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

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Abstract

Although reelection prospects can increase policy effort by incumbents, it can also create incentives for politically-motivated targeting of resources, which might jeopardize both distributional efficiency and electoral competition. While existing empirical tests examine the potential countervailing effects of accountability and targeting in isolation, this article analyzes their net effect in the context of the world's largest cash transfers program, *Bolsa Família* (BF) in Brazil. Using administrative data on more than 11 million households from both the BF registry and party membership rolls, we estimate these effects using a regression discontinuity design based on mayoral term limits. The evidence supports political targeting over accountability: although reelection prospects only weakly improve effort in program expansion, they drive mayors to aggressively extend BF benefits to ineligible, non-poor households that are core party supporters. Furthermore, only politically-motivated targeting is positively correlated with future reelection probability.

Word Count: 8370

The quality of democracy depends upon the existence of institutional mechanisms that allow voters to keep politicians accountable. The prospect of reelection has been singled out as an effective disciplining device by both 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, while reelection incentives may increase the incumbent's effort, they may also create a risk of politically-motivated allocation of policy resources. When incumbents can collect electoral rewards from targeting voters outside the original scope of the policy, we should expect both welfare losses coming from inefficient budget allocation and distortions in the balance of political power.[1] The literature provides ample support for the existence of politically-motivated manipulation of spending across time, geographies, and groups of voters (Lindbeck and Weibull, 1987; Dixit and Londregan, 1996; Levitt and Snyder, 1997; Solé-Ollé and Sorribas-Navarro, 2008; Camacho and Conover, 2011; Gans-Morse, Mazzuca, and Nichter, 2014; Klein and Sakurai, 2015; Labonne, 2016; Gottlieb et al., 2018).

Given these often competing incentives, does the prospect of reelection enhance or hinder the quality of local policy implementation? The extant empirical scholarship provides little or no evidence of the net effect of these incentives on both distributional outcomes and the consequent level of electoral competition.[2] Nevertheless, this question is especially relevant for the many developing democracies that have decentralized policy implementation in an environment where clientelistic distribution is still electorally attractive.

We propose an empirical test to compare accountability and targeting in the local implementation of Brazil's *Bolsa Família* (BF), during the mayoral tenure of 2009-2012. Access to BF is programmatic, i.e., all households self-reporting monthly *per capita* income below R$140 can enroll, and benefits are approved and paid directly by the central government without the participation of local incumbents. Nevertheless, given that mayors are responsible for

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

collecting and verifying the enrollment information, they have leeway to effectively conduct one type of politically-motivated program misallocation: the inclusion of nonpoor, illegible beneficiaries in exchange for support. This institutional framework makes BF a perfect case study for the net effects of accountability and targeting, given that good program implementation and effective political use of benefits are mutually exclusive.

Our first step is to code four main outcomes that proxy the effects of those incentives in BF using a novel combination of election results, administrative data on more than 11 million BF beneficiaries, and membership rolls of parties with more than 12 million voters. Policy effort is measured using the expansion in program coverage during the mayoral tenure, which was extensive between 2009 and 2012. At the beginning of the period, BF covered only around 80% of its targeted public.

We use three different measures of politically-motivated targeting. Our main outcome is derived from country-wide cuts in BF benefits conducted immediately after the end of the mayoral tenure (first-half of 2013). The Ministry of Social Development (MDS) identified and excluded more than 700,000 households misrepresenting income in the registry, on the basis of cross-check of government databases. This measure is particularly helpful for disentangling accountability and targeting in BF in the period 2009-12, as it indicates how much of the effort in program expansion was dedicated to irregular enrollments.

The second targeting measure relies on anomalous patterns of income reporting, in the spirit of the literature on electoral fraud detection (Rundlett and Svolik, 2016). We observe whether households enrolling during the mayoral tenure are disproportionately reporting income that is equal or slightly below the eligibility level. Such 'bunching' should constitute evidence of fraud if it is observed only in municipalities with reelection incentives, only for this specific income level, and it is absent from the contemporaneous Census survey (2010). Our third variable is obtained by matching party rolls with the BF individual registry,[3] in order to observe whether the mayor's partisans are disproportionately enrolled in BF.

The next step is to compare mayors with and without reelection incentives on the basis of these outcomes. Our empirical strategy has to overcome two sources of potential bias here.

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[3]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).

First, there could be unobserved municipality characteristics that influence both the implementation of BF and election results. Thus, we use a regression discontinuity design (RDD) to compare municipalities were incumbents barely won the 2008 election (and were ineligible in 2012), to ones where they barely lost (and challengers were eligible in 2012). 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 can be 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.[4]

Our results provide only weak support for reelection prospects improving effort in good policy implementation in this context. Although coverage increases 2.9 percentage points (pp) more with reelection incentives (a 10% effect), this effect is not statistically significant under any specification. The results from the 2013 program audits tell a different story. Municipalities governed by first-term mayors in 2009-2012 experienced benefit cuts that where 2.5 times larger than the ones for locations with lame-duck mayors (or 3.2pp higher). A comparison of these effects implies that all BF expansion was generated by misallocated policy effort and the targeting of ineligible households.[5] What is more, for income within a small interval around R$140, 88% of households declared themselves eligible to BF when enrolled by first-term mayors, which is significantly more than the 76% share under lame-duck ones.

Even if a 3pp misallocation cost does not seem high for a program of the scope and scale of BF, it puts incumbents in an disproportionately attractive position during the election cycle. These resources are equivalent to nearly three times the cost of running a mayoral campaign in these locations. They also represent 150% of the resources spent in managing the program at the local level, i.e., the misallocation could pay for the implementation of another BF.

As for the political component of targeting, the share of mayor's partisans enrolling with income just below R$140 is 3.9 times larger (35%) in the presence of reelection incentives than the share in municipalities without them (9%). Mayors not only target their core supporters, they also focus on the voters that enroll within the income range that shows evidence of misreporting fraud. This result provides some insight to the ongoing debate on "whether politi-

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[4](Ferraz and Finan, 2011) use a similar strategy.
[5]Even if we had the statistical confidence to link reelection with higher policy expansion.

cians allocate goods to their core constituents or whether they target swing voters" (Golden and Min, 2013). While the intuition behind swing targeting is better electoral returns,[6] politicians target core supporters to ensure mobilization (Nichter, 2008), in the presence of risk aversion (Cox and Mccubbins, 1986), or in the case "where partisan loyalties are conditional on particularistic benefits received in the past" (Gans-Morse, Mazzuca, and Nichter, 2014). All these arguments fit the present case. Local elections have a weak ideological component and party loyalty is often contingent on the continuous exchange between patrons and voters (Nichter and Peress, 2017). Furthermore, the risk of electoral prosecution for vote buying might lead incumbents to target 'low risk' party activists, which can also be mobilized for campaigning. To our knowledge, the present article provides one of the first pieces of evidence based solely on administrative data on how politicians target individual voters.[7]

