

# What You See and What You Get: Direct and Indirect Political Dividends of Public Policies

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## Abstract

We investigate both direct and indirect political dividends of public policies by examining *Minha Casa, Minha Vida*, a programmatic housing policy in Brazil that selects beneficiaries through a lottery. We surveyed MCMV lottery participants and found that winners are not more likely to support the incumbent. We find that nonbeneficiaries, a much larger group than beneficiaries, are aware of the program and evaluate it very well, and difference-in-differences analysis leveraging the roll-out of the program across municipalities finds that presidential and mayoral incumbent candidates had higher electoral performance in localities that implemented the program. We present evidence that beneficiaries and nonbeneficiaries' distinct electoral returns are due to differences in how they experience and therefore evaluate the program. As a consequence, when beneficiaries are a relatively small group, the benefits are conspicuous, and program's objectives have support, government programs can generate electoral returns independently of how they are perceived by beneficiaries.

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When we consider the political and electoral effects of government policies in general, and social programs in particular, we tend to think primarily of the direct effects on beneficiaries. That is, to what extent do beneficiaries alter their political behaviors and attitudes in response to the benefit received? Answering this question typically involves a comparison between beneficiaries and otherwise similar nonbeneficiaries (Campbell 2012).

It is well established, however, that policy feedback, when it exists and can be documented, can take on forms beyond direct effects on beneficiaries (Jacobs and Mettler 2018). In this paper we argue that electoral returns of government programs follow a counter-intuitive logic in which a program delivering a very large and visible benefit – such as a home – might generate electoral returns primarily through its impact on nonbeneficiaries. This claim stands in contrast to programs delivering small and continued benefits to many, which can generate sizeable direct electoral returns from content beneficiaries.

Large benefits, while desirable to beneficiaries, often require very complex arrangements in order to be delivered. This complexity implies that program implementation can fail in many different stages of the provision process, at least to some beneficiaries. Ironically, this might not matter electorally because large benefits are also, almost by definition, delivered to few beneficiaries. While few individuals experience failure directly, the delivery of large benefits can be conspicuously advertised to nonbeneficiaries. As a form of advertising, government programs can signal politicians' priorities and convey perceptions of competence to the general electorate, thereby generating policy feedback from voters not directly affected by the policy. This channel is particularly interesting because nonbeneficiaries tend to have less direct knowledge of how programs are run than beneficiaries do, and nonbeneficiaries might then support policies regardless of the quality with which they are implemented.

We show the existence of these different feedback channels by examining the direct and indirect effects of one of the largest housing programs in the world, *Minha Casa, Minha Vida* (MCMV), in Brazil. MCMV is a highly visible and well-known policy that transfers new housing units to eligible beneficiaries. Due to its visibility and design, we can unpack the impacts of MCMV on beneficiaries and on the broader electorate.

Learning about the direct political effects of housing programs is typically challenging. Becoming a beneficiary in a housing program is highly correlated with income and place of residence. This correlation is true by design in means-tested programs; in more politicized initiatives, program selection can also depend on hard-to-observe political connections. We overcome these challenges by leveraging the lottery system implemented to select MCMV beneficiaries. We conduct two original surveys in which we interview approximately 3,000 applicants to compare winners with nonwinners. In our first survey, which we label the *recent lotteries* survey, we interviewed subjects just after they had been randomly selected (or not) for a new MCMV housing unit, but before any unit was delivered. In our second survey, which we call *early lotteries* survey, we interviewed winners and nonwinners around eight years after the delivery of the housing units. With both surveys, we can measure the effects of recently winning the lottery and the long-term effects of winning the lottery on individuals' support of politicians and on their self-reported political behavior. Furthermore, random selection of beneficiaries allows us to estimate the causal effect of becoming a program beneficiary on support for incumbents, which we refer to as the direct effect of MCMV.

Using differences-in-differences analysis leveraging the staggered roll-out of the program across municipalities, we also examine the indirect payoffs for the broader electorate. This analysis allows us to determine whether it is associated with the electoral performance of local and national level politicians.

We show that MCMV lottery winners, when compared to lottery nonwinners, do not provide any significant direct electoral bonus to the politicians in charge of funding and implementing the program. We also show that despite these null direct political effects, the program is well known and well regarded both by nonbeneficiary applicants and by voters in general, who vastly outnumber beneficiaries. More importantly, we show that MCMV is associated with improved electoral performance of national and local incumbents. As the direct effects analysis showed that beneficiaries do not vote for incumbents at higher rates, this improved performance must come primarily from nonbeneficiaries.

To investigate the interpretation that beneficiaries react to their experiences with the program, we analyzed 15 in-depth semi-structured qualitative interviews with MCMV applicants, winners and nonwinners. These interviews suggest that winners had uneven experiences with the program, and these experiences colored their views of MCMV. For nonwinners, in contrast, MCMV is a low salience but generally positive possibility. Based on these interviews, we further explore our survey and we find that lottery winners who had negative experiences with MCMV, via longer wait times, had more negative evaluations of incumbents than those who had positive experiences, via shorter wait times, with MCMV.

Our findings contribute to studies of policy feedback. By examining both direct and indirect effects in a single study, we are able to better assess policies' distributional consequences and to highlight possible tensions and complementarities between different policy constituencies. We also offer an interpretation of our results that is based the different ways in which beneficiaries and the broader electorate, composed almost entirely of nonbeneficiaries, assess the same policies, thereby contributing to studies on how policy design affects attitudes and views about policy and politics for both those directly affected and for larger audiences.

We start with a discussion of our argument in more detail; then we provide some context about MCMV and Brazil's political system. We move to our empirical tests, first with our original surveys of MCMV applicants, and subsequently through an examination of nonbeneficiaries' views on MCMV and its impact on voter choice. We then discuss the possible drivers of the null electoral returns among beneficiaries and positive dividends among nonbeneficiaries. We conclude by discussing the implications for public policy and accountability.

## 1 Beneficiaries and the Electorate

The evidence on the direct electoral effects of social policies is mixed. On the one hand, studies on conditional cash transfers (De La O 2012, Manacorda, Miguel and Vigorito 2011, Zucco 2013), disaster relief spending (Bechtel and Hainmueller 2011), and other governmental transfers (Pop-Eleches and Pop-Eleches 2012) find that beneficiaries reward incumbents for social programs. On the other hand, several studies cast doubt on these claims.

De Kadt and Lieberman (2020), examining improvements in service provisions across several southern African countries, found negative electoral returns to the incumbents. Imai, King and Velasco Rivera’s (2020) analysis of a health insurance scheme and of Mexico’s *Oportunidades* finds that beneficiaries did not vote in favor of politicians who initiated the program. Blattman, Emeriau and Fiala (2018) find that cash grants had, in fact, an anti-incumbent effect. Examining the U.S. context, Clinton and Sances (2018) show that the highly salient Affordable Care Act had a limited effect in mobilizing beneficiaries to vote.

With respect to housing policies, findings are fairly consistent regarding the effects on political engagement. In the U.S. context, homeowners turn out at higher rates in local elections (Hall and Yoder forthcoming) and property owners are more likely to participate in local council meetings and donate during elections (Yoder 2020). Kumar (forthcoming), examining a housing lottery in India, finds that becoming a homeowner increases local nonelectoral mobilization. Relatedly, in Mexico, a government program that issued property rights over agricultural lands increased support of the right-wing party but did not cause an increase in support for the benefactor party (de Janvry, Gonzalez-Navarro and Sadoulet 2014).

All in all, providing a coveted and important benefit such as a home, which is unattainable without government assistance for the overwhelming majority of individuals who qualify for governmental social programs, could lead to change in vote choice in favor of politicians who created and supported such programs. If voters are expected to respond politically to government efforts to provide any good or service, housing is a leading contender to generate large political effects (Ansell 2019). Our pre-registered hypotheses were in this direction.<sup>1</sup> Yet, as we noted earlier, we fail to find such electoral rewards among beneficiaries.

Our unexpected results are, in fact, consistent with the absence of documented *pro-incumbent*

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<sup>1</sup>We expected winners to support the Workers’ Party incumbents at higher rates than non-winners; we did not anticipate the null effects for incumbent support that we describe next and we did not pre-register our observational analysis of the indirect effects among nonbeneficiaries. See Appendix J for our pre-analysis plan and deviations from it.

electoral effects in government sponsored housing or homeownership programs (e.g., Huberty 2011). One interpretation of studies that find that beneficiaries do not reward benefactors is that these policies backfired politically. Electorally motivated politicians, by this logic, should not insist on housing schemes. The fact that these policies often survive suggests either that politicians are making mistakes or that political benefits lie elsewhere.

Policies are not just about beneficiaries, however. They also function as signals by which the electorate learns about the incumbents' priorities (Boas, Hidalgo and Toral forthcoming), redistributive commitments (Corrêa and Cheibub 2016, Holland 2017), and competence. As Weitz-Shapiro notes, "a policy's 'audience' – that is, not its direct targets – may reward a politician for particularly effective implementation, just as they may punish him for poor implementation. [This point] also suggest[s] an alternative mechanism that can help explain prior findings that identified the sometimes null or negative electoral effects of spending on patronage and social programs" (Weitz-Shapiro 2014, p. 23). Corrêa and Cheibub (2016) examine the reception of social policy among beneficiaries and nonbeneficiaries as it relates to conditional cash transfers in Latin America, and they find that part of the conditional cash-transfer (CCT) electoral bonus received by incumbents is diminished by backlash against these programs. Lü (2014) makes a related point when he finds that the untargeted population, influenced by campaigns to increase policy awareness and not by the policy benefit itself, is responsible for the effects of policy on government responsibility.

For such a signaling effect to exist, nonbeneficiaries must, at a minimum, be aware of the policy. While this awareness may be a challenge in certain policy areas such as tariffs or subsidies that only are only visible to those involved (Mettler 2011), housing projects are tangible and visible to all, and therefore particularly suited for reaching broader audiences. As Gilbert and Ward (1985) point out, building public housing serves the purpose of showing that the state is working for the poor. Politicians often seek to further enhance visibility of housing projects by building them close to major thoroughfares, and ribbon-cutting ceremonies and housing-related stories invite media coverage that shapes awareness among the wider public (Folha de São Paulo 2002).

