000
	(0.029)	(0.032)	(0.022)	(0.018)	(0.034)	(0.033)
BFP/electorate	-0.139	-0.055	-0.113	-0.029	1.065***	0.881**
	(0.115)	(0.080)	(0.100)	(0.069)	(0.400)	(0.369)
Observations	320	320	653	653	128	128
R-squared	0.017	0.373	0.017	0.484	0.055	0.120
Municipality FE	no	yes	no	yes	no	yes

Notes. Results are displayed for regressions as in equation 1. *Fraction* denotes the fraction of beneficiaries with missed attendance. *Fraction treated* denotes the fraction of beneficiaries warned in the week days before the elections with missed attendance. *BFP/electorate* is the number of families (beneficiaries) over electorate. 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 vote share of the municipal incumbent party. in Columns 5 and 6 the vote share of the municipal incumbent mayor. Panel A display the results for regressions where the fraction of beneficiaries (families) that get any warning divided by those with low attendance during the monitoring period, regardless their warning stage. Panel B display the results for regressions where the fraction of beneficiaries (families) that get a warning, and suffer financial loss according to their warning stage (warning stage from 2 to 5, divided by those with low attendance during the monitoring period. Regressions in columns 2, 4, 6 and 8 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: Placebo (November): Do Voters Respond to the Enforcement of Program Conditionalities?

	(1)	(2)	(3)	(4)	(5)
	Panel A: Fraction: warn/fail				
	share PT	share coalition	share inc PT	share inc party	share inc
Fraction treated	0.004	-0.006	0.034*	0.011	-0.001
	(0.011)	(0.010)	(0.019)	(0.011)	(0.015)
Fraction	-0.004	0.000	0.007	0.040***	0.036**
	(0.014)	(0.011)	(0.036)	(0.014)	(0.018)
BFP/electorate	0.028	-0.018	0.878**	-0.114	-0.179*
	(0.057)	(0.052)	(0.375)	(0.091)	(0.103)
Observations	570	894	164	798	407
R-squared	0.416	0.336	0.044	0.022	0.028
	Panel B: Fraction: warn loss/fail				
	share PT	share coalition	share inc PT	share inc party	share inc
Fraction treated	-0.010	-0.015	0.001	-0.001	0.021
	(0.010)	(0.010)	(0.015)	(0.008)	(0.013)
Fraction	0.002	0.009	-0.005	0.003	0.000
	(0.009)	(0.008)	(0.015)	(0.007)	(0.010)
BFP/electorate	0.029	-0.016	0.775**	-0.081	-0.099
	(0.057)	(0.052)	(0.353)	(0.056)	(0.063)
Observations	570	894	164	798	407
R-squared	0.417	0.337	0.113	0.462	0.365
Municipality FE	yes	yes	yes	yes	

Notes. Results are displayed for regressions as in equation 1. Warnings received in Nov. 2008 (month after election) by beneficiaries w/ last digit NIS 1-5 do not affect electoral shares. Panel A display the results for regressions where the fraction of beneficiaries (families) that get a warn divided by those with low attendance during the monitoring period, regardless their warning stage. Panel B display the results for regressions where the fraction of beneficiaries (families) that get a warn divided by those with low attendance during the monitoring period and suffer financial loss according to their warning stage (warning stage from 2 to 5). *Fraction* denotes the fraction of beneficiaries with missed attendance. *Fraction treated* denotes the fraction of beneficiaries with missed attendance, with last digit NIS from 1 to 5, warned in November. *BFP/electorate* is the number of families (beneficiaries) over electorate. The dependent variable in regressions reported in column 1 is the vote share of the PT candidate. In column 2 the dependent variable is the vote share of the candidate affiliated with the coalition of the President. In column 3 the dependent variable is the vote share of the candidate affiliated with the incumbent when the incumbent is affiliated with PT. In column 4 the dependent variable is the vote share of the candidate affiliated with incumbent party. In column 5 the dependent variable is the vote share of the incumbent. All regressions in this table include municipality fixed effects. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 5: The Effects of Political Alignment on Enforcement, DID Estimates