The final step is to examine the electoral returns of these competing incentives. Although the present empirical strategy does not allow us to precisely establish a causal link, we find that our main misallocation outcome (audit cuts) is highly correlated with electoral success for incumbents attempting reelection in 2012. The fact that the same is not observed for coverage expansion might explain why mayors fail to put effort on good BF implementation, and focus on clientelism. The extant work on the political rewards of programmatic policies such as CCTs 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,[8] Imai, King, and Rivera (2018) argue the opposite suggesting that "most policies studied in prior research are partly programmatic and partly clientelistic." This is precisely the case of BF in regards to its local implementation,[9] which is largely programmatic, but allows for clientelistic exchanges when mayors illegally extend benefits to nonpoor, ineligible households in exchange for political support.

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[6]Core supporters are already likely to vote for the party for ideological reasons (Dixit and Londregan, 1996; Stokes, 2005).

[7]The extant empirical literature using administrative data focuses mostly on targeting across geographies and groups. The literature providing evidence of individual targeting is mostly based on survey work.

[8]This literature does not disentangle what is a result of (1) undeserved credit claiming (see an example in Cruz and Schneider (2017)); (2) clientelistic misallocation of resources; (3) or rewards for effort that signals the politician's ability.

[9]Zucco (2013) examines the case of credit claiming by the party at the federal level in presidential elections.

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, generating an average 50% increase on the monthly income of the targeted population. Both the scale and scope of the program make it an attractive tool for political manipulation by local incumbents. Although beneficiaries tend to credit the federal government under President Lula (PT, 2003-2010) for these transfers (Sugiyama and Hunter, 2013; Zucco, 2013), BF registration rules allow local politicians from other parties to also extract rents from the program.

Different from many other CCTs (e.g. Mexico, Philippines, Colombia), BF benefits are granted mainly based on self-reported income (Handa and Davis, 2006). All households that declare monthly *per capita* (pc) 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 pc income below R$140 are eligible to BF benefits.[11] Simpler enrollment rules generally allow for more inclusion, even if at a risk of higher program fraud coming from ineligible households underreporting income. As an example, a 2016 audit by the Ministry of Social Development (MDS) found 1.1 million households receiving the benefit irregularly as they had actual income above the threshold (8% of all beneficiaries).[12]

The federal government has limited tools to detect and curtail this type of program fraud directly. The local BF administration is periodically audited as part of the CGU initiative that audits local governments,[13] but these audits reach less than 3% of all municipalities every year. In addition to CGU audits, the Ministry of Social Development (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 for

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[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.

[11]This was the eligibility threshold for the period between Aug 2009 and 2012, it has increased ever since. 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 pc income below R$140, conditional on school attendance and regular health check-ups.

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

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

BF eligibility. MDS also requires beneficiaries to update their information every two years, under the risk of losing the benefit. 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.

Program rules state that municipal governments are responsible for enrollment, data collection through CadUnico, and for verifying the accuracy of the information. Once households are enrolled, the central government approves and pays the benefit directly through a debit card. Given that municipalities are effectively the main gatekeepers of the registry, they can influence the level of income underreporting in the program.[14] Not surprisingly, there are many reported attempts of program manipulation by local politicians for personal gain. As examples, MDS found that 2,168 elected politicians enrolled themselves in BF, and were receiving benefits illegally in 2013.[15] 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 in 2014, which consisted mainly of manufacturing false CadUnico entries (with low reported income) to trigger the irregular payment of benefits.[16]

On the other hand, the opportunities for the clientelistic use of BF by local incumbents are limited, given that poor and therefore eligible households do not need to be selected nor approved by municipalities to receive benefits. In regards to poor, eligible households, politicians are limited to two strategies. They can try to claim credit over the program's arrival, or they can threaten to remove beneficiaries from the registry if they withhold political support. Both strategies seem to be ineffective. The literature has shown that households correctly attribute transfers to the federal government (Sugiyama and Hunter, 2013; Zucco, 2013), and that credit claiming gains for mayors are limited (Frey, 2017).

What is more, households have institutional mechanisms to denounce such practices, and vote buying attempts could cost incumbents their office. More than 600 hundreds mayors were impeached in Brazil since vote buying became an electoral crime in the late 90s. As specific examples, the mayor of Bocaina (PI) was impeached for vote buying, after offering the

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[14]For a more extensive analysis, see Lindert et al. (2007).
[15]O Globo (Oct, 2013). See the news in Portuguese: http://goo.gl/3RsfaW.
[16]Jusbrasil (2014). See the news in Portuguese: http://goo.gl/a40TYX.

BF benefit in exchange for votes.[17] In a different case, the federal police intercepted calls from a mayor in the Northeast, also asking for votes in exchange for the BF benefit.[18]

In this context, BF benefits are conducive to clientelism only when the program resources are misallocated. Conditional exchanges that elicit the commitment of voters are only possible when politicians have monopoly over the goods (Medina and Stokes, 2007), and "when the goods or benefits that can be exchanged are valuable to citizens... and ongoing, yet reversible" (Weitz-Shapiro, 2014). Only when nonpoor households access the benefit by means of misreporting income, mayors have actual power to recall the benefit if conditions are not met (for example, if the incumbent is not reelected). Furthermore, households do not have any incentive tor denounce the practice, as they might not obtain the benefit under a different municipal administration.

These characteristics make BF a perfect case to study to net effects of accountability vs. targeting in policy delivery, given that good program implementation and effective politically-motivated allocation of benefits are mutually exclusive in this context. It follows from the canonical accountability models that mayors seeking reelection should put more effort on good program implementation.[19] However, if mayors can also extend BF eligibility on the basis of political preferences and not the poverty of voters, reelection incentives might generate counterbalancing welfare losses to the program.

Measuring BF Targeting and Performance

When designing the eligibility mechanism for any CCT program, policy makers faces a trade-off between inclusion and the quality of targeting. In the case of *Bolsa Família* (BF), the program was designed to prioritize inclusion over targeting (Soares, Ribas, and Osório, 2010), as underscored by the simple self-reporting enrollment rule that is easily understood by beneficiaries, but also easily violated.

We use the change in program coverage during the mayoral tenure as our measure of

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[17]Portal Saiba Mais (2016). See the news in Portuguese: http://goo.gl/D2LJTg.

[18]Hoje em Dia (2014). See the news in Portuguese: http://goo.gl/9GtLOy.