To fully understand the electoral effects of housing programs, therefore, it is critical to focus on the different ways that beneficiaries and nonbeneficiaries are touched by these programs. Beneficiaries assess a policy, and the incumbents tied to it, based on their own well-being, rewarding officeholders for positive changes and penalizing them for negative ones (Larsen et al. 2019). Government benefits might also induce a reciprocity bond between beneficiaries and politicians, irrespective of actual changes in well-being, even if a program is not necessarily corrupted by clientelism (Labonne 2013). Finally, the experience of participating in social programs and interacting with the state can influence electoral choices, attitudes, and broader forms of involvement in politics (Soss 1999). In sum, experience with a particular program is a vital engine in shaping beneficiaries' reactions to a policy.

Nonbeneficiaries often use different benchmarks to evaluate policies when compared to beneficiaries. Arguably, implementation matters less to nonbeneficiaries because they have less information on a program's execution. That is not to say that there is no interpersonal communication between beneficiaries and nonbeneficiaries; nonbeneficiaries may learn about a program via secondhand experience. Yet whether a program improves others' well-being may be relevant in principle for observers in their judgment a policy, it is certainly less salient than for those whose well-being is actually influenced. Secondhand information on personal experiences likely has more limited scope than other public and media sources of information on programs that are visible yet concentrated on few beneficiaries.

As the extensive literature on policy feedback shows, the public's perception of social programs' beneficiaries (e.g. race) and the stereotypes associated with them tend to influence whether public opinions favors a policy (Gilens 2009). Furthermore, characteristics of the policymaker, particularly her party affiliation, often weights heavily in how the public evaluates the policies she initiates. Finally, a program may be simply evaluated by observers based on its stated goals (e.g., access to higher education, reduction in homelessness, etc.) and the attributed importance of that policy goal.

From the policymaker perspective, the relative electoral importance of beneficiaries and nonbeneficiaries' depends on numbers in ways that are not always obvious. For many policies

and programs, MCMV included, the number of beneficiaries is much smaller than that of nonbeneficiaries. The electoral importance of beneficiaries, consequently, might pale in comparison with that of nonbeneficiaries.

Part of the general appeal of the program is precisely that it creates high expectations about receiving a very desirable benefit across a large number of potential beneficiaries. These high expectations about the program are tough to meet, but frustration of expectations is only ever experienced by some of the few who are selected as beneficiaries. In other words, one of the reasons that the program is so popular among nonbeneficiaries also might be because nonbeneficiaries do not experience the frustrations with program delivery that beneficiaries do. But the relative sizes of these groups make the whole enterprise electorally worthwhile.

## 2 The MCMV program

The Workers' Party (*Partido dos Trabalhadores*, or PT) government created the *Minha Casa, Minha Vida* (MCMV) program in 2009 with the explicit goal of reducing the housing deficit but also as part of an economic stimulus package. As such, the MCMV aims to tackle different aspects of the housing problem by using a combination of subsidized credit and private provision for the middle classes with government provision and *heavily* subsidized financing for the low and lower-middle classes. Between 2009 and 2016, the MCMV program delivered 3.14 million housing units (out of 4.4 million contracted ones) at a total cost of about R\$ 319 billion (USD 53 billion) (Biderman 2019, p. 60–65). As a comparison, the well-known conditional cash transfer *Bolsa Família* benefits 14 million families at a cost of about R\$ 30 billion per year. Eight years of MCMV cost more than a decade of *Bolsa Família* while covering less than a fourth of the number of families.

We focus on Tier 1 of the program, geared towards lower-income families, which accounts for about 36% of the MCMV housing units (1.15 million) and about 26% of its spending (R\$ 84 billion). Families that do not own a home and with income lower than R\$ 1,800 (approximately USD 300) per month are eligible for the program, which subsidizes up to



90% of the value of the unit.<sup>2</sup> Families that are selected make monthly payments varying from R\$ 80 to R\$ 270, depending solely on their income, for up to 120 months. MCMV Tier 1 selects beneficiaries through public lotteries. Evidence from existing research (Bueno forthcoming) and official audits suggests that incumbents have limited ability to distribute MCMV housing units as outright clientelism.

The program turns beneficiaries into *de jure* “borrowers”, but they are *de facto* homeowners because monthly payments are minimal, procedures to evict defaulting beneficiaries are cumbersome, and anecdotal evidence suggests that beneficiaries informally sell and rent their homes (against the program’s rules) prior to completing their payments. MCMV Tier 1 is all but a gift to its beneficiaries.

The MCMV program’s institutional setting involves federal and local governments, public (and in a few instances, private) banks, and private developers. The federal government sets the general rules and provides financing. Local governments and private developers submit proposals to the state-run bank and/or to the Ministry in charge of the program for the construction of housing units. Local governments select beneficiaries and also provide tax breaks, the infrastructure surrounding the housing project, a matching grant to offset construction costs, and social assistance for the beneficiaries once they move.

Although spearheaded by the PT, the MCMV program was widely supported by left- and right-wing parties, business elites, and housing social movements. The program is considered so important by politicians that the ministry in charge of it was ranked as the most “politically desirable” among the almost 40 ministries that were recently evaluated by members of Congress (Zucco, Batista and Power 2019). Yet, despite this widespread support, the program faces many criticisms. It tends to build housing in the outskirts of cities where land is cheaper, creating urban sprawl into areas that are often ill-equipped with public services (Hiroto 2018). In fact, studies that leverage on MCMV lotteries find that lottery winners are not more (and sometimes less) likely than nonwinners to hold on to jobs and

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<sup>2</sup>The other tiers of the program provide much smaller subsidies and credit for the lower middle class.

more likely to sign up for CCT programs, presumably because the projects' locations are far from economic centers (Chagas and Rocha 2019). Not surprisingly, despite being almost a gift, many lottery winners decline the unit that is offered to them, as we discuss in more detail later. The program is also criticized for achieving too little while spending too much as the housing deficit for Brazil's lowest-income stratum remains at about 3.8 million homes, and smaller municipalities have difficulty meeting the MCMV requirements and enticing contractors.

To the general public, however, the program's shortcomings are much less known than its achievements. Even though MCMV distributes a private good, MCMV Tier 1 projects in larger cities are typically large and conspicuous complexes. In small towns, even isolated houses or complexes with few units are visible to voters, as they follow predetermined architectural standards, and look very similar to one another across the country. Therefore, to the broader electorate, the MCMV projects stand out as a very visible government program.

### 3 Do MCMV Beneficiaries Reward Incumbents?

To determine whether program recipients are more likely than nonrecipients to support incumbents, we conducted two surveys based on the MCMV lotteries used to select beneficiaries. We fielded our surveys in Rio de Janeiro, the second-largest municipality in Brazil, and one which has conducted multiple MCMV lotteries since 2011. As of 2017, about 743,000 individuals had participated at least once in these lotteries, 46,000 people had been selected, and close to 29,000 housing units had been built and delivered. During most of the period under analysis (2011-2016), Rio was governed, at the local and state levels, by politicians affiliated with the *Movimento Democrático Brasileiro* (MDB) – Mayor Paes (2012–2018) and Governors Cabral (2007–2014) and Pezão (2014–2019). At the federal level, the PT held the presidency from 2003 to 2016. Until 2016, the state MDB supported the PT government at the federal level.

### 3.1 Data

In our recent lotteries survey, we interviewed 795 lottery winners and nonwinners who participated in lotteries in 2016. We used two lotteries carried out within less than a month of each other and were the most recent at the time we conducted our survey in 2017. Lottery winners had not yet signed an MCMV agreement or received their new home. However, the winners had been informed of the lottery outcome, except for those who could not be found by city hall, a point we discuss later. In this survey, treatment is defined as knowing that one had won a housing lottery, which is considered a credible promise of receiving a benefit in the near future.

In our early lotteries survey, we interviewed 2,119 individuals who participated in two lotteries in 2011, carried out within three months of each other. Lottery winners who signed the MCMV agreement moved into their units in 2012 and 2013. We selected a sample of lottery winners and nonwinners and conducted interviews in the first half of 2020, so our treated sample had moved into their new homes about eight years earlier. Therefore, in this second survey, treatment is defined here as being a MCMV beneficiary.

To select our samples, we selected all winners and a random sample of nonwinners with the pre-registered expectation that we would be able to interview about 10% of our samples. We purchased sampled subjects' contact information and interviewed them via phone. Refusal rates were relatively low, but the main challenge was finding the subjects as most phone numbers we procured were nonworking because individuals of lower socioeconomic status tend to change (mostly pre-paid) phone numbers frequently.<sup>3</sup>

### 3.2 Design

The estimation strategy is similar in both recent and early lotteries surveys. Our assignment to treatment variable is the indicator for whether an applicant  $i$  won a lottery  $j$ . Randomization takes place at the lottery level, which allows us to estimate of the effect of winning lottery  $j$ . Applicants may apply to multiple lotteries, so our unit of analysis is at

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<sup>3</sup>See [Appendix A](#) for details about the sampling procedures.

the applicant-lottery level.

