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 of resources, which might jeopardize both distributional efficiency and electoral competition. 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 nearly 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. This inclusion also improves the performance of incumbents in their reelection attempt. Evidence from both a survey with 11,000+ households and the heterogeneity in the main results suggests that the poor fails to keep mayors accountable due to the lack of precise information about the policy. This mechanism is framed within the context of a simple accountability model. Finally, anomalous income reporting patterns in the registry also show that this electorally-driven fraud 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 local 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 the consequent level of electoral competition.[3] Nevertheless, this question is especially relevant for the many developing democracies that have decentralized policy implementation in an environment where nonprogrammatic distribution is still electorally attractive.

This paper examines 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). Access to BF is programmatic, i.e., all households self-reporting monthly *per capita* income below R$140 can enroll. The program is both financed and primarily run by the federal government, which approves individual benefits and pays them directly to the beneficiaries, without intermediaries. Not surprisingly, voters often associate the program with that sphere of government, and to President Lula

[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.

(PT) and his party (Sugiyama and Hunter, 2013; Zucco, 2013; Brollo, Kaufmann, and La Ferrara, 2017). Nevertheless, mayors are responsible for collecting and verifying the enrollment information, which gives them leeway to conduct one type of benefit misallocation: the inclusion of nonpoor, ineligible beneficiaries. Due to quotas, these households effectively take the benefit from the deserving poor.[4] Thus, this paper focuses on one single metric of program implementation for local administrations: whether or not BF benefits reach the eligible poor.

The mayor's problem is framed within a simple model where incumbents choose whether or not to put effort in screening beneficiaries. Positive (negative) screening means that less (more) nonpoor households receive the benefit, and the quality of targeting improves (deteriorates). Thus, mayors face the following trade-off: poor households, when excluded from the policy, punish the incumbent in the voting booth if they were informed about the program. These incentives generate 'good' accountability, and positive screening becomes attractive to mayors. Included nonpoor families, however, always prefer to reelect mayors that allow negative screening. In regards to this group, fraud is electorally attractive for mayors. This framework generates two straightforward predictions: (i) if the cost of effort is low enough, incumbents always manipulate screening; and (ii) they choose negative screening whenever excluded but eligible voters do not have enough information to hold them accountable.

The empirical exercise compares the quality of screening for mayors with and without reelection incentives during the 2009-2012 tenure. Here, the challenge is to overcome two sources of bias. First, there could be unobserved municipality characteristics that influence both BF implementation and election results. This is addressed with a regression discontinuity design (RDD) that compares municipalities were incumbents barely won the 2008 election (and were ineligible in 2012), to ones where they barely lost (and challengers were re-eligible). Second, Brazil imposes a limit of two consecutive terms, so second-term mayors (reelected incumbents) have more experience in the office than first-term ones. This is addressed by comparing only first- and second-term mayors in municipalities where the race was between an incumbent and a challenger that had previous experience in the same office.[5]

[4]This problem was recently emphasized by Minister Osmar Terra, which said: "Bolsa Família used to have waiting lists of millions 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.

As a primary measure of income reporting fraud, this paper uses a timely and unprecedented round of program cuts conducted immediately after the end of the mayoral tenure (first-half of 2013). The government excluded more than 700,000 households misrepresenting income in the program registry (CadUnico), on the basis of cross-checks with federal databases. Combined with the date of program entry for individual households obtained from CadUnico microdata, these cuts provide a particularly precise measure of irregular enrollment in all municipalities, for families that entered BF during the 2009-2012 electoral cycle.

The first set of results indicate that first-term mayors set nonzero screening effort, in line with their reelection incentives. What is more, these incentives trigger negative beneficiary screening in the context of the BF program. Under lame-duck mayors, the average post-audit coverage reduction was 1.3 percentage points (pp). In the presence of reelection incentives, these cuts were 5.3pp higher (4x higher). To put this number in perspective, it corresponds to more than twice the total cost of running the overall program enrollment and verification system at the municipal level, and nearly five times the average budget for mayoral campaigns.

This failure of accountability could seem surprising at first, given that BF is arguably the most visible government policy in Brazil. Nevertheless, this paper shows alternative evidence, in line with the theory, that many eligible families lack the most basic information on how to obtain the benefit, despite having superficial program knowledge. First, a 2009 government-sponsored survey with 11,000+ households shows that 40% of respondents were not receiving benefit, in spite of considering themselves eligible. Among this group, only half ever attempted to enroll or talk to a public employee about it, and they were significantly more likely to have obtained BF knowledge form a second-hand, unofficial source. Second, local BF promotion and enrollment is handled by the local departments of Social Services in 97% of the cases. The results show that, in locations with less employees assigned to this role, the effects of reelection incentives on negative screening are much higher (a 9.5pp cut in benefits).

This paper also examines the electoral returns of negative screening. Using a feature of CadUnico that allows me to link enrolled households to voting machines, I examine the correlation between the number of post-audit BF beneficiaries to votes for the incumbent mayors in the subsequent mayoral election in 2012. Using only variation across voting machines in the same polling station, I show that incumbents gain significantly more votes (vs. 2008) in machines where poor voters had less BF benefits, post-audit.

The last empirical exercise provides alternative evidence of negative screening in the presence of reelection incentives, based on anomalous patterns of income reporting around the eligibility threshold.[6] It shows that households that enrolled in municipalities where mayors are re-eligible were 24% more likely to report eligible income, among all households that enrolled within a small income interval around the eligibility threshold. Such effect on 'bunching' is only observed for this specific income level, and it is absent from the contemporaneous Census survey (2010). What is more, beneficiaries that were enrolled with this income pattern (just below the threshold) by a public servant politically connected to the mayor were more likely to have been excluded from the program by the 2013 cuts. The same pattern is not observed for enrollment by members of other parties, or in locations where mayors did not have reelection incentives, but it was stronger where the 2008 election was more competitive.

In addition to being related to the vast literatures on term limits and strategic spending cited before, this paper is closer to the applied work dealing directly with how local politicians influence the implementation of CCT 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.[7] In Brazil, two papers provide opposing evidence on how term limits affect local implementation of CCTs. Brollo, Kaufmann, and La Ferrara (2017) focus on the enforcement of the school attendance requirements for BF beneficiaries. They show how local politicians aligned with the President (and PT party) are more lenient when enforcing program conditionalities before elections. Although they investigate a different dimension of program implementation, and focus on politicians connected with PT at the federal level, their results are very much 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 lead mayors to implement better management practices in the policy, which lead to superior outcomes for the beneficiaries. These results are not fully comparable to the present paper, which focuses solely on the quality of

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

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

program targeting.[8] Nevertheless, they also emphasize what I see as 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 among the beneficiaries for their role in the policy implementation (p.673), and excluded but deserving households were highly informed about the program (p.683), which put them in a position to keep politicians accountable for policy 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 credit claiming over the institution of programmatic policies,[9] Imai, King, and Rivera (2018) argue the opposite. They suggest that "most policies studied in prior research are partly programmatic and partly clientelistic." This paper's main contribution to this discussion is to provide well identified empirical evidence that even in policies that have a largely programmatic design, politicians might exploit opportunities for manipulation and expropriation of political rents.

Mayors, Bolsa Família and Elections

Bolsa Família (BF) reaches roughly 20% of Brazilian households, with resources that represented 13% of all non-discretionary federal transfers to municipalities in 2008. What is more, it generates an average 50% increase on the monthly income of its targeted population. Different from many other CCTs (e.g. Mexico, Philippines), BF benefits are granted mainly based on self-reported income (Handa and Davis, 2006). 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, the *Cadastro Unico* (CadUnico).[10] However, only families with income

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

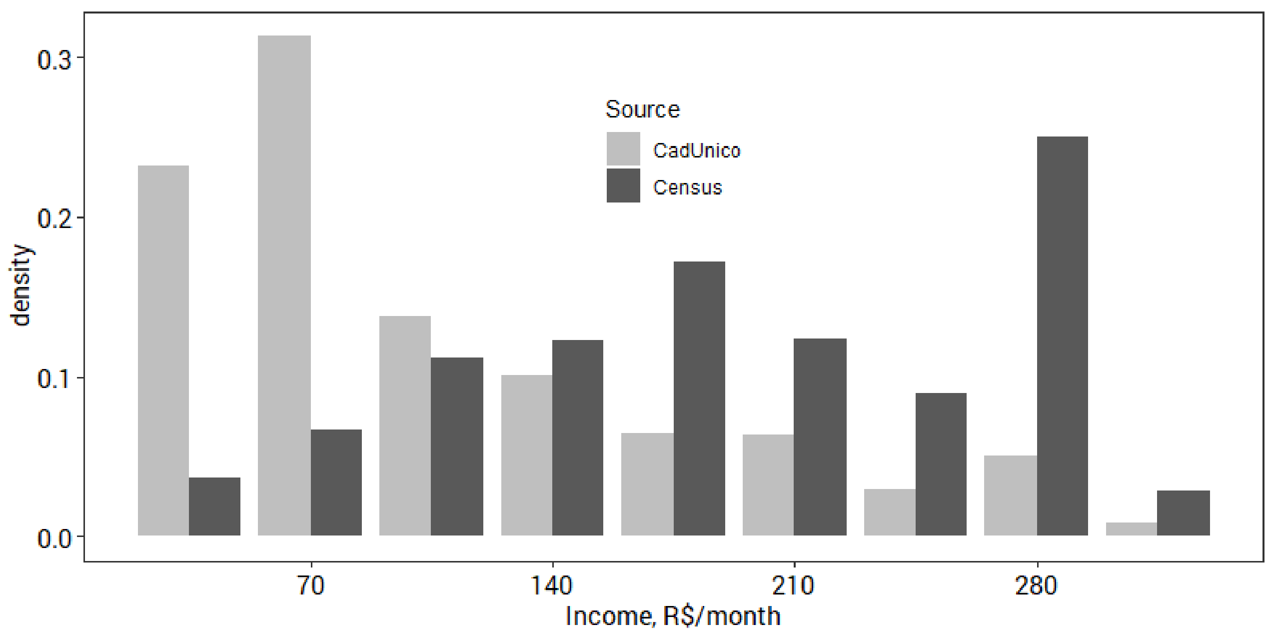
[9] This literature does not disentangle what is a result of (i) undeserved credit claiming (see an example in Cruz and Schneider (2017)); (ii) clientelistic misallocation of resources; (iii) or rewards for effort that signals the politician's ability.