[19]As an example of a similar mechanism, De Janvry, Finan, and Sadoulet (2012) show that Brazilian mayors with reelection incentives were better at implementing a former CCT program aimed to reduce school dropout rates (*Bolsa Escola*).

effort in good program implementation for mayors, as program enrollment is one of the main responsibilities of municipalities, and it is salient to voters. Coverage is calculated as the ratio between the number of beneficiaries, and the estimated number of poor households in each municipality.[20] At the beginning of the mayoral tenure (end of 2008), municipalities in the sample covered on average around 80% of their targeted households, and had ample room to expand the program in the following four years. Accordingly, the average BF coverage for this sample was above 110% of the target at the end of the mayoral tenure in 2012.

Measuring politically-motivated misallocation of benefits is more challenging, given that we only observe individual reported (and not actual) income. An ideal measure would show that reelection incentives drive mayors to extend BF to nonpoor voters, while also establishing a political pattern for such distribution. We use three variables for this purpose. The first is based on a timely round of program audits that the Ministry of Social Development conducted in the first semester of 2013, based on a cross-check of federal government databases, without any input from the municipalities. These audits triggered the interruption of benefit payments to more than 700,000 households due to income misreporting fraud (MDS, 2013a,b). We measure the change in BF coverage (defined above) between the end of the mayoral tenure (Dec 2012), and the end of the audits (Jun 2013). We emphasize that even mayors reelected for a new tenure in 2013-2016 would have no room to avoid or manipulate these audits, due to their technical and top-down profile.

The second variable is based on microdata from the administrative registry of the BF program (CadUnico).[21] CadUnico contains the timing of enrollment, and the income declared by households for program eligibility. For most of the mayoral tenure (Aug 2009 to Dec 2012), the per capita income threshold for BF eligibility was R$140. Figure 1 shows the benefits granted to households enrolling with income around this threshold from Aug 2009 to Dec 2012. In line with program eligibility rules, the share reporting income below the threshold was much more likely to be receiving BF benefits as of Dec 2012.[22] Accordingly, for every municipality

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[20]Coverage targets are not binding (there are many municipalities with coverage above 100% (MDS, 2012)). We calculate all our coverage variables based on the updated target from 2011. Very similar results are found using the target valid in 2009 and 2010.

[21]We have access to the CadUnico registry dated Dec 2012.

[22]Not all households reporting eligible income will be receiving the BF benefits at all times, and sometimes households with higher income will be receiving it. The reasons for some of these disparities are discussed in the online appendix, Section B on page 3.

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we measure the share of families reporting eligible income (below R$ 140), out of all households that declared income in a small interval around R$140 (for example: between 131 and 149).[23] We call this the 'just eligible' share in every municipality.

Figure 1: Share of Households Receiving the Benefit by Declared Income
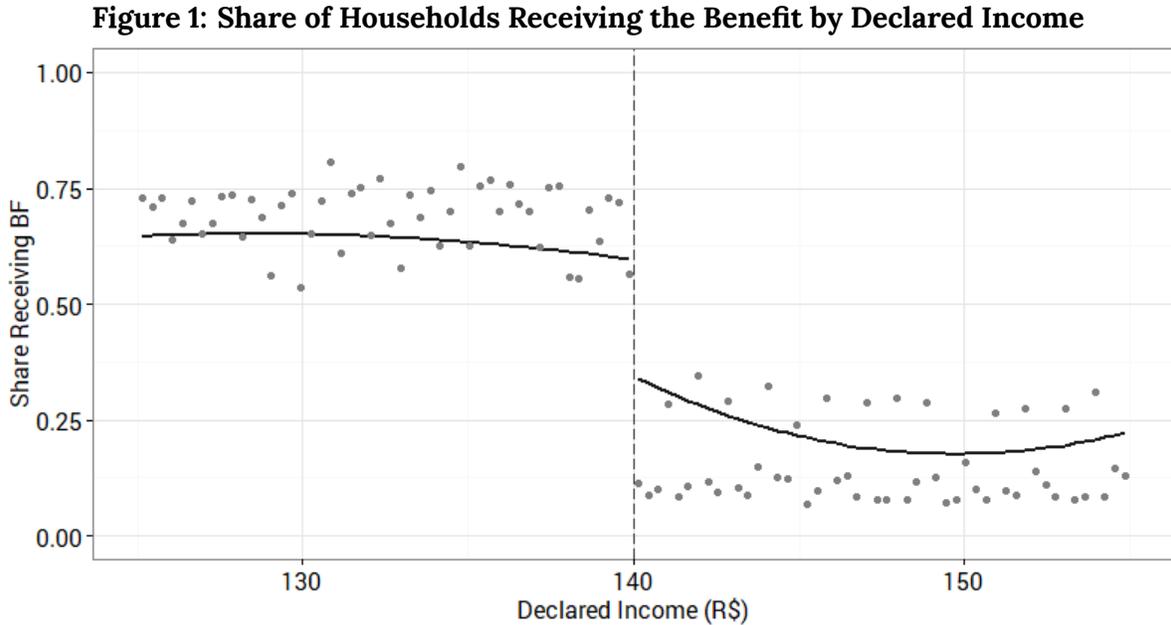
The dots represent the average of households receiving the BF benefit for that specific value of declared income. R$140 is the threshold for benefit eligibility, as established in 2009. The plot includes households enrolled between Aug 2009 and Dec 2012. The solid lines are the local fit of a second degree polynomial.

The idea is that an unusual bunching of households at an income level just below eligibility represents evidence of misreporting fraud, if and only if this pattern is not observed in other income levels (CadUnico), nor in other contemporaneous surveys for the same R$140 level (as the 2010 Census). Moreover, if this bunching in income declaration is generated only by the volition of households themselves, a 'quasi-random' assignment of reelection incentives across municipalities should not have any effect on this measure. However, if reelection incentives also motivate mayors to foster program misallocation, this measure would vary across locations according to the assignment of reelection.

There are two other important things to highlight about this variable. First, it does not capture other types of misallocation, e.g., if mayors allow ineligible households to report zero

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[23]We show that our estimates are also robust to the choice of window around R$140.

income for eligibility. This means that this variable potentially underestimates the full effects of reelection incentives on BF misallocation. Second, one could argue that this variable could also be measuring an over-diligence from the mayor, as opposed to program misallocation. In other words, mayors with reelection incentives would be more likely to push the marginal ineligible household into the program when presented with such a case. Although this is still a technical violation of BF rules, we are really interested in cases where ineligible households have an actual income that is clearly above the threshold, but are reporting below R$140 anyway. Although this measure alone cannot disentangle those two behaviors, we find complementary support in the other misallocation measures, especially the audit-based variable, for the idea that we are properly measuring a more significant misallocation of resources.