As each of the lotteries implies different probabilities of assignment to treatment, we follow Lin (2013) and include demeaned lottery indicators (and controls, whenever appropriate) that are cross multiplied with the treatment indicator.<sup>4</sup> We estimate equation 1,

$$Y_i = \beta_0 + \beta_1 Z_{ij} + \sum_{m=1}^M (\mu_m \mathbf{X}_i + \sigma_m Z_{ij} \times \mathbf{X}_i) + \sum_{j=1}^{J-1} (\Lambda_j B_{ji} + \gamma_j Z_{ij} \times B_{ji}) + u_{ij} \quad (1)$$

where  $Y_i$  is an outcome measure for an applicant  $i$ ,  $Z_{ij} = 1$  represents a winning applicant  $i$  in lottery  $j$ , and  $Z_{ij} = 0$  represents a non-winning applicant  $i$  in lottery  $j$ . The coefficient  $\beta_1$  represents the effect of winning a lottery.<sup>5</sup>  $\mathbf{X}_i$  is a matrix of  $m$  demeaned pre-treatment covariates,<sup>6</sup>  $\mu_m$  is the coefficients vector for the demeaned pre-treatment covariates, and  $\sigma_m$  is the coefficients vector on the interaction between these covariates and the treatment indicator. In this equation,  $J = 2$ , for there are only two lotteries in each survey sample,  $B$  is a demeaned dummy indicator for whether the observation belongs to either lottery,  $\Lambda_j$  is lottery  $j$ 's effect, and  $\gamma_j$  is the coefficient on the interaction between the demeaned lottery indicator and treatment indicator. Standard errors are heteroskedasticity-robust clustered at the applicant level.

Although we did not administer the lotteries ourselves, the process is transparent; individu-

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<sup>4</sup>Survey design, outcomes, and analysis follow our pre-analysis plan, as reported in Appendix J.

<sup>5</sup>We report the complier average causal effects in Appendix F.

<sup>6</sup>Our (pre-registered) pre-treatment covariates are age, sex, race, having a formal job, (logged) formal wages, and indicator for being in the national registry for social programs (not pre-registered). In the first survey, we also include religion, years of schooling, and whether children are living at home (not available pre-treatment in the second survey). In the lotteries used in the first survey only, lottery winners were sorted by age in decreasing order. For this survey, we control for age even in the estimation “without controls.”

als are publicly assigned numbers prior to the drawing date, and winners are selected based on results from a national lottery. Not surprisingly, analysis of balance (Appendix C) does not reveal any systematic differences between treatment and control groups on pre-treatment covariates. Still, we report results with and without pre-registered sociodemographic controls.

Not all lottery winners decided to become program recipients or even learned about being lottery winners. All lotteries are public; however, only lottery winners are notified directly, and they are often not found by city officials. Also, many winners are deemed ineligible or decide to decline the offer – a point to which we return later. We focus on the intent to treatment analysis because we believe it to be the most politically relevant quantity. This is because noncompliance is a feature of the program, and we are interested in learning about effects of the program on political behavior.<sup>7</sup>

As in many studies of government-run lotteries in low-income countries, attrition was an ex-ante concern. We did not, however, find evidence of differential attrition rates across treatment and control groups in either of our surveys (Appendix D). We also took several steps to minimize the possibility that attrition systematically related to potential outcomes: our enumerators were blind to treatment assignment, the order for contacting subjects was randomly determined, and our protocol for contacting control and treatment subjects was the same.

We also compared our survey sample estimates to an administrative population data “benchmark,” for which there is no non-response on two formal employment related outcomes. In the administrative data, these variables are measured for all 623,598 individual-lottery observations for the two 2011 lotteries from which we selected our early lotteries survey sample.<sup>8</sup>

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<sup>7</sup>Overall conclusions are similar had we focused on the treatment effects among compliers (Appendix F).

<sup>8</sup>We do not have post-treatment administrative outcome data collected for the recent lotteries survey sample.

Results based on our sample are statistically indistinguishable from those using this population data, and both indicate that there is a null effect of winning an MCMV lottery on earnings from a formal job and having a formal job (see Appendix Table D5).

### 3.3 Outcome Variables

The key variables in our analysis are respondents' evaluations of incumbents and electoral mobilization, which we grouped into indices of items related to each conceptual outcome of interest, as described in Table 1. For brevity, we focus here on the summary indices only and report results for all individual outcome items in Appendix E. All indices were created by adding variables' z-scores. This procedure was pre-registered in our PAP, but we had not planned on constructing indices for all outcome groups. The use of indices, however, simplifies presentation and clearly conveys that results were null for almost all items.

Table 1 lists all outcomes examined in the paper. We analyze respondents' attribution of responsibility for the MCMV program, which indicate whether respondents connect the program to the incumbents being evaluated. The key outcome is the *Incumbent at Treatment Evaluation Index*, referring to subjects' evaluation of incumbents at any tier of government at the time of treatment assignment. Given the PT's status as the creator of MCMV, we also assess survey respondents' assessment of PT politicians and the party itself in the *PT Evaluation Index*. We created a *Current Incumbent Evaluation Index* to examine effects on incumbents at the time of the survey. We also examine evaluations of incumbents at the national and local levels, since MCMV is a policy implemented by both tiers of government. Finally, we include a *Mobilization Index* that captures other electorally relevant behavior. All outcome variables were reoriented, meaning that higher values indicate positive effects.

### 3.4 Results

Figure F1 reports results. We find, in both surveys that a large plurality of nonwinners attribute the program to PT presidents Lula and Dilma, or the PT in general and that this share is even larger among lottery winners. This attribution bolsters the expectation that

Table 1: Survey Outcomes Indices and Variables

Outcome	Survey Item	Type	Survey
PT Evaluation Index	Lula Evaluation	5-point	Both
	Dilma Evaluation	5-point	Both
	PT ID	Binary	Both
	Vote Haddad 2018	Binary	Early
Incumbent at Treatment Evaluation Index	Dilma Evaluation	5-point	Both
	Temer Evaluation	5-point	Recent
	Paes Evaluation	5-point	Both
	Pezão Evaluation	5-point	Recent
Current Incumbent Evaluation Index	Bolsonaro Evaluation	5-point	Early
	Crivella Evaluation	5-point	Early
	Witzel Evaluation	5-point	Early
Local Incumbent Evaluation Index	Paes Evaluation	5-point	Recent
	Pezão Evaluation	5-point	Recent
	Vote Pedro Paulo	Binary	Recent
	Crivella Evaluation	5-point	Early
National Incumbent Evaluation Index	Dilma Evaluation	5-point	Recent
	Temer Evaluation	5-point	Recent
	Bolsonaro Evaluation	5-point	Early
Mobilization Index	Talk to a Candidate	Binary	Early
	Support a Candidate	Binary	Early
	Turnout in 2016	Binary	Early
	Turnout in 2018	Binary	Early
	Expected Turnout 2020	Binary	Early
Attribute to Lula, Dilma, or PT	Responsibility for MCMV	Binary	Both
Attribute to Paes, Pezão, Temer, or MDB	Responsibility for MCMV	Binary	Both

Notes: All variables were standardized prior to analysis. Survey question wording available in Appendix B.

winners could reward PT incumbents since they are the ones seen as responsible for the program.

However, we also find that lottery winners *do not* have better evaluations of the PT than nonwinners. Furthermore, lottery winners are not more likely than nonwinners to vote in elections, support a candidate, or talk to a candidate, as reported in the null result for the Mobilization Index, which suggest that MCMV did not lead to pro-PT or pro-incumbent mobilization.

This lack of effects towards PT politicians, however surprising, cannot be pegged on a hypothetical “ceiling” effect either. Lula – the most popular politician in our data – was only rated in the highest response category by 16.3% of those in the control group in our survey, and his average rating was only slightly (0.12) above the middle category on the five-point scale we used. We can also dismiss the idea that null findings are driven by the fact that the beneficiaries do not have full property rights. Lottery winners are more likely to self-identify as homeowners (0.0619,  $p < 0.01$ ) and about 81% of beneficiaries do so.

The fact that Lula’s evaluation was not particularly high should also help dispel fears that these null effects could be the result of heavily pro-incumbent individuals signing up for MCMV in the first place. We already knew that enrollment in MCMV was easy for all, widespread, and not politically targeted; we now know that those who signed up were not particularly pro-incumbent. Moreover, the support for the PT in this sociodemographic in Rio is low, with less than 7% of our respondents in the control group expressing support for the PT. While we would need a comparison of applicants and non-applicants to definitely rule out that partisans were more likely to apply, the evidence that we do have weighs strongly against this possibility. Importantly, the survey was conducted in a period of aversion towards past governments, particularly those from the PT. While this might have depressed support for some of the politicians evaluated in our surveys, this lower support should have affected both winners and nonwinners equally, so it cannot easily explain the lack of differences between the two groups.