[10] CadUnico is an integrated registry that contains a vast array of demographic information reported by households at the time of their enrollment/update. It can be accessed and updated by the local offices, and the data is used by the federal office to grant or deny BF benefits.

below R$140 are eligible to transfers.[11]

In any CCT program, policy makers faces a trade-off between inclusion and the quality of targeting. Simpler enrollment rules generally allow for more inclusion, but increase the risk of program fraud coming from ineligible households underreporting income. *Bolsa Família* (BF) was designed to prioritize inclusion over targeting (Soares, Ribas, and Osório, 2010). As an example, a 2016 audit by the Ministry of Social Development (MDS) found 1.1 million households irregularly receiving the benefit, given that their actual income was above the threshold (8% of all beneficiaries).[12] Figure 2 further illustrates this potential for fraud. It shows that households are much more likely to underreport income when registering in CadUnico than they are for the contemporaneous Census survey (2010), which is inconsequential for eligibility in any social program.

Figure 1: 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 registers.

[11]This was the eligibility threshold for the period between Aug 2009 and 2012, it was R$120 in the first six months of 2009. There are several types of BF benefits, the main ones being the basic and variable benefits. The basic benefit of R$70 is granted to all households with pc income below R$70, and it is not conditional. The variable benefit (R$32-R$38 per eligible household member) targets children (below 18) and pregnant women, and it is available to all households with eligible income, conditional on school attendance and health check-ups.

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

The federal government has limited tools to detect this type of program fraud. Even though the municipal BF administration is periodically audited as part of the CGU initiative that oversees mayors,[13] these audits reach less than 3% of all municipalities every year. In addition, 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 their declared value.[14] 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 simple enrollment rules, combined with the scale and scope of BF, make it an attractive tool for political manipulation by mayors. Even though it is the federal government that ultimately pays households directly through a debit card, municipal governments are effectively the gatekeepers of the CadUnico registry: they are responsible for enrollment, data collection, and for verifying the accuracy of the information.[15] In this context, the most effective tool to extract political rents from the BF program is negative screening, i.e., the inclusion of nonpoor, noneligible households in the roll of beneficiaries. Potential gains from simply fostering program expansion are limited for two main reasons. First, the number of benefits is limited by coverage quotas, both at the national and local levels.[16] Second, once they enter the payment list, poor (and legally eligible) households are much more likely to credit President Lula (PT, 2003-2010) and his party for the transfers (Sugiyama and Hunter, 2013; Zucco, 2013; Brollo, Kaufmann, and La Ferrara, 2017). At that point, only the federal government can cancel the benefit, and families are given information and resources (such as a toll free number) to denounce local program manipulation. This is not the case for noneligible households, which can have the benefit recalled at any time by local officials.

[13] Ferraz and Finan (2011) provide additional details on the CGU program in Brazil.

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

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

[16] The program went through two significant expansion periods. between inception (2003) and 2006, where roughly 6 million new families were added, and between 2009 and 2012, when the quotas were increased and other 2 million registered. Even though local coverage quotas are sometimes negotiable, the global program quota is always binding.

Not surprisingly, there are many reported attempts of program manipulation by local politicians for personal gain. In 2013, MDS found that 2,168 elected politicians enrolled themselves in BF, and were receiving benefits illegally.[17] In Santa Tereza de Goiás (GO), the public prosecutor indicted both the mayor and the local BF coordinator for a widespread fraud in the program, which consisted mainly of manufacturing false CadUnico entries to trigger the irregular payment of benefits.[18] In 2016, the mayor of Bocaina (PI) was impeached for vote buying, after offering the BF benefit in exchange for votes.[19]

Accordingly, this paper focuses on cases where mayors allow nonpoor households into BF if this improves their reelection chances. In this context, mayors always face a trade-off when setting program screening. Due to the quotas, the inclusion of nonpoor households necessarily implies the exclusion of eligible ones.[20] If the excluded poor has enough information to effectively blame local administrations for the negative screening, then we have a scenario of 'positive' accountability where mayors are rewarded by good program targeting. On the other hand, the included nonpoor are always better off under the incumbent if there is any chance that a newcomer is elected with a different screening policy.[21]

Theoretical Framework

This section proposes a simple theory to illustrate when good program implementation (positive screening) becomes an optimal strategy for reelection seeking incumbents. I use a basic accountability framework where an incumbent with reelection incentives exercises costly effort to influence the implementation of policy whenever this effort provides a signal to voters about the politician's type, and increases his reelection probability. Specifically, the present model deals directly with the incumbent's decision to manipulate program screening, i.e., to exercise effort in either denying or granting more benefits to nonpoor households.

[17] O Globo (Oct, 2013). See the news in Portuguese: http://goo.gl/3RsfaW.

[18] Jusbrasil (2014). See the news in Portuguese: http://goo.gl/a40TYX.

[19] Portal Saiba Mais (2016). See the news in Portuguese: http://bit.ly/2JplVjh.

[20] The program has, since 2006, kept an extensive waiting list for millions of households that reported eligible income, but were not receiving the benefit. See the following MDS press release: http://bit.ly/2Jpk6TF.

[21] This is in line with one of the basic principles of clientelistic politics: voters are more likely to reciprocate when politicians have the monopoly over the goods (Medina and Stokes, 2007), and when the benefit is reversible (Weitz-Shapiro, 2014).

Accordingly, consider a simple two-period model where mayors are limited to one reelection, and run against a challenger at the end of period 1. The local population is comprised of P poor and N nonpoor voters. A fixed number P of policy transfers (t) is available to poor citizens. The nonpoor population only obtains t by means of income underreporting fraud. The benefit provides utility $u(t)$ and $v(t)$ to poor and nonpoor, respectively; with $u(t) > v(t)$. 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 to incumbents, and generate a disutility of $\kappa s^2/2$.

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 therefore bounded by $[\bar{n} - P/N, \bar{n}]$. Politicians are one of two types: opportunistic or pro-poor. Pro-poor types always set the maximum possible level of positive screening ($\bar{n}$). Opportunistic politicians set a level that maximizes their chances of reelection in the first period, and $s = 0$ in period 2, if reelected. The prior probability that a politician is pro-poor is given by π.

Incumbents decide on the amount of screening in period 1, according to their type that is unknown to voters. Voters do not observe screening directly, but learn about the incumbent's type by their inclusion status. Voters are prospective, and they choose between the incumbent and a challenger based on their expected policy benefit in period 2, plus the challenger's overall popularity in the municipality given by ξ, assumed to be exogenous with uniform distribution in $[-1/2, 1/2]$.

Consider the prospects of all households included in the program in period 1. The deserving poor cannot differentiate between opportunistic and pro-poor incumbents, and they are also indifferent between candidates, given that they cannot be excluded from the policy by either politician. Nonpoor households, however, learn that the incumbent is opportunistic, and know that they lose the benefit in period 2 if a pro-poor type is elected. The excluded poor might also learn the type of the incumbent if they had the information that they were eligible, and are able to correctly attribute their exclusion to the local administration's screening policy. Define α_m as the share of the poor that are informed. Finally, both the excluded nonpoor and the uninformed poor vote solely based on the challenger's popularity. Thus, the expected benefit in period 2 for the relevant groups of voters is given below:

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

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 for misallocation. Thus, the probability that an incumbent is reelected is given by:

$$\mathbb{P}[n(\bar{n}-s)\pi\Big(v(t)-\alpha_m u(t)\Big)-\xi > \frac{1}{2}] \tag{2}$$

Incumbents maximize their reelection probability subject to the screening cost.[22] Given the distributional assumptions, the optimal screening for an interior solution is given by:

$$s^* = \frac{\pi n}{\kappa}\Big[\alpha_m u(t)-v(t)\Big] \tag{3}$$

The equation above provides a few insights on the mayor's decision. First, incumbents exercise effort in screening as long as there are net electoral gains. If the 'accountability' gains, expressed by $\alpha_m u(t)$, are canceled out by the political targeting effects ($v(t)$), mayors with reelection incentives set the same screening level as lame-duck mayors ($s=0$).

Second, it is easy to see that screening is negative only when $\alpha_m u(t) < v(t)$. This necessarily implies a severe asymmetry of information between included and excluded households. In other words, α_m must be small enough to compensate for the higher marginal utility the poor receives from the benefit. In words, 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 lack of information, which makes the inneficient and political targeting of resources more attractive to mayors.

