Do Reelection Incentives Improve Policy Implementation? Accountability vs. Political Targeting*

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Abstract

Although reelection prospects can improve policy implementation by incumbents, they can also create incentives for politically-motivated targeting, which might jeopardize distributional efficiency. While existing empirical tests typically focus on these potential countervailing incentives in isolation, this paper analyzes their net effect in the context of *Bolsa Família* (BF) in Brazil, using a regression discontinuity design and data on seven million households from the program's registry. The evidence supports political targeting over accountability: mayors with reelection incentives are four times more likely to include nonpoor, ineligible households into the policy. These results cannot be explained by higher effort in indiscriminate program expansion. On the contrary, evidence from both a survey with 11,000+ households and the heterogeneity in the estimates suggests that they reflect a breakdown on the information channels that lead the excluded poor to hold local administrations accountable. On the other hand, the included nonpoor are more likely to support incumbents in their reelection attempts, as they fear losing the benefit should the administration change. Finally, anomalous income reporting patterns also show that this electorally-driven targeting is more common for households enrolled by public servants politically connected to the mayor.

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The quality of democracy depends upon the existence of institutional mechanisms that allow voters to keep politicians in check. The prospect of reelection has been singled out as an effective disciplining device by the literature on political accountability (Barro, 1973; Ferejohn, 1986; Besley and Case, 1995; Persson and Tabellini, 2000; Alt, de Mesquita, and Rose, 2011; Ferraz and Finan, 2011; Ashworth, 2012; Duggan and Martinelli, 2017). The simple version of the argument is straightforward: politicians are more likely to forfeit short term gains from shirking when more effort in good policy implementation could earn them the reelection. On the other hand, reelection prospects may also create incentives for politically-motivated distribution, and lead incumbents to target voters outside the scope of the policy. This is likely to generate both distortions in local electoral dynamics and welfare losses coming from inefficient resource allocation.[1] The literature has ample evidence for the existence of such politically-motivated targeting across time, geographies, and groups of voters.[2]

Given these often competing incentives, does the prospect of reelection enhance or hinder the quality of policy implementation? The extant empirical scholarship provides mixed results, and little or no evidence on what drives the net effect on both distributional outcomes and electoral competition.[3] Nevertheless, this question remains relevant for the many developing democracies that have decentralized policy implementation in an environment where nonprogrammatic redistribution is still prevalent.

I examine this trade-off between accountability and politically-motivated misallocation using microdata from the 2009-2012 expansion in the largest conditional cash transfer (CCT) program in the world, Brazil's *Bolsa Família* (BF). During this period, BF included additional 2.6mn families across the country, a 23% increase. Access to BF is programmatic, i.e., all households self-reporting monthly *per capita* income below R$140 are eligible. The program is financed and run by the federal government, which approves individual benefits and pays them directly to the beneficiaries. Nevertheless, municipalities are responsible for collecting and verifying the enrollment information, which gives mayors leeway to include nonpoor (and ineligible) beneficiaries in the payroll. Due to the existence of a benefit quota, these households effectively take the resources from the deserving poor.[4]

[1]Examples such practices are vote buying (which is illegal in many countries) and patronage payments – see a review on Hicken (2011) and recent examples in Hidalgo and Nichter (2015); Larreguy, Marshall, and Querubín (2016); Rueda (2016); or tactical redistribution, machine politics and pork barrel politics (Lindbeck and Weibull, 1987; Dixit and Londregan, 1996; Stokes, 2005; Nichter, 2008).

[2]See Lindbeck and Weibull (1987); Dixit and Londregan (1996); Levitt and Snyder (1997); Solé-Ollé and Sorribas-Navarro (2008); Pop-Eleches and Pop-Eleches (2012); Brollo and Nannicini (2012); Gans-Morse, Mazzuca, and Nichter (2014); Klein and Sakurai (2015); Labonne (2016); Gottlieb et al. (2019).

[3]The extensive empirical literature cited above focuses on separate tests of one mechanism or the other.

[4]This problem was recently emphasized by a cabinet member, which said: "Bolsa Família used to have waiting lists of mil-

In this context, this paper focuses on one dimension of BF implementation in municipalities: whether or not the benefits reach the targeted poor. The measure of targeting comes from an unprecedented round of benefit cuts conducted unilaterally by the central government in the first half of 2013, which identified ineligible households using cross-checks of CadUnico and other federal databases. More than 700,000 families were excluded from BF in this process. Combined with the information obtained from CadUnico microdata on the reported income and enrollment date of individual households, these cuts provide a precise measure of irregular BF enrollment during the 2009-2012 mayoral tenure.

This exercise also takes advantage of the Brazilian electoral legislation that imposes a limit of two consecutive terms for mayors. This allows me to compare municipalities governed by mayors with and without reelection incentives. The main challenge is to overcome two sources of bias: one comes from unobserved municipal characteristics that influence both BF targeting and elections. This is addressed with a regression discontinuity (RD) design. The RD compares only municipalities where incumbents barely won the 2008 election (control group), to municipalities where they barely lost, and were replaced by first-term mayors that had reelection incentives in 2009-2012 (treatment group). The RD, however, does not ensure that candidate-specific traits are held constant across groups. The most likely sources of candidate differentiation are experience and proven competence. Relative to first-time mayors, reelected incumbents have more experience in the office, and were the only ones elected after revealing their performance. This is addressed by comparing only municipalities where the race was between an incumbent and a challenger that had previous experience as local mayor.[5]

The main results show that the quality of BF targeting is worse in municipalities where experienced mayors had reelection incentives. Under lame-duck mayors, the average post-audit coverage reduction was 2.21 percentage points (pp) for families that entered the program in 2009-2012. In the presence of reelection incentives, these cuts were 6.40pp higher (4x higher). To put this number in perspective, it corresponds to three times the average cost of running the BF enrollment and monitoring system at the municipal level, and five times the average budget for mayoral campaigns.

I interpret these findings within a simple theory where reelection seeking mayors choose how to screen new beneficiaries. I define positive screening as the inclusion of deserving, poor households in

lions of people that deserved the benefit but could not get it because their spot was occupied by someone that was ineligible." http://bit.ly/2Jpk6TF.

[5] Ferraz and Finan (2011) use a similar strategy. In the text, I also discuss and assess other secondary sources of potential differences between control and treatment mayors – page 16.

BF. On the other hand, negative screening deteriorates program targeting, and happens when mayors actively allow nonpoor families to enter the payroll. In the BF context, the results above imply that reelection incentives led to negative screening, which is only attractive to mayors when (i) the included nonpoor is more likely to reward them for the transfers than the included poor; and (ii) the excluded poor is unlikely to hold them accountable for the exclusion. This is a scenario of failed accountability that might lead to welfare losses. Given that BF is the most visible policy in Brazil, and that poor households vastly outnumber the nonpoor in the program,[6] these sizable effects seem surprising at first. Nevertheless, I combine evidence from institutional features of BF, heterogeneous treatment effects, and a government survey with poor households, to argue that this accountability failure reflects a breakdown in the ability of households to obtain reliable information about BF enrollment from the municipal bureaucracies.

First, negative screening is only electorally attractive if the included poor fails to reward mayors for the program. Accordingly, the literature has shown extensive evidence that local politicians have limited ability to claim credit for BF in Brazil. Once poor households enter the payroll, they are likely to credit President Lula and PT for the money (Sugiyama and Hunter, 2013; Zucco, 2013; Brollo, Kaufmann, and La Ferrara, 2017; Frey, 2019). They are also given resources (e.g. a toll free number) to protect them against attempts by local politicians to manipulate their enrollment status.

Second, the excluded but eligible poor often lacks the information to blame the municipal administration for not enrolling them. A government-sponsored survey with 11,000+ poor households (MDS, 2009) shows that BF is widely known in Brazil. However, although 95% of respondents know its eligibility criteria, 40% of them were not receiving benefits in spite of considering themselves eligible. Interestingly, only half of the excluded group ever enrolled in CadUnico. The survey suggests that this reflects a breakdown of local information channels: the excluded poor was more likely to have learned about BF from an indirect source such as media or friends, whereas included families more often had contact with the municipal bureaucracy (social services or health care workers).[7] The heterogeneity of the RD results reinforces the importance of these information channels for enrollment. With the help of two proxies for the strength of the municipal bureaucracy, I show that nearly all the effects of reelection incentives on screening come from the group of municipalities with weak bureaucratic capacity.

[6] The 2013 audit excluded slightly more than 5% of all beneficiaries.

[7] Employees in these areas are often the ones that enroll new households in CadUnico (MDS, 2009; Frey, 2019).

On the other hand, noneligible households that were included through negative screening have incentives to support incumbents in their reelection attempts. Because they understand the eligibility criteria (MDS, 2009), they know that their inclusion bends the program rules. BF also requires regular updates, where the municipal bureaucracy reassesses the eligibility of households. The included-nonpoor thus have incentives to reelect an administration that, actively or inadvertently, allowed them to illegally access the transfers. Additional evidence to this effect comes from a feature of CadUnico that allows me to link 80% of the households enrolled in 2009-2012 to voting machines, and to examine the correlation between BF screening and the share of votes for incumbents in their 2012 reelection attempt. Using only variation across voting machines in the same polling station, I show that mayors had significantly more votes in machines where poor voters had lost more BF benefits, post-audit.

These results provide a precise measure of the effects of reelection incentives on BF targeting. However, they cannot be taken as conclusive evidence that the electorally motivated negative screening is the only mechanism at play. I use several measures of incumbents' effort in expansion and monitoring of BF during 2009-2012 to rule out the plausibility of two alternative narratives for these findings. The first is that bad targeting is a consequence of a concerted local effort in program expansion. Here, mayors do not care whether poor or nonpoor are included, they just want to maximize benefits. This leads them to put more effort in enrollment and less effort in the enforcement of the eligibility criteria. The second explanation generates opposite predictions: reelection seeking mayors know that BF does not brings electoral rewards, so they shift effort away from the program, and targeting deteriorates. The results do not support either of these narratives: treatment and lame duck mayors expand the program at similar rates; they enroll a similar share of households with eligible income; and show indistinguishable metrics of BF management and monitoring in several categories.

The last empirical exercise provides alternative evidence of negative screening in the presence of reelection incentives, based on anomalous patterns of income reporting.[8] I measure the share of households with eligible income, among all that enrolled within a small interval around R$140. Households in municipalities where mayors could be reelected were 27.2% more likely to report eligible income than families under lame-duck mayors. Such *bunching* effect just below R$140 is only observed for this specific income level, and it is absent from the 2010 Census survey. What is more, when these beneficiaries were enrolled by a public servant that was politically connected to the mayor, they were

[8]This is in the spirit of the literature on electoral fraud detection (Cantú, 2014; Rundlett and Svolik, 2016).

more likely to have been excluded from BF by the 2013 cuts. This is not observed when the enrollment was conducted by members of other parties, or in locations governed by lame-duck mayors.

In addition to being related to the vast literature on term limits and strategic spending cited before, this paper is also close to the work on how local politicians influence the implementation of social programs. Camacho and Conover (2011) document how incumbents in Colombia falsify a poverty census in order to increase local enrollment, in environments of high electoral competition.[9] In Brazil, two papers provide mixed evidence on the effects of term limits on local implementation of CCTs. Brollo, Kaufmann, and La Ferrara (2017) focus on the enforcement of the school attendance requirements for BF beneficiaries. They show that mayors aligned with the President (PT mayors) were less likely to enforce BF conditionalities before elections. Although they investigate a different dimension of program implementation, their results are in line with the spirit of this paper's findings. On the other hand, De Janvry, Finan, and Sadoulet (2012) investigate *Bolsa Escola* (BE), a precursor to BF. They show that reelection incentives led mayors to implement better management practices in BE, with superior outcomes for the beneficiaries. These results are not fully comparable to the present paper, which focuses solely on the quality of program targeting.[10] Nevertheless, they emphasize the likely reasons for why reelection incentives triggered positive accountability in BE (and not in BF, as shown in this paper). In BE, mayors were able to claim ample credit for their role in implementation of the policy (p.673), and excluded but deserving households were highly informed about the program (p.683). This put them in a position to keep politicians accountable for their performance.

Finally, this paper also informs a more general empirical literature on the political rewards of programmatic policies such as CCTs, which presents mixed evidence. While De La O (2013); Manacorda, Miguel, and Vigorito (2011); Pop-Eleches and Pop-Eleches (2012); Labonne (2013); Zucco (2013) show that incumbents benefit from claiming credit over programmatic policies,[11] Imai, King, and Rivera (2018) argue that this is only possible when policies are "partly programmatic and partly clientelistic." This paper's main contribution is to provide well identified evidence that even in a largely programmatic policy, politicians might exploit opportunities for manipulation and rent extraction.

[9] The paper does not investigate the effects of reelection incentives, given that mayors have a one-term limit in Colombia.

[10] De Janvry, Finan, and Sadoulet (2012) examine some enrollment practices that might suggest better targeting quality, but this variable is not directly measured in their work.

[11] This literature does not disentangle what is a result of (i) undeserved credit claiming (see an example in Cruz and Schneider (2017)); (ii) clientelistic misallocation of resources; (iii) or rewards for effort that signals the politician's ability.