	fraction justified		fraction warned		fraction warned with financial loss	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Simple difference estimates						
Before	0.0877***	0.0841***	-0.176***	-0.177***	-0.184***	-0.186***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)
Observations	30,318	30,318	30,318	30,318	30,318	30,318
R-squared	0.041	0.522	0.032	0.423	0.058	0.446
Municipality FE	no	yes	no	yes	no	yes
Panel B: Difference in difference estimates						
Coalition	0.009		0.006		0.012***	
	(0.006)		(0.006)		(0.005)	
CoalitionxBefore	-0.002	0.001	-0.009	-0.010	-0.010*	-0.008
	(0.007)	(0.008)	(0.008)	(0.008)	(0.005)	(0.006)
Before	0.088***	0.084***	-0.173***	-0.174***	-0.181***	-0.183***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.005)
Observations	30,318	30,318	30,318	30,318	30,318	30,318
R-squared	0.041	0.522	0.032	0.423	0.059	0.446
Panel C: DID estimates – Heterogeneous effects, First term mayors						
CoalitionxBeforexFirst	0.056***	0.044**	-0.040**	-0.026	0.010	0.016
	(0.018)	(0.019)	(0.019)	(0.021)	(0.014)	(0.015)
CoalitionxFirst	0.000		-0.003		-0.011	
	(0.014)		(0.015)		(0.012)	
CoalitionxBefore	-0.047***	-0.035**	0.023	0.011	-0.018	-0.021
	(0.016)	(0.017)	(0.017)	(0.018)	(0.012)	(0.014)
BeforexFirst	-0.001	-0.001	0.010	0.011	-0.002	0.000
	(0.010)	(0.011)	(0.010)	(0.011)	(0.007)	(0.008)
Coalition	0.009		0.009		0.022**	
	(0.013)		(0.013)		(0.011)	
First	0.001		-0.011		-0.012**	
	(0.008)		(0.008)		(0.006)	
Before	0.089***	0.085***	-0.181***	-0.182***	-0.179***	-0.183***
	(0.009)	(0.010)	(0.010)	(0.010)	(0.007)	(0.008)
Observations	30,318	30,318	30,318	30,318	30,318	30,318
R-squared	0.042	0.522	0.033	0.423	0.060	0.446
Months linear trend	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Municipality FE	no	yes	no	yes	no	yes

Notes. Results are displayed for regressions as in equation 3. Panel A reports the results when the triple interaction is not included. Panel B displays the difference-in-difference estimates: Panel C displays the difference-in-difference estimates with heterogeneous effects – whether the mayor is eligible for a consecutive mandate (first-term mayors).
Columns 1 and 2 report the results of regressions that consider as dependent variable the fraction of families that missed attendance during the reference period and get a warning. Columns 3 and 4 report the results of regressions that consider as dependent variable the fraction of families that missed attendance during the reference period and get a warning. Columns 5 and 6 report the results of regressions that consider as dependent variable the fraction of families that missed attendance during the reference period and get a warning, incurring in financial loss (warning stage 2-5). All regressions include year fixed effects and month linear trend. Regressions displayed in columns 2, 4, and 6 include municipality fixed effects. Robust standard errors clustered at the municipality level in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 6: The impact of political alignment on enforcement, RD estimates

Specification:	OLS	OLS	OLS	RDD	RDD	RDD
Dep. Variable:	*fraction justified*	*fraction warned*	*fraction warned with financial loss*	*fraction justified*	*fraction warned*	*fraction warned with financial loss*
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Sample – two-candidates races						
Coalition	0.013	-0.006	-0.020*	-0.040	0.020	-0.040
	(0.019)	(0.017)	(0.011)	(0.045)	(0.041)	(0.026)
Observations	1,128	1,128	1,128	1,128	1,128	1,128
Panel B: Sample – two-candidates broader races						
Coalition	-0.007	0.008	0.000	-0.017	0.022	0.010
	(0.014)	(0.013)	(0.008)	(0.032)	(0.030)	(0.019)
Observations	1,911	1,911	1,911	1,911	1,911	1,911

Notes. Results are displayed for OLS (columns 1, 2 and 3) and RDD 3^{rd} order spline polynomial (columns 4, 5 and 6). Panel A considers *two-candidates races*; races with only two candidates where one candidate is politically aligned with the coalition of the president and the other is not. Panel B considers *two-candidates races broad*; races with two or more candidates where one of the first two is politically aligned with the coalition of the president and the other is not. Dependent variables: *number of beneficiaries (families) who are justified divided by the number of beneficiaries (families) who fail to comply attendance*, *number of beneficiaries (families) who get a warning divided by the number of beneficiaries (families) who fail to comply attendance* and *number of beneficiaries (families) who get a warning that imply a financial loss divided by the number of beneficiaries (families) who fail to comply attendance*. RDD specifications with split polynomial as in equation (5). Robust standard errors in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 7: The impact of political alignment on enforcement, RD estimates – heterogeneous effects