[22] $\max_s \; n(\bar{n}-s)c\Big(\alpha_m u(t)-v(t)\Big)-\kappa\frac{e^2}{2}$

Data and Research Design

This paper tests the effects of reelection incentives on the quality of program implementation using data on the expansion in *Bolsa Família* (BF) enrollment during the mayoral tenure of 2009-2012.[23] Although the program reached full global coverage in 2006, the Ministry of Social Development (MDS) significantly increased the global target of benefits after 2009 to include more than 2 million additional families. In early 2009, municipalities in the sample covered only around 80% of their targeted households, on average, and had ample room to expand enrollment in the following four years.[24]

Given that CadUnico only records the household's reported (as opposed to actual) income, a direct measure of the quality of screening at the municipal level is not readily available. This paper takes 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 the CadUnico data. 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, and provide a reliable measure of income misreporting across locations, triggered by an event that was exogenous to the behavior of individual mayors.

Accordingly, the quality of local program screening is defined as the change in the total number of benefits between Jun 2013 and Dec 2012 in the municipality, only for households that enrolled during the previous mayoral administration in 2009-2012. The lists of households receiving payments at the end of that period (December, 2012), and after the audits (June, 2013), was obtained from the MDS. This information was linked to the CadUnico registry extracted in Dec 2012 by the NIS number, allowing me to observe which households enrolled during the period of interest. I also build three other variables that provide different measures of change in both enrollment and coverage for the period. *Newly Registered* is the ratio of new CadUnico enrollments in 2009-2012 to the local coverage target; *Newly Eligible* is the percentage of these CadUnico new entrants that declared income below the eligibility threshold, and *Change in Benefits* is calculated as the difference between the local number of

[23]The CadUnico microdata was only made available by MDS for December 2012.

[24]The average BF coverage for this sample was above 110% of the target at the end of the mayoral tenure in 2012. Municipal coverage targets are not binding, and there are many municipalities with coverage above 100% (MDS, 2012). Only the global coverage target of the program is binding.

BF benefits in 2012 and 2008, divided by the local coverage target.[25]

Mayors with reelection incentives were elected in 2008 for their first non-consecutive term.[26] I estimate the effect of reelection incentives by comparing municipalities ran by mayors elected for a first term (treatment group) to municipalities ran by mayors reelected for a second and last term (control group). A naive comparison using all 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 comes from the fact that second-term mayors are systematically different from first-term ones in both experience and proven competence. They have more consecutive years of experience on the job (by design), and they were reelected after spending at least one tenure in office. To correct for this problem, we use a sample of municipalities in which an incumbent ran against a challenger that has been mayor in that municipality during the period of 2001-2004. The two-term limit in Brazil only applies to consecutive terms, and running for higher office is a very uncommon career path for Brazilian mayors (Klašnja and Titiunik, 2017).

The second source of bias comes from unobserved municipal characteristics that could potentially increase the incumbent's reelection probability. 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 are confounded by the level of poverty across municipalities. This problem is addressed with a regression discontinuity design (RDD). The spirit of the RDD is to compare municipalities where incumbent mayors barely won the election (and therefore had no reelection incentives), to municipalities where they barely lost, and were replaced by first-term experienced candidates that could be reelected. This strategy provides a quasi-random assignment of reelection incentives in places where elections were close (Lee, 2008; Eggers et al., 2015).[27] The main estimating equation is shown below:

[25]This last variable is built using the total number of benefits in the municipality, based on the aggregated numbers reported monthly by MDS. Thus, it also includes households that enrolled in the program before 2009.

[26]The sample includes only locations where elections happens in one round. Only municipalities with more than 200,000 voters have elections in two rounds in Brazil. All election data comes from the Superior Electoral Authority (TSE) and includes election results and personal information of candidates such as party, gender, education, age and former occupation. Other demographic data comes surveys conducted by the Brazilian Institute of Statistics and Geography (IBGE). More details on the data gathering process are found in the online appendix (Section A on page 1).

[27]The application of the RDD to close elections has been widely used in Brazil (Boas and Hidalgo, 2011; Ferraz and Finan, 2011; Klašnja and Titiunik, 2017).

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

where for municipality m the outcome is denoted by y_m, reelection incentives are given by the dummy T_m, and the treatment effect is denoted by β_1. The margin of victory MV_m is the difference in percentage points between the winner and the runner-up. As usual, the local linear regression is weighted by the edge kernel, and estimated for a sample limited by a bandwidth around the discontinuity.[28] Figure A.I in the appendix shows that the sample is balanced for 18 variables that include characteristics of the municipality, elected party, and mayor. I emphasize that municipalities in both sides of the discontinuity have similar levels of BF coverage in 2008 (before the election), and also similar poverty levels. The data construction process is described in the appendix (Section A).[29]

Results

Table 1 shows the main results for the variables described above. The first (unnumbered) column presents the pre-treatment mean of each variable for a municipality at the threshold. Columns (1) though (3) show the effect of reelection incentives for three different bandwidths. The graphic representation of these effects for the screening variable can be found in Figure 2. In the appendix, I show the sensitivity of these estimates to the choice of polynomial, and the inclusion of covariates and state fixed-effects (see Table A.I). Unless otherwise noted, all comments are based on the results obtained with the optimal bandwidth (column 2).

The estimates clearly indicate that first-term mayors set nonzero screening effort, in line with their reelection incentives. Not only that, but there is also strong evidence that these incentives result in a negative screening level in the context of the BF program. As a result of the program cuts conducted in the first half of 2013, municipalities without reelection incentives in 2009-2012 had an average coverage reduction of 1.3pp for the households that entered in 2009-2012. However, in municipalities where the mayor could attempt reelection, these cuts

[28]The optimal bandwidth is calculated as the minimum bandwidth estimated using different specifications in the plug-in method proposed by Calonico, Cattaneo, and Titiunik (2014). As usual, we show results for different multiples of the optimal bandwidth in Table 1.

[29]As it is also usual in RD designs (McCrary, 2008), the appendix shows that the running variable is not being manipulated at the discontinuity (Figure A.IV).

were 5.3pp higher (4x higher). I highlight that the 2013 cuts were progressively executed from January through May (MDS, 2013a,b), which means that during this period some households in the waiting list were added to the program to replace the excluded families. The audit documents do not specify if these newly included households were also audited in the same way at that time. If they were not, the estimate here might be underestimating the true effect of reelection incentives on program mistargeting.

Table 1: Screening, Coverage and Enrollment in BF

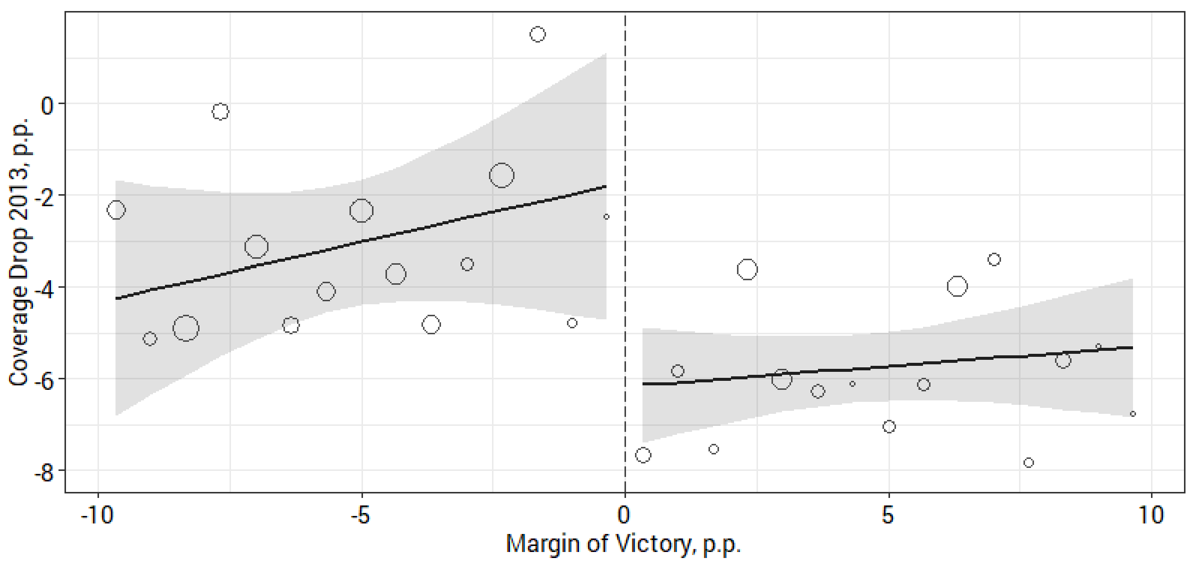
Dependent Variable:	Mean	(1)	(2)	(3)
Coverage Drop 2013	-1.345	-4.918	-5.306	-6.601
S.E.	(1.194)	(1.373)	(1.586)	(2.564)
Bandwidth	8.8	13.2	8.8	4.4
Observations	300	405	300	143
Change in Benefits, pp	28.645	6.208	2.279	-2.077
S.E.	(3.484)	(3.841)	(4.621)	(7.485)
Bandwidth	8.5	12.7	8.5	4.2
Observations	288	388	288	136
Newly Registered	54.732	5.662	3.074	2.349
S.E.	(5.486)	(6.105)	(7.267)	(10.567)
Bandwidth	8.2	12.3	8.2	4.1
Observations	279	382	279	132
Newly Eligible	80.820	-1.169	-1.277	-1.611
S.E.	(2.332)	(2.660)	(3.097)	(4.427)
Bandwidth	8.7	13.0	8.7	4.3
Observations	295	398	295	142
bandwidth rules	*optimal*	*1.5 x opt.*	*optimal*	*0.5 x opt.*

Heteroskedasticity robust standard errors in parenthesis. The mean is calculated for a municipality at the discontinuity ($MV = 0$) using only data in the control group (i.e. no reelection incentives).