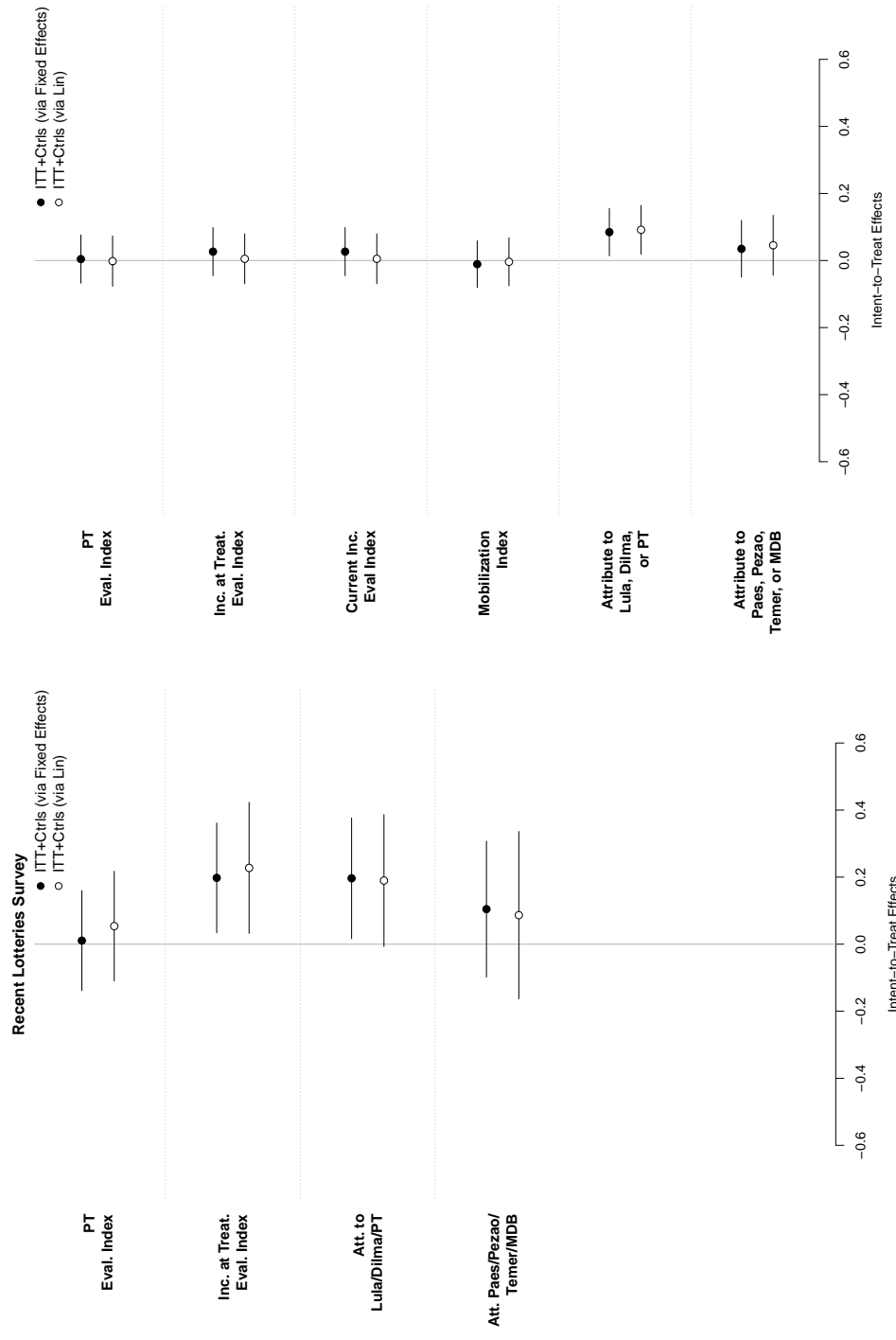


Figure 1: MCMV Effects on Incumbent Evaluation and Mobilization

Notes: Figure reports intent-to-treat effects on outcomes listed in Table 1. See Appendix E for results for each item used to construct these indices.

Despite some effects of winning MCMV were found for other incumbents, the few positive results are very weak. In our early lotteries survey, comparing winners and nonwinners about 8 years after winners had moved to their new homes, we find that winners do not have better evaluations of the incumbents who governed at the time they won the lottery compared to nonwinners, and that winners do not hold current incumbents in higher regard. Yet, for our recent lotteries survey that compares winners and nonwinners after the lottery results were public, but before moving to the new house, we find that winners have higher evaluations of incumbents at treatment. This effect is almost entirely driven by recent lottery winners' higher evaluations of local-level incumbents at the time of treatment (mayor Paes and governor Pezão), but they do not hold higher evaluations of national incumbents at time of treatment (Dilma and Temer), or a mayoral candidate supported by mayor Paes.

One interpretation is that MCMV has a short-term effect that then dissipates over time. This is a plausible interpretation, but, based on our findings, any electoral short-term bonus to incumbents is likely to be electorally negligible. As a preliminary consideration, winners do not attribute MCMV to Paes or Pezão at higher rates than nonwinners and, overall, only about 6% of respondents attribute MCMV to Paes and Pezão, combined. More importantly, the political magnitude of these statistically significant effects for Paes is *very* small. Paes's evaluation is 0.23 points higher among lottery winners in an evaluation scale ranging from  $-2$  to  $2$ . Considering that lottery nonwinners, on average, rate Paes's administration at  $-0.39$ , winning MCMV's lottery would not tip them past the center of the scale (i.e., "regular"). Using simulation, and generously assuming every individual assigned to treatment receives a boost of 0.23 points in Paes's evaluation, and that any evaluation of Paes as doing a good/excellent job implies that an individual would vote for him with certainty, we estimate Paes would get, on average, 292 more votes out of the 2,911 lottery winners. Finally, this boost in Paes's support did not translate into more votes for the candidate backed by Paes in 2016 or more support for Paes in 2020 (Appendix E).

Granted, our results cannot determine whether beneficiaries who just moved are the ones that yield the greatest payoff to incumbents. This explanation would require an inverted-V shaped response: voters do not reciprocate when informed that they won, they reciprocate

just after moving, and their support decays over time. All in all, at an average cost of at least 38,000 reais (6,300 dollars) per housing unit (without considering implementation and local costs), the MCMV seems like a poor electoral investment.

## 4 Do Voters Reward Incumbents for MCMV?

MCMV provides weak to no direct electoral returns among beneficiaries. Tier 1 beneficiaries, however, are a fairly small group (less than 0.6% of the population), while the program's broader electoral audience is many times larger.

The potential return of MCMV to win votes among nonbeneficiaries is enormous. In the city of Rio, for example, only 42,000 individuals became recipients, but more than 700,000 signed up for MCMV and many more know about the program. Even if beneficiaries do not reciprocate electorally, the opinions of eligible nonbeneficiaries or even of non-eligible observers might matter more for incumbents' electoral fortunes.

This strategy of using MCMV as advertisement makes sense when we consider that the program is held in high regard by the general public. In a nationally representative survey conducted during the 2014 presidential campaign, 95% of Brazilians approved of the MCMV program. Notably, the MCMV's approval rate was then about 20 percentage points higher than other PT flagship social programs like *Bolsa Família* (a CCT) and *Mais Médicos* (a provision of basic medical assistance to under-served areas), and only 2 percentage points lower than the *Benefício de Prestação Continuada*, which is a pension for low income elderly and those with special needs that is written into the 1988 constitution and was not initiated by the PT.

Another survey, carried out by one of the main polling companies in Brazil in November of 2015,<sup>9</sup> shows that MCMV was known by 50% of respondents. MCMV was the second most

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<sup>9</sup>This survey interviewed more than 15 thousand respondents in all 27 states and included detailed questions about public policies and government programs. It also produced very plausible estimates of *Bolsa Família* and MCMV coverage (23% and 1.5%, respectively).

well-known government program or initiative at the time, behind only *Bolsa Família* (59%), which is striking given that MCMV’s coverage is substantially smaller.

MCMV was not only well known, but also well evaluated. It was rated third among the eight most well-known programs, rated higher than *Bolsa Família*, particularly if we consider only nonbeneficiaries of both programs. Potential MCMV beneficiaries – those who signed up for the program but are not recipients – also hold the program in high regard. Using data from our own survey of lottery participants in Rio de Janeiro, we find that 80% of nonbeneficiaries believed MCMV made beneficiaries’ lives better or much better.

These bits of evidence suggest that MCMV might be a political asset even if it does not produce any direct political effects on beneficiaries. To systematically test whether MCMV produces electoral effects on the broader electorate, however, we cannot leverage on the lottery as we did in the previous section’s comparison between beneficiaries and nonbeneficiaries. We explore, instead, the staggered introduction of MCMV across Brazilian municipalities and electoral cycles to tease out its association with the electoral performance of presidential and local incumbent candidates.

## 4.1 Empirical Setup

Through multiple freedom of information requests we obtained the lists of all beneficiaries of the MCMV Faixa I through late 2021, with location of the unit and date it was delivered. The program data spans three four-year national-level electoral cycles. The first beneficiaries received their units in late 2009, and by the 2010 election, 60,000 had been delivered in about 700 municipalities. The program expanded greatly in the next cycle, reaching more than 700,000 beneficiaries in almost 4 thousand municipalities by the time of the 2014 election. The MCMV delivered more than 150,000 units in both 2015 and 2016, but lost momentum after that, with the acute political and economic crisis that engulfed the country. By the 2018 presidential election 1.3 million units had been delivered in more than 4,200 thousand municipalities in the country.

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See [Appendix G](#) for more details.

Our main outcome of interest is the vote share for the incumbent candidates in presidential elections in each municipality, which in the period under consideration were always from the PT. We estimate simple two-period difference-in-differences models for each of the three presidential electoral cycles in our data. For each cycle, we include only municipalities that had *not* implemented MCMV in past cycles. This implies that all 5,570 municipalities are included in the first cycle (of which 2,888 implemented the program) but substantially fewer enter the analysis in the second cycle (2,682/1,107) and even less in the third (1,575/158). Analysis of the 2014–2018 cycle is further impacted by Dilma Rousseff’s removal from office in 2016 and replaced with her vice-president who was not from the PT, weakening the connection between the MCMV and voting for the PT candidate in 2018.

The other outcome of interest is the vote share for incumbent mayoral candidates. Mayoral elections in Brazil are also held every four years, but with a two-year offset relative to the presidential elections. The three local election cycles spanned by the MCMV, therefore, are 2008–2012, 2012–2016, and 2016–2020. As only about 100 municipalities were “treated” for the first time in the third local cycle, hardly any units were delivered in the last two of its years, and the 2020 election was first postponed and then held in the throngs of the Covid-19 pandemic, we only examine the first two cycles.

An important consideration for the analysis of local elections is determining who is the incumbent, which varies across municipalities. While cases in which the mayor personally runs for reelection are straightforward, this happens in only about half of the municipalities in each cycle. For cases in which the mayor did not run for reelection, we coded as the incumbent the candidate affiliated with the party of the mayor. As mayors often switch parties during their term, we updated the mayor’s party affiliation using the party membership data. If this procedure did not allow us to identify any candidate in the municipality, we then coded as the incumbent the candidate who was a member of a party that was in the mayor’s electoral coalition in the previous cycle. By employing these criteria we identified incumbent candidates in 93% and 90% of municipalities in the 2012 and 2016 elections, respectively. In the remaining cases we assumed that the incumbent mayor did not support any candidate

and coded the incumbent vote share as zero.<sup>10</sup>

For each cycle and for each office (presidential or mayoral), we then estimate variations of the following regression:

$$V_{ij} = \beta_0 + \beta_1 MCMV_i + \beta_2 Post_j + \beta_3 MCMV_i \times Post_j + u_{ij} \quad (2)$$

where  $i$  represents a municipality and  $j$  an election (presidential or mayoral). Here,  $V_{ij}$  is the vote share (between 0 and 1) obtained by the incumbent party candidate in each election  $j$  in municipality  $i$ . Then  $MCMV_i$  is, initially, a binary indicator of whether a municipality implemented the MCMV program in the election cycle being analyzed, and  $Post_j$  indicates whether the data point is the pre- ( $Post_j = 0$ ) or the post-treatment observation ( $Post_j = 1$ ) in the cycle. Here,  $\beta_3$  is our coefficient of interest, describing the vote share change in MCMV adopting municipalities relative to non-adopting ones. We cluster our standard errors at the municipality level. In [Appendix H](#), we examine two variations of this model in which we substitute the binary indicator of MCMV presence by a continuous indicator based on the number of beneficiaries per 1000 inhabitants and another indicator based the number of beneficiaries relative to the housing deficit.