Background: Bolsa Família, Mayors and Elections

Bolsa Família (BF) reaches roughly 20% of Brazilian households, and increases the monthly income of its targeted population by 50%, on average. Different from other CCTs (e.g. Mexico, Philippines), these transfers are granted mainly based on the self-reported income of the beneficiary. In a nutshell, all households that declare monthly per capita income below R$140 (US$67 in 2012) are eligible to some form of benefit.[12] The enrollment procedure is simple: households need to provide this income information to a local program office to enter the BF registry, called *Cadastro Unico* (CadUnico).[13] Their information is then uploaded to the central program administration, which approves or denies the benefit. Once approved, benefits are paid directly by the central government to the beneficiary through a debit card. Thus, although the municipal bureaucracy does not intermediate the distribution of transfers, it plays the important role of gatekeeper in the program: municipal employees are responsible for enrollment, data collection, and for verifying the accuracy of the information.[14]

In addition to fully financing the program, the federal government also sets the target number of benefits for the country, based on its estimates of the poor population. Figure 1 shows the evolution of total beneficiaries in BF, and its coverage rate. In the early stages, it took nearly three years for BF to enroll the target number of 11.1mn households (met in mid-2006). This target was only increased again after 2009 (twice in 2009 and 2011), to finally reach 13.7mn families. Because the global quota is binding, many poor households that enrolled in CadUnico with BF-eligible income after 2006 did not receive the benefit. Since 2006 the program has been at capacity at most times, exception made to the 2009-11 expansion window. As a result, BF has had long waiting lists in several moments, with many poor and eligible households excluded from the policy (e.g. in 2006-09, and again starting in 2012).[15]

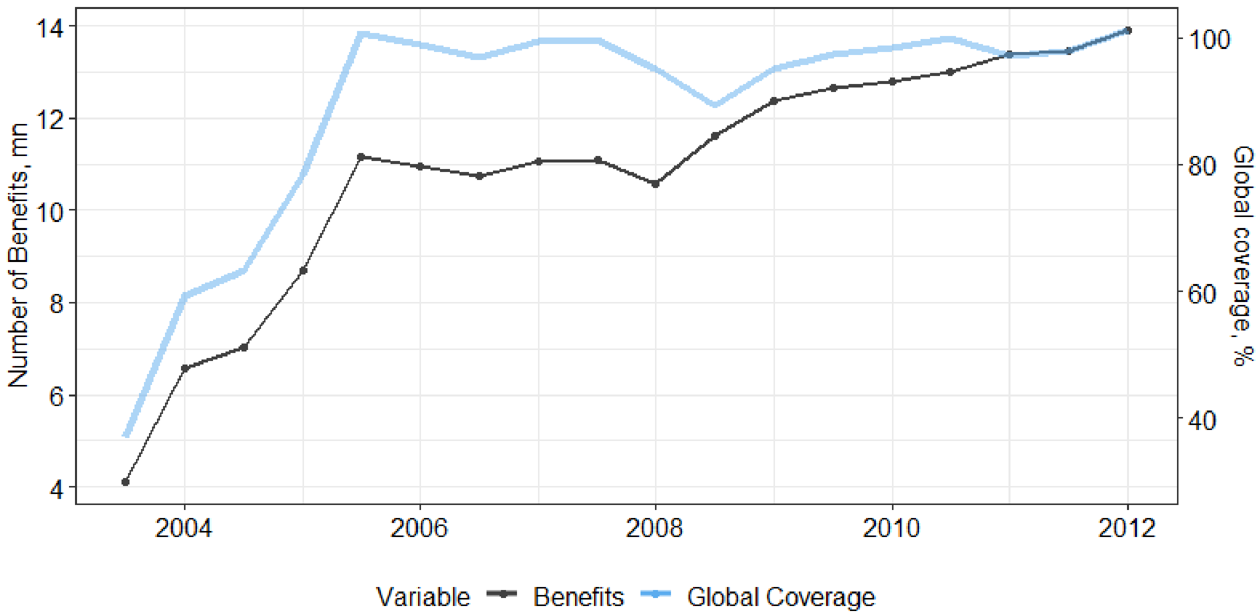
[12] This was the eligibility threshold for the period between Aug 2009 and Dec 2012. R$120 was the threshold for the first six months of 2009. There are several types of BF benefits, the most important are the basic and variable benefits. The basic benefit of R$70 is granted to all households with income below R$70, without any conditions. The variable benefit (R$32-R$38 per eligible household member) targets children (below 18) and pregnant women. It is available to households with income below R$140, and it is conditional on school attendance and health check-ups.

[13] All households that declare monthly per capita income below half the minimum wage (R$311/US$150 in 2012) are eligible to enroll in the program registry, even if they are not BF-eligible. CadUnico can be accessed and updated by the local offices, and the data is used by the federal office to grant or deny BF benefits.

[14] For a more extensive analysis, see Lindert et al. (2007).

[15] See the following MDS press release: http://bit.ly/2Jpk6TF.

Figure 1: Global coverage of the BF program



The left-side axis has the number of households receiving BF benefits each year. The right-side axis contains the global coverage rate of BF, based on the total estimate of eligible families.

Each municipality also receives an estimate of the local poor population that needs coverage. These are not strict *quotas*, given that MDS allows municipalities to cover more than their targets, even though this necessarily implies that other locations remain under-covered due to the global binding quota. They are, however, *partially* binding. MDS constantly pushes high-coverage municipalities to review their registries and the quality of their income information (MDS, 2012). What is more, the Ministry is also significantly less likely to approve benefits for newly enrolled families in these locations, even when they are eligible. Figure A.IV (appendix) shows that, in high-coverage places, the approval rate for new BF-eligible entrants is below 70%, as opposed to nearly 100% for less covered municipalities. Figure A.III (appendix) shows that 42% of this paper's sample covered more than their target in Dec 2008. Also, although the median coverage rate was 95%, none of these municipalities were able to expand the program before 2009-11, given that the global quota was binding in 2008 (this was again the case in 2012 when the new global quota was reached).[16]

[16]I emphasize these quotas could have been met with nonpoor households that passed the screening process, as this paper suggests it is the case. It is important to mention that these existing coverage disparities are mostly due to the fact that some municipalities were able to enroll newcomers at a faster rate than others before the quota first became binding in 2006, and the coverage levels became persistent (Frey, 2019). That being said, treatment and control municipalities enrolled newcomers at similar rates in 2009-12, as I discuss the Results section.

Two factors at large contribute to the exclusion of poor, eligible households from BF, and remain a threat to the effectiveness of this policy. First, imperfect targeting is caused by the enrollment of ineligible households that obtain the transfers by means of misrepresenting their income. Not surprisingly, BF's simple enrollment rules also increase the risk of income underreporting fraud.[17] This practice effectively leads to the exclusion of poor households from the benefit due to limited quotas. As an example, a 2016 audit by the Ministry of Social Development (MDS) found 1.1mn households (8% of all beneficiaries) with actual income above the threshold receiving the benefit.[18]

The federal government has limited tools to detect income underreporting, and the bulk of this responsibility falls on local program offices that are managed by municipal governments. In addition to sporadic audits,[19] the MDS runs its own internal checks, which mainly consist of matching BF beneficiaries with lists from other government databases such as RAIS (wage data for all formal sector employees), and CNIS (retirement benefits), in order to identify households that have actual income above the declared value.[20] During the first semester of 2013, more than 700,000 households were excluded from BF in an unprecedented effort to identify ineligible beneficiaries using the internal checks described above (MDS, 2013a,b), and without any input from the municipal program administrators.

The second factor that leads to program mistargeting is the inability of some eligible households to enroll in the program due to the lack of proper information. In a nutshell, while general knowledge about BF and its benefits is widespread across the country, a significant share of the poor still lack the practical information on how to register in CadUnico. In most municipalities, this information is obtained from the municipal bureaucracy that is responsible for running BF's data collection, namely social service and health care workers, and its quality is directly linked to the ability of poor families to easily access this bureaucracy. This is illustrated by a 2009 survey conducted by the MDS with households that fit the profile targeted by this policy (MDS, 2009). MDS surveyed 11,372 families, of which 6,053 were not receiving BF benefits at the time. Table 1 summarizes the findings.

[17] Figure A.II in the appendix illustrates how the income reporting patterns of households differ between the CadUnico registry and the Census survey conducted in 2010. For all households that reported monthly income below R$311 in either survey, the distribution of reported income in that range is much more biased towards zero in the CadUnico survey than in the Census. I emphasize that the Census survey is not used to determine eligibility to any social program in the country.

[18] O Globo (2016). See the news in Portuguese: http://goo.gl/yeVpWA.

[19] Even though BF is also audited as part of the government initiative that oversees mayoral administrations (CGU), these audits reach less than 3% of all municipalities every year.

[20] MDS also requires beneficiaries to update their information every two years, under the risk of losing the benefit.

Table 1: Program knowledge among the households targeted by the policy

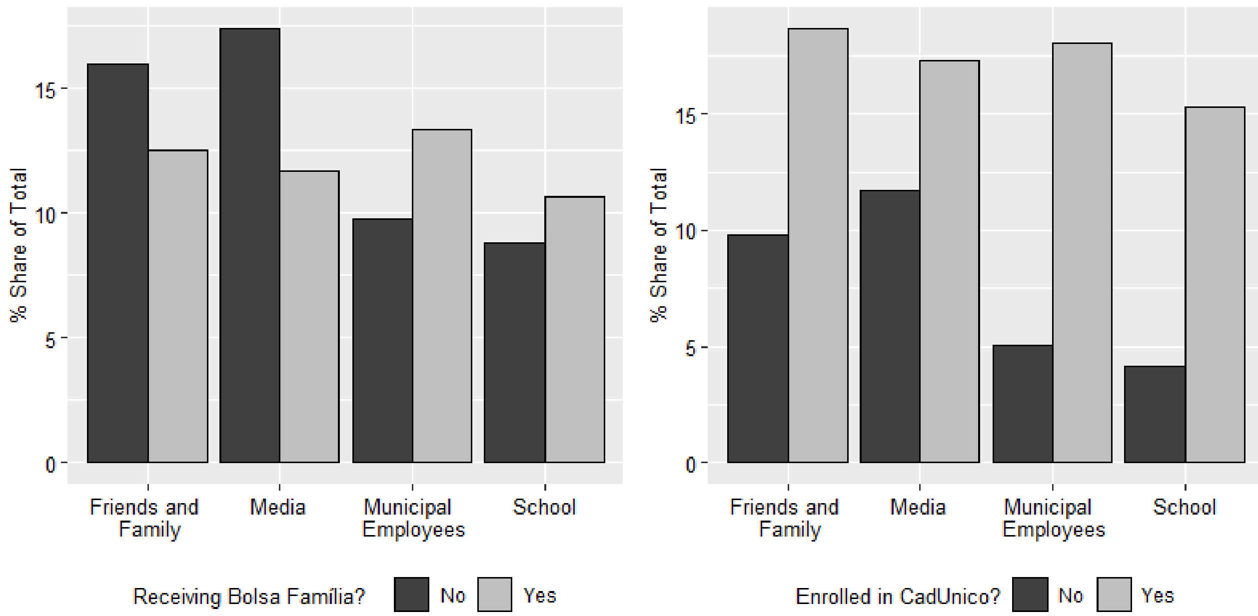
	Know the program	Know that they are eligible	Know program rules	Enrolled in CadUnico	Indirect information	Direct information
	(1)	(2)	(3)	(4)	(5)	(6)
BF Beneficiary	97.6	98.6	94.8	93.1	51.0	49.0
Non-Beneficiary	93.8	76.8	94.3	51.8	62.0	38.0
Observations	11098	8860	8860	7754	7754	7754

The Table shows the average percentage of households in each group that gave a positive answer to the questions related to items listed in each column. The sample in Column (1) has all households that answered the questions about being in BF or CadUnico. The sample in Columns (2) and (3) only include households that know about the program, and that answered the questions about eligibility and knowledge of program rules. Columns (4),(5) and (6) include the households from column (2) that think they are BF-eligible.

First, column (1) shows that BF is well known by both beneficiaries and excluded households alike: 95% of all families said that they knew the program. More interestingly, 77% of the excluded households that knew BF said that they were eligible, and 94% of them were able to correctly answer the questions about the eligibility thresholds, indicating that they actually knew the basic program rules. If these households knew the program and the eligibility rules, why were they excluded? One potential explanation is found in columns (4) through (6). From all the non-beneficiaries that said that they were eligible to the benefit, only 52% ever enrolled in CadUnico, the most basic step to enter the program.[21] What is more, included and excluded households obtained information about BF from different sources: while 50% of beneficiaries said that they learned about BF from a *direct* source (municipal employees, which also includes health care workers, and school employees), around two-thirds of excluded households obtained program information from an indirect source (media, friends or family). In summary, the survey suggests that the source of program information matters: households that were introduced to BF by the municipal bureaucracy display a higher probability to enroll in CadUnico, the first step to obtain BF benefits – see also Figure 2 below.

[21] There might be some reporting error in this variable, since *only* 93% of beneficiaries said that they were enrolled. In any case, the gap in enrollment between excluded and included households is striking.

Figure 2: Sources of BF information for poor households



The left-side plot shows the information source of BF beneficiaries and excluded households. The right-side plot compares households enrolled in CadUnico to non-enrolled households.

Mayors and negative program screening

Due to the influential role of the municipal bureaucracy in program enrollment and data collection, it is not surprising that local politicians, notably mayors, employ program manipulation for both personal and political gain. In 2013, MDS found that 2,168 elected politicians were enrolled in BF, and were receiving benefits illegally.[22] In Santa Tereza de Goiás (GO), the public prosecutor indicted both the mayor and the local BF coordinator for widespread program fraud, which consisted of manufacturing false CadUnico entries to trigger the irregular payment of benefits.[23] In 2016, the mayor of Bocaina (PI) was impeached for vote buying, after offering the BF benefit in exchange for votes.[24]

This paper focuses on the specific case where mayors allow noneligible households into BF if this improves their reelection chances. I frame the mayor's problem in terms of screening the income of new beneficiaries, given that this is the main responsibility of local program offices. Thus, mayors face the choice between negative screening, i.e., the inclusion of nonpoor-ineligible households, or positive

[22] O Globo (Oct, 2013). See the news in Portuguese: http://goo.gl/3RsfaW.
[23] Jusbrasil (2014). See the news in Portuguese: http://goo.gl/a40TYX.
[24] Portal Saiba Mais (2016). See the news in Portuguese: http://bit.ly/2JplVjh.

screening, the inclusion of poor families that are the intended target of the policy. Given that the quotas limit program expansion efforts, negative screening necessarily crowds out eligible households.

Within this framework, I argue that the most effective tool to extract political rents from BF is negative screening. This argument relies on a series of institutional factors that affect the way in which three groups of voters relate to this program. First, consider the poor and eligible families included in BF by positive screening. The literature has shown that local politicians have limited ability to extract electoral rewards from this group. Once families enter the payroll, they are much more likely to credit President Lula and his party (PT) for the transfers (Sugiyama and Hunter, 2013; Zucco, 2013; Brollo, Kaufmann, and La Ferrara, 2017). What is more, the families are given information and resources (such as a toll free number) to denounce local program manipulation by the municipal bureaucracy. In practice, this means that only the federal government can cancel their benefit after entry. In fact, Frey (2019) uses exogenous BF coverage variation across municipalities to show that the arrival of benefits generated severe vote losses for mayors where the overall local coverage was high.[25]

Now consider the poor excluded from BF. Negative screening is only more attractive when they do not have enough information to effectively blame the local bureaucracy for failing to enroll them. Again, the AIBF survey indicates that the excluded poor are much less likely to have had contact with municipal workers, and thus lack even the basic information on how to enter CadUnico (even when they know that they are eligible). What is more, given that this group was more likely to have learned about BF from indirect sources, it is also likely that they associate the program with the federal government. This is the scenario where accountability fails and mayors are not punished for negative screening.

Finally, consider the nonpoor included in BF through negative screening. I argue that they have strong incentives to support incumbents in a reelection run. The AIBF survey shows that nearly 95% of them knows the BF eligibility criteria well, which means that these households understand that their inclusion bends program rules. In this context, their willingness to support the incumbent most likely follows from one of these two scenarios. First, the municipal bureaucracy might actively inform voters that the eligible enrollment was a *gift* from the administration, so the expectation of electoral reciprocity is clear, as it is the role of the bureaucracy in the process. I expect that this explains part of the results, given that the practice is in line with the qualitative examples presented above.

Second, these voters might simply think that they got away with underreporting. The key to their

[25] The author argues that BF reduced the clientelistic transactions between poor voters and local politicians.

behavior in this scenario is the program update rule: beneficiaries are required to come in to update their CadUnico information periodically (two years at most), otherwise they lose the benefit.[26] By design, the households thus know that this review is done by the municipal bureaucracy, and because it includes an income screening, there is a risk that they might be cut-off. As a consequence, the included nonpoor prefer to face the active (and lenient) incumbent that they know, instead of an unknown challenger at the next round of reviews.