To put this misallocation of resources in perspective, the BF budget for 2012 was around R$20 billion (roughly US$8bn). A loss of 5.3% in the program resources, although apparently small, corresponds to more than twice the total cost of running the CadUnico enrollment and verification system at the municipality level. In terms of resources available for persuading voters, the mistargeted amount corresponds to nearly five times the average budget for mayoral campaigns in Brazilian municipalities.

The remaining three variables in Table 1 provide support to this paper's narrative that reelection incentives affect BF implementation mainly trough screening, as opposed to differential effort in program enrollment. A potential alternative explanation for the main result would be that mayors with reelection incentives put effort in enrollment, so they can claim credit for the benefit's arrival. This could potentially explain the main result if higher enrollment, under certain conditions, also implies more nonpoor households in the program. Nevertheless, the estimates show that mayors with reelection incentives do not increase coverage more than lame-duck mayors. Also, they do not enroll more beneficiaries in CadUnico, and among the newly enrolled, they do not enroll more households with an income level that makes them eligible to BF. In short, theres is no evidence that administrative effort goes into program expansion as opposed to screening.

Figure 2: Coverage Drop in 2013 at the Discontinuity



The right-side reflects municipalities where the mayor had reelection incentives. The dots represent the average of each outcome variable for that specific value of margin of victory. The solid lines are the local linear fit.

Negative Screening and Lack of Program Information

In the context of the BF program, the results above show that income reporting fraud is a more attractive electoral tool to mayors than the efficient allocation of benefits. In line with the proposed theoretical framework, I interpret this lack of 'positive' accountability as a consequence of the poor quality of program information available for eligible households excluded from the policy (a low α_m in the model). In other words, either the excluded poor does not know that they are eligible or cannot correctly attribute blame for the program misallocation. This is does not come as a surprise. The very marketing campaign that makes voters much more likely to associate this policy with the federal government run by Lula and PT, might also undermine their ability to keep the local administrations accountable for good program performance.

In order to further explore this mechanism, this paper examines a survey from the Ministry of Social Development (MDS), and the pattern of heterogeneity in the results for the main variable. In 2009, MDS surveyed poor households with the intent to asses the impacts of BF (MDS, 2009) on several dimensions. Among the 11,372 respondent families, 6,053 were not receiving BF benefits at the time. Table 2 shows the main survey patterns.

Table 2: Program Knowledge Among the Poor Outside BF in 2009

	Know about the program (1)	Think they are eligible (2)	Enrolled in CadUnico (3)	Second hand information (4)	Talked to someone (5)
% of Respondents	93.0	76.3[a]	52.4	64.3[b]	46.1

[a] This ratio is 99% among the benefit recipients in the same period. [b] This ratio is 50% among the benefit recipients in the same period.

First, program knowledge is extremely high: 93% of the non-beneficiaries knew about BF, and 76% of them believed that they were eligible to the benefit. Second, general program knowledge does not seem to translate into enrollment. Among the ones that see themselves as eligible, only 52% ever enrolled in CadUnico, and only 46% ever talked to a local official about the program. This shows that even poor households that know about the policy lack the most basic information on how to obtain it. Third, the source of information seems to play a role in the gap between knowledge and enrollment. While 64.3% of the households in that group indicate that they heard about BF from a second-hand, unofficial source as media, family or friends, only 50% of the actual beneficiaries report the same. This again underscores the existing asymmetry in program information between excluded and included households.

If the gap between knowledge and enrollment is correlated with the household receiving first-hand information about this policy, than the size of the municipal infrastructure dedicated to spread information about BF can be used as a reasonable proxy for α_m.[30] In the vast majority of Brazilian municipalities (97% in 2013), Cadunico enrollment is, at least officially, one of the attributions of the Social Service (SS) workers. There is a lot of heterogeneity in how well local SS departments are staffed: some exist only on paper, others are fully staffed with dedicated full-time employees. The Brazilian Institute of Geography and Statistics (IBGE) published in 2009 the number of public servants employed in the field, for every municipality (IBGE, 2009). This paper classifies municipalities into small and large, according to the number of employees assigned to SS, and use this as a proxy for the ability of households to receive reliable enrollment information on BF. The larger the SS infrastructure, the more likely we are to observe the realization of the positive accountability scenario described in the model.[31]

Table 3 show the heterogeneous results for the variable *Coverage Drop 2013*, for different bandwidths. Figure 3 shows the separate plots for the two groups of municipalities (with small and large SS Infrastructure).[32] The results indicate that the negative screening effects of reelection incentives are much higher in municipalities that employed less SS workers in the beginning of the mayoral tenure (line 1). Accordingly, the effects are small and not statistically significant for the group where social services departments were fully staffed, indicating the absence of any incentives for negative program screening in these locations (line 4).

[30] Frey (2017), for example, shows that the number of family doctors in a municipality was key to spread information about BF enrollment in the early years of the policy.

[31] The number of employees in social services is also balanced at the discontinuity (Figure A.I in the appendix).

[32] Balance of covariates for each subsample is shown in Figure A.II and Figure A.III, in the appendix. Out of 68 coefficients shown in these plots, only one is statistically significant from zero.

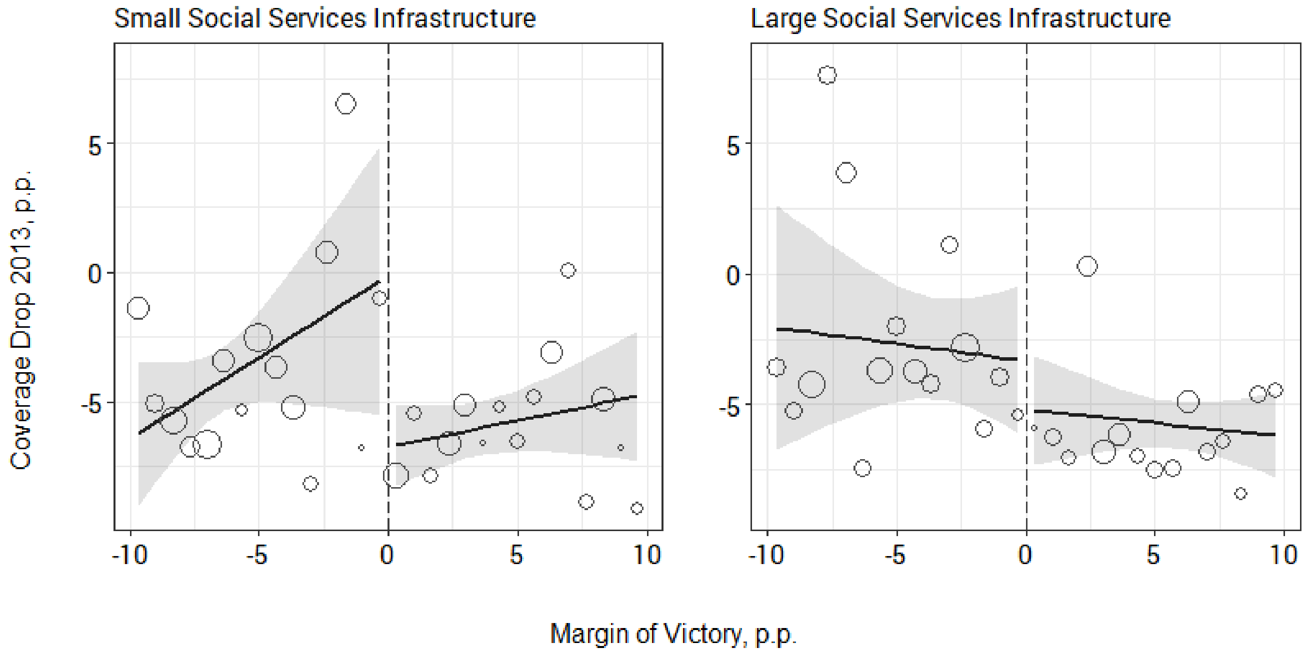
Table 3: Heterogeneity of Effects by the Level of Infrastructure in Social Services

Dependent Variable: *Coverage Drop* 2013	(1)	(2)	(3)
Reelection (a)	-7.278	-9.426	-11.226
S.E.	(1.861)	(2.085)	(3.238)
Large Social Serv. (b)	-3.632	-6.513	-9.151
S.E.	(2.051)	(2.365)	(3.981)
Reelection x Large Social Serv. (c)	5.002	9.473	11.242
S.E.	(2.788)	(3.259)	(5.625)
(a) + (c)	-2.276	0.047	0.016
S.E.	(2.076)	(2.505)	(4.599)
Bandwidth	13.2	8.8	4.4
Observations	405	300	143

Heteroskedasticity robust standard errors in parenthesis.