Our third variable comes from matching CadUnico data with the membership rolls of political parties at the 2008 election. We use the electoral ID (*título de eleitor*), present in both databases, to match partisans to CadUnico registrants. Nearly 10% of the country's population is affiliated to a political party, and this group is usually regarded as the core supporters and party activists (Speck, Braga, and Costa, 2015). For party members enrolling in BF with a 'just eligible' income (equal or just below R$140), we calculate the share that belongs to the mayor's party, and we observe how it responds to reelection incentives. Alternatively, we produce the same variable for partisans enrolling in BF with an income outside the just eligible window, i.e., between R$0 and R$130.

This variable allows us to test both the existence and type of political motivation behind the targeting of BF benefits in the presence of reelection incentives. More importantly, it shows whether the evidence of political targeting is congruent with the misallocation results obtained with the previous two variables. This is key to our hypothesis that BF resources can only be used in clientelistic exchanges when incumbents are extending benefits to nonpoor, ineligible households. If that is the case, we expect to observe a pattern of political allocation of BF benefits only for households enrolling with income within the range where we also observe evidence of fraud, i.e., just below R$140.

We focus on the 4-year mayoral tenure between 2009 and 2012, as our CadUnico microdata is only available for this period. Mayors with reelection incentives are the ones elected in 2008 for their first non-consecutive term. Mayors reelected in 2008 for a second consec-

utive term are the lame-duck mayors.[24] 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).

Empirical Strategy

We 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) in the 2008 municipal election. 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 elected mayors 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 (Ferraz and Finan, 2011; 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 observed treatment effects are confounded by the level of poverty across municipalities. We address this problem using 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,

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[24]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.

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).

We define the margin of victory mv_m in municipality m as the difference in percentage points between the winner and the runner-up.[25] Under the RDD assumptions (Lee and Lemieux, 2010), the local average treatment effect (LATE) is identified for $mv_m = 0$. We estimate this effect non-parametrically using a local linear regression, as shown in equation 1.

$$y_m = \beta_0 + \beta_1 \delta_m + \beta_2 mv_m + \beta_3 \delta_m mv_m + \epsilon_m \tag{1}$$

where for municipality m the outcome is denoted by y_m, reelection incentives are given by the dummy δ_m, and the treatment effect is denoted by β_1. As usual, the local linear regression is weighted by the edge kernel, and estimated for a sample limited by a bandwidth around $mv_m = 0$.[26] As it is also usual in RD designs (McCrary, 2008), we present in the appendix a test showing that the running variable is not being manipulated at the discontinuity (Figure A.1).

The RDD is only valid if the covariates that are either fixed or determined before treatment are balanced around the discontinuity, implying that the control and treatment groups are not significantly different. Figure 2 shows that the sample is balanced for 19 variables that include characteristics of the municipality, elected party, and mayor. We emphasize that municipalities in both sides of the discontinuity have similar levels of BF coverage and total spending in 2008 (before the election), and also similar poverty levels. All these variables are described in the appendix (Section A on page 1).

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[25]The application of the RDD to close elections has been widely used in Brazil (Boas and Hidalgo, 2011; Ferraz and Finan, 2011; Brollo and Nannicini, 2012; Brollo and Troiano, 2016; Klašnja and Titiunik, 2017)

[26]The optimal bandwidth is calculated as the minimum between the bandwidths estimated using the plug-in methods proposed by Calonico, Cattaneo, and Titiunik (2014), and Imbens and Kalyanaraman (2012). As usual, we show results for different multiples of the optimal bandwidth in Table 1.

Figure 2: Balance of Covariates
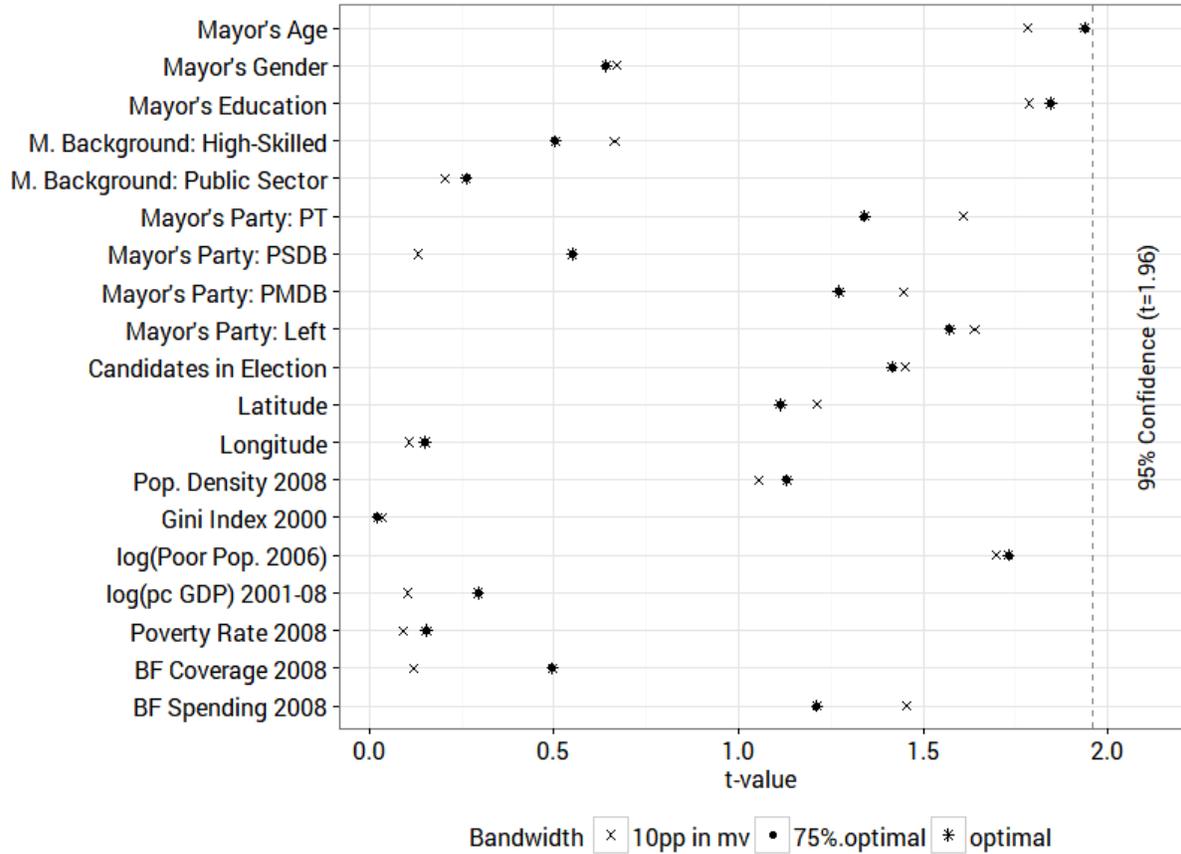
The dots represent the heteroskedasticity robust t-value for the coefficients at the discontinuity under different bandwidths.