Theoretical Framework

This section proposes a simple theory to illustrate the trade-off described above between good program implementation (positive screening) and political targeting of the benefits (negative screening), for reelection seeking incumbents. Consider a simple two-period probabilistic voting model where mayors are limited to one reelection, and run against a challenger at the end of period 1. The population has P poor and N nonpoor voters. A fixed number P of policy transfers (t) is available, and are filled in period 1. Nonpoor citizens only obtain t when they lie about their income. The benefit provides utility $u(t)$ and $v(t)$ to poor and nonpoor, respectively; where $u(t) > v(t)$. All households know whether they are eligible to the benefit or not, but only a share α of the population is informed, i.e., they know that program screening is the attribution of the local bureaucracy. Mayors cannot exclude poor households once they become registered, but they can manipulate the initial registration process by setting the screening level for new entrants $(s \in [\underline{s}, \bar{s}])$. Screening is costly, and generates a disutility of $\kappa s^2/2$.

Incumbents set their preferred screening level in their first tenure in office. If reelected for a second and last term, they put no effort and keep screening unchaged from their previous tenure. In period 1, $(\bar{n} - s)N$ nonpoor households enter the policy, where $\bar{n}$ is the share of the nonpoor that take the place of eligible poor families if there is no screening. Screening can assume negative values, and is thus bounded by $[\bar{n} - P/N, \bar{n}]$. Politicians are one of two types: opportunistic or pro-poor, and their type is unknown to voters. Pro-poor types always set the maximum possible level of positive screening $(s = \bar{n})$. Opportunistic politicians set a level that maximizes their chances of reelection in the first period. The prior probability that a politician is pro-poor is given by π. Voters do not observe screening directly, they only observe their inclusion status. Voters are prospective, and they choose between the incumbent and

[26] MDS enforces this rule on a regular basis, and cancels benefits of non-compliant households without the interference of local administrators.

a challenger based on their individual expected policy benefit in period 2, and the challenger's overall popularity (ξ), which I assume to be exogenous with uniform distribution in $[-1/2, 1/2]$.

Consider the prospects for period 2 of all households that were included in the program. The deserving poor cannot be excluded by either politician in period 2, by design, so they are indifferent between candidates at the end of period 1.[27] The included nonpoor, however, learn that the incumbent is opportunistic, and know that they lose the benefit in period 2 if a pro-poor type is elected.

Now consider the poor that have been excluded in period 1. Only a share α of these households know that they should have been included by the mayor, and therefore learn that the incumbent is opportunistic. In that case, they only obtain the benefit in period 2 if a pro-poor type is elected. The uninformed, excluded poor do not learn anything, and are also indifferent between candidates in period 2.[28] Thus, the expected benefit in period 2 for the relevant groups of voters is given below:

$$\mathbb{P}[t=1] = \begin{cases} 1 & \text{for the included poor, numbered: } P - (\bar{n} - s)N \\ \pi & \text{for the excluded and informed poor, numbered: } \alpha(\bar{n} - s)N \\ 1 - \pi & \text{for the included nonpoor, numbered: } (\bar{n} - s)N \end{cases} \tag{1}$$

In this simple framework, it is easy to see that only two groups care about the type of politician they elect: the included nonpoor, which prefers an opportunistic politician in power, and the excluded and informed poor, which is willing to punish opportunistic incumbents. Thus, the probability that an incumbent is reelected is given by $\mathbb{P}[n(\bar{n} - s)\pi(v(t) - \alpha u(t)) - \xi > 1/2]$. Incumbents maximize their reelection probability subject to the screening cost.[29] Given the distributional assumptions, the optimal screening for an interior solution is given by:

$$s^* = \frac{\pi n}{\kappa}[\alpha u(t) - v(t)] \tag{2}$$

The equation above provides two insights on the mayor's decision. First, incumbents exercise effort in screening as long as there are net electoral gains. If the accountability gains ($\alpha u(t)$) are canceled out by the political targeting gains ($v(t)$), mayors with reelection incentives set $s = 0$. Second, screening is negative only when $\alpha u(t) < v(t)$. This necessarily implies a severe asymmetry of information between

[27] This is the case even though they do not differentiate between opportunistic and pro-poor incumbents after inclusion.

[28] These households vote solely based on the challenger's popularity, same as the excluded nonpoor.

[29] $\max_s \; n(\bar{n} - s)\pi(\alpha u(t) - v(t)) - \kappa\frac{s^2}{2}$

included and excluded households. In other words, α must be small enough to compensate for the higher marginal utility the poor receives from the benefit. If the poor were perfectly informed about how and why they were excluded from the benefit, their incentives to keep incumbents in check should always be higher than the incentives of nonpoor households to elect a politician that allows fraud. In this framework, negative screening only arises due to the lack of information.

Data and Research Design

This paper estimates the effects of reelection incentives on program screening using data from the global expansion of *Bolsa Família* (BF) during the mayoral tenure of 2009-2012, when the program quota was increased to include additional 2.6mn households. Given that CadUnico only records the household's reported (as opposed to actual) income, a direct measure of the quality of municipal screening is not readily available. Instead, I take advantage of a timely round of top-down audits conducted by the MDS in the first semester of 2013, based on a cross-check of federal government databases with CadUnico. These audits triggered the interruption of BF payments to more than 700,000 households due to income misreporting fraud (MDS, 2013a,b) in all Brazilian municipalities. They were conducted without the influence or input from municipal administrations, and provide a reliable measure of income misreporting across locations.

The variable that proxies BF targeting quality is called *screening*, and it is calculated using all households that enrolled in CadUnico in 2009-12 for municipality m, as follows:

$$Screening_m = \frac{(bf13_m - bf12_m)}{eligible_m} \times 100 \tag{3}$$

where $bf13_m$ and $bf12_m$ are the number of households that enrolled in CadUnico in 2009-12 that had the BF benefit in June 2013 (post-audit), and Dec 2012; respectively. The denominator $eligible_m$ is the number of CadUnico entrants with eligible income (below R\$140) in 2009-12.[30]

I also build five other variables that measure different dimensions of the municipal administration's effort in expanding and managing BF. The variable *new enrolled* is the ratio of new CadUnico registrations in 2009-2012 to the local enrollment target, in percentage points; *eligible share* is the percentage

[30] I also provide in the appendix (Table A.V) an alternative specification using the percentage of ALL households that entered CadUnico in the period, which also includes the ones with income above the BF eligibility threshold in the denominator. The estimated effects are similar for the two definitions.

of these new entrants that declared income below the eligibility threshold for benefits. The variable *new benefits* is the percentage of BF benefits granted to these CadUnico newcomers, as of Dec 2012; and *bf yield* is the percentage of BF benefits granted to CadUnico newcomers with eligible income. Although the approval of benefits is the attribution of the federal government, it sometimes reflects local enrollment procedures. For example, benefits are denied when CadUnico has missing or inconsistent data, even if the income is eligible. Finally, *IGD* is an index calculated by the MDS for each municipality to evaluate the quality of local program management. Its components account for the performance of the local bureaucracy in monitoring the compliance of beneficiaries with the health and school conditionalities; in keeping CadUnico updated with recent information; and in keeping enrollment numbers high. This IGD measure was calculated for the end of 2012. I provide more details on data construction in the appendix (page 1 and page 8), including the description of other variables used as covariates.

The Regression Discontinuity Design

I focus on candidate rather than party incumbency. The treatment effect for each municipality is defined as the victory, in 2008, of a mayor with reelection incentives for the 2009-12 tenure. Brazilian mayors face a limit of two consecutive terms, so this effect is estimated with the comparison between municipalities governed by mayors elected for a first non-consecutive term in office (treatment mayors), and municipalities governed by mayors reelected for a second and last term (control mayors).

A naive comparison of all 5,563 municipalities in the country, however, would carry two main sources of bias (Alt, de Mesquita, and Rose, 2011; Ferraz and Finan, 2011; De Janvry, Finan, and Sadoulet, 2012). The first problem comes from unobserved municipal characteristics might affect both the incumbent's reelection probability and the outcomes of interest. For example, if mayors are more likely to be reelected in poor areas, then treatment and control groups are not randomly assigned, and the effects on screening might be confounded by the influence of poverty levels.

This problem is addressed with a regression discontinuity design (RDD). The spirit of the RD is to compare municipalities where incumbents barely won the 2008 election (control), to municipalities where they barely lost, and were replaced by first-term mayors that could be reelected (treatment). This strategy provides a quasi-random assignment of reelection incentives in places where elections were close (Lee, 2008; Eggers et al., 2015).[31] This also means that locations where the incumbent did not

[31]The application of RD to close elections has been widely used in Brazil for both candidates (Boas and Hidalgo, 2011;

run for reelection in 2008 are excluded from the analysis (nearly half of the country's municipalities).[32]

The second source of bias comes from the fact that even the RDD does not hold all characteristics of elected politicians constant (Eggers, 2017). For example, the comparison made here is between reelected incumbents and first-term mayors. If these politicians are also different in other dimensions that are systematically correlated with their performance in office, then the treatment effects could be biased. Even though I cannot fully eliminate this threat to identification, I take several steps to ensure that the comparison between the treatment and control groups is fair in the present empirical design.

First, the most significant difference between treatment and control mayors comes from experience and proven competence (Ferraz and Finan, 2011; De Janvry, Finan, and Sadoulet, 2012). Lame duck mayors have more consecutive years on the job, by design, and they were reelected after voters observed their performance in office, which is not the case of challengers. I avoid these potential biases using only a sample of municipalities in which an incumbent ran against a challenger that had been mayor in that location during the tenure of 2001-04 (the *experienced* sample). The two-term limit in Brazil only applies to consecutive terms, and rerunning is a very common path for mayors (Klašnja and Titiunik, 2017). This sample guarantees that challengers and incumbents are comparable in experience and proven competence. This brings down the number of available municipalities from 2824 to 693.[33]

Second, I also assess a second salient source of potential candidate differentiation. The experienced sample now has treatment mayors that were incumbents in 2001-04, and ran for office in 2008. This implies that they were not mayors in 2005-08, which means one of three things: (i) they did not run for reelection in 2004; (ii) they did run but lost; or (iii) they were already in their second consecutive term in 2001-04, so they faced the two-term limit and could not run. If the decision to rerun after one term in office (or after a loss) is systematically correlated with a relevant trait of politicians, then depending on the profile of the challenger, treatment and control mayors could be fundamentally different.[34]

Ferraz and Finan, 2011) and parties (Klašnja and Titiunik, 2017).

[32]This is equivalent to excluding the open seat sample in Klašnja and Titiunik (2017). I emphasize that the present article and Klašnja and Titiunik (2017) define their treatment and control groups in the opposite way. While I am interested in the effects of reelection incentives, they frame their analysis in terms of incumbency advantage. In addition, the design here also automatically excludes 145 municipalities that had only one candidate in the 2008 race, and the 83 municipalities that in 2008 had an election with the possibility of two stages (these are the state capitals, or cities with more than 200,000 voters). Finally, 22 municipalities that had the 2008 election canceled by the courts are also excluded.

[33]In the appendix, page 3, I show the estimation of treatment effects for the potentially biased sample (the inexperienced sample). I also examine its balance of covariates, and discuss the distinctions between the control groups of the experienced and inexperienced samples. This comparison suggests that the impact of bias might be severe in the inexperienced sample, as the differences in treatment effects between samples cannot be solely explained by differences in pre-existing conditions that would drive heterogeneous treatment effects.

[34]As an example, if only the high-quality challengers that lost in 2004 decide to run again in 2008, this could make the

Fortunately, I can trace the history of 99% of the challengers in the sample to observe why they did not run in 2004. This allows me to estimate the effects of reelection incentives on screening for two different subsamples: a group where challengers faced term limits in 2004 (64% of the sample); and a group where either they did not run or ran and lost (36%). The first group is likely more comparable to control mayors in 2008: they decided to run after one term in office, and won their reelection attempt.[35] In any case, these results are presented in Table A.X in the appendix, and they show that the difference in treatment effects is not statistically significant across these groups.[36] This suggests that the top-line results in this paper were not driven by these pre-existing differences between candidates.

Third, Table A.IV in the appendix shows that the sample is balanced at the discontinuity for 16 variables that include characteristics of the municipality, elected party, and more importantly, the mayor. Accordingly, The main estimating equation is shown below:

$$y_m = \beta_0 + \beta_1 T_m + \beta_2 MV_m + \beta_3 T_m MV_m + \xi_m \tag{4}$$

where the outcome is denoted by y_m, for municipality m. The presence of reelection incentives is given by the T_m, and the treatment effect by β_1. The dummy T_m assumes value of one when an experienced challenger wins the 2008 election, and has reelection incentives in 2009-12. The margin of victory MV_m is the difference in percentage points between the winner and the runner-up in 2008. The local linear regression is weighted by the edge kernel, and estimated using a bandwidth around the discontinuity, following Cattaneo, Idrobo, and Titiunik (2020). I emphasize that municipalities in both sides of the discontinuity have similar levels of BF coverage in 2008, and also similar poverty levels.[37]

Main Results

Table 2 shows the RD effects from equation 4 and the *screening* variable, for different polynomial and bandwidth specifications. The graphic representation of these effects is shown in Figure 3.

treatment group to have a higher quality on average than lame duck mayors in 2009-12.

[35]If I restrict the sample even further to only this group, however, I reduce my number of municipalities severely, and also the statistical power of the estimates.

[36]If anything, the more comparable group has treatment effects that are slightly stronger in magnitude, which suggests that the even the direction of potential bias is not a threat to the present estimation.

[37]Figure A.I (appendix) shows no evidence of manipulation of the running variable at the discontinuity (McCrary, 2008).

Table 2: Reelection incentives lead to negative screening

Dependent Variable: *screening*	(1)	(2)	(3)	(4)
RD Effect (Reelection incentives)	-6.396	-8.339	-8.769	-8.315
S.E.	(2.813)	(3.949)	(4.349)	(4.038)
C.I.	[-12.21,-1.18]	[-16.11,-0.63]	[-17.41,-0.37]	[-16.50,-0.68]
Pre-treatment mean	-2.205	-0.978	-0.788	-1.401
Bandwidth	10.2	11.5	15.1	5.1
Observations	332	359	439	162
Bandwidth rule	*optimal*	*optimal*	*optimal*	*1/2 optimal*
Polynomial	*linear*	*quadratic*	*cubic*	*linear*

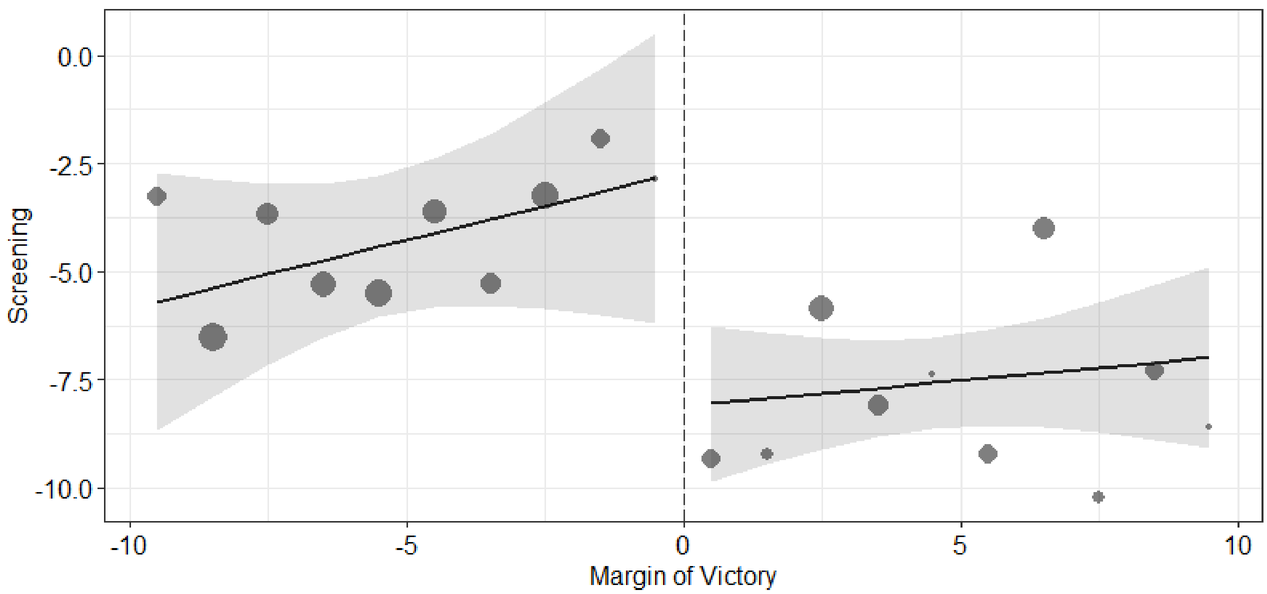
Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020). The pre-treatment mean is calculated for the control group (lame duck mayors) at the discontinuity (β_0 in equation 4). The RD effect corresponds to β_1 in equation 4.