One of the challenges with this empirical exercise is that the local SS employment is not random across municipalities. On the contrary, it is likely correlated with local levels of poverty and income inequality. Hence, the estimates here might be simply a proxy for other channels, correlated with SS, that would impact the magnitude of the effects of reelection incentives on program screening. I approach this problem by showing that such heterogeneous effects are not present when the sample is split by the value of other variables that are potentially correlated with the local SS employments, namely: Poverty rate, inequality (gini), and per capita GDP. All variables produce results that are weak in magnitude, and not statistically significant (see the appendix, Table A.III).

Figure 3: Heterogeneous RDD Results



The right-side of each plot reflects municipalities where the mayor had reelection incentives. The dots represent the average of each outcome variable for that specific value of margin of victory. The solid lines are the local fit of a third degree polynomial.

Electoral Rewards

Do voters reward incumbents for negative screening? This paper attempts to shed some light on this question using the results of the subsequent mayoral election in 2012, and the variation in the number of BF beneficiaries across voting machines in the same polling station. In a nutshell, I show that incumbents gain more votes (vs. 2008) in machines where voters had less BF benefits, post-audit, already adjusting for the 2012 benefit status.

First-term mayors actually ran for reelection in 133 municipalities. In each of them, on average, there are 11.3 voting locations (polling stations), with 3.7 voting machines each. CadUnico contains the voting machine number for a significant part of its enrolled households (81% of the 2009-12 entrants), which allows me to observe the correlation between votes for the incumbent and the benefit status at the voting machine level. Brazilian voters 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, the electoral offices typically allocate the voter to the nearest location to her residence, in order to facilitate voting. Within that location, they are usually assigned to a machine that has less voters at that moment in time. In this sample, the average size of a machine is 264 voters, with a 56 standard deviation.[33]

The empirical strategy relies on the variation in BF benefit status across voting machines, within voting locations. For each voting machine v, I add the number of 2009-12 entrants that had the BF benefit in Dec 2012 ($BF12_v$), and Jun 2013 ($BF13_v$), and also the total number of votes for the incumbent VI_v. The variable $BF13_v$ measures the number of households that retained the benefit, post-audit, in that specific machine. After controlling for number of benefits in 2012, this variable becomes a proxy for the impact of the audit on benefits, and allows me to examine the correlation between negative program screening and incumbent performance. Accordingly, I estimate the equation below:

$$VI_v = \gamma_0 + \gamma_1 BF13_v + \gamma_2 BF12_v + \gamma_3 X_v + \delta_l + \epsilon_v \tag{5}$$

where δ_l are fixed-effects by voting location, and X_v are machine-level controls.[34]

The parameter γ_1 is expected to be negative. The intuition is straightforward: machines with less benefits in 2013 have more voters included in BF due to negative program screening.[35] Thus, they are also more likely to show an increase in support for the mayor if (nonpoor) voters reward the incumbent for being included in the program. The results are shown in Table 4. The first column estimates the effects in number of votes, the second uses ratios (it regresses the vote percentage on the percentage of entrants with benefits), and the third uses the natural logarithm of the number of votes.

[33] Voting machines have at most 400 voters, 500 in state capitals.

[34] They include the total number of voters in that machine in the elections of both 2012 and 2008, and the votes for the incumbent in the 2008 election. They also include the average value of some CadUnico variables for the 2009-12 entrants in that specific voting machine, namely: Declared Income, year of enrollment, declared monthly expenses with food, and existence of a public water connection in the household.

[35] Adjusted by the number of benefits in 2012.

Table 4: BF Benefits and Votes for the Incumbent

Dependent Variable: *Votes for the Mayor*	(1)	(2)	(3)
Benefit 2013	-0.243	-0.012	-0.013
S.E.	(0.136)	(0.005)	(0.005)
Benefit 2012	-0.086	0.003	-0.003
S.E.	(0.127)	(0.005)	(0.005)
Past Votes	0.326	0.362	0.380
S.E.	(0.016)	(0.016)	(0.018)
Voting Locations	1504	1504	1504
Observations (Voting Machines)	5537	5537	5537

Column (1) has the count of votes and benefits, columns (2) has ratios (vote percentage in the machine, and the ratio of entrants with benefits in 2012 and 2013), and column (3) has the log of counts.

All three specifications produce consistent estimates, which indicate that the voting machines that had less households with BF benefits in 2013 (post-audit, post-election) also had more votes for the incumbent in the 2012 election. Column (1) indicates that for every four marginal voters that 'lost' the benefit in 2013, there is a marginal increase of one vote for the incumbent when compared to the 2008 election.[36] Nevertheless, the magnitude of these estimates should be taken with a grain of salt, given the ecological nature of the inference made here. The main contribution of this section is to provide a robust correlation between BF benefits post-audit and votes for the incumbent, with reasonably benign identification assumptions on the nature of the allocation of households across voting machines.[37]

Patterns of Income Reporting Fraud

The primary measure of income underreporting fraud (or negative screening) in this paper comes from a country-wide audit that excluded 700,000 households from the BF program. In this section, I provide alternative evidence that reelection incentives lead incumbents to include noneligible households in BF, in the spirit of the literature that uses statistical anomalies

[36]Even though the estimated effect is less precise in column (1) when compared to the other two, it provides a more straightforward interpretation of the magnitude of the results

[37]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.

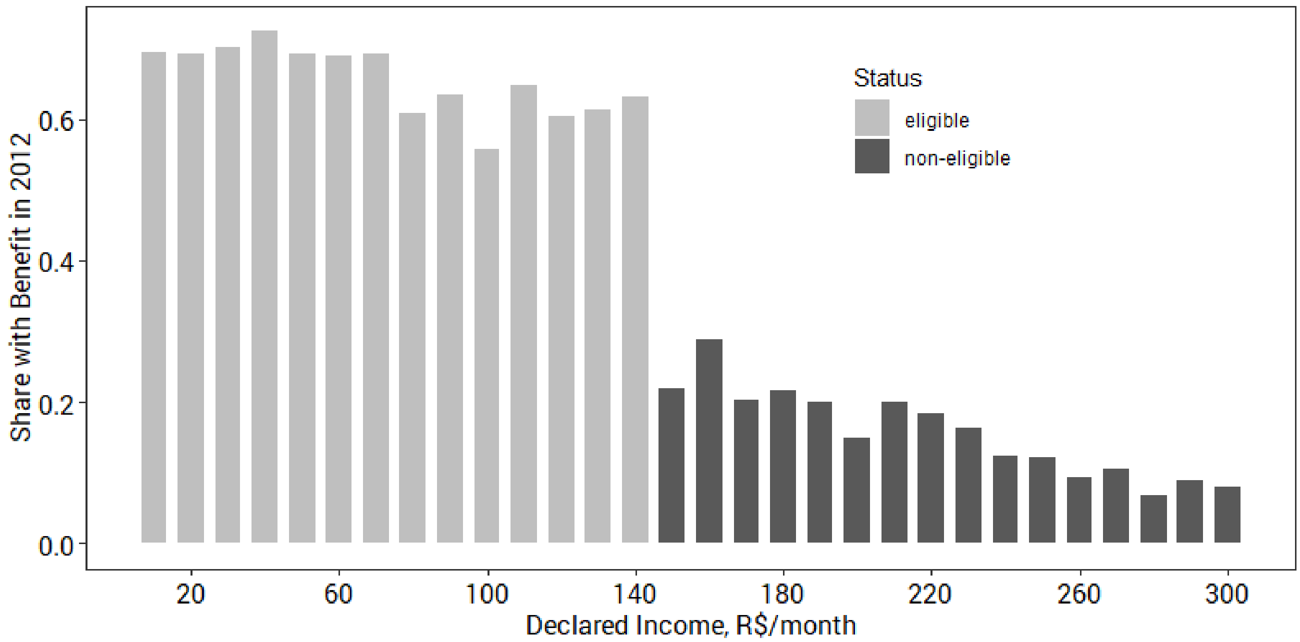
to detect electoral fraud (Cantú, 2014; Rundlett and Svolik, 2016). In a nutshell, within a small range around the income eligibility threshold (R$140), households are much more likely to register an income that makes them eligible to the BF benefit in municipalities where reelection incentives are present. The idea is that this unusual bunching of households at an income level just below eligibility represents evidence of misreporting fraud if and only if this pattern is neither observed in other income levels (CadUnico) nor in other contemporaneous surveys for the same R$140 level (as the 2010 Census).

Accordingly, for every municipality, the variable *Just Eligible* is defined as the share of families reporting eligible income, out of all that declared income in a small interval around the threshold (for example: 10% higher or lower). Even though enrollment with eligible income does not necessarily guarantee the benefit,[38] Figure 4 shows that the vast majority of the households that enrolled as such in 2009-2012 were receiving the benefit in Dec 2012.[39] It is not surprising that nearly 80% of all CadUnico households report eligible income, on average.

[38]The benefit is ultimately approved by the federal government. The actual approval can take several months, and it might be subject to the analysis of municipal quotas. Also, the global quota of benefit is binding, and households might be put on a waiting list even if their municipality is below the local target.

[39]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 reported after registration are still eligible to keep their former benefit for a grace period that could last up to two years.

Figure 4: Share of Households Receiving the Benefit by Declared Income



The dots represent the average of households receiving the BF benefit for that specific value of declared income. R$140 is the threshold for benefit eligibility, as established in 2009. The plot includes households enrolled between Aug 2009 and Dec 2012. The solid lines are the local fit of a second degree polynomial.