Dep. Variable:	*fraction justified*	*fraction warned*	*fraction warned with financial loss*
	(1)	(2)	(3)
Panel A:		two-candidates races sample	
Coalition (π_0)	-0.166**	0.190***	0.000
	(0.084)	(0.079)	(0.051)
Coalition*First term (β_0)	0.199***	-0.242***	-0.051
	(0.097)	(0.091)	(0.059)
Observations	1,128	1,128	1,128
$\pi_0 + \beta_0$	0.032***	-0.051***	-0.051***
	(0.001)	(0.001)	(0.000)
Panel B:		two-candidates broader races sample	
Coalition (π_0)	-0.102	0.108*	-0.003
	(0.065)	(0.062)	(0.039)
Coalition*First term (β_0)	0.123*	-0.127*	0.006
	(0.074)	(0.070)	(0.044)
Observations	1,911	1,911	1,911
$\pi_0 + \beta_0$	0.021***	-0.019***	0.003***
	(0.001)	(0.001)	(0.000)

Notes. Results are displayed for RDD 3^{rd} order spline polynomial. Panel A considers *two-candidates races*; races with only two candidates where one candidate is politically aligned with the coalition of the president and the other is not. Panel B considers *two-candidates races broad*; races with two or more candidates where one of the first two is politically aligned with the coalition of the president and the other is not. Dependent variables: *number of beneficiaries (families) who are justified divided by the number of beneficiaries (families) who fail to comply attendance*, *number of beneficiaries (families) who get a warning divided by the number of beneficiaries (families) who fail to comply attendance* and *number of beneficiaries (families) who get a warning that imply a financial loss divided by the number of beneficiaries (families) who fail to comply attendance*. RDD specifications with split polynomial as in equation 6, $\hat{\pi_0}$ identifies the treatment effect in $D_i = 0$, $\hat{\pi_0} + \hat{\beta_0}$ in $D_i = 1$, and $\hat{\beta_0}$ the difference between the two. D_i equals one if the incumbent mayor is eligible to run for re-election (first term mayors). Robust standard errors in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

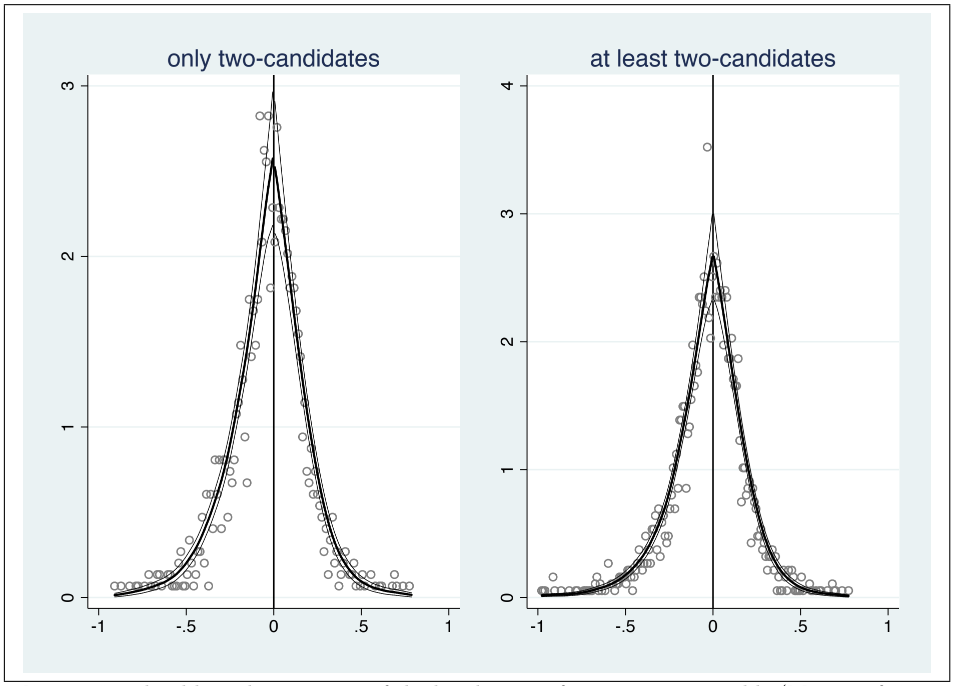
Table 8: The effects of politically connected schools on enforcement

	fraction justified			fraction warned			fraction warned with financial loss		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pol	0.032***	0.037***		-0.023***	0.001		-0.063***	-0.002	
	(0.003)	(0.003)		(0.003)	(0.003)		(0.003)	(0.003)	
Pol*Before	0.015***	0.008**	0.009**	-0.005	0.002	0.002	0.005	0.006	0.007
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.004)	(0.004)	(0.005)
Before	0.063***	0.057***	0.049***	0.049***	0.048***	0.049***	0.071***	0.064***	0.066***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Observations	158,517	158,517	158,517	158,517	158,517	158,517	158,517	158,517 1	58,517
R-squared	0.008	0.222	0.604	0.035	0.222	0.522	0.027	0.176	0.399
Months FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Municipality FE	no	yes	no	no	yes	no	no	yes	no
School FE	no	no	yes	no	no	yes	no	no	yes