The estimates show that municipalities governed by mayors with reelection incentives in 2009-12 enrolled significantly more ineligible, nonpoor households in BF during the mayoral tenure. From column (1), the BF cuts conducted by MDS in the first half of 2013 triggered an average reduction of 2.21 percentage points (pp) in the number of benefits for these households, in municipalities governed by lame duck mayors (pre-treatment mean). However, in municipalities with a treatment mayor, these cuts were 6.40pp higher (4x as large). The remaining columns show that these results are robust to polynomial and bandwidth choices.[38]

To put this misallocation of resources in perspective, the global BF budget for 2012 was around R$20 billion (roughly US$8bn). A loss of 6% in the program, although apparently small, corresponds to more than three times the total cost of running the CadUnico enrollment and verification system in municipalities. In terms of resources available for persuading voters, the mistargeted amount corresponds to five times the average budget for mayoral campaigns in Brazil.

[38] In the appendix, Table A.V also shows that they are robust to the the inclusion of covariates, and the use of a slightly different variable definition.

Figure 3: BF screening at the discontinuity



The outcome variable is *screening*, as defined in the text. The right-side is the treatment group (municipalities where the mayor had reelection incentives). The dots represent the average of the outcome variable for that specific value of margin of victory. Their size reflect the number of observations within that range of margin of victory. The solid lines are the local linear fit, and the shaded area is the 95% confidence interval.

The previous results provide a precise measure of the effects of reelection incentives on the quality of BF targeting across municipalities. However, they cannot be taken as conclusive evidence that this inclusion of nonpoor households is driven by the electorally motivated negative screening proposed by this paper. In the sequence, I discuss two potential alternative explanations for the results, and use data on local expansion and management of BF during the mayoral tenure to assess their plausibility.

First, bad targeting could be a consequence of a concerted effort by the municipal administration in program expansion. Instead of actively enrolling nonpoor households that could reward them in reelection attempts (the argument of this paper), mayors simply want more beneficiaries in their location. These results are then consistent with a scenario where they do so with a combination of more enrollment and less enforcement of the eligibility criteria. The nuance here is that mayors do not really care about who enters the program (poor or nonpoor), they just want to maximize benefits. In support of this explanation, reelection incentives should generate at least one of two patterns: (i) higher enrollment in CadUnico (there are no binding quotas for that); and (ii) among new CadUnico entrants, higher enrollment with declared income below the eligibility threshold for BF. Table 3 shows the RD effects for the variables *New enrolled* and *Eligible share*, which measure precisely these two patterns.[39]

Table 3: Alternative explanations: expansion and management quality of BF in 2009-2012

Outcome Variable:	New enrolled	Eligible share	New benefits	BF yield	IGD index
RD Effect (R. incentives)	1.294	-1.030	-0.230	0.454	0.005
S.E.	(5.054)	(4.133)	(4.248)	(4.429)	(0.016)
C.I.	[-9.22,10.59]	[-9.14,7.06]	[-8.08,8.57]	[-7.81,9.55]	[-0.02,0.04]
Pre-treatment mean	35.225	80.646	54.191	67.989	0.857
Bandwidth	8.3	9.1	8.7	10.2	12.6
Observations	284	305	295	333	386

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020), using optimal bandwidths for a linear polynomial. The pre-treatment mean is calculated for the control group (lame duck mayors) at the discontinuity (β_0 in equation 4). The RD effect corresponds to β_1 in equation 1.

Table 3 indicates that the combination of program expansion and lax enforcement of the eligibility criteria does not explain the screening results: reelection incentives do not lead treatment mayors to behave differently than lame duck mayors on these areas. The growth in CadUnico registration is similar across treatment and control municipalities (*New enrolled*). What is more, the average newcomer in CadUnico is no more or less likely to report income below R$140 in treatment municipalities (variable *Eligible share*).[40] Finally, the variable *new benefits* shows that, at the end of 2012, a similar share of new entrants had the BF benefit in both groups of municipalities. This is not surprising, given the institutional features of BF: the existence of program quotas that cap the expansion efforts by mayors, and the salience of the federal program brand that leads beneficiaries to associate BF with Lula and PT.

I use a second empirical strategy to assess this alternative explanation. If negative screening is simply a spillover from effort in enrollment and a lax enforcement of eligibility, then the variable *screening* should be negatively correlated with *new enrolled* and *eligible share* in the sample. I calculate and present this correlation, at the discontinuity point, in Table A.XI in the appendix.[41] In summary, the

[39] Table A.VI in the appendix shows that the results from Table 3 are robust to the inclusion of covariates, and different polynomials and bandwidths.

[40] The effect is actually negative, but small and not statistically significant.

[41] I split the sample by the median value of these variables, and estimate the difference in the average value of the variable screening across the samples with low- and high-enrollment (or *eligible share*). I estimate this difference at the discontinuity where the value of margin of victory is zero. In that, the exercise is similar to the RD estimation in this paper, but the treatment and control groups are redefined in terms of high and low value of the new running variables (*new enrolled* and *eligible share*).

estimates show the correlation between *screening* and both these variables is not statistically significant, and its signal is actually positive under most specifications.

The second alternative explanation for the screening results is that, if mayors in fact know that BF expansion and good management do not generate electoral rewards, they might shift effort away from BF into other areas when they are seeking reelection. However, the results from Table 3 do not support this narrative: reelection incentives do not lead to less effort in program expansion, given that treatment and control municipalities have similar levels of total enrollment, and enrollment with eligible income.

The variable *bf yield* measures the rate of approval of benefits by the federal government, for eligible households. Even though mayors do not influence this process directly, benefits might be denied due to errors and inconsistencies in the CadUnico data. Under this alternative narrative, if reduced effort in BF management triggers more benefit denials, this would happen more often in treatment municipalities, which is not the case. Finally, the IGD index is a direct measure of the quality in BF management at the municipal level. Again, the estimated effect for this variable does not support the idea of differential effort in BF management across treatment and control groups.

Heterogeneous effects by the size of the relevant municipal bureaucracy

In page 8, I present survey results that suggest that the lack of information on how to access CadUnico explains why many poor households fail to receive BF benefits. The survey also indicates that individual CadUnico enrollment is strongly correlated with having obtained BF information from the municipal bureaucracy, as opposed to an indirect source as media, friends or family. I use two proxies for the strength of the relevant bureaucracy across municipalities, in order to split the sample into locations with strong or weak information channels. I then estimate heterogeneous treatment effects on *screening* for these subsamples. I expect that, in municipalities with better information channels, reelection incentives are less likely to lead mayors to conduct negative screening. In these places, poor households are better informed about the role of the municipal government in BF, and can punish the incumbent for their exclusion.[42]

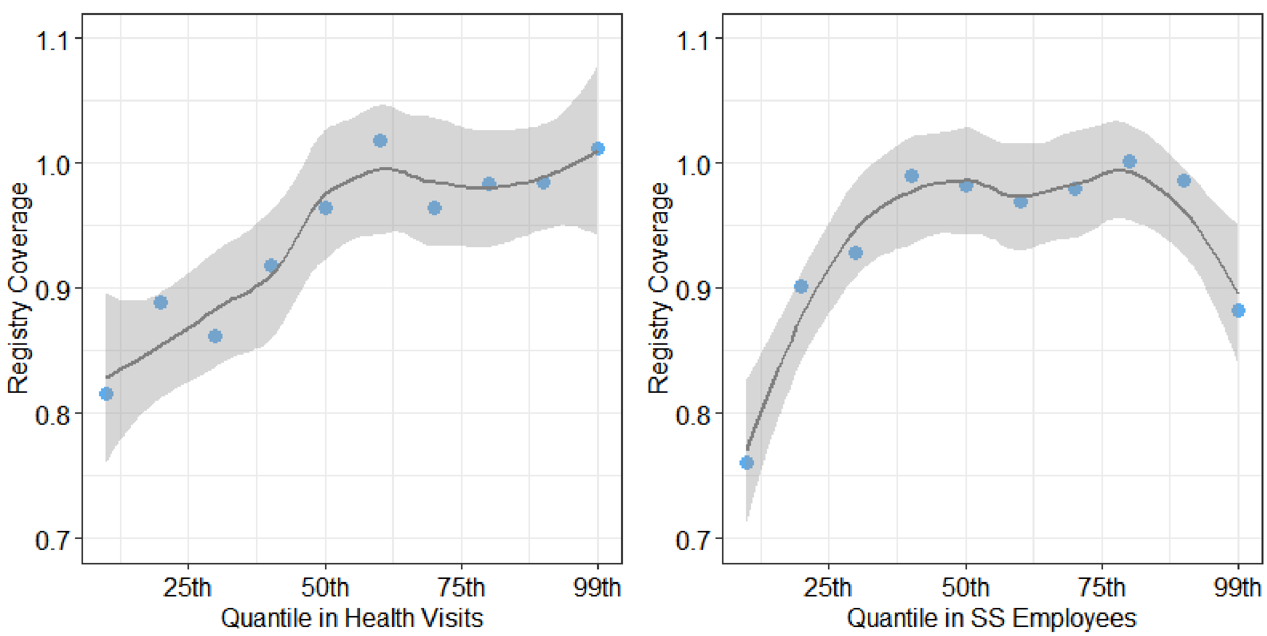
The first proxy is the number of employees allocated to the municipal social service department, obtained from a 2009 survey conducted by the Brazilian Institute of Geography and Statistics (IBGE,

I also emphasize that the effects have no causal interpretation. I provide details in the appendix, page 16.

[42]These are high α municipalities, according to the theory developed before.

2009). In nearly all municipalities (97%), Cadunico enrollment is primarily conducted by Social Service (SS) workers. What is more, there is a lot of heterogeneity in how well these departments are staffed: some have a single temporary employee, others are fully staffed with full-time workers. The second proxy follows directly from Frey (2019), which shows that home visits by health care workers in Brazil were instrumental in spreading information about BF, and in fostering program enrollment.[43] With data from the Ministry of Health from 2006-08 (before elections), I calculate the average number of home visits per covered family for these three years. For both these variables, municipalities with strong (weak) information channels are the ones where the proxy value is above (below) the median. I also check whether these variables are good predictors of direct information channels in municipalities by showing that they are positively correlated with past CadUnico enrollment. Figure 4 plots this relationship for each one of these proxies, using CadUnico enrollment from Dec 2008.[44]

Figure 4: Municipal bureaucracy and past CadUnico enrollment



In both plots, the y-axis has the number of families enrolled CadUnico, as of 2008, divided by the local CadUnico enrollment target (this is different than the BF coverage target). The x-axis orders the municipalities by the value of each one of the proxies described in the text. The dots represent the average CadUnico enrollment rate for different quantiles in municipal bureaucracy.

[43]More specifically, the paper uses an exogenous differential in funding across municipalities for the Family Health Program. This is a health care program in Brazil that finances home visits by teams of doctors, nurses and health agents. The additional funding led to more visits by health professionals, and consequently to more enrollment in BF.

[44]The value of both these variables is also balanced at the discontinuity, as shown in Table A.IV in the appendix.

Table 4 shows the heterogeneous effects on *screening*. The first column has the results for the group with strong information channels, column (2) shows the results for the weak group, and column (3) shows their difference.[45] Overall, the results indicate that the effects of reelection incentives on screening are concentrated in the sample where the information channels are weak, for both proxies.[46]

Table 4: Heterogeneity of effects by the strength of the information channels

	Municipal bureaucracy (proxy for information channel)		
	Strong	Weak	Difference
Sample split by the size of the SS bureaucracy in 2009			
RD effect (Reelection incentives)	-2.375	-9.989	-7.614
S.E.	(2.774)	(2.773)	(3.922)
C.I.	[-8.08,2.79]	[-15.70,-4.83]	[-15.30,0.07]
Bandwidth	10.2	10.2	10.2
Observations	155	177	332
Sample split by the number of health visits per family in 2006-08			
RD effect (Reelection incentives)	0.951	-13.252	-14.203
S.E.	(2.855)	(2.857)	(4.039)
C.I.	[-5.01,6.18]	[-19.22,-8.02]	[-22.12,-6.29]
Bandwidth	10.3	10.3	10.3
Observations	155	164	319

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020), using optimal bandwidths and a linear polynomial. The RD effect corresponds to β_1 in equation 1.

One of the challenges with this empirical exercise is that the bureaucracy is not randomly assigned across municipalities. The results here might be driven by other channels, correlated with bureaucracy, that affect the heterogeneity of the effects of reelection incentives on screening. In order to alleviate this concern, I show that such heterogeneous effects are not present when the sample is split by the value of other variables, such as the poverty rate and inequality (gini), which are likely correlated with bureaucratic capacity. These results are shown in Table A.VII (appendix).

I also run a falsification test of this mechanism, in which I split the sample using a proxy for media

[45] The balance of covariates at the discontinuity, for each subsample, is shown in the appendix, Table A.XII and Table A.XIII.

[46] The difference in treatment effects between the two samples split by the SS bureaucracy is only significant at a 90% confidence level.

access (local radio ownership, measured by the 2010 census).[47] The AIBF survey shows that, although the media helped to spread BF knowledge, it is not the most reliable source of information for the purpose of enrollment: households that learned about BF from the media were less likely to be in CadUnico. Thus, the treatment effect should not vary significantly across municipalities with better or worse media access. Table A.VII (appendix) shows that this is the case.

Electoral Rewards

Do voters reward incumbents for negative screening? I shed some light on this question using the results of the subsequent mayoral election in 2012, and the variation in the screening variable, aggregated by voting machine. Comparing voting machines with a similar share of CadUnico-enrolled voters within the same polling station, I show that the vote percentage for the incumbent is significantly higher when poor voters assigned to that machine experience a larger loss in BF benefits.

First-term mayors ran for reelection in 133 municipalities in the sample. CadUnico contains the voting machine number for roughly 80% of the households enrolled in 2009-12. This allows me to observe the correlation between votes for the incumbent and screening at the machine level. Brazilians are assigned to a voting machine at the time they first register to vote in a given municipality. This allocation does not change across elections, unless voters move and re-register. At the time of registration, voters are typically allocated to the polling station closer to their residence, in order to facilitate voting. Within that voting location, they are usually assigned to the machine that has less voters at that moment in time.[48] The estimation uses 4,946 voting machines in 966 different locations.