Table 5 shows the estimation of equation 4, using the variable *Just Eligible* as the outcome. It shows the results for different bandwidths, and specifies the variable for three ranges around the threshold. The first line shows that, in the absence of reelection incentives, 64% of households that declare income in an interval of 10% around the eligibility threshold, declare themselves eligible. This share is 15pp higher (a 24% effect) when the mayor has reelection incentives. The results are consistent across both bandwidth and income range around the threshold. Table A.II in the appendix shows falsification tests using other income thresholds in CadUnico, and also the R$140 income level in the Census survey taken in all municipalities in 2010, which is not consequential for any social program eligibility. These tests show no evidence of placebo effects outside the eligibility threshold.

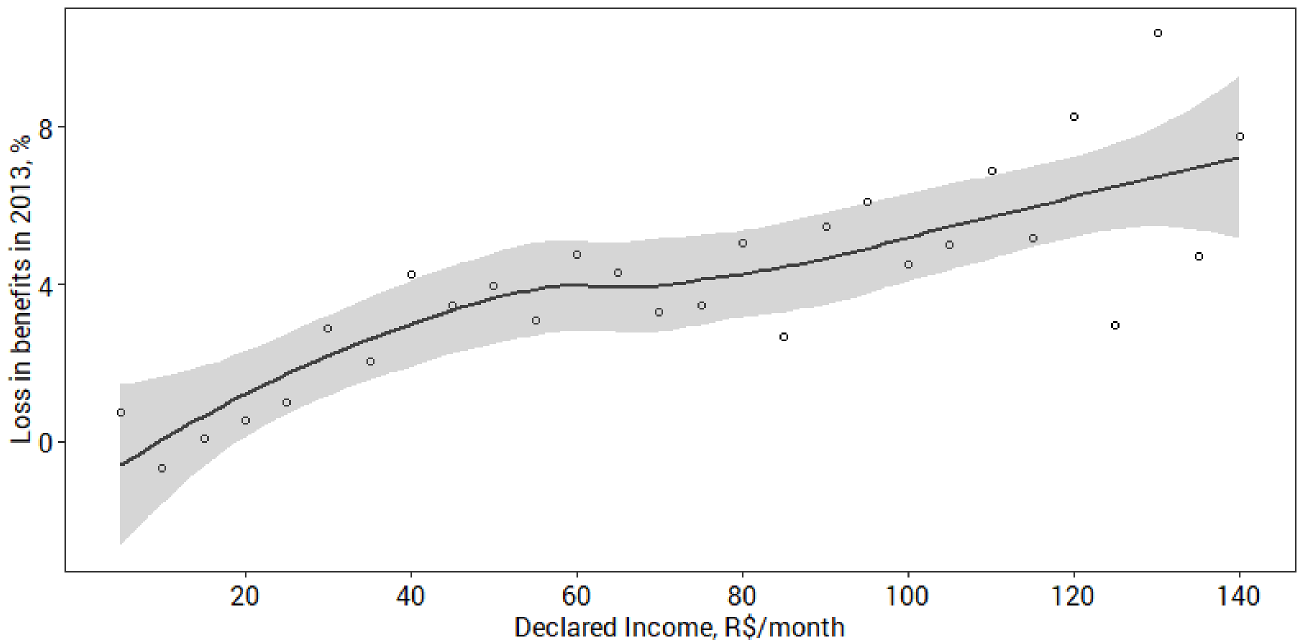
Table 5: Patterns of Reported Income Around the Eligibility Threshold

Dependent Variable:	Mean	(1)	(2)	(3)
Just Eligible (± 5R$)	67.703	12.853**	18.425**	31.003**
S.E.	(3.870)	(4.396)	(5.140)	(7.691)
Bandwidth	8.7	13.1	8.7	4.4
Observations	271	368	271	128
Just Eligible (± 7R$)	63.657	10.379**	15.266**	27.393**
S.E.	(3.623)	(4.234)	(4.829)	(7.498)
Bandwidth	9.0	13.5	9.0	4.5
Observations	288	391	288	136
Just Eligible (± 10R$)	70.175	4.662	8.824**	15.520**
S.E.	(3.024)	(3.555)	(4.027)	(5.924)
Bandwidth	8.6	12.9	8.6	4.3
Observations	283	384	283	135
Entrants within range (± 5R$)	1.333	0.393	0.565	0.588
S.E.	(0.301)	(0.348)	(0.400)	(0.524)
Bandwidth	9.0	13.4	9.0	4.5
Observations	305	411	305	145
bandwidth rules	*optimal*	1.5 *x opt.*	*optimal*	0.5 *x opt.*

Heteroskedasticity robust standard errors in parenthesis. The mean is calculated for a municipality at the discontinuity ($mv = 0$) using only data for control group (i.e. no reelection incentives).

I emphasize that this type of fraud is obviously not the only one possible, given that non-poor households could rather misreport any level of eligible income. Thus, although the estimates here strongly suggest that this type of enrollment is consistent with CadUnico fraud, their magnitude cannot be directly mapped to the main results obtained in Table 1. Nevertheless, this finding is also consistent with Figure 5 that shows that households that reported eligible income closer to the threshold were more likely to lose the benefit after the 2013 audit.

Figure 5: Loss of Benefit Post-Audit by Income Level



Percentage of households that lost the benefit after the audit, according to their reported income as of Dec 2012.

Additional Evidence of Political Manipulation in Enrollment

For many families that enrolled in CadUnico in 2009-2012, the register records the name of the interviewer that collected the household's data. In this section, I combine the names of these interviewers with the party membership rolls in Brazil,[40] and show that households enrolled by partisans of the mayor with a just eligible income level (as defined before) were more likely to lose the benefit after the 2013 audit.

I use the same household-level data from the exercise on electoral returns of screening, with the subset for which the interviewer's name is available. First, I test whether the partisanship of the interviewer is correlated with the enrollment status. The results are shown in the Columns (1) through (3) of Table 6. In short, partisan (of the mayor) interviewers are no more or less likely to enroll households with an eligible status (column 1), or within a small range around the threshold (column 2), or with just eligible income (column 3). All regressions include fixed-effects for municipality-quarter, and therefore employ only the variation across

[40]Around 10% of all Brazilian voters are formally affiliated to parties, and this group is generally seen as the core supporters and party activists (Speck, Braga, and Costa, 2015).

households that registered in the same municipality, in the same 3-month period.[41]

Table 6: Loss of Benefit When Registered by a Partisan of the Mayor

	Dependent Variable:				
	Eligible	Around Threshold	Just Eligible	Change in Benefit	
	(1)	(2)	(3)	(4)	(5)
Partisan (a)	0.005	0.002	0.003	0.011	0.012
S.E.	(0.008)	(0.004)	(0.004)	(0.007)	(0.007)
Just Eligible (b)				-0.024	-0.022
S.E.				(0.010)	(0.010)
Partisan x Just Eligible (c)					-0.047
S.E.					(0.031)
(b) + (c)					-0.069
S.E.					(0.029)
Observations	72876	72876	72876	72876	72876

Cluster-robust standard errors by municipality in parenthesis. All regressions include quarter-municipality fixed effects, and the household-level covariates described in the text.

The most interesting results are shown in columns (4) and (5). The outcome variable is defined as the change in benefit generated by the audit ($BF13_h - BF12_h$, for household h). I regress this variable on (i) a dummy that assumes value of one when the interviewer is a partisan of the mayor (*Partisan*), (ii) on a dummy indicating whether the household enrolled in the just eligible range (income $\in [R\$131, R\$140]$),[42] and (iii) their interaction. Line 1 in column (4) shows that households enrolled by a partisan of the mayor are no less likely than other households to lose the benefit post-audit. However, line 4 in column (5) shows that, among these households, the ones with just eligible income are 6.9pp more likely to have lost the benefit in 2013 than the ones enrolling with other incomes.

In the appendix, I show that this pattern is: (i) stronger for municipalities where the previous elections were more competitive; (ii) non-existent in municipalities without reelection incentives; and (iii) also non-existent when the *Partisan* variable is calculated with interviewers that belong to the the main opposition party.

[41] As before, I also control for the households declared income, monthly expenses with food, and existence of water connection in the household.

[42] Fixed-effects and covariates are the same as above.

Conclusion

In the context of a conditional cash transfer program (CCT) in Brazil, the present 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. The trade-off is straightforward: while nonpoor households might 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 RDD estimates indicate that politically-motivated screening targeting trumps 'good' accountability in this policy: mayors with reelection incentives are four times more likely to include nonpoor households into the policy. This inclusion also improves the performance of incumbents in their reelection attempt, and it is more common in locations where households do not have easy access to obtain program information. Finally, anomalous income reporting patterns also 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.[43] The present 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 triggering 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.

Nevertheless, 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

[43] 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.

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.

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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. All households that had a registry marked as invalid were excluded, as they were not enrolled in Dec 2012. I also 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 for which the reported income is in line with the registry threshold, i.e., below R$311 in 2012. Around 8% of the total households had reported income above this threshold. This could have been caused by registration errors, or the fact that some households can be enrolled in CadUnico with higher pc income under special circumstances.[44]

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 entry. 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 of households in each location.

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

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.