Notes. Results are displayed for regressions as in equation 7. The dummy variable $before$ equals to one for months before the election and zero otherwise. Columns 1, 2 and 3 report the results of regressions that consider as dependent variable the fraction of families that missed attendance and get low attendance justified by the school principal during the monitoring period. Columns 4, 5 and 6 report the results of regressions that consider as dependent variable the fraction of families that missed attendance during the reference period and get a warning. Columns 7, 8 and 9 report the results of regressions that consider as dependent variable the fraction of families that missed attendance during the reference period and get a warning, incurring in financial loss (warning stage 2-5). All regressions include year fixed effects and month fixed effects. Regressions displayed in columns 2, 5, and 8 include municipality fixed effects. Regressions displayed in columns 3, 6, and 9 include school fixed effects. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

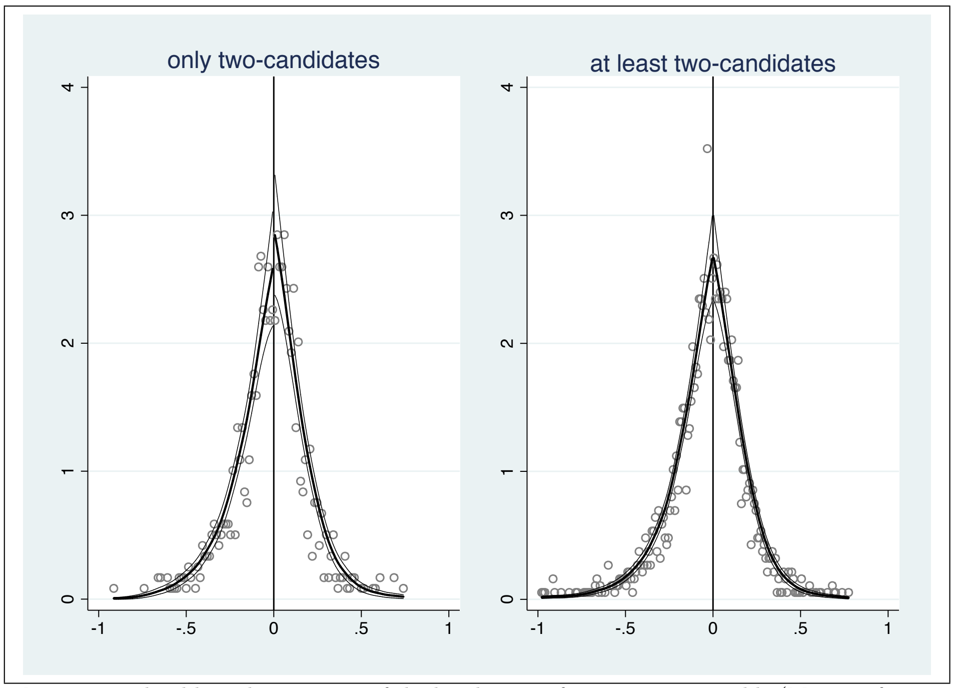
7 Appendix

Figure A: McCrary Density Test



Notes. Weighted kernel estimation of the log density of our running variable (Margin of Victory of the politically aligned candidate) performed separately on either side of the threshold $MV_{it} = 0$ $MV_{it} > 0$ when the winning candidate in the municipality i is politically aligned, $MV_{it} < 0$ when the winning candidate is is politically aligned. Left-hand side graph: races with two-candidates in 2004 elections; discontinuity estimate: point estimate -0.022 and standard error 0.116. Right-hand side graph: races with more than two-candidates in 2004 elections; discontinuity estimate: point estimate -0.001 and standard error 0.094

Figure B: McCrary Test, Two Candidates Races, First-Term Mayors



Notes. Weighted kernel estimation of the log density of our running variable (Margin of Victory of the politically aligned candidate) performed separately on either side of the threshold $MV_{it} = 0$ $MV_{it} > 0$ when the winning candidate in the municipality i is politically aligned, $MV_{it} < 0$ when the winning candidate is is politically aligned. Left-hand side graph: races with two-candidates in 2004 elections; discontinuity estimate: point estimate -0.339 and standard error 0.195. Right-hand side graph: races with more than two-candidates in 2004 elections; discontinuity estimate: point estimate 0.152 and standard error 0.104