The main independent variable is the machine-equivalent of screening *screening*, which is defined as the difference in the total number of BF benefits between Jun 2013 and Dec 2012, as a share of all CadUnico entrants in 2009-12 in machine v.[49]. The main outcome is the percentage of votes for the incumbent (PCT_v). Accordingly, I estimate the equation below:

$$PCT_v = \gamma_0 + \gamma_1 screening_v + \gamma_2 PCTP_v + \gamma_3 X_v + \delta_l + \epsilon_v \tag{5}$$

[47] Radio ownership has been previously used as a proxy for local access to political information in Brazil by Ferraz and Finan (2008).

[48] Machines are capped in size, and these caps vary by state. On average, this sample's machine size is 343, with a standard deviation of 56. I exclude all machines that more than doubled in size between 2008 and 2012, 1% of the sample.

[49] $covdrop_v = \frac{bf13_v - bf12_v}{entrants_v}$

where δ_l are voting location fixed-effects, $PCTP_v$ is the percentage of votes for the mayor in the 2008 election, and X_v are machine-level controls.[50] The parameter γ_1 is expected to be negative, given that machines with more negative screening had more nonpoor voters included in BF. These voters, according to the theory, are more likely to support the incumbent in a reelection run.

The results are shown in Table 5. The preferred specification is shown in column (3), but both the coefficient and the standard errors are fairly stable across alternative specifications.

Table 5: Loss of BF Benefits is correlated with votes for the incumbent mayor

Dependent Variable: *Percentage of votes for the incumbent*	(1)	(2)	(3)
screening (γ_1)	-0.823	-0.809	-0.887
S.E.	(0.408)	(0.407)	(0.412)
Past pct. vote (γ_2)	0.368	0.372	0.372
S.E.	(0.015)	(0.015)	(0.015)
Voting Locations	966	966	966
Observations	4946	4946	4946
Control for New Entrants	No	Yes	Yes
Other Covariates	No	No	Yes

All regressions include fixed-effects by voting location. The average percentage of voters in a machine that are CadUnico entrants in 2009-12 is 4.27%.

The results show that the percentage of votes for the incumbent in the reelection run is positively correlated with the votes in 2008 (γ_2), but negatively correlated with screening (γ_1). The magnitude of γ_1 is better interpreted when re-scaled by the average share of machine voters that entered CadUnico in 2009-12 (4.27%).[51] It implies that, if the percentage of voters that lost the benefit in 2013 increases by 10pp, the vote percentage of the incumbent increases by 2.1pp in the same machine. Nevertheless, the magnitude of this estimate should be taken with a grain of salt, given the ecological nature of this inference. The main contribution of this section is simply to provide a robust correlation between BF screening and votes for the incumbent, with reasonably benign identification assumptions about the

[50] They include the share of voters in the machine that entered CadUnico in 2009-12 (New entrants), and the average value of some CadUnico variables for these group, namely: declared income, enrollment year, monthly expenses with food, and existence of water connection in the household. These covariates are measured contemporaneously with enrollment (not post-treatment).

[51] One should divide -0.887 from column (3) by 4.27 to obtain the coefficient in equivalent pct of machine votes.

nature of the allocation of voters across voting machines.[52]

Anomalous Patterns of Income Reporting

The primary measure of income underreporting in this paper comes from a national audit that excluded 700,000 families from BF in 2013. In this section, I provide alternative evidence that reelection incentives led incumbents to include noneligible households in the program, in the spirit of the literature that uses statistical anomalies to detect electoral fraud (Cantú, 2014; Rundlett and Svolik, 2016). I show that, within a small range around the income eligibility threshold (R$140), households from municipalities with reelection incentives in 2009-12 were more likely to declare an income that was less than or equal to R$140. The intuition for this exercise is the following: this unusual bunching of income declarations just below R$140 is consistent with fraud if and only if this pattern is neither observed in other income levels (CadUnico) nor in the 2010 census survey for the R$140 level.

For every municipality, *just eligible* is defined as the percentage share of families declaring eligible income, out of all with declared income within a small interval around the threshold (e.g., R$140 $\pm$R$5). Even though the enrollment with eligible income does not guarantee benefit approval,[53] Figure 5 shows that the majority of households that enrolled as such in 2009-12 were receiving transfers by Dec 2012.[54] Table 6 shows the estimation of equation 4, using *just eligible* as the outcome.

[52]The parameter γ_1 captures the effect of program screening on the mayor's electoral performance under the assumption that, within voting locations, assignment to machines is uncorrelated the household's benefit status.

[53]The benefit is ultimately approved by the federal government.

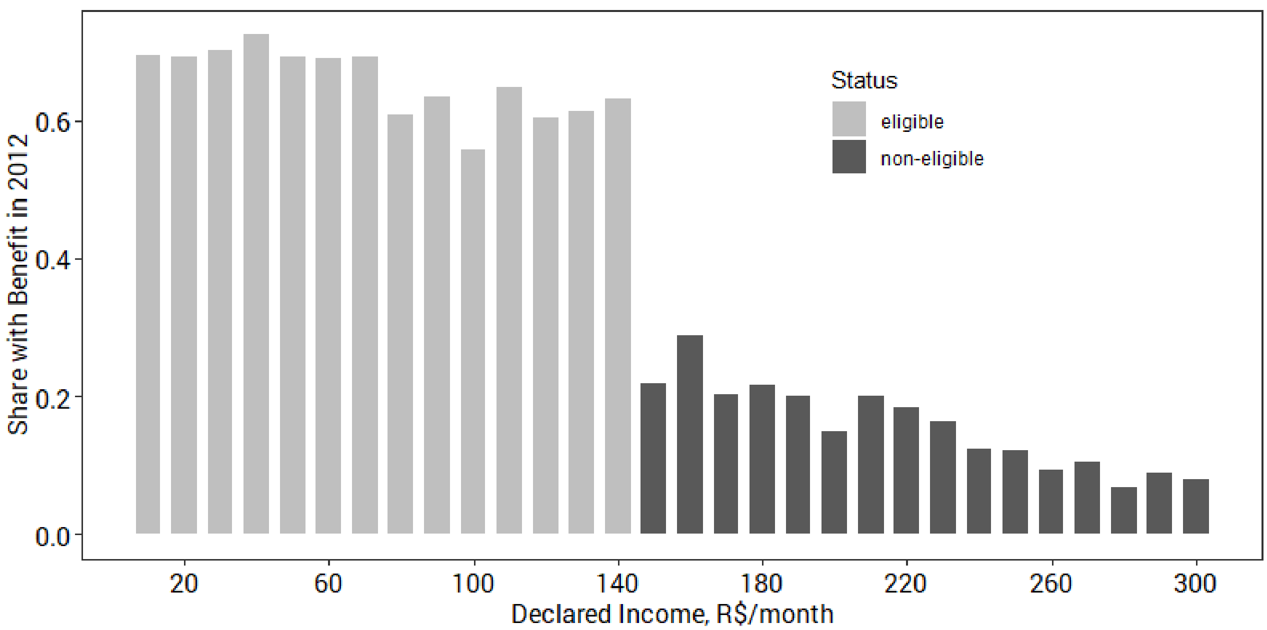
[54]It is also possible for households with declared income above the threshold to receive the benefit. BF has a permanence rule since 2009, stating that households that increased their income at some point after the initial registration are still eligible to keep the benefit for a grace period that could last up to two years.

Table 6: Reelection incentives generate anomalous income reporting patterns

Dependent Variable: *Just eligible*	(1)	(2)	(3)	(4)
RD effect (Reelection incentives)	18.425	20.830	34.567	31.003
S.E.	(7.649)	(8.698)	(14.267)	(13.668)
C.I.	[4.30,34.28]	[4.18,38.27]	[7.41,63.33]	[6.65,60.23]
Pre-treatment mean	67.703	65.447	50.792	53.814
Bandwidth	8.7	15.0	13.3	4.4
Observations	271	400	373	128
Bandwidth rules	*optimal*	*optimal*	*optimal*	*1/2 optimal*
Polynomial	*linear*	*quadratic*	*cubic*	*linear*

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020). The pre-treatment mean is calculated for the control group (lame duck mayors) at the discontinuity. The RD effect corresponds to β_1 in equation 1.

Figure 5: Share of households receiving BF benefits, by declared income

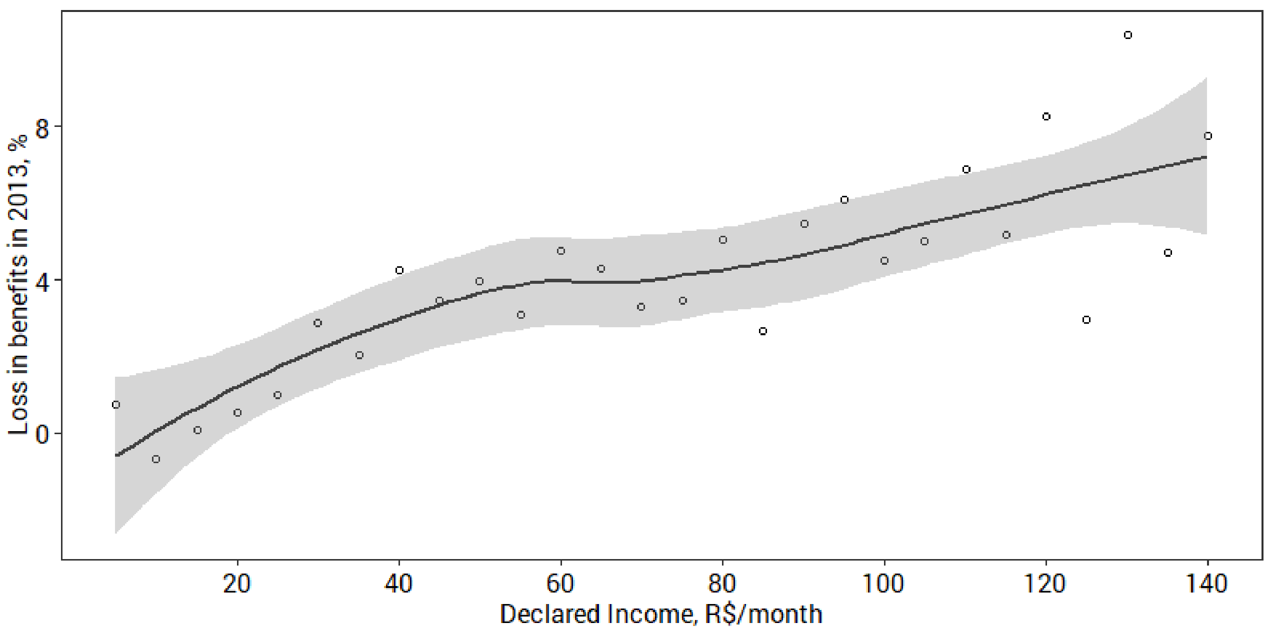


The light colored bars show the share with benefit among households that declared eligible income. The bars aggregate all income declarations in R$10 intervals.

The pre-treament mean indicates that, in the absence of reelection incentives, 67.7% of households that declare income within an interval of $\pm$R$5 around the eligibility threshold, declare themselves eligible (column 1). This share is 18.4pp higher (a 27.2% effect) when the mayor has reelection incentives. These findings are consistent across bandwidths and choices of polynomial. The appendix shows additional robustness and placebo tests. Table A.VIII replicates these results including covariates, and includes alternative specifications where *just eligible* is built for wider intervals around the threshold.[55] Table A.IX has two placebo tests: the first varies the eligibility level by ±R$20, and the second uses the R$140 threshold, but with income declaration data from the census survey from 2010. Both these tests show no evidence of placebo effects outside the eligibility threshold in CadUnico.

I emphasize that this type of fraud is not the only one possible in CadUnico,i.e., nonpoor households could rather misreport any level of eligible income. Thus, although the estimates here suggest that this type of enrollment is consistent with fraud, their magnitude cannot be directly mapped to the main results in Table 2. Nevertheless, they are consistent with the following pattern observed in the data: households that reported eligible income closer to the threshold were also more likely to have lost their benefit after the 2013 audit (see Figure 6).

Figure 6: Loss of benefit post-audit, by level of declared income



Pct of households that lost the benefit post-audit, according to their reported income, as of Dec 2012.

The CadUnico registry also records the name of the interviewer that collected the household's data

[55]It also shows that the share of people that enter CadUnico within the interval around R$140 is similar in treatment and control groups, as expected.

for many of the families. In this section, I merge the names of these interviewers with the party membership rolls in Brazil.[56] Around 10% of all Brazilian voters are formally affiliated to parties, and this group is seen as the core supporters and party activists (Speck, Braga, and Costa, 2015). I then show that, for all households enrolled with a *just eligible* income level, the ones registered by a partisan of the mayor were more likely to lose the benefit after the audit.

I use the subset of CadUnico households for which the interviewer's name is available. First, I focus on two binary outcomes: a dummy that indicates whether the household enrolled with eligible income, and another that indicates whether enrollment happened with just eligible income. I regress these outcomes on *Partisan*, which assumes value of one when the interviewer belongs to the mayor's party. The results are shown in columns (1) and (2) of Table 7. In short, partisan interviewers are no more or less likely to enroll households with either eligible or just eligible income.

Table 7: Loss of benefit when registered by a partisan of the mayor with just eligible income

	Dependent Variable:			
	Eligible	Just eligible	Change in benefit	
	(1)	(2)	(3)	(4)
Partisan	0.008	0.002	0.008	0.010
S.E.	(0.009)	(0.005)	(0.007)	(0.008)
Just eligible			-0.012	-0.010
S.E.			(0.007)	(0.007)
Partisan x Just eligible				-0.074
S.E.				(0.033)
Observations	116436	116436	116436	116436

Cluster-robust standard errors by both municipality in parenthesis. All regressions include fixed effects by municipality and by quarter-year of enrollment (time trends), and the household-level covariates described in the text.

The most interesting results are shown in columns (3) and (4). The outcome variable is now defined as the change in benefit between Jun 2013 and Dec 2012 for household h. I first regress this variable on the *partisan* dummy, and on a dummy indicating whether the household enrolled with just eligible

[56]These two datasets are merged by the exact full name of the interviewer. Thus, some measurement error is expected from either spelling errors in the registries or incorrect matches of two different people with the exact same full name. I do not believe that this is a significant problem in the sample, given that Brazilian full names are often composed of three or more different names. In any case, in order to avoid this type of error, I have excluded from the party membership rolls all duplicated names within the same municipality. This was a reduction of less than 2% in the list of party members.

income (income $\in [R\$131, R\$140]$) – column (4). Column (5) includes the interaction between these variables. Negative coefficients indicate that households are more likely to have lost the benefit in 2013.

In line with the evidence from Figure 6 above, column (4) shows that households that enroll with just eligible income are slightly (1.2pp) more likely to lose their benefit than households that do not (even though the coefficient is not statistically significant here). However, when the just eligible enrollment is conducted by a partisan of the mayor, the probability of benefit loss is 7.4pp higher (when compared to enrollment with just eligible income by a nonpartisan interviewer). In the appendix, I show that this pattern is non-existent in municipalities without reelection incentives; and also non-existent when the *Partisan* variable is calculated with interviewers that belong to the the main opposition party.