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 aggregate is also used as the (binding) cap of benefits provided to the population. The cap valid for 2008 was calculated by MDS using the PNAD 2006 survey, the cap valid at the end of the mayoral tenure (2012) was based on the 2010 Census and first implemented in 2011.

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).

B Additional Tables and Figures

Table A.I: Robustness to Controls, and Polynomial Specification

Dependent Variable:	(1)	(2)	(3)	(4)
Coverage Drop 2013	-5.306	-4.605	-6.433	-5.040
S.E.	(1.586)	(1.136)	(2.223)	(1.560)
Bandwidth	8.8	8.8	11.3	11.3
Observations	300	300	355	355
Change in Benefits, pp	0.023	0.081	-0.014	0.064
S.E.	(0.046)	(0.046)	(0.059)	(0.057)
Bandwidth	8.5	8.5	12.3	12.3
Observations	288	288	378	378
Newly Registered	3.074	5.458	0.865	5.902
S.E.	(7.267)	(6.343)	(9.839)	(8.469)
Bandwidth	8.2	8.2	11.0	11.0
Observations	279	279	351	351
Newly Eligible	-1.277	0.941	-2.268	0.872
S.E.	(3.097)	(2.059)	(4.308)	(2.852)
Bandwidth	8.7	8.7	11.3	11.3
Observations	295	295	357	357
Degree of Polynomial	*1st*	*1st*	*2nd*	*2nd*
Controls	No	Yes	No	Yes

Heteroskedasticity robust standard errors in parenthesis. The list of controls includes all variables from Figure A.I.

Table A.II: Placebo Tests for the Just Eligible Variable

Dependent Variable:	(1)	(2)	(3)	(4)
Just Eligible (Placebo 1)	79.424	-3.708	-1.808	-0.559
S.E.	(3.701)	(4.074)	(4.976)	(7.984)
Bandwidth	11.4	17.1	11.4	5.7
Observations	320	429	320	166
Just Eligible (Placebo 2)	57.172	-3.844	-11.629	-18.019
S.E.	(4.917)	(5.701)	(6.656)	(10.300)
Bandwidth	9.0	13.5	9.0	4.5
Observations	273	370	273	127
Just Eligible (Placebo 3)	66.920	1.231	1.966	-3.316
S.E.	(5.680)	(6.281)	(7.583)	(11.844)
Bandwidth	8.5	12.7	8.5	4.2
Observations	223	298	223	111
Just Eligible (Census)	69.060	6.702	6.884	5.765
S.E	(3.979)	(4.644)	(5.286)	(8.107)
Bandwidth	8.6	12.9	8.6	4.3
Observations	277	373	277	131

Heteroskedasticity robust standard errors in parenthesis. All results for a range of R$5 around the income eligibility threshold.

Table A.III: Falsification Test for the Heteroegeneous Results

Dependent Variable: *Coverage Drop* 2013			
Interaction Variable:	Poverty Rate	per capita GDP	Inequality
	(1)	(2)	(3)
Reelection (a)	-4.711	-6.860	-5.660
S.E.	(2.157)	(2.308)	(2.224)
Variable (b)	3.412	-4.471	0.119
S.E.	(2.406)	(2.390)	(2.395)
Reelection x Variable (c)	-1.413	2.937	0.968
S.E.	(3.189)	(3.176)	(3.182)
(a) + (c)	-6.124	-3.923	-4.692
S.E.	(2.349)	(2.181)	(2.277)
Bandwidth	8.8	8.8	8.8
Observations	300	300	300

Heteroskedasticity robust standard errors in parenthesis.

Table A.IV: Loss of Benefit When Registered by a Partisan of the Opposition Party

	Dependent Variable:				
	Eligible	Around Threshold	Just Eligible	Change in Benefit	
	(1)	(2)	(3)	(4)	(5)
Partisan (a)	0.006	0.002	0.001	-0.009	-0.009
S.E.	(0.011)	(0.002)	(0.003)	(0.007)	(0.008)
Just Eligible (b)				-0.024	-0.025
S.E.				(0.010)	(0.010)
Partisan x Just Eligible (c)					0.031
S.E.					(0.045)
(b) + (c)					0.006
S.E.					(0.045)
Observations	72876	72876	72876	72876	72876

Cluster-robust standard errors by municipality in parenthesis. Opposition party is defined as the runner-up party in the 2008 election. All regressions include quarter-municipality fixed effects, and also control for the declared income of households.

Table A.V: Loss of Benefit When Registered by a Partisan of the Mayor (No Reelection Incentives)

	Dependent Variable:				
	Eligible	Around Threshold	Just Eligible	Change in Benefit	
	(1)	(2)	(3)	(4)	(5)
Partisan (a)	-0.004	-0.002	0.001	0.004	0.004
S.E.	(0.007)	(0.003)	(0.003)	(0.007)	(0.007)
Just Eligible (b)				-0.014	-0.015
S.E.				(0.007)	(0.007)
Partisan x Just Eligible (c)					0.008
S.E.					(0.040)
(b) + (c)					-0.007
S.E.					(0.039)
Observations	210347	210347	210347	210347	210347

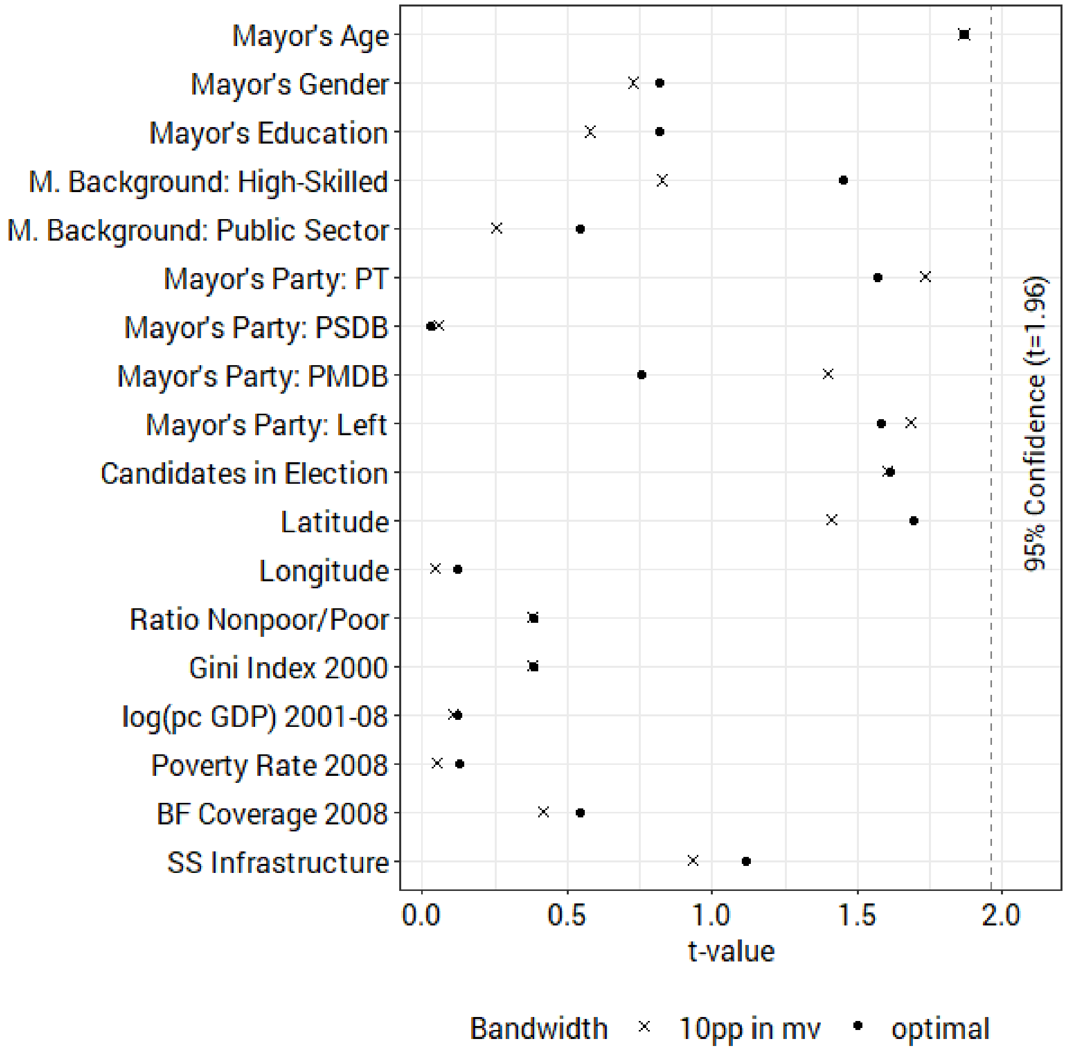
Cluster-robust standard errors by municipality in parenthesis. All regressions include quarter-municipality fixed effects, and the household-level covariates described in the text.