MCMV's rollout was not as-if random, so municipalities that implemented MCMV could have been on a different economic or political path than those that did not implement it. Municipalities had to apply for MCMV and be approved by the federal government to receive funds, contract companies for housing construction, and distribute units to beneficiaries. We present evidence, via a regression discontinuity-design (in [Appendix I](#)) that MCMV was not targeted to mayors belonging to the president's or to the minister's party. We also do not find evidence that MCMV funds were given earlier to mayors who are members of president's party. In [Appendix H](#) we also report robustness tests employing matching municipalities on pre-treatment observables. None of these variations change the results.

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<sup>10</sup>See [Novaes and Schiumerini \(2021\)](#) for a discussion of local incumbency in Brazil.

## 4.2 Results

Table 2a reports results for presidential elections, and Table 2b for mayoral elections. Each panel in the table reports results using the dichotomous operationalization of MCMV presence by cycle and pooling all cycles.

For presidential elections, we see positive and statistically significant electoral effects in the first two cycles. In municipalities that delivered MCMV homes, PT presidential candidates obtained 1.1 and 2.5 percentage points (p.p.) more votes in municipalities that delivered MCMV homes, in each cycle, respectively. Even with the expected null effects on the third cycle, the pooled estimate shows average positive effects of almost 2 p.p. The estimated increase in vote shares for presidential candidates shown in Table 2 results in about half to three-quarters of a million votes for incumbent (PT) presidential candidates in those localities were due to MCMV.

Results for the mayoral elections are concentrated in the first cycle. In that year, incumbents in municipalities with MCMV received just under 2 p.p. more votes than in other municipalities.<sup>11</sup> As shown in Appendix H, for both mayoral and presidential elections, results from the models using continuous indicator based on the number of beneficiaries are substantively similar.

The less clear results at the local level could be driven by weaker attribution, but political and institutional features also limit our analysis. While at the national level the same party was in charge for 13 years, turnover varies considerably across municipalities and incumbency is therefore much harder to measure. Moreover, the 2016 election came only weeks after the impeachment of President Rousseff and other major political events that sparked a very strong anti-incumbent wave. Furthermore, our survey data from Rio indicates that a large majority of recent lottery participants (77.8%) attributed MCMV to presidents Lula, Dilma and/or the PT, which stands in stark comparison to the meager 6% that attributed it to the mayor and/or governor of Rio. This finding is consistent with our aggregate results,

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<sup>11</sup>See Appendix H for details on these estimations.

Table 2: MCMV effects on incumbent candidates vote share

(a) Presidential Elections

	MCMV Presence			
	2006–10	2010–14	2014–18	Pooled
MCMV	0.011*** (0.003)	0.025*** (0.003)	0.001 (0.007)	0.019*** (0.003)
N	11132	5361	3150	19643
Mun.	5567	2682	1575	5570

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

(b) Local Elections

	MCMV Presence		
	2008–12	2012–16	Pooled
MCMV	0.018** (0.009)	-0.003 (0.011)	0.004 (0.005)
N	11140	5640	14340
Mun.	5570	2820	5455

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$



suggesting that electoral payoffs may be more consistent at the national rather than local level.

## 5 Discussion

Contra our pre-registered expectations, our results reveal weak to null electoral returns among beneficiaries. Yet we also find positive evaluations among a large audience of non-beneficiaries, electoral payoffs for presidential candidates and, to some extent, local incumbents.

Based on the existing literature and our own ex-post conjectures about these results, we interpret these weak and null effects for incumbents among beneficiaries as a result of the firsthand experience with the program, and, more specifically, its implementation and consequences for well-being. More specifically, the MCMV program generates high expectations among beneficiaries and nonbeneficiaries alike, but delivers uneven experiences for beneficiaries. To further explore the interpretation that beneficiaries' meager electoral returns to incumbents result from a disappointing experience with the program by at least part of the beneficiaries, we examine the program's consequences on well-being and analyze whether beneficiaries with more positive (negative) experiences with MCMV supported incumbents at higher (lower) rates.

If beneficiaries reward incumbents due to improvements in their welfare, we expect that null electoral returns are accompanied by no or negative improvements in well-being for lottery winners. And, as shown in Table D5, early lottery winners do not report higher levels employment and formal income, which is consistent with findings by Chagas and Rocha (2019). Bueno, Nunes and Zucco (forthcoming), examining several dimensions of welfare and happiness, also do not find evidence that participating in MCMV leads to greater well-being.

Furthermore, our research suggests that MCMV delivers an uneven experience rather than a uniformly disappointing one to beneficiaries. To examine this heterogeneity, we com-

pare individuals' evaluation of incumbents based on their wait time after registering for the program.<sup>12</sup> Given the large interest in the program, demand outstrips supply and most applicants never receive a housing unit. Thus lottery winners can wait for years in the program's rolls until they are selected via lottery.

Furthermore, our understanding for why wait time is salient to individuals is informed by 15 qualitative in-depth interviews (with 10 winners and 5 nonwinners) that we conducted with MCMV applicants in Rio de Janeiro in 2018. Many of our interviewed subjects did not recall when they signed up because too much time had elapsed. Indeed, a few of them used important life events to try and remember when they first registered, such as the birth of their children or divorce. For instance, S5 told us the following:

**I:** Talking about MCMV, do you remember when you signed up for the lotteries? **S5:** It has been too long. I can't tell you what year because I really don't know. For you to have a sense, let's calculate. I got separated, my daughter was [omitted] and now she is [omitted] and I had already signed up by then. And then you can say... I was [omitted] then, and now I am [omitted]. Man, I really have no idea. **I:** But if you daughter is [omitted] and she was [omitted] then, it has been 17 years... **S5:** Yeah, it may be that. I don't know what that was, honestly, even if I tried, I couldn't remember.<sup>13</sup>

This wait time caused frustration and many indicated that it was their main complaint about the program. Take, for example, interview S8's statement:

**I:** In your opinion, what was the main disappointment with the program? **S8:** The delay in getting selected in the lottery.

S2, a lottery winner, was also frustrated by such a long wait. For her, long wait times were evidence of political corruption:

**I:** And do you think that what made you lose hope and faith was all this wait? **S2:** Yes. Yes, how come it takes so many years? There is so much money and things involved. **I:** And who do you think is responsible for all this wait? What caused all this wait, in your

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<sup>12</sup>Another possibility would be to analyze winners' evaluation based on the location of their housing projects, but we lack sufficient variation for that variable.

<sup>13</sup>MCMV was implemented in Rio in 2011 and the interview was conducted in 2018.

opinion? **S2:** Why this wait? I think there is too much corruption. It can only be too much corruption and stealing. **I:** Can you point to who is responsible? A party or group? **S2:** I think it's all of them.

On the other hand, winners with shorter wait times describe better experiences with the program. S6, for instance, remembered exactly when she signed up (2-3 years prior to being selected):

**I:** On the balance, what was your main expectation with the program? [...] **S6:** I received what I expected. [...] **I:** Did you have any disappointment with the program? **S6:** No, none so far.

To further explore these insights from the qualitative interviews, we turn to our survey of recent lottery participants.<sup>14</sup> Using administrative data, we measure the time elapsed since a participant registered for the program until our interview for both winners and nonwinners. We then compute the difference, between winners and nonwinners, in incumbent evaluation for those who had lower-than-median wait time and for those who had higher-than-median wait time.

Results are shown in Figure 2. Winners who had high wait times since enrollment had markedly lower evaluations of the PT ( $-0.21$ ,  $p\text{-value} = 0.06$ ), Lula ( $-0.29$ ,  $p\text{-value} = 0.04$ ), and Dilma ( $-0.24$ ,  $p\text{-value} = 0.08$ ) than the control group, whereas those with low wait times since enrollment tended to have more positive evaluations of these incumbents than the control group (even if not always statistically significant at conventional levels). The differences between groups with high and low wait times are statistically significant for PT Inc./Lula/Dilma evaluations. Interestingly, we see much less variation in the evaluation of local level incumbents, who are seen more positively overall. PT incumbents appear to receive both credit and blame for the program, as expected by subjects' higher levels of attribution to them. Overall, this evidence is consistent with the interpretation that individuals' evaluation of incumbents varies with their experience with the program, particularly for incumbents more closely associated with the program.

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<sup>14</sup>All of our early lotteries survey winners won early in the program.