Results and Discussion

The estimates obtained with the RDD for the variables described in the previous Section are shown in Table 1. The first (unnumbered) column presents the pre-treatment mean of each variable for a municipality at the threshold ($mv = 0$). Columns (1) though (4) present the effect of reelection incentives for four different bandwidths. The graphic representation of these effects for each main variable can be found in Figure 3. Also, for the main four outcomes, we show the sensitivity of the estimates to the choice of polynomial in the RDD estimation, and the inclusion of covariates and state fixed-effects (see Table A.2, in Section C of the online appendix, on page 3). Finally, our comments are based on the results obtained with the optimal

bandwidth (column 3).

Table 1: Measures of Effort and Targeting in BF

Dependent Variable:	Mean	(1)	(2)	(3)	(4)
Cov. chg. (2008-2012)	0.293	0.001	0.017	0.029	0.056
(benefits–target ratio)	(0.046)	(0.116)	(0.089)	(0.071)	(0.061)
observations	254	119	185	254	320
bandwidth	7.84	3.92	5.88	7.84	10.00
Cov. chg. audits (2013)	-0.022	-0.037*	-0.035**	-0.032**	-0.034**
(benefits–target ratio)	(0.009)	(0.022)	(0.017)	(0.014)	(0.017)
observations	386	206	317	386	320
bandwidth	12.88	6.44	9.66	12.88	10.00
Just eligible	0.758	0.212**	0.176***	0.118**	0.102**
(share eligible to BF)	(0.045)	(0.085)	(0.063)	(0.052)	(0.047)
observations	287	138	208	287	320
bandwidth	8.65	4.32	6.49	8.65	10.00
Pol. Targeting (in RS130-140)	0.089	0.483***	0.329**	0.258**	0.278**
(share of party affiliates)	(0.063)	(0.157)	(0.135)	(0.122)	(0.128)
observations	156	80	122	156	142
bandwidth	11.26	5.63	8.45	11.26	10.00
Pol. Targeting (below RS130)	0.158	0.094	0.054	0.013	0.009
(share of party affiliates)	(0.037)	(0.092)	(0.069)	(0.057)	(0.056)
observations	308	146	230	308	311
bandwidth	9.65	4.82	7.24	9.65	10.00
bandwidth rules	*optimal*	*0.50 x opt.*	*0.75 x opt.*	*optimal*	*fixed*

Confidence level: *90%, **95%, ***99%. Heteroskedasticity robust standard errors in parenthesis. The mean is calculated for a municipality at the discontinuity ($mv = 0$), using only the data for the subsample in the control group (i.e. no reelection incentives).
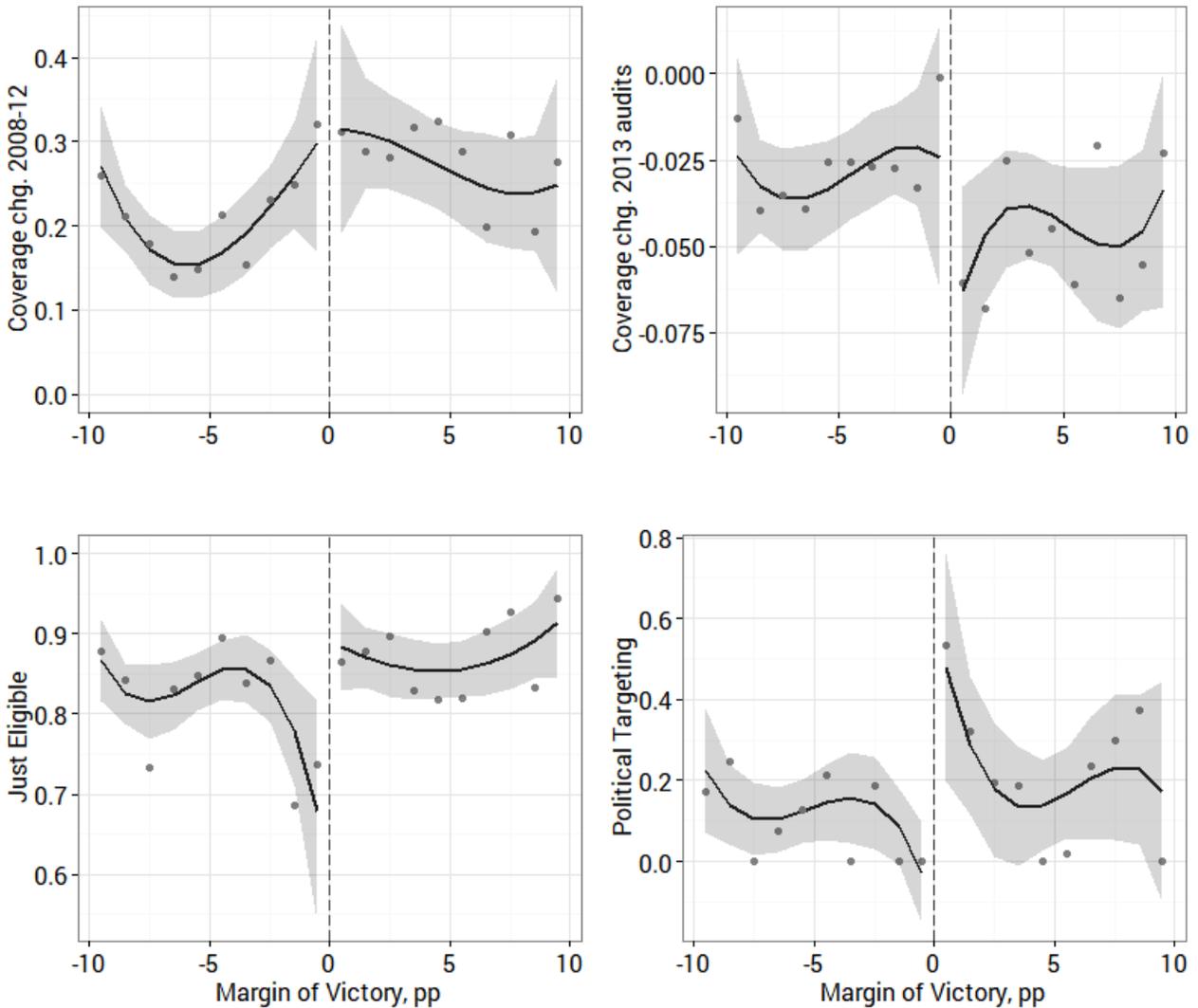
The right-side reflects municipalities where the mayor had reelection incentives. The dots represent the average of each outcome variable for that specific value of margin of victory. The solid lines are the local fit of a third degree polynomial.