Conclusion

In the context of *Bolsa Família* (BF) in Brazil, this paper examines the countervailing effects of local reelection incentives on the quality of decentralized policy implementation, which is measured by the incumbent's success in curtailing the enrollment of nonpoor households into the program with negative screening. The trade-off is straightforward: while nonpoor households prefer an incumbent that allows this type of fraud, poor voters excluded from the policy punish incumbents for this distortion. The net effect of these incentives on the mayor's actions depends on how much each group values the benefit, and how well the excluded poor are informed about the incumbent's role in program screening.

The RD estimates indicate that politically-motivated targeting trumps *good* accountability in this policy: mayors with reelection incentives are four times more likely to enroll nonpoor households into the policy. This behavior cannot be explained by either effort in program expansion or a different approach to program management. It is also shown to improve the performance of incumbents in their reelection attempts, and it is more common in locations where households do not have access to the most common information channel for program enrollment, measured by local bureaucratic capacity. Finally, anomalous income reporting patterns show that this electorally-driven fraud is more common for households enrolled by public servants politically connected to the mayor.

The present analysis is one of the first steps in the direction of a broader understanding on how competing incentives generated by institutional rules such as term limits jointly motivate individual politicians, given that most of the existing literature focus on specific mechanisms in isolation.[57] This paper argues that, in the context of Brazil's CCT, it is an asymmetry of information about the policy that refrains voters to keep incumbents accountable for their program implementation practices. However, the specific mechanisms that trigger accountability likely vary across different institutional environments and program designs. It is beyond the scope of this paper to provide a more general framework that presents the conditions in which these different incentives would prevail upon one another.

Finally, I believe that the conclusions here could be extended to policy implementation in similar institutional environments where (i) clientelism is still a prevalent form of politician-voter linkage, as opposed to ideological and programmatic considerations; (ii) the social policy design is only partially programmatic, allowing space for manipulation by local incumbents; (iii) the local infrastructure that allows the spread of information about public services is precarious; and (iv) the policy delivery is decentralized, with local incumbents controlling resources that were not necessarily raised by local taxes. In this case, there are less mechanisms for accountability, and top-down policy audits might be the most effective way to discipline politicians.

[57] One notable exception is Alt, de Mesquita, and Rose (2011). The authors use term limits for US state governors to disentangle the effects of accountability and competence in policy implementation.

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Online Appendix

Contents

A Details on the Data Construction Process

CadUnico The raw data from CadUnico contains the answers from each household to an extensive questionnaire that can be found at: http://goo.gl/27b9OG. I excluded all households with invalid entries for the dates of their last update or enrollment (i.e. whenever they had invalid years of update, or the last update happened before enrollment), and missing some relevant information as declared income, monthly expenses with food and electricity, and basic information about the conditions of the family dwelling (such as the existence of a Bathroom, or public water service). Finally, I use only households with reported income below the registry threshold of R$311 in 2012. Around 8% of all households had reported income above this value. This could have been caused by registration errors, or the fact that some households can be enrolled in CadUnico with higher income under special circumstances.[58]

For all households that entered CadUnico in 2009-2012, I only observe the reported income valid in Dec 2012. For some households that updated their information at least once after enrollment, this income could have changed from the amount reported at the time of registration. I do not see this as a threat to the analysis, given that any manipulation of reported income within that period would have been done under the same set of reelection incentives for all municipalities. For example, if a household first enrolled with income of R$145 in Aug 09 (not eligible to BF), and then updated the income to R$139 (eligible) a few months later, I only observe the R$139. Both the enrollment and the update were still conducted by the same municipal administration, with the same reelection prospects.

Census As for the income declaration measure constructed using census data, I use the sample of the Census 2010. The sample was conducted with a questionnaire for ~11% of the Brazilian households, and the aggregation of households within each municipality considers the sample weights determined by the IBGE in order to closely reflect the income distribution in each location.

Party Membership Rolls Party membership rolls were downloaded from the TSE website. I only considered the party members as of Dec 2008 (i.e. pre-treatment), which had their status recorded as active in the list. Any duplicated entries were eliminated, keeping the most recent party enrollment in case the same voter appeared to be enrolled in two or more different parties.

[58]The income threshold can be waived for households with total monthly income below three times the minimum wage.

Other Data The Ministry of Social Development (MDS) provides monthly data on municipal CCT coverage. This was used to compute the number of benefits in both December 2012 and June 2013, after the round of audits by MDS. MDS also provides an estimate of households that are BF-eligible and CadUnico-eligible in each municipality. These numbers are used as non-binding targets for the number of registries and benefits provided for each municipality. Their sum is the (binding) cap of benefits provided to the population. Election data comes from the Superior Electoral Authority (TSE), for both the 2008 and 2012 municipal elections. It has biographical data on the candidates, and the number of votes at the voting machine level, for each municipality. Finally, whenever I categorize parties in the left-right spectrum, I use the DALP survey produced by Duke University. A score of 4 or less indicates a leftist party (in the 0 to 10 scale).

Construction of Variables The construction of the outcome variables is always described in the main text. The construction of the covariates is described in the footnotes of Table A.IV in this appendix.

B Competence and Experience Biases

This paper uses a sample of Brazilian municipalities where incumbent mayors ran against challengers with previous mayoral experience. I call this the *experienced* sample. This design aims to avoid potential bias in the treatment effects coming from differences in experience and proven competence between newly elected challengers and reelected incumbents, which occur by design in municipalities where incumbents ran against inexperienced challengers in 2008 (the *inexperienced* sample). I emphasize that any differences in treatment effects between the inexperienced and experienced samples could come from two sources. The first is the bias discussed above. The second is potentially heterogeneous treatment effects. In other words, if municipalities in the two samples are fundamentally different from each other, this could lead to different effects of reelection incentives as well.[59]

Although these two sources of differences in effects cannot be disentangled within the scope of the present analysis, this section provides suggestive evidence that the effects of reelection incentives on negative screening should be relatively weaker in the experienced sample, based on observed differences between the control groups of these two samples. Interestingly enough, this is not the case. These findings therefore suggest that the experience and ability biases might play a relevant role in determining the behavior of first-term mayors in BF screening. The remaining of the section describes both the empirical exercise and the findings in more detail.

I first show the estimated effect of reelection incentives for the *inexperienced* sample. I then compare the BF outcomes and the pre-determined covariates across experienced and inexperienced municipalities. The comparison is restricted to the control group because all mayors in these municipalities are reelected incumbents. This means that biases coming from differences in competence and experience do not play a role in the BF outcomes observed across samples (they should only reflect differences in observed or unobserved traits of experienced and inexperienced municipalities).[60] I use this comparison to provide insights on the potential nature of heterogeneous treatment effects, and consequently, the relative importance of the experience and ability biases.

Table A.I below shows that reelection incentives have no significant effect on BF screening, expansion or management, for the inexperienced group, i.e., challengers that were not mayors before were less

[59]This latter source of differentiation does not threaten the RDD identification strategy as the bias does, it just indicates that the results should be interpreted accordingly.

[60]This is not the case for treatment municipalities, where the elected challengers in the experienced sample are different from the ones in the inexperienced sample, by design.

likely to engage in the type of negative program screening observed in the experienced group.

Table A.I: RD effects for the inexperienced sample

Outcome Variable:	BF screening	New enrolled	Eligible share	BF yield
RD effect (Reelection Incentives)	1.588	-0.133	-1.967	2.693
S.E.	(1.980)	(2.028)	(2.527)	(2.886)
C.I.	[-2.10,5.66]	[-4.22,3.73]	[-7.14,2.76]	[-2.78,8.54]
Pre-treatment mean	-7.051	27.701	78.804	70.062
Bandwidth	8.6	7.9	8.3	8.9
Observations	779	712	754	794

Robust standard errors in parenthesis and 95% confidence intervals in brackets. The RD uses a linear polynomial and optimal bandwidths. The pre-treatment mean is calculated for the control group at the discontinuity.

Table A.II shows the balance of covariates between the treatment and control groups. Even though most covariates are balanced, the treatment group has mayors that are significantly younger (by 5 years). This is not surprising, given that this sample compares newcomers to reelected incumbents.

Table A.II: Balance of covariates in the inexperienced sample

Dependent Variable:	Coeff.	S.E.	C.I.	Band.	Obs.
Mayor's Age	-4.853	1.697	[-8.250,-1.600]	8.9	791
Mayor's Gender	0.004	0.060	[-0.120,0.110]	8.6	781
Mayor's Education	-0.051	0.080	[-0.210,0.100]	9.4	825
M. Background: Public Sector	-0.022	0.045	[-0.110,0.070]	12.3	1036
M. Background: High-Skilled	0.118	0.078	[-0.030,0.270]	8.0	721
Mayor's Party: PT	0.060	0.056	[-0.050,0.170]	8.4	756
Mayor's Party: PMDB	0.057	0.071	[-0.080,0.200]	8.6	766
Mayor's Party: PSDB	0.034	0.059	[-0.080,0.150]	9.6	847
Mayor's Party: Left	0.007	0.077	[-0.150,0.150]	7.3	676
Turnout, 2008	-0.015	0.906	[-1.690,1.860]	8.4	757
Gini Coefficient, 2000	0.018	0.014	[-0.010,0.050]	7.8	705
Per Capita GDP, 2008	0.134	0.104	[-0.060,0.340]	8.6	778
Poverty Rate, 2008	0.007	0.026	[-0.040,0.060]	8.8	787
BF Coverage, 2008	0.005	0.034	[-0.060,0.070]	9.0	797
SS Employees, 2009	-0.064	0.189	[-0.440,0.300]	8.6	770
Visits per Family, 2006-08	0.209	0.189	[-0.140,0.600]	6.0	536

Coefficient is the treatment effect of reelection incentives. Robust standard errors in parenthesis and 95% confidence intervals in brackets, and bandwidths are optimal for a linear polynomial.

I now compare the control groups of the experienced and inexperienced samples, as follows:

$$y_m = \beta_0 + \beta_1 M_m + \beta_2 MV_m + \beta_3 M_m MV_m + \xi_m \tag{6}$$

where M_m indicates whether the incumbent won the 2008 election over an experienced challenger, i.e., it takes value one for the experienced sample.[61] Table A.III shows the estimation results.

[61] The coefficient β_1 here does not identify a causal effect of mayoral experience, but rather shows the average difference between these two samples, at the discontinuity.

Table A.III: Differences between experienced and inexperienced control groups

Dependent Variable:	Coeff.	S.E.	C.I.	Band.	Obs.
Outcome Variables					
Bf Drop 1, 2013	5.018	(3.013)	[-0.77,11.04]	9.2	649
Bf Drop 2, 2013	3.789	(2.557)	[-1.10,8.92]	9.4	659
New enrolled, 2013	9.100	(3.146)	[3.53,15.86]	6.3	446
Eligible share, 2012	2.045	(3.213)	[-4.22,8.38]	9.0	632
New benefits, 2012	-0.760	(3.152)	[-7.13,5.23]	11.3	764
BF yield, 2012	-2.286	(3.770)	[-9.81,4.97]	10.8	740
IGD index, 2012	-0.005	(0.014)	[-0.03,0.02]	10.8	742
Pre-determined covariates					
Mayor's Age	0.649	(1.894)	[-3.07,4.36]	10.0	693
Mayor's Gender	0.004	(0.052)	[-0.10,0.11]	15.5	1003
Mayor's Education	-0.019	(0.113)	[-0.24,0.20]	9.5	662
M. Background: Public Sector	-0.056	(0.054)	[-0.17,0.05]	10.9	746
M. Background: High-Skilled	0.135	(0.118)	[-0.08,0.38]	8.6	609
Mayor's Party: PT	0.066	(0.080)	[-0.08,0.23]	9.6	675
Mayor's Party: PMDB	-0.072	(0.098)	[-0.28,0.11]	7.2	517
Mayor's Party: PSDB	0.038	(0.077)	[-0.11,0.20]	12.5	848
Mayor's Party: Left	0.221	(0.116)	[0.01,0.46]	9.0	633
Turnout, 2008	-0.633	(1.274)	[-3.13,1.86]	9.9	688
Gini Coefficient, 2000	-0.003	(0.014)	[-0.03,0.02]	12.3	831
Per Capita GDP, 2008	0.053	(0.134)	[-0.20,0.32]	10.7	731
Poverty Rate, 2008	0.001	(0.034)	[-0.07,0.07]	10.8	744
BF Coverage, 2008	0.005	(0.044)	[-0.08,0.09]	8.7	617
SS Employees, 2009	0.763	(0.332)	[0.16,1.46]	6.7	475
Visits per Family, 2006-08	-0.213	(0.145)	[-0.50,0.07]	8.1	549

The coefficient shows the effect of having a lame duck mayor that won against an experienced challenger (vs. having a lame duck mayor that won against an inexperienced challenger). Robust standard errors in parenthesis and 95% confidence intervals in brackets, and bandwidths are optimal for a linear polynomial.

Not surprisingly, mayoral characteristics are similar across groups. This is also the case of most municipal covariates and 2009-12 outcomes in terms of BF screening and management. However, two remarkable differences provide some insight on the potential heterogeneity of treatment effects. First, municipalities in the experienced sample have a higher number of SS employees. Second, the 2009-12 increase in CadUnico enrollment is also higher for this sample.

The AIBF survey results discussed in this paper suggest a strong association between CadUnico enrollment and the local SS bureaucracy. Thus, it is possible that municipalities in the experienced sample have, as a whole, better channels for disseminating enrollment information than their counterparts in the inexperienced sample. The main results in this paper also suggest that, in areas with such characteristics, reelection incentives are less likely to cause negative screening (Table 4). All in, these findings here imply that the treatment effects of reelection incentives in the experienced sample should be lower than the ones in the inexperienced sample. They are, however, stronger. This indicates that the experience and competence biases play a significant role in determining the willingness and capacity of first-term mayors to take electoral advantage of political targeting of BF.

C Additional Tables and Figures

Table A.IV: Covariate balance at the discontinuity

Dependent Variable:	Coeff.	S.E.	C.I.	Band.	Obs.
Mayor's Age	4.306	(2.289)	[-0.200,8.770]	13.2	404
Mayor's Gender	-0.054	0.071	[-0.190,0.090]	10.9	348
Mayor's Education	-0.129	0.180	[-0.500,0.210]	6.7	223
M. Background: Public Sector	0.032	0.075	[-0.110,0.190]	7.9	261
M. Background: High-Skilled	-0.211	0.161	[-0.540,0.090]	6.8	225
Mayor's Party: PT	-0.119	0.088	[-0.300,0.040]	9.3	310
Mayor's Party: PMDB	0.127	0.115	[-0.080,0.370]	8.2	275
Mayor's Party: PSDB	0.003	0.115	[-0.240,0.220]	9.8	326
Mayor's Party: Left	-0.206	0.150	[-0.520,0.070]	8.5	288
Turnout, 2008	2.017	1.789	[-1.360,5.650]	8.8	297
Gini Coefficient, 2000	-0.007	0.024	[-0.050,0.040]	9.4	313
Per Capita GDP, 2008	0.103	0.191	[-0.270,0.470]	8.6	290
Poverty Rate, 2008	-0.006	0.050	[-0.100,0.090]	8.1	266
BF Coverage, 2008	-0.001	0.058	[-0.110,0.120]	9.3	309
SS Employees, 2009	-0.606	0.338	[-1.310,0.010]	8.0	262
Visits per Family, 2006-08	0.011	0.185	[-0.350,0.370]	6.8	216

The treatment effect is having a mayor with reelection incentives. Robust standard errors in parenthesis and 95% confidence intervals in brackets for optimal bandwidths and for a linear polynomial (Cattaneo, Idrobo, and Titiunik, 2020).