Table A.VI: Loss of Benefit When Registered by a Partisan of the Mayor (Competitive Environments)

	Dependent Variable:				
	Eligible	Around Threshold	Just Eligible	Change in Benefit	
	(1)	(2)	(3)	(4)	(5)
Partisan (a)	0.000	0.004	0.006	0.010	0.012
S.E.	(0.011)	(0.005)	(0.005)	(0.011)	(0.011)
Just Eligible (b)				-0.025	-0.022
S.E.				(0.012)	(0.013)
Partisan x Just Eligible (c)					-0.087
S.E.					(0.035)
(b) + (c)					-0.109
S.E.					(0.033)
Observations	50928	50928	50928	50928	50928

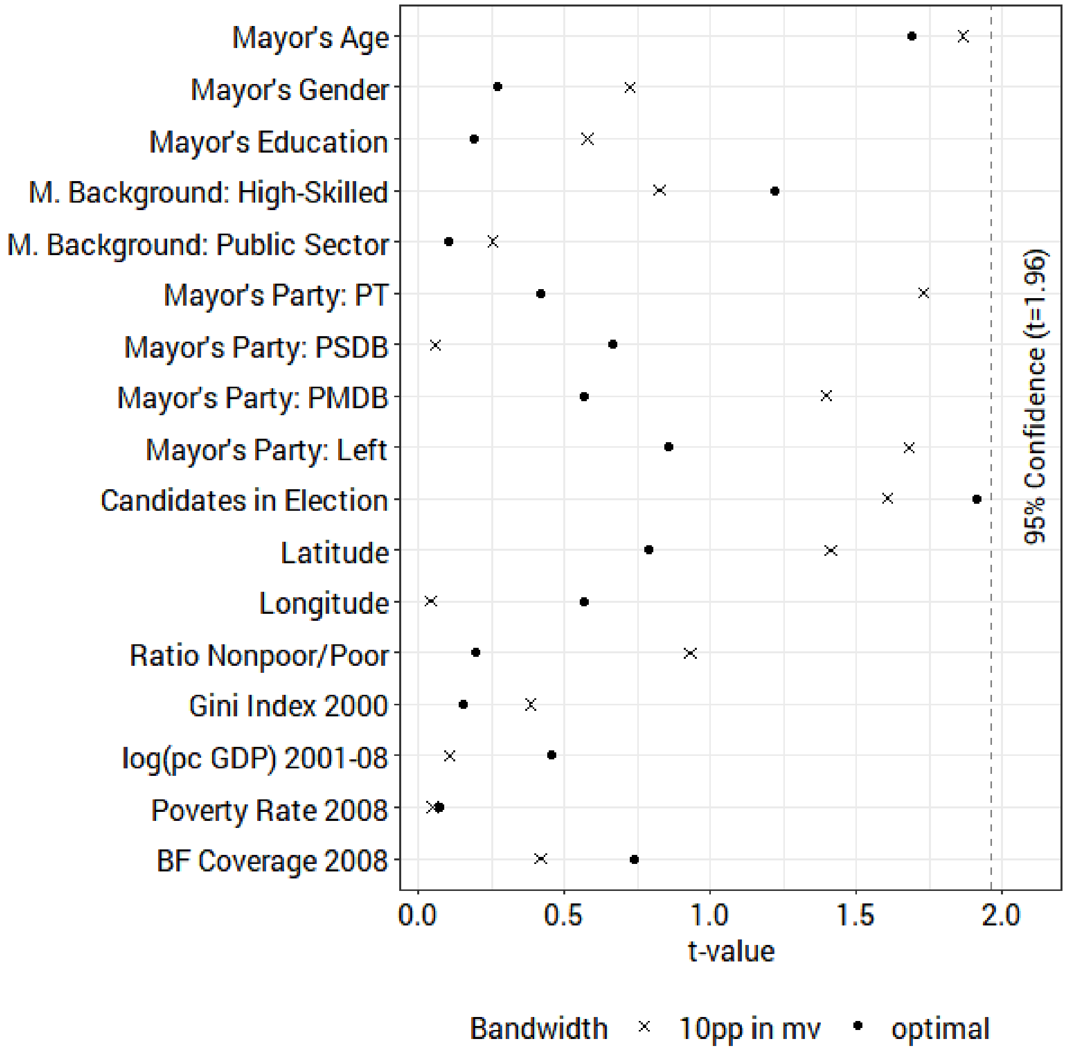
Cluster-robust standard errors by municipality in parenthesis. The sample only includes municipalities with margin of victory in 2008 below the median value of 11pp. All regressions include quarter-municipality fixed effects, and the household-level covariates described in the text.

Figure A.I: Balance of Covariates



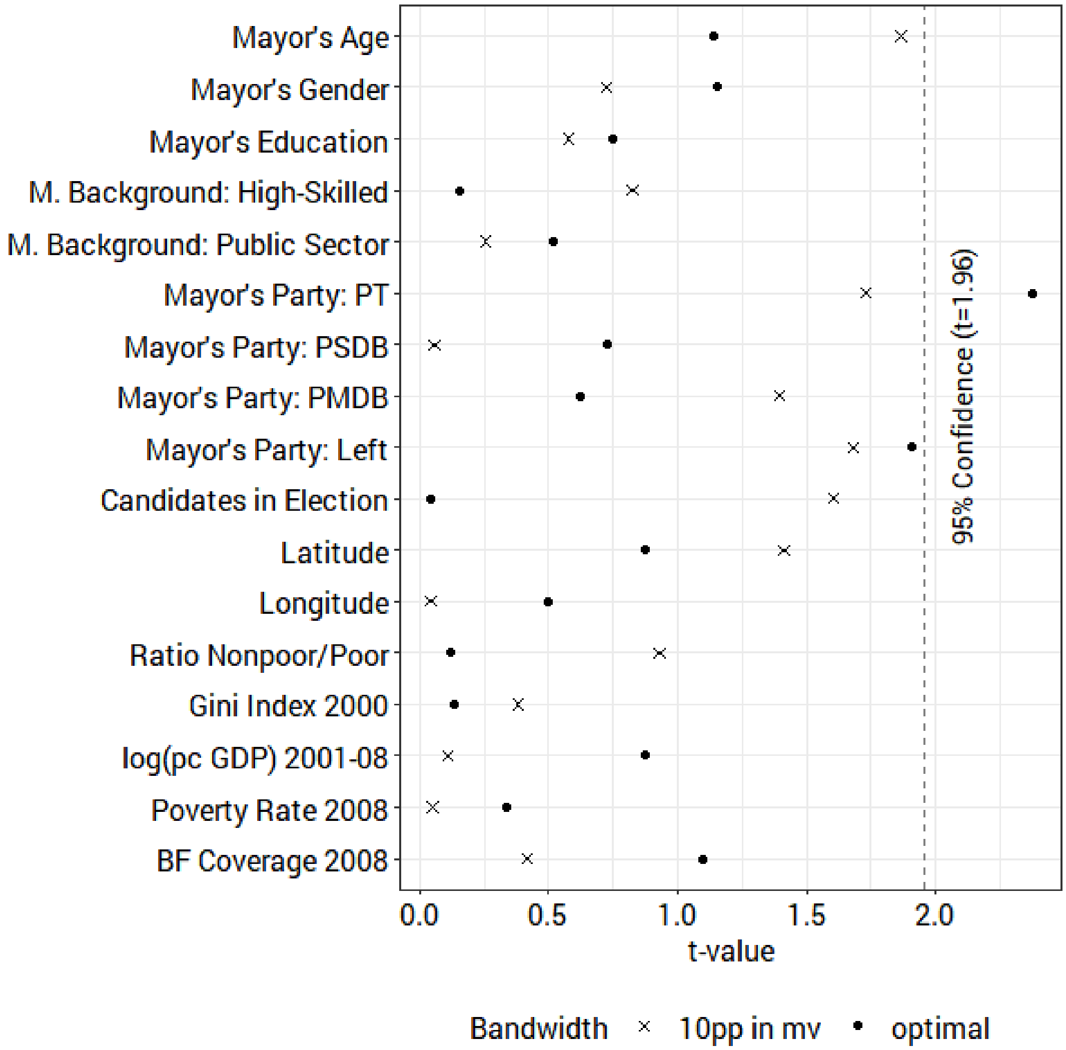
The dots represent the heteroskedasticity robust t-value for the coefficients at the discontinuity under different bandwidths.

Figure A.II: Balance of Covariates (Small SS Infrastructure)



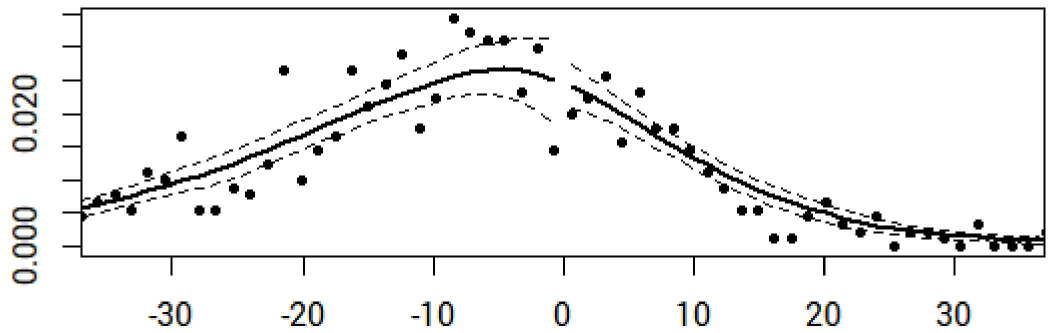
The dots represent the heteroskedasticity robust t-value for the coefficients at the discontinuity under different bandwidths.

Figure A.III: Balance of Covariates (Large SS Infrastructure)



The dots represent the heteroskedasticity robust t-value for the coefficients at the discontinuity under different bandwidths.

Figure A.IV: McCrary Test of the Manipulation of the Running Variable



The p-statistic equals 0.96.