Policy Effort   Municipalities where mayors have reelection incentives had higher program expansion (BF coverage) during the 2009-2012 mayoral tenure. The 2.9 percentage points (pp) coverage increase was 10% higher than the pre-treatment level of 29pp. However, these results are not statistically significant under any alternative, and the estimated coefficient presents a remarkable drop as the bandwidth narrows. This indicates that its positive magnitude, although insignificant, is likely driven by estimation bias.

Our null result is not aligned with the expectation of accountability models, which predict that mayors exercise more effort in good program implementation in the presence of reelection incentives. We believe that there are two likely explanations for the lack of significant effects here. First, even though mayors could claim credit for enrollment, i.e., for providing households the information on how to access the program, they are ultimately not in control of benefit approval and continuation. BF's marketing material strongly emphasizes the program as federal policy, and voters associate it with Lula's presidency, as opposed to municipal administrations (Sugiyama and Hunter, 2013; Zucco, 2013). Once households enroll and the source of resources becomes clear to them, the credit claiming opportunities for local incumbents are limited.

Second, the program generates a significant increase in the income of poor households. Thus, once voters dissociate the benefit from local incumbents, it could actually undermine the ability of these politicians to conduct clientelistic exchanges with the beneficiaries, given that their marginal utility from handouts falls as their income increases (Frey, 2017).

Benefit Misallocation   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 2.2pp. However, in municipalities where the mayor could have been reelected, these cuts were 3.2pp higher (or 2.5 times as large). This is strong evidence that there were many more truly ineligible households enrolled in these municipalities.

We 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 would have been added to the program to replace the excluded ones. Thus, our estimate here might be underestimating the true effect of reelection incentives on program mistargeting.

To put this misallocation in perspective, the BF budget for 2012 was around R$20 billion (roughly US$8bn). A loss of 3% of the program resources, although apparently small, corresponds to one and a half times 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 three times the average budget for mayoral campaigns in Brazilian municipalities.

Our 'Just eligible' variable shows that for all households with declared income in the interval [R\$131,R\$149], the average share reporting eligible income (below R\$140) is 76% in the absence of reelection incentives. In places where mayors had reelection prospects, this share increases by 12pp (Column 3), indicating that nearly 90% of households in that income range are declaring themselves eligible to the BF benefit in these municipalities. This represents an effect of 16% over the baseline measure.

We show in Figure 4 that this result is robust to the choice of interval around R\$140 (left-side plot). We also show (right-hand plot) two placebo tests for this variable. First, we code the variable for other income thresholds, as follows: (a) the multiple of R\$10 immediately above and below R\$140, i.e. R\$130 and R\$150; (b) The BF eligibility threshold that was in place in the previous mayoral tenure (R\$120); and (c) R\$100, which could be a natural choice of round number to be reported for ineligible households looking to receive the benefit. If this variable precisely measures manipulation of eligibility, none of the effects for these alternative specifications should be statistically significant, which is the case here.

Second, we code the variable for the R\$140 threshold, but now using the income survey from the 2010 census. The Census is conducted by a different government institute (IBGE), and its data does not influence the eligibility of households for BF benefits. Thus, different assignment of reelection incentives should not influence the way in which households report their income around the R\$140 level for the Census. As expected, this variable is not statistically significant.

We have speculated before that the effect on 'just eligible' could be a consequence of two slightly different types of manipulation. First, a first-term mayor might be willing to include marginal households in the program when they declare income that is only slightly above the threshold, for example, R\$143. This 'mild' type of fraud generates minimal mistargeting of resources, and we argue that neither program administrators nor researchers are (or should be) very concerned about this type of violation. On the other hand, this effect could be consequence of a more relevant type of fraud in which mayors extend eligibility to households that have actual earnings that are significantly higher than the threshold. We turn to our previous audit -driven variable to argue that large part of the effect on 'just eligible' comes form this more severe violation.

Figure 4: Robustness and Placebo Tests
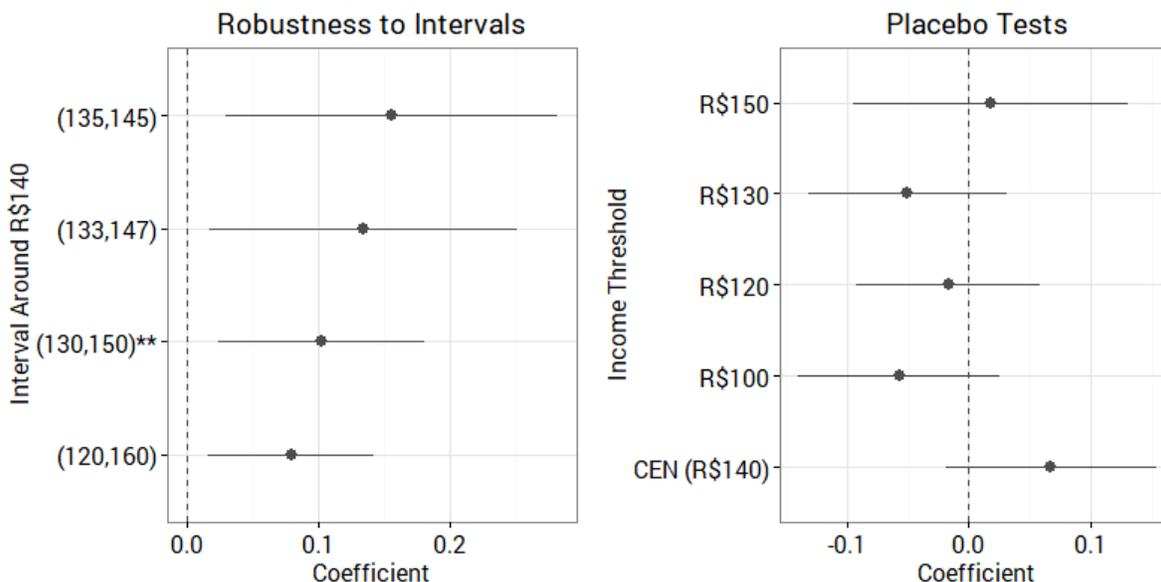
The left-hand plot shows the targeting coefficient for subsamples with different intervals around the declared income threshold of R$140. The right-hand plot shows the coefficient for intervals of ± R$10 around different income thresholds in declared income for CadUnico: R$100, R$120, R$130, R$150, and around the income of R$140 for the 2010 census survey.