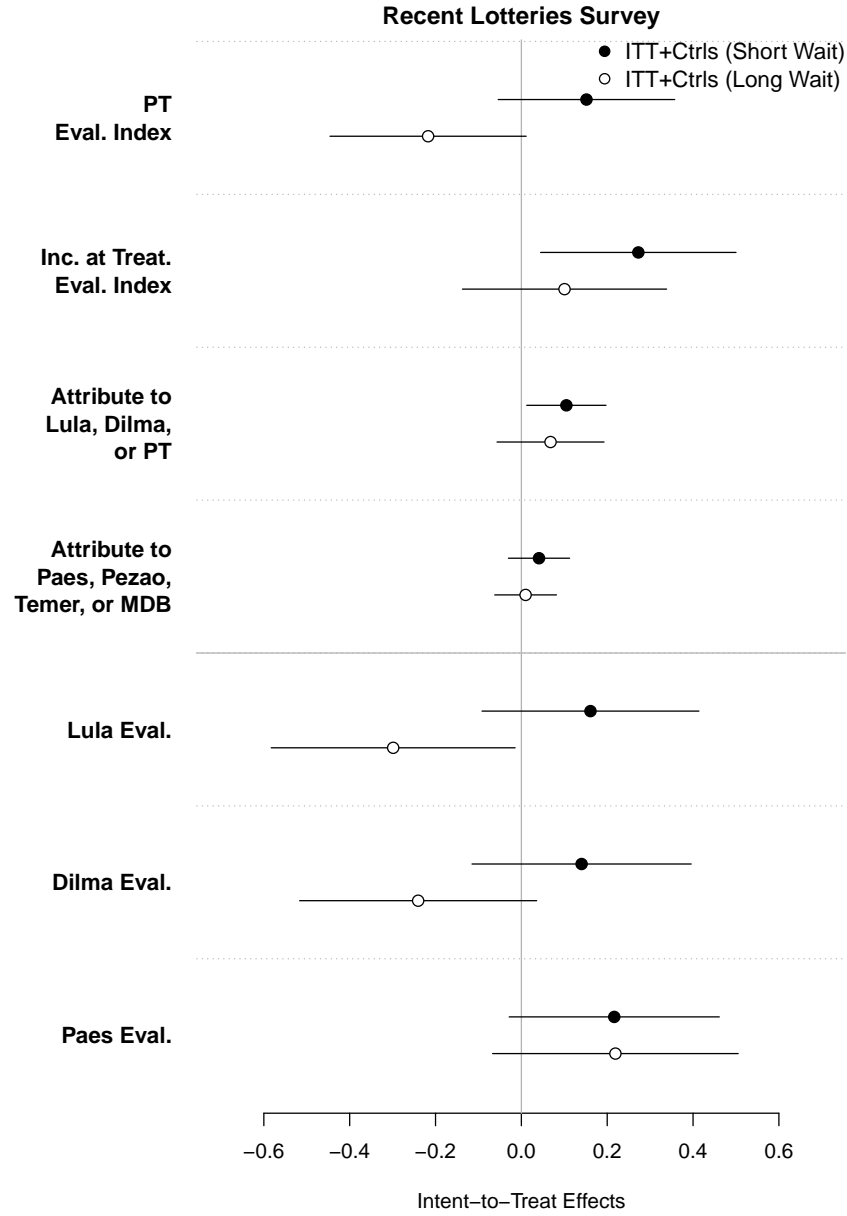


Figure 2: Effects of MCMV Condition on wait time Since Enrollment  
 Notes: Bars indicate 95% confidence intervals. Low and high wait times at the median.  
 All estimates using lotteries as fixed effects.

Regarding nonbeneficiaries, we also conjectured that their sources of support for incumbents due to the MCMV stemmed from pathways other than the program’s implementation or its impact on well-being. We believe that part of the electoral returns is at least partially driven by official advertisement and credit-claiming attempts by incumbents. Greater association of

the MCMV with the PT presidents could be a consequence of the centrality of the program in both the 2010 and 2014 presidential campaigns. In personal communication with the authors, João Santana, President Dilma’s publicist, noted that MCMV was cited 31 times in the 40 TV and radio campaign advertisements aired in 2010, and 45 times in the 39 spots broadcast in her 2014 reelection campaign. According to Santana, “MCMV was part of a broader context that encompassed upward social mobility, the dream of owning a home, buying a car, having meat on the table, etc. In other words, it was part of a package that Brazilians in general – and the poorest in particular – were experiencing at the time.”<sup>15</sup> MCMV was an important part of a communication strategy targeted far beyond just beneficiaries.

Electoral rewards for mayors were confined to the first cycle only, but mayors still behave as if they perceive the MCMV as an electoral asset. Bueno (forthcoming), for instance, shows that in local election years, politicians time the delivery of MCMV housing to the period in which they can legally attend public ribbon-cutting events and post coverage thereof on social media.

Furthermore, MCMV can create electoral dividends among beneficiaries because of its goals and its conspicuous and concrete benefits, which combined create broad social consensus. Providing homeownership to low-income individuals, primarily through newly built units, is a program goal that is not likely to invite backlash. In our survey in Rio de Janeiro, for instance, more than 90% of participants – winners or nonwinners – agreed with the statement that “homeownership is a dream.” Moreover, the survey data that we presented in Section 4 also show that evaluations of the MCMV vary less with income than the evaluation of other social programs, suggesting broad-based support, as opposed to some CCTs (Corrêa and Cheibub 2016).

What the general public sees in the case of MCMV are the new houses being built and people in need moving into them. For most outside observers, delays in delivering the units or the low desirability of their location may not be as salient as it is to beneficiaries. Crucially, the very large group of nonbeneficiaries will never have direct contact with the program and

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<sup>15</sup>Personal communication on 08/12/2020.

will therefore not experience any disappointment. Hence, a very visible program that caters to the dreams of the masses can be a political asset even if it does not completely fulfill expectations of beneficiaries.

Importantly, our interpretation relies on the assumption that the aggregate effects we document in the difference-in-differences analysis must come from nonbeneficiaries. This assumption implies that these direct effects are also weak to null in places other than Rio.

While our evidence on the direct effects comes from a single very large city, we have reasons to expect similar null or weak electoral effects among beneficiaries to be found in other contexts as well. Like in Rio, beneficiaries elsewhere are not better off in terms of their labor market prospects (Chagas and Rocha 2019). MCMV housing projects in other metropolitan areas are also often located in the outskirts of cities, where land is cheaper. In fact, Hiroto (2018) documented that MCMV contributes to urban sprawl in 20 metropolitan areas.<sup>16</sup> Based on our own qualitative understanding of the program and anecdotal evidence, long wait times are also common elsewhere. In sum, the reasons why we believe we find weak and null direct effects of MCMV are present elsewhere.

Finally, we estimated that the aggregate increase in votes associated with MCMV is about half to three-quarters of a million votes in support of the presidential incumbent, using estimates from Table 2. If this number of votes were to come exclusively from beneficiaries, we would be assuming that a majority of all beneficiaries (combined across all election cycles) both turned out to vote and voted for the incumbent with certainty. While possible, we find this case implausible.

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<sup>16</sup>Barnhardt, Field and Pande (2015) examine a housing policy in India similar to MCMV and find that lottery winners were worse off in terms of access to public transportation, job opportunities, and social support.

## 6 Conclusion

We compared winners and nonwinners housing lotteries for the MCMV program, in Brazil, on several electoral outcomes. We find that beneficiaries are not more likely to vote for or support politicians associated with the program. Despite the lack of effects on beneficiaries, we identify an aggregate boost in the electoral performance of incumbents through a difference-in-difference analysis that leverages the staggered implementation of the MCMV across thousands of Brazilian municipalities. Our combined evidence also suggests these different effects for beneficiaries and non-beneficiaries are due to differences in how they experience and therefore evaluate the program.

While the notion that policies create constituencies and politics is not novel, further empirical developments and theoretical refinements are needed. It is clear that the existence of multiple feedback channels leads to questions about their relative importance, whether they are substitutes or complements, the conditions under which politicians favor one channel over others, and their significance in the creation of coalitions in support of different policies. Moreover, several unanswered questions remain about the potential trade-offs between these feedback channels. In the case of the MCMV program, we found that promoting tangible improvements to direct beneficiaries was not a pre-requisite for generating positive indirect political dividends through other feedback channels (nonbeneficiaries). Future studies can elaborate under which circumstances the electorate evaluates policies differently from the direct beneficiaries and what other feedback channels can influence the electoral dividends of public policy.

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# What You See and What You Get: Direct and Indirect Political Dividends of Public Policies

— Online Appendix<sup>1</sup> —

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<sup>1</sup>Parts of this online appendix may overlap or partly reproduce background information from other manuscripts currently under review or preparation that use the same datasets.

## Appendix A Summary Statistics

Table A1 shows information about the population and sample for each survey. Tables A3 and A2, that follow, present summary statistics for the main variables in each survey. The complete question wording is reported in Appendix B.

Table A1: Lotteries Included in the Surveys

	Early Lotteries Survey		Recent Lotteries Survey	
	2019-12 – 2020-07		2017-05 – 2018-01	
Edital	03/2011	06/2011	17.2016	20.2016
Lottery Date	2011-11-06	2011-08-13	2016-10-19	2016-11-05
Non-Winners	295,235	318,789	580,983	484,151
Winners	2,983	6,505	2,299	612
Delivery Dates	2011-12 – 2012-11		2017-12 – 2018-12	
Pre-Sample	22,157		8,032	
Contacted	3,772		1,283	
Interviewed	2,119		795	

Notes: Early lotteries were for the Park Imperial, Park Royal, Destri, Toledo, Rio Bonito, Estoril, Sevilha, Taroni, and Cascais housing projects, while the late lotteries included units in the Vila Carioca, Safira, Porto Fino, and Ametista projects. “Contacted” is defined as whether the selected person was found by our field team.