Mayor's Age: Age of elected mayor, as of 2008.

Mayor's Gender: Binary variable that assumes one if the elected mayor is a female.

Mayor's Education: Binary variable that assumes one if the elected mayor has a college education.

M. Background: Public Sector: Binary variable that assumes one if the elected mayor had a previous career as a civil servant.

M. Background: High-Skilled: Binary variable that assumes one if the elected had a previous career in a high-skilled profession in the private sector.

Mayor's Party: PT: Binary variable that assumes one if the elected mayor belongs to PT.

Mayor's Party: PMDB: Binary variable that assumes one if the elected mayor belongs to PMDB.

Mayor's Party: PSDB: Binary variable that assumes one if the elected mayor belongs to PSDB.

Turnout, 2008: Turnout in the 2008 municipal election, in %.

Gini Coefficient, 2000: Index calculated with data from the 2000 census survey.

Per Capita GDP, 2008: annual per capita GDP in the municipality for 2008, in log(R$ '000).

Poverty Rate, 2008: The number of households that should be eligible to BF benefits, estimated by the MDS in 2006, divided by the number of households in the municipality, from 2008 (IBGE).

BF Coverage, 2008: The ratio of households covered by BF benefits in 2008, divided by the local MDS coverage target.

SS Employees, 2009: The size of the social service bureaucracy in the municipality in 2009 (IBGE), in log(variable). This is the only variable here that is measured slightly after the 2008 election.

Visits per Family, 2006-08: The average ratio of annual health care visits per family in the period. Includes all families covered by the public health system.

Table A.V: Reelection incentives lead to negative screening (Robustness)

Dependent Variable: *screening*	(1)	(2)	(3)	(4)
Includes Covariates				
RD effect (Reelection incentives)	-7.348	-8.216	-8.307	-8.938
S.E.	(2.842)	(3.439)	(3.659)	(3.481)
C.I.	[-13.12,-1.98]	[-15.07,-1.59]	[-15.51,-1.17]	[-15.98,-2.33]
Bandwidth	7.4	10.5	16.2	3.7
Observations	248	338	461	120
Alternative Outcome Specification				
RD effect (Reelection incentives)	-5.177	-6.430	-6.765	-6.611
S.E.	(2.407)	(3.215)	(3.518)	(3.345)
C.I.	[-10.09,-0.66]	[-12.73,-0.12]	[-13.74,0.05]	[-13.47,-0.36]
Bandwidth	9.7	11.4	14.9	4.9
Observations	325	358	431	152
Bandwidth rules	*optimal*	*optimal*	*optimal*	*1/2 optimal*
Polynomial	*linear*	*quadratic*	*cubic*	*linear*

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020). The first set of results includes all the first 14 covariates from Table A.IV. The remaining two variables are not included because they are available only for a subset of the main sample. These results are, however, robust to their inclusion, and available upon request. The alternative outcome specification is described in text, page 14.

Table A.VI: Expansion and management quality of BF in 2009-2012 (Robustness)

Outcome Variable:	New enrolled	Eligible share	New benefits	BF yield	IGD index
Quadratic Polynomial, Optimal Bandwidth					
RD effect (R. incentives)	0.099	-2.268	0.654	2.847	0.025
S.E.	(6.339)	(5.454)	(5.551)	(6.732)	(0.028)
C.I.	[-12.75,12.10]	[-13.09,8.29]	[-9.97,11.79]	[-9.90,16.49]	[-0.03,0.08]
Bandwidth	11.5	11.3	11.2	11.0	11.3
Observations	359	357	354	352	355
Cubic Polynomial, Optimal Bandwidth					
RD effect (R. incentives)	0.301	-3.030	1.245	3.243	0.030
S.E.	(7.432)	(6.363)	(6.497)	(7.321)	(0.029)
C.I.	[-14.50,14.63]	[-15.59,9.35]	[-11.39,14.07]	[-10.83,17.87]	[-0.03,0.09]
Bandwidth	14.9	14.0	14.2	16.6	17.6
Observations	431	422	423	467	481
Linear Polynomial, Optimal Bandwidth, Includes Covariates					
RD effect (R. incentives)	1.616	-1.751	0.979	1.478	0.003
S.E.	(4.489)	(3.016)	(3.872)	(4.138)	(0.016)
C.I.	[-7.60,10.00]	[-7.92,3.90]	[-6.10,9.08]	[-6.12,10.10]	[-0.03,0.03]
Bandwidth	8.3	7.0	7.8	9.7	10.2
Observations	283	235	258	325	332

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020). The last set of results includes all the first 14 covariates from Table A.IV. The remaining two covariates are not included because they are available only for a subset of the main sample. These results are, however, robust to their inclusion, and available upon request.

Table A.VII: Heterogeneity of effects of reelection incentives by other variables

	Value of the variable used to split the sample		
	High	Low	Difference
Sample split by radio ownership in 2010			
RD effect (Reelection incentives)	-7.186	-4.540	2.646
S.E.	(2.753)	(2.754)	(3.894)
C.I.	[-12.85,-2.06]	[-10.20,0.59]	[-4.99,10.28]
Bandwidth	10.2	10.2	10.2
Observations	168	164	332
Sample split by the poverty rate in 2008			
RD effect (Reelection incentives)	-6.709	-6.142	0.567
S.E.	(2.738)	(2.737)	(3.871)
C.I.	[-12.40,-1.67]	[-11.83,-1.10]	[-7.02,8.15]
Bandwidth	10.2	10.2	10.2
Observations	169	163	332
Sample split by the gini coefficient in 2000			
RD effect (Reelection incentives)	-5.582	-6.963	-1.381
S.E.	(2.811)	(2.811)	(3.975)
C.I.	[-11.41,-0.39]	[-12.79,-1.77]	[-9.17,6.41]
Bandwidth	10.2	10.2	10.2
Observations	167	165	332

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020) for a linear polynomial and optimal bandwidths. The RD effect corresponds to β_1 in equation 1.

Table A.VIII: Anomalous income reporting patterns (Robustness)

Dependent Variable: *Just eligible*	(1)	(2)	(3)	(4)
$\pm$R$5 range around the threshold, includes covariates				
RD effect (Reelection Incentives)	17.452	25.447	35.553	29.717
S.E.	(6.740)	(9.519)	(14.158)	(11.944)
C.I.	[5.08,31.50]	[7.44,44.75]	[8.58,64.08]	[7.98,54.80]
Bandwidth	9.2	12.4	12.6	4.6
Observations	282	349	355	134
$\pm$R$7 range around the threshold				
RD effect (Reelection incentives)	15.266	17.756	31.761	27.393
S.E.	(6.922)	(7.954)	(12.668)	(11.509)
C.I.	[2.16,29.29]	[2.37,33.55]	[7.64,57.29]	[7.40,52.52]
Bandwidth	9.0	14.7	12.8	4.5
Observations	288	408	374	136
$\pm$R$10 range around the threshold				
RD effect (Reelection incentives)	8.824	14.478	18.787	15.520
S.E.	(4.865)	(6.233)	(8.384)	(7.705)
C.I.	[-0.59,18.48]	[2.66,27.09]	[2.81,35.68]	[2.10,32.30]
Bandwidth	8.6	11.4	12.6	4.3
Observations	283	345	373	135
Dependent Variable: Share of total income declarations that fall within $\pm$R$5 of the threshold				
RD effect (Reelection incentives)	0.135	0.578	0.625	0.228
S.E.	(0.772)	(1.052)	(1.140)	(1.028)
C.I.	[-1.40,1.62]	[-1.45,2.68]	[-1.58,2.89]	[-1.73,2.30]
Bandwidth	10.2	10.1	14.0	5.1
Observations	333	330	422	163
Bandwidth rules	*optimal*	*optimal*	*optimal*	*1/2 optimal*
Polynomial	*linear*	*quadratic*	*cubic*	*linear*

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020). The RD effect corresponds to β_1 in equation 1.

Table A.IX: Anomalous income reporting patterns (Placebo Tests)

Dependent Variable: *Just eligible*	(1)	(2)	(3)	(4)
$\pm$R$5 range around a placebo level equal to the eligibility threshold +R$20 (R$160)				
RD effect (Reelection incentives)	2.538	1.963	1.568	-1.978
S.E.	(10.091)	(12.774)	(13.870)	(13.783)
C.I.	[-16.81,22.74]	[-23.04,27.04]	[-25.47,28.90]	[-29.33,24.70]
Pre-treatment mean	66.540	67.812	69.156	72.969
Bandwidth	9.7	11.9	16.8	4.9
Observations	255	283	368	123
$\pm$R$5 range around a placebo level equal to the eligibility threshold -R$20 (R$120)				
RD effect (Reelection incentives)	6.884	7.322	7.761	5.765
S.E.	(7.697)	(9.288)	(10.593)	(10.981)
C.I.	[-7.62,22.55]	[-10.98,25.43]	[-13.20,28.33]	[-15.34,27.70]
Pre-treatment mean	69.060	68.724	68.298	70.081
Bandwidth	8.6	12.3	16.1	4.3
Observations	277	354	437	131
$\pm$R$5 range around the original R$140 eligibility threshold, data from the 2010 census survey				
RD effect (Reelection incentives)	5.132	5.294	4.520	5.238
S.E.	(7.747)	(10.537)	(11.157)	(11.768)
C.I.	[-10.91,19.45]	[-15.62,25.68]	[-17.47,26.26]	[-17.61,28.52]
Pre-treatment mean	61.883	58.248	58.446	58.205
Bandwidth	8.8	10.9	16.8	4.4
Observations	276	321	431	131
Bandwidth rules	*optimal*	*optimal*	*optimal*	*1/2 optimal*
Polynomial	*linear*	*quadratic*	*cubic*	*linear*

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020). The pre-treatment mean is calculated for the control group (lame duck mayors) at the discontinuity. The RD effect corresponds to β_1 in equation 1.

Table A.X: Heterogeneity of effects of reelection incentives effects by the past of the challenger

	Challenger did not run in 2004 due to term limits?		
	No	Yes	Difference
RD effect (Reelection incentives)	-4.695	-7.858	-3.163
S.E.	(2.845)	(2.845)	(4.023)
C.I.	[-10.65,0.50]	[-13.82,-2.66]	[-11.05,4.72]
Bandwidth	10.3	10.3	10.3
Observations	127	207	334

Robust standard errors in parenthesis and 95% confidence intervals in brackets, all calculated according to Cattaneo, Idrobo, and Titiunik (2020), using optimal bandwidths for a linear polynomial.

Here, I estimate the correlation between *screening* and the variables *new enrolled* and *eligible share*. The intuition is that, if negative screening is simply a spillover of program enrollment and a lax enforcement of eligibility criteria, the value of *screening* should be more negative in the samples where the value of these two variables is higher, at the discontinuity. Accordingly, I split the municipalities in two groups by the median value of *new enrolled* (and *eligible share*). Then, I estimate the difference in screening between these groups, at the discontinuity point where the margin of victory is zero. This estimate is shown in Table A.XI below. The effects estimated with this regression have no causal interpretation, they simply show how much worse is screening for the group of municipalities that had high enrollment (or a more lax enforcement of eligibility). In summary, the correlation between screening and these variables is not statistically significant at the discontinuity. If anything, it is actually positive in magnitude in most specifications, indicating that negative screening is not associated with high CadUnico enrollment and a lax enforcement of BF eligibility criteria in this sample.

Table A.XI: Correlation between screening, enrollment and enforcement of eligibility criteria

Dependent Variable: *screening*	(1)	(2)	(3)	(4)
The *treatment* dummy indicates that the municipality has a HIGH value of *new enrolled*				
RD effect of high enrollment	-0.659	-0.273	2.137	3.126
S.E.	(2.839)	(3.214)	(3.854)	(3.655)
C.I.	[-5.98,5.15]	[-6.49,6.11]	[-5.39,9.72]	[-3.80,10.53]
Pre-treatment mean	-5.089	-5.560	-7.220	-8.459
Bandwidth	10.3	16.6	16.5	5.1
Observations	333	467	467	163
The *treatment* dummy indicates that the municipality has a HIGH value of *eligible share*				
RD effect of lax enforcement	3.361	3.626	3.742	3.924
S.E.	(2.455)	(3.244)	(3.563)	(3.265)
C.I.	[-1.57,8.06]	[-2.68,10.03]	[-3.14,10.83]	[-2.39,10.41]
Pre-treatment mean	-7.645	-7.634	-8.010	-8.052
Bandwidth	12.2	14.2	15.7	6.1
Observations	375	423	453	196
Bandwidth rules	*optimal*	*optimal*	*optimal*	*1/2 optimal*
Polynomial	*linear*	*quadratic*	*cubic*	*linear*

Robust standard errors in parenthesis and 95% confidence intervals in brackets. Means are calculated for the groups with low value of the splitting variables, at the discontinuity.

Table A.XII: Covariate balance at the discontinuity (Subset 1)

Dependent Variable:	Coeff.	S.E.	C.I.	Band.	Obs.
SS Bureaucracy BELOW Median Size					
Mayor's Age	3.542	(5.790)	[-7.560,15.140]	6.8	120
Mayor's Gender	-0.058	(0.074)	[-0.210,0.080]	7.4	134
Mayor's Education	0.218	(0.226)	[-0.220,0.670]	7.8	138
M. Background: Public Sector	0.112	(0.110)	[-0.080,0.350]	4.7	74
Mayor's Party: PT	-0.027	(0.210)	[-0.420,0.400]	5.9	98
Mayor's Party: PSDB	-0.326	(0.379)	[-1.110,0.380]	4.2	66
Mayor's Party: Left	-0.076	(0.287)	[-0.640,0.490]	6.5	110
Gini Coefficient, 2000	-0.025	(0.030)	[-0.080,0.040]	8.9	162
Per Capita GDP, 2008	-0.021	(0.318)	[-0.650,0.590]	5.3	86
Poverty Rate, 2008	0.022	(0.079)	[-0.120,0.190]	6.3	108
BF Coverage, 2008	-0.013	(0.111)	[-0.240,0.200]	8.0	140
SS Employees, 2009	0.008	(0.318)	[-0.650,0.600]	5.2	80
Visits per Family, 2006-08	-0.086	(0.273)	[-0.600,0.470]	5.5	86
SS Bureaucracy ABOVE Median Size					
Mayor's Age	5.406	(3.230)	[-0.620,12.040]	10.2	155
Mayor's Gender	0.026	(0.146)	[-0.250,0.330]	8.9	142
Mayor's Education	-0.223	(0.248)	[-0.740,0.230]	7.7	119
M. Background: Public Sector	-0.010	(0.094)	[-0.190,0.180]	7.5	117
Mayor's Party: PT	-0.147	(0.124)	[-0.410,0.070]	6.0	94
Mayor's Party: PSDB	0.081	(0.166)	[-0.240,0.410]	9.2	142
Mayor's Party: Left	-0.300	(0.171)	[-0.660,0.010]	9.8	150
Gini Coefficient, 2000	0.026	(0.027)	[-0.030,0.080]	11.6	172
Per Capita GDP, 2008	0.236	(0.352)	[-0.420,0.960]	6.8	104
Poverty Rate, 2008	0.032	(0.074)	[-0.110,0.170]	7.8	120
BF Coverage, 2008	0.011	(0.061)	[-0.110,0.130]	8.8	139
SS Employees, 2009	-0.130	(0.293)	[-0.720,0.430]	9.7	150
Visits per Family, 2006-08	0.168	(0.276)	[-0.380,0.700]	7.5	113

The treatment effect is having a mayor with reelection incentives. Robust standard errors in parenthesis and 95% confidence intervals in brackets for optimal bandwidths and for a linear polynomial (Cattaneo, Idrobo, and Titiunik, 2020).