Political Targeting It remains to show that the benefit misallocation evidenced by the two previous measures has a clear political pattern. Accordingly, the share of program entrants that are partisans of the mayor is only 8.9% in the absence of reelection incentives, but strikingly higher (35%) in municipalities where mayors can be reelected. This is observed for households enrolling with 'just eligible' income (between R$131 and R$140). In other words, most of the same households whose enrollment shows a pattern of income reporting fraud are also copartisans of the mayor.

The targeting of core supporters here probably stems from the fact that this clientelistic exchange carries a relatively high risk for the politician. Brazil has prosecuted hundreds of politicians since vote buying became a electoral crime in the late 1990s. Given that the exchange of illegal BF benefits for support could trigger the loss of the mayoral mandate, local incumbents might focus on their 'less risky' partisans. The practice also conforms with the idea that, in areas where clientelism is effective, party loyalties are often the result of a continuous stream of patronage provided by politicians (Anderson, Francois, and Kotwal, 2015;

Gans-Morse, Mazzuca, and Nichter, 2014), as opposed to ideological considerations.

We also code the political targeting variable using households declaring income outside the just eligible range, i.e., between R$0 and R$130. In this case, reelection incentives have no effect on the share of partisans of the mayor that are enrolled in the program. This result underscores our argument that politically-motivated targeting of BF benefits is only effective in the specific case of income misreporting fraud. For households that are actually poor and therefore BF-eligible, politicians have little room to extract commitment and future guarantees of support.

Electoral Rewards Are incumbents being rewarded in their reelection attempts for either coverage expansion or benefit misallocation? Unfortunately the empirical strategy in the present article does not allow us to precisely disentangle the returns to those different strategies. We are limited to observing correlations between our measures and future reelection probabilities of mayors that could, and effectively decide to run again. Figure 5 indicates that the electoral returns to program misallocation are higher, which likely justifies the responses of local incumbents to the competing incentives raised by reelection prospects, as shown by our previous results.

Coverage expansion in 2009-2012 is not correlated with reelection in 2012 for incumbent-candidates. However, mayors that ran for reelection in municipalities that later (in 2013) had severe coverage reductions with the MDS benefit cuts were more likely to have been reelected in 2012. In Table 2 we show that these correlations are robust to the inclusion of covariates and state fixed-effects.

Figure 5: Correlation between reelection and BF inclusion and targeting
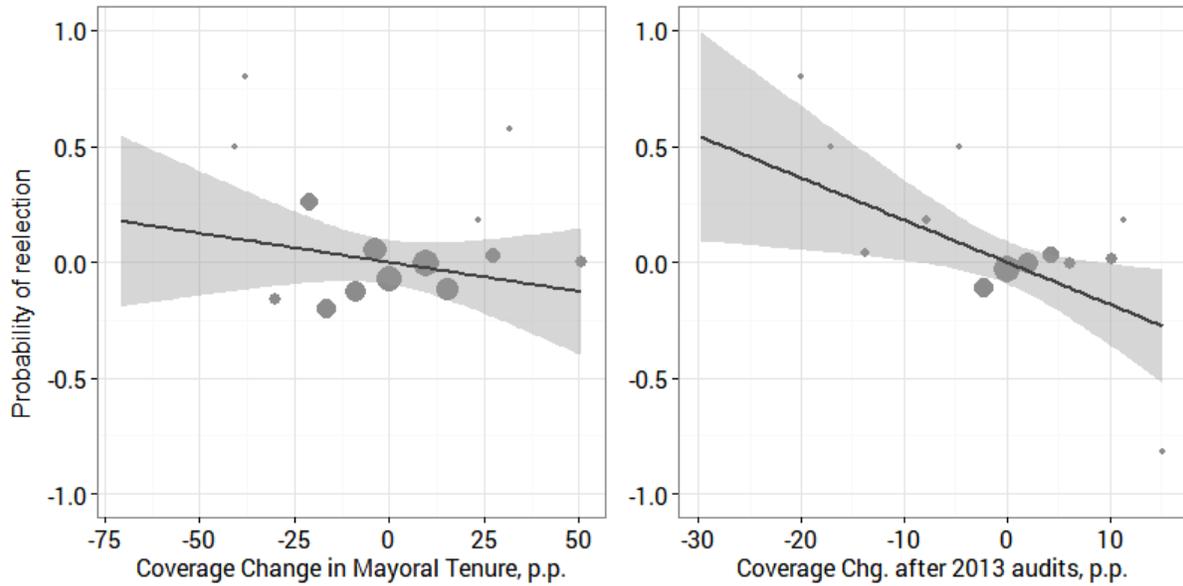
These plots reflect the regressions shown in Table 2. The left-side plot correlates the coverage change during the mayoral tenure, and the reelection rate for mayors that actually ran in the 2012 election. The right-side plot does the same for the coverage change that is a result of the 2013 program cuts. The points show the average of the outcome variable along the x-axis for a sample of mayors elected in a close election (or $mv < 10$p.p.).

Table 2: Main Results: BF Coverage, Targeting and Benefits Cut

Dependent Variable:	(1)	(2)	(3)	(4)
Coverage chg. during Tenure	-0.364	-0.209	-0.213	-0.139
(p.p.)	(0.236)	(0.293)	(0.252)	(0.352)
Coverage chg. due to 2013 cuts	1.659***	1.774***	1.219*	1.422*
(p.p.)	(0.555)	(0.689)	(0.646)	(0.726)
Share of Votes in 2008			0.006	0.005
(pct.)			(0.011)	(0.018)
BF Coverage in 2008			0.368*	0.138
(Benefits / Target)			(0.221)	(0.311)
Poverty Level			-0.683*	-1.240
(Target Benefits / Pop.)			(0.350)	(0.834)
Observations	83	83	83	83
State Effects	No	Yes	No	Yes
Covariates	No	No	Yes	Yes

Confidence level: *90%, **95%, ***99%.

Conclusion

In the context of a conditional cash transfer program (CCT) in Brazil, the present article examines the countervailing effects of local reelection incentives on both the incumbent's effort in policy implementation and politically-motivated targeting of resources. The trade-off is straightforward: the mayor's reelection chances might improve if she puts more effort on program implementation, and more households are receiving benefits. On the other hand, reelection prospects might lead mayors to allocate transfers based on political loyalties rather than on the objective program criteria (i.e., poverty), which would translate into inefficient public good provision and reduced local electoral competition.