Table A2: Summary Statistics 2016 Lotteries Survey

	N	Min	Mean	Median	Std. Dev.	Max	Missing
Win. Lottery‡	1267	0.00	0.23	0.00	0.42	1.00	0
Compliance‡	1267	0.00	0.21	0.00	0.41	1.00	0
Age†	1267	22.28	47.07	46.13	13.25	85.55	0
Sex (male)	1267	0.00	0.30	0.00	0.46	1.00	0
Race (white)	1267	0.00	0.25	0.00	0.44	1.00	0
Religion (any)	1267	0.00	0.85	1.00	0.36	1.00	0
Children (N)	1267	0.00	0.90	1.00	1.24	11.00	0
Schooling (years)	1267	0.00	9.01	11.00	4.14	15.00	0
Registry (in CadUnico)†	1267	0.00	0.16	0.00	0.37	1.00	0
Yrs. Formal Emplmt†	1267	0.00	2.61	3.00	2.20	5.00	0
Av. Earnings Formal Emplmt†	1267	0.00	4.35	6.28	3.23	9.13	0
Workers' Party Eval. Index	1267	-1.50	0.00	-0.05	1.00	3.54	0
Lula Eval.	1197	-2.00	0.19	0.00	1.24	2.00	70
Dilma Eval.	1195	-2.00	-0.30	0.00	1.20	2.00	72
Partisanship Workers' Party	1267	0.00	0.05	0.00	0.21	1.00	0
Local Inc. Eval. Index	1224	-1.19	0.04	-0.10	1.01	4.71	43
Paes Eval.	1192	-2.00	-0.35	0.00	1.17	2.00	75
Pezão Eval.	1195	-2.00	-1.35	-2.00	0.87	2.00	72
Vote for Inc. 2016	725	0.00	0.03	0.00	0.18	1.00	542
Inc. at Treat. Eval. Index	1211	-1.62	0.04	-0.07	1.03	2.78	56
Nat. Inc. Eval. Index	1210	-1.81	0.00	0.14	1.01	2.61	57
Temer Eval.	1166	-2.00	-1.19	-1.00	0.89	2.00	101
Attrib. to Lula/Dilma/PT	753	0.00	0.79	1.00	0.40	1.00	514
to Paes/Pezão/Temer/MDB	753	0.00	0.07	0.00	0.26	1.00	514
Any Attribution	1267	0.00	0.59	1.00	0.49	1.00	0
MCMV Improved Lives	1192	-2.00	1.34	2.00	0.98	2.00	75
Would Move	1260	-1.00	0.58	1.00	0.70	1.00	7

Notes: †Age was measured using data from private vendors, employment and earnings from employment was measured using RAIS data, and Registry was measured using CadUnico data. ‡ See Appendix F for a discussion of compliance measures and winning the lottery was obtained by the researchers by digitizing lottery rolls and results from public sources.

Table A3: Summary Statistics 2011 Lotteries Survey

	N	Min.	Mean	Med.	Std Dev.	Max	Missing
Win. Lottery†	3924	0.00	0.23	0.00	0.42	1.00	0
Compliance‡	3924	0.00	0.12	0.00	0.32	1.00	0
Age†	3490	27.59	49.50	47.81	11.34	87.85	434
Sex (male)	3924	0.00	0.38	0.00	0.49	1.00	0
Race (white)	3924	0.00	0.24	0.00	0.43	1.00	0
Registry (in CadUnico)†	3924	0.00	0.16	0.00	0.37	1.00	0
Yrs. Formal Emplmt†	3924	0.00	4.43	5.00	3.17	8.00	0
Av. Earnings Formal Emplmt†	3924	0.00	4.68	5.92	2.67	8.67	0
PT Eval. Index	3924	-1.20	0.00	-0.16	1.00	3.33	0
Lula Eval.	3783	-2.00	0.12	1.00	1.41	2.00	141
Dilma Eval.	3771	-2.00	-0.70	-1.00	1.24	2.00	153
Partisanship PT	3924	0.00	0.06	0.00	0.24	1.00	0
Vote PT 2018	2887	0.00	0.22	0.00	0.42	1.00	1037
Local Inc. Eval. Index	3824	-1.70	0.01	0.10	1.01	5.51	100
Paes Eval.	3701	-2.00	0.02	0.00	1.29	2.00	223
Pezão Eval.	3730	-2.00	-1.43	-2.00	0.82	2.00	194
Vote for Inc. 2016	2088	0.00	0.02	0.00	0.14	1.00	1836
Vote for Paes 2020	3190	0.00	0.48	0.00	0.50	1.00	734
Nat. Inc. Eval. Index	3849	-2.22	0.01	-0.06	1.00	3.80	75
Temer Eval.	3674	-2.00	-1.13	-1.00	0.99	2.00	250
Bolso. Eval.	3717	-2.00	-0.08	0.00	1.40	2.00	207
Inc. at Treat. Eval. Index	3834	-1.69	0.01	-0.14	1.01	2.41	90
Curr. Inc. Eval Index	3834	-1.69	0.01	-0.14	1.01	2.41	90
Crivella Eval.	3718	-2.00	-0.80	-1.00	1.24	2.00	206
Mobilization Index	3894	-2.67	-0.01	0.48	1.00	0.48	30
Turnout 2016	3857	0.00	0.83	1.00	0.38	1.00	67
Turnout 2018	3871	0.00	0.87	1.00	0.34	1.00	53
Attrib. to Lula/Dilma/PT	3904	0.00	0.42	0.00	0.49	1.00	20
to Paes/Pezão/Temer/MDB	3904	0.00	0.01	0.00	0.12	1.00	20
Any Attribution	3924	0.00	0.61	1.00	0.49	1.00	0
MCMV Improved Lives	3481	-2.00	<sup>4</sup> 1.13	2.00	1.21	2.00	443
Would Move	3924	0.00	0.58	1.00	0.49	1.00	0

Notes: †Age was measured using data from private vendors, employment and earnings

## Appendix B Survey Question Wording

The outcomes in the survey analysis were based on the following survey questions. If there are differences in the question wording between surveys, we note the survey in square brackets.

### Politicians' Evaluations

- Lula evaluation: E como avalia o trabalho que o ex-presidente Lula realizou durante seus mandatos?
- Temer evaluation: E como avalia o trabalho que o atual presidente Michel Temer está realizando [recent lotteries survey]? E como avalia o trabalho que o ex-presidente Michel Temer realizou durante seu mandato [early lotteries survey]?
- Dilma evaluation: Como você avalia o trabalho que a ex-presidente Dilma Rousseff realizou durante os seus mandatos? Diria que foi ótimo, bom, ruim, ou péssimo?
- Paes evaluation: E como avalia o trabalho que o ex-prefeito do Rio de Janeiro, Eduardo Paes, realizou durante seus mandatos?
- Pezão evaluation: E como avalia o trabalho que o governador do Rio de Janeiro, Pezão, está realizando durante seu mandato [recent lotteries survey]? E como avalia o trabalho que ex-governador do Rio de Janeiro, Luiz Fernando Pezão realizou durante seu mandato [early lotteries survey]?
- Bolsonaro evaluation: Como você avalia o trabalho que o presidente Jair Bolsonaro está realizando? Diria que é ótimo, bom, ruim, ou péssimo?
- Crivella evaluation: Pesando agora na prefeitura da cidade do Rio, como avalia o trabalho que o Prefeito Marcelo Crivella está realizando?
- Witzel evaluation: E como avalia o trabalho que o governador do Rio de Janeiro, Wilson Witzel, está realizando durante seu mandato? Diria que foi ótimo, bom, ruim, ou péssimo?

### Party ID (Workers' Party ID)

- Você simpatiza com algum partido político? Sim, Não
- Com qual partido você simpatiza? [Spontaneous: coded from a list of parties]

### Vote choice

- Vote for Haddad in 2018: Em quem você votou para presidente no segundo turno? Coded from the list of candidates plus null/blank.



- Vote for Paes in 2020: Neste ano haverá nova eleição para prefeito. Imagine que os candidatos fossem o atual prefeito Marcelo Crivella e o ex-prefeito Eduardo Paes, em quem você votaria?
- Vote Pedro Paulo in 2016: Em quem você votou para prefeito do Rio no primeiro turno?

#### Mobilization

- Support a Candidate: Nas eleições de 2018, você declarou apoio a algum candidato? Por exemplo, colocou cartaz ou faixa na sua casa, adesivo na roupa, circulou mensagens de apoio no whatsapp, no facebook, foi em eventos de campanha?
- Talk to Candidate: Nas eleições de 2018, você conversou com algum candidato (a qualquer cargo: deputado estadual ou federal, governador, senador, presidente)?
- Clientelism: Você sabe se as pessoas, de forma geral, recebem alguma ajuda de políticos e líderes comunitários em troca de voto e apoio político?
- Turnout in 2016: Você votou nas últimas eleições municipais de 2016, quando Crivella foi eleito?
- Turnout in 2018: Você votou nas últimas eleições para presidente, em 2018?
- Expected Turnout in 2020 [same question as Vote for Paes in 2020, but coding whether or not respondents say they are likely to vote]

#### Attribution

- Quem você considera o maior responsável pelo Minha Casa Minha Vida? [spontaneous; coded if mentioned politicians, parties, institutions]

## Appendix C Balance Tests

We present balance tests for the joint null hypothesis (by regressing the treatment assignment indicator on the pre-treatment covariates) as well as the test for each covariate. For the balance tests regarding the recent lotteries, we also include the same survey weights we included in the main analysis. Also, for pooled analyses (including both lotteries in each survey), we either include lottery fixed effects or follow (Lin 2013).

Table C1: Wald Test for Joint Null Hypothesis Test for Balance: Pooled Recent Lotteries

Res.Df	Df	F	Pr(>F)
1,256			
1,264	-8	1.54	0.1380

Note: Regression of treatment assignment on pre-treatment covariates (null model includes age). All standard errors are clustered at the individual level. Permutation p-value = 0.27

Table C2: F-Test for Joint Null Hypothesis Test for Balance: Pooled Early Lotteries

F	Df	p-value
0.49	6.00	0.82

Note: Regression of treatment assignment on pre-treatment covariates. All standard errors are clustered at the individual level. Permutation p-value = 0.804

Table C3: Pooled Balance Tests Recent Lotteries Survey: Covariates

	Estimate	Std. Error	t value	Pr(> t )	N
Sex (male)	0.05	0.04	1.19	0.24	1267
Race (white)	-0.01	0.04	-0.33	0.74	1267
Religion (any)	0.04	0.03	1.24	0.21	1267
Children (N)	-0.06	0.10	-0.62	0.53	1267
Schooling (years)	0.61	0.33	1.88	0.06	1267
Registry (CadUnico)	-0.04	0.03	-1.32	0.19	1267
Yrs in Formal Employment	0.39	0.17	2.26	0.02	1267
(Logged) Av. Formal Wages	0.50	0.23	2.12	0.03	1267
Not conditioning on age					
Sex (male)	0.04	0.04	0.95	0.34	1267
Race (white)	-0.01	0.04	-0.29	0.77	1267
Religion (any)	0.04	0.03	1.27	0.20	1267
Children (N)	-0.10	0.10	-0.98	0.33	1267
Schooling (years)	0.34	0.36	0.94	0.35	1267
Registry (CadUnico)	-0.04	0.03	-1.38	0.17	1267
Yrs in Formal Employment	0.27	0.19	1.41	0.16	1267
(Logged) Av. Formal Wages	0.29	0.27	1.07	0.28	1267

Notes: All standard errors are clustered at the individual level.