Table A.XIII: Covariate balance at the discontinuity (Subset 2)

Dependent Variable:	Coeff.	S.E.	C.I.	Band.	Obs.
Health Visits BELOW Median					
Mayor's Age	1.902	(4.451)	[-6.790,10.660]	6.0	94
Mayor's Gender	-0.025	(0.056)	[-0.130,0.090]	10.7	167
Mayor's Education	0.082	(0.254)	[-0.400,0.590]	7.0	114
M. Background: Public Sector	-0.001	(0.159)	[-0.300,0.320]	7.2	117
Mayor's Party: PT	-0.040	(0.111)	[-0.250,0.190]	6.2	96
Mayor's Party: PSDB	0.196	(0.170)	[-0.160,0.510]	6.8	109
Mayor's Party: Left	-0.239	(0.190)	[-0.620,0.120]	10.7	167
Gini Coefficient, 2000	-0.031	(0.033)	[-0.100,0.030]	9.6	155
Per Capita GDP, 2008	0.257	(0.271)	[-0.250,0.810]	9.2	150
Poverty Rate, 2008	-0.066	(0.068)	[-0.200,0.070]	9.6	155
BF Coverage, 2008	-0.026	(0.098)	[-0.220,0.160]	6.8	109
SS Employees, 2009	-0.913	(0.531)	[-2.080,0.000]	5.1	80
Visits per Family, 2006-08	-0.057	(0.134)	[-0.330,0.190]	10.1	162
Health Visits ABOVE Median					
Mayor's Age	7.864	(4.442)	[-0.310,17.110]	9.2	146
Mayor's Gender	-0.086	(0.160)	[-0.400,0.230]	9.9	153
Mayor's Education	-0.253	(0.272)	[-0.830,0.240]	6.8	104
M. Background: Public Sector	0.000	(0.004)	[0.000,0.020]	3.7	56
Mayor's Party: PT	-0.142	(0.181)	[-0.510,0.200]	9.4	146
Mayor's Party: PSDB	-0.105	(0.109)	[-0.340,0.090]	6.1	93
Mayor's Party: Left	-0.209	(0.201)	[-0.640,0.150]	6.8	104
Gini Coefficient, 2000	0.017	(0.035)	[-0.050,0.090]	8.0	124
Per Capita GDP, 2008	-0.108	(0.305)	[-0.740,0.450]	7.2	114
Poverty Rate, 2008	0.052	(0.064)	[-0.070,0.180]	7.4	116
BF Coverage, 2008	0.029	(0.074)	[-0.120,0.170]	10.3	159
SS Employees, 2009	-0.323	(0.472)	[-1.260,0.590]	10.4	159
Visits per Family, 2006-08	0.307	(0.204)	[-0.080,0.720]	6.1	95

The treatment effect is having a mayor with reelection incentives. Robust standard errors in parenthesis and 95% confidence intervals in brackets for optimal bandwidths and for a linear polynomial (Cattaneo, Idrobo, and Titiunik, 2020).

Table A.XIV: Loss of benefit when registered by a partisan of the opposition with just eligible income

	Dependent Variable:			
	Eligible	Just eligible	Change in benefit	
	(1)	(2)	(3)	(4)
Partisan	0.004	-0.003	-0.005	-0.006
S.E.	(0.008)	(0.005)	(0.008)	(0.008)
Just eligible			-0.012	-0.012
S.E.			(0.007)	(0.007)
Partisan x Just eligible				0.013
S.E.				(0.042)
Observations	116436	116436	116436	116436

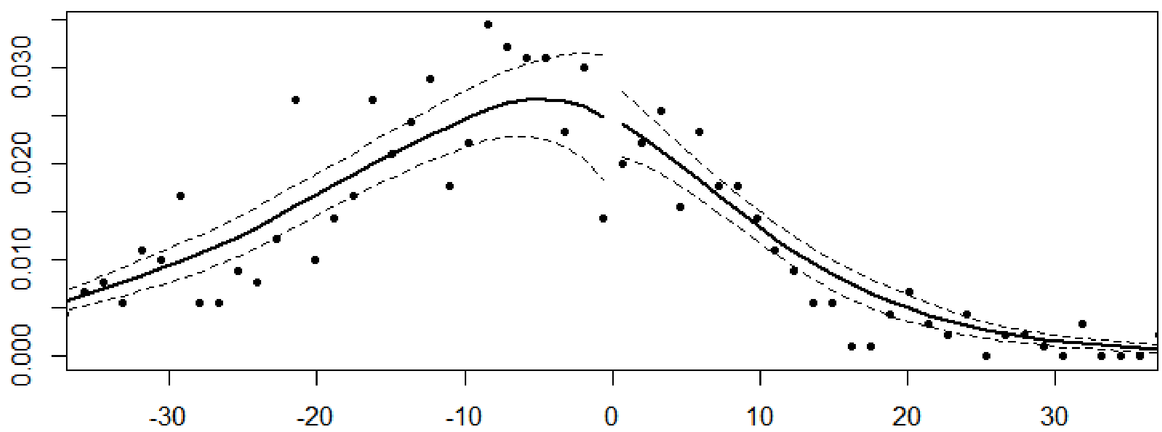
Cluster-robust standard errors by both municipality in parenthesis. All regressions include fixed effects by municipality and by quarter-year of enrollment (time trends), and the household-level covariates described in the text.

Table A.XV: Loss of benefit when registered by a partisan of a lame duck mayor with just eligible income

	Dependent Variable:			
	Eligible	Just eligible	Change in benefit	
	(1)	(2)	(3)	(4)
Partisan	-0.002	0.002	-0.002	-0.002
S.E.	(0.007)	(0.003)	(0.007)	(0.008)
Just eligible			-0.005	-0.005
S.E.			(0.005)	(0.005)
Partisan x Just eligible				0.002
S.E.				(0.037)
Observations	305258	305258	305258	305258

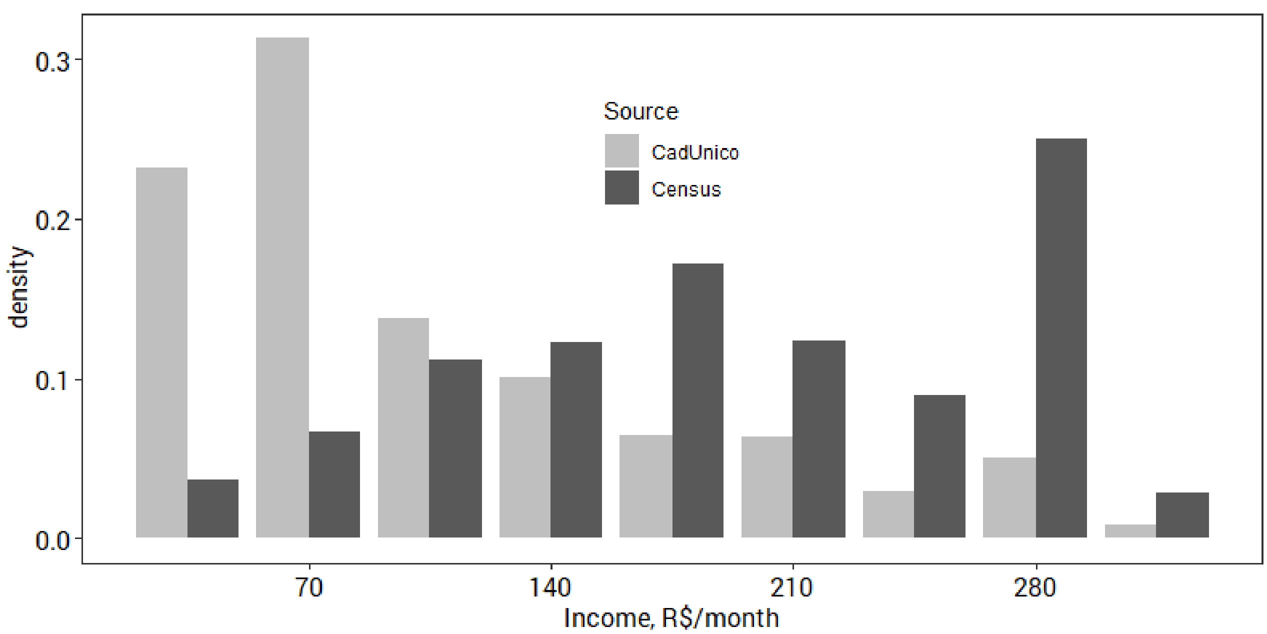
Cluster-robust standard errors by both municipality in parenthesis. All regressions include fixed effects by municipality and by quarter-year of enrollment (time trends), and the household-level covariates described in the text.

Figure A.I: McCrary test of the manipulation of the running variable



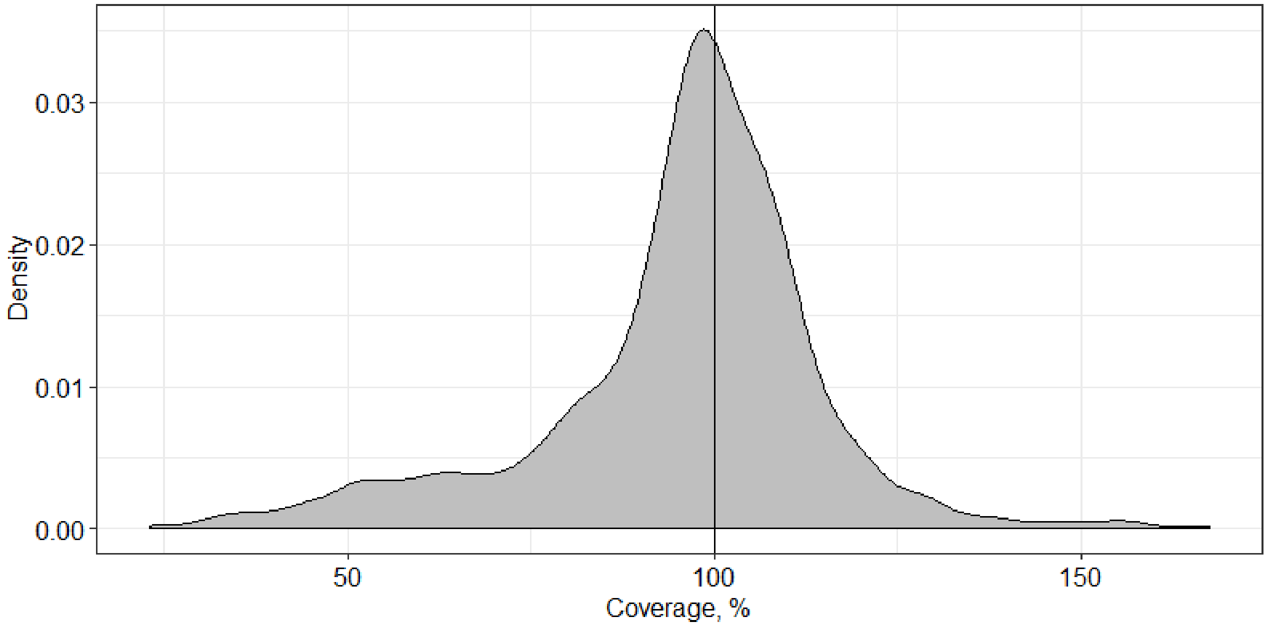
The p-statistic equals 0.96.

Figure A.II: Distribution of the reported income: CadUnico vs. Census



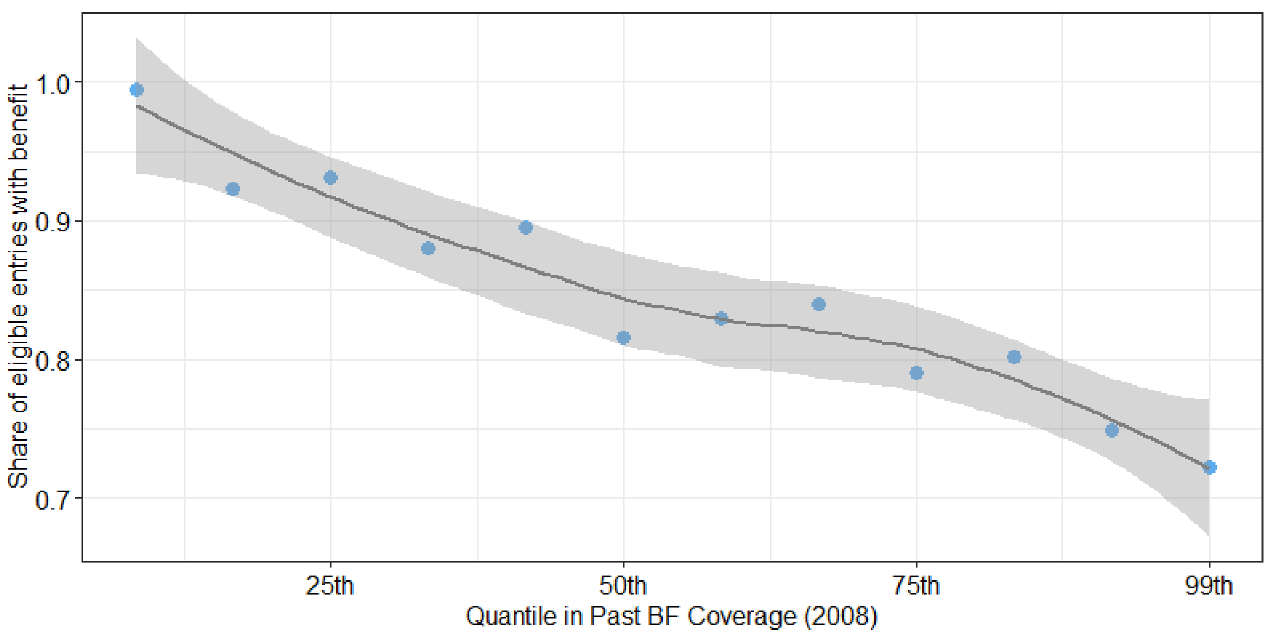
Data from the sample of the 2010 Census, and CadUnico updated as of Dec 2012. For presentation purposes, it excludes the households that reported zero income, which are similar in both registries.

Figure A.III: Distribution of the pre-existing BF coverage in the sample (2008), by municipality



Coverage is calculated as the number of local BF benefits divided by the coverage target set by MDS for the municipality. For presentation purposes, the plot excludes one outlier with coverage of 217%..

Figure A.IV: Less benefits are approved for eligible families in high-coverage municipalities



The y-axis contains the share of eligible entrants in CadUnico, in 2009-12, that had the benefit approved before Dec 2012. The x-axis aggregates municipalities according to their pre-existing coverage levels.