Our RDD estimates indicate that it is politically-motivated targeting and not accountability concerns that drive the local enrollment process of BF. While there is only weak evidence that mayors with reelection prospects are better at expanding the program coverage, the results show that they are more likely to enroll nonpoor, ineligible households in the CCT program,

and that this group is disproportionately composed by the mayor's partisans. What is more, while this clientelistic allocation is highly correlated with future reelection probability, program expansion is not.

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.[27] While we clearly show that clientelistic motivations trump the incentives for better policy implementation in the context of Brazil's CCT, it is beyond the scope of this article to provide a more general framework that presents the conditions in which these different incentives would prevail upon one another.

Nevertheless, we believe that our conclusions 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; and (iii) 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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[27]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. We eliminated all households that had a registry marked as invalid (i.e. they were not enrolled at that time), and 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).

We 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.[28]

For the construction of our 'just eligible' and political targeting variables, we had access to the CadUnico extraction of Dec 2012. CadUnico contains the entry date for each household in the registry, allowing us to select only households that joined the program during the mayoral term of 2009-2012. Even though the mayoral tenure starts in January 2009, we focus only on the period after August 2009, for which the eligibility income threshold was constant at R$140. All our data is aggregated at the municipality level, given that our source of variation for reelection incentives is at this level.

For all households that entered CadUnico between Aug 2009 and Dec 2012, we only observe the reported income of households for the CadUnico extracted 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. We do not see this as threat to our analysis, as 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, we only observe the R$139. Both the enrollment and the update were still conducted by the same municipal administration, with the same reelection prospects.

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

Census As for the income declaration measure constructed using census data, we 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.

Party Membership Rolls We downloaded the party membership rolls from the TSE website. We only considered party members as of 2008 (i.e. pre-treatment), which had their status recorded as active in the list. We eliminated any duplicated entries, 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, and the total value of transfers related to BF, from 2004 to 2015. This data, although only reported at the aggregated level, complements the CadUnico information for the period after the 2012 election, for which CadUnico data is not available. This was used to compute the number of benefits cut in 2013 after the round of audits by MDS.

MDS also provides the 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.

As described before, election data comes from the Superior Electoral Authority (TSE). We categorized parties in the left-right spectrum using the DALP survey produced by Duke University. A score of 4 or less indicates a leftist party (in the 0 to 10 scale).

B Income Reporting and Distribution of BF Benefits

There are two main reasons why the BF-eligibility based on income reported in CadUnico does not always match the actual benefit distribution for a few households. First, local offices cannot manipulate the timing or approval of the actual benefit. Even after enrollment, the actual approval of benefits can take several months, and it might be subject to the analysis fo municipality quotas. Second, BF has a permanence rule since 2009, stating that households that increased their reported income above the threshold are still eligible to keep their former benefit for a grace period that could last up to two years (i.e. until their next scheduled update).

Accordingly, Table A.1 below shows that 26% of the CadUnico households are not receiving BF benefits (LESS cells), even though they should be (mostly due to recent enrollment/updates). Also, 11% of households are receiving benefits while they should not (MORE cells), mostly due to the permanence rule. This data is from 2012.

Table A.1: Bolsa Família Targeting

% of Households	Reported p.c. Monthly Income, R$ [0,70]	(70,140]	(140,311]	Total
Full Benefits	43.3	7.6MORE	3.5MORE	54.3
Variable Benefit Only	0.3LESS	4.7	0.9MORE	6.0
No Benefit	14.4LESS	11.0LESS	14.3	39.7
Total	58.0	23.4	18.6	100.0

The cells contain the percentage of households in each category. The three cells on the bottom-left side represent households that are receiving less benefits than they should. The three cells on the upper-right side are the households that are receiving more benefits than they should.
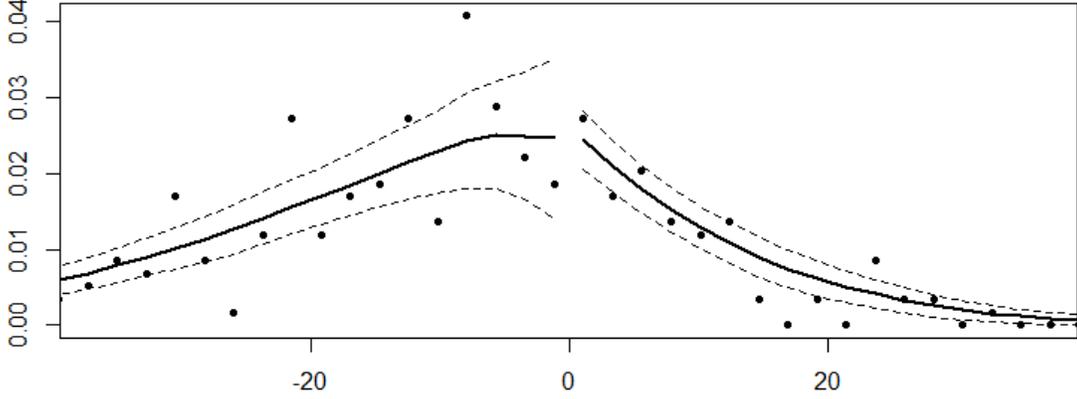
C   Additional Tables and Figures

Table A.2: Robustness to Controls, and Polynomial Specification

Dependent Variable:	(1)	(2)	(3)	(4)
Coverage chg. 2008-2012	0.078	0.066	0.007	0.106
(coverage-target ratio)	(0.086)	(0.076)	(0.125)	(0.100)
observations	185	185	230	230
bandwidth	5.88	5.88	7.01	7.01
Cov. chg. audits (2013)	0.168***	0.153**	0.241**	0.190*
(coverage-target ratio)	(0.057)	(0.062)	(0.103)	(0.105)
observations	208	208	219	219
bandwidth	6.49	6.49	6.76	6.76
Just eligible	-0.033**	-0.038**	-0.045*	-0.050**
(share eligible to BF)	(0.016)	(0.017)	(0.026)	(0.024)
observations	317	317	339	339
bandwidth	9.66	9.66	10.85	10.85
Political Targeting (in RS130-140)	0.470**	0.539***	0.610***	0.989***
(share of party affiliates)	(0.198)	(0.206)	(0.192)	(0.345)
observations	122	122	112	112
bandwidth	8.45	8.45	7.94	7.94
Degree of Polynomial	*1st*	*1st*	*2nd*	*2nd*
State Effects	Yes	Yes	No	Yes
Controls	No	Yes	No	Yes

Confidence level: *90%, **95%, ***99%. Heteroskedasticity robust standard errors in parenthesis. The list of controls includes all variables from Figure **??**. The bandwidth is 75% of the optimal for each degree of polynomial.

Figure A.1: McCrary Test of the Manipulation of the Running Variable

The p-statistic equals 0.94.