Table C4: Pooled Balance Tests Early Lotteries Survey: Covariates

	Estimate	Std. Error	t value	Pr(> t )	N
Lin Model					
Age	0.42	0.41	1.02	0.31	3923
Sex (male)	-0.02	0.02	-0.95	0.34	3923
Race (white)	0.00	0.01	0.15	0.88	3923
Registry (Cadunico)	-0.00	0.01	-0.34	0.73	3923
Years in Formal Employment	0.06	0.11	0.51	0.61	3923
Avg. Formal Wages	0.02	0.09	0.18	0.85	3923
Fixed-Effects Model					
Age	0.33	0.40	0.85	0.40	3923
Sex (male)	-0.01	0.02	-0.43	0.66	3923
Race (white)	0.00	0.01	0.12	0.90	3923
Registry (Cadunico)	-0.01	0.01	-0.70	0.48	3923
Years in Formal Employment	0.10	0.11	0.96	0.34	3923
Avg. Formal Wages	0.07	0.09	0.72	0.47	3923

Notes: All standard errors are clustered at the individual level.

Table C5: Wald Test for Joint Null Hypothesis Test for Balance: Individual Lotteries - Recent Lotteries Survey

Lottery 17/2016				Lottery 20/2016			
Res.Df	Df	F	Pr(>F)	Res.Df	Df	F	Pr(>F)
786				461			
794	-8	1.52	0.1464	469	-8	0.49	0.8642

Notes: Regression of treatment assignment on pre-treatment covariates (null model includes age). All standard errors are clustered at the individual level. Permutation p-value 0.21 (Lottery 17/2016) and Permutation p-value 0.90 (Lottery 20/2016)

Table C6: Wald Test for Joint Null Hypothesis Test for Balance: Individual Lotteries - Early Lotteries Survey

Lottery 03/2011				Lottery 06/2011			
Res.Df	Df	F	Pr(>F)	Res.Df	Df	F	Pr(>F)
1846				2063			
1852	-6	0.76	0.5979	2069	-6	0.74	0.6172

Notes: Regression of treatment assignment on pre-treatment covariates. All standard errors are clustered at the individual level. Permutation p-value 0.573 (Lottery 03/2011) and Permutation p-value 0.601 (Lottery 06/2011)

Table C7: Lottery 17/2016 Balance Tests: Covariates

	Estimate	Std. Error	t value	Pr(> t )	N
Sex (male)	0.07	0.04	1.77	0.08	796
Race (white)	-0.03	0.03	-0.94	0.35	796
Religion (any)	0.04	0.03	1.58	0.11	796
Children (N)	-0.02	0.10	-0.19	0.85	796
Schooling (years)	0.66	0.28	2.36	0.02	796
Registry (CadUnico)	-0.06	0.03	-2.33	0.02	796
Yrs in Formal Employment	0.24	0.17	1.46	0.14	796
(Logged) Av. Formal Wages	0.34	0.24	1.45	0.15	796
No conditioning on age					
Sex (male)	0.05	0.04	1.41	0.16	796
Race (white)	-0.03	0.03	-0.93	0.35	796
Religion (any)	0.05	0.03	1.73	0.08	796
Children (N)	-0.06	0.09	-0.63	0.53	796
Schooling (years)	0.25	0.33	0.78	0.44	796
Registry (CadUnico)	-0.06	0.03	-2.27	0.02	796
Yrs in Formal Employment	0.07	0.17	0.39	0.70	796
(Logged) Av. Formal Wages	0.06	0.26	0.24	0.81	796

Notes: All standard errors are clustered at the individual level.

Table C8: Lottery 20/2016 Balance Tests: Covariates

	Estimate	Std. Error	t value	Pr(> t )	N
Sex (male)	0.02	0.07	0.34	0.73	471
Race (white)	0.01	0.07	0.16	0.87	471
Religion (any)	0.03	0.05	0.60	0.55	471
Children (N)	-0.11	0.16	-0.68	0.50	471
Schooling (years)	0.55	0.57	0.96	0.34	471
Registry (CadUnico)	-0.01	0.05	-0.25	0.81	471
Yrs in Formal Employment	0.58	0.30	1.95	0.05	471
(Logged) Av. Formal Wages	0.69	0.38	1.81	0.07	471
No conditioning on age					
Sex (male)	0.02	0.07	0.29	0.77	471
Race (white)	0.01	0.07	0.20	0.84	471
Religion (any)	0.03	0.05	0.54	0.59	471
Children (N)	-0.14	0.16	-0.87	0.38	471
Schooling (years)	0.44	0.63	0.70	0.48	471
Registry (CadUnico)	-0.02	0.05	-0.38	0.70	471
Yrs in Formal Employment	0.51	0.33	1.55	0.12	471
(Logged) Av. Formal Wages	0.56	0.45	1.24	0.21	471

Notes: All standard errors are clustered at the individual level.

Table C9: Early Lotteries Balance Tests: Covariates

	Estimate	Std. Error	t value	Pr(> t )	N
Lottery 03/2011					
Age	0.79	0.75	1.05	0.30	1853
Sex (male)	-0.05	0.03	-1.80	0.07	1853
Race (white)	0.00	0.03	0.15	0.88	1853
Registry (Cadunico)	0.01	0.02	0.60	0.55	1853
Years in Formal Employment	-0.16	0.21	-0.75	0.45	1853
Avg. Formal Wages	-0.20	0.18	-1.11	0.27	1853
Lottery 06/2011					
Age	0.09	0.54	0.16	0.87	2070
Sex (male)	0.02	0.02	0.82	0.42	2070
Race (white)	0.00	0.02	0.02	0.99	2070
Registry (Cadunico)	-0.02	0.02	-1.29	0.20	2070
Years in Formal Employment	0.25	0.15	1.63	0.10	2070
Avg. Formal Wages	0.21	0.13	1.66	0.10	2070

Notes: All standard errors are clustered at the individual level.



## Appendix D Attrition

We compare attrition patterns by regressing indicators for response and response conditional on “picking up” (the phone call) indicator on treatment assignment, respondent’s sex,<sup>2</sup> pre-treatment earnings from a formal job, having a formal job, and interactions between treatment and these covariates. We then estimate a F-test of the hypothesis that all interaction coefficients are zero. In all models, we cluster standard error at the respondent level and we include sampling weights for the analysis of the 2011 survey. We find no systematic difference between in attrition patterns between treatment and control groups in both surveys.

In Tables D3 and D4 we show comparisons of attrition rates across treatment arms. In Columns (1) and (2) of both Tables, we regress a response indicator on treatment assignment, using Lin’s (2013) model and fixed-effects for each lottery, respectively. In columns (3) and (4) of both Tables, we compare attrition rates across treatment and control by regression a response indicator conditional of picking up the phone on treatment assignment. In all models, we cluster standard errors at the respondent level and we include sampling weights for the analysis of the 2011 survey. Overall, we do not observe different response rates across treatment and control groups even though there is some evidence of in models (1) and (2) of Table D3, at the 10% level of statistical significance. Unfortunately, we do not have age information available for all subjects we attempted (only for those interviewed). This means that we cannot examine attrition conditional on age, as we did for our main analyses in the paper, since randomization was (marginally) conditional on age in the lotteries conducted in 2016.

Table D1: Wald Test for Attrition Patterns: Survey Recent Lotteries

Interviewed Indicator				Interviewed Conditional on Picking up			
Res.Df	Df	Chisq	Pr(>Chisq)	Res.Df	Df	Chisq	Pr(>Chisq)
11,716				1,533			
11,719	-3	3.03	0.3877	1,536	-3	0.88	0.8295

Notes: All standard errors are clustered at the individual level.

In an effort to assess whether nonresponse could be leading to biased estimates, we compare estimates for administrative outcomes obtained using our survey sample against a “benchmark” estimates for the same outcomes using the whole population for which these data are observed and that does not suffer from nonresponse (attrition). Specifically, we estimate the effect of winning a lottery in 2011 on earnings in our survey sample and in the whole population. Table D5 shows that, in both the survey sample and in the whole population there

<sup>2</sup>Estimated based on subject’s name using genderBR.

Table D2: Wald Test for Attrition Patterns Survey Early Lotteries

Interviewed Indicator				Interviewed Conditional on Picking up			
Res.Df	Df	Chisq	Pr(>Chisq)	Res.Df	Df	Chisq	Pr(>Chisq)
29,624				6,432			
29,627	-3	2.39	0.4950	6,432	-3	0.19	0.9808

Notes: All standard errors are clustered at the individual level.

is a null effect of winning a lottery on these labor outcomes. Furthermore, estimates are statistically indistinguishable from each other (p-value = 0.66, earnings outcomes; p-value = 0.40, formal job outcome). Unfortunately we do not have any post-treatment outcome using administrative data for the survey with 2016 lottery participants.

Table D5: Comparison of Survey Estimates to  
“Benchmark” Administrative Data Estimates

	Administrative Average (logged) Earnings	Administrative Formal Job	Survey Average (logged) Earnings	Survey Formal Job
Winning a lottery	-0.03 (0.04)	-0.01 (0.01)	0.03 (0.12)	0.02 (0.03)
R <sup>2</sup>	0.00	0.00	0.00	0.00
Adj. R <sup>2</sup>	0.00	0.00	-0.00	-0.00
Num. obs.	623411	623411	3923	3923
RMSE	3.53	0.95	3.47	0.93
N Clusters	350,690	350,690	2,119	2,119

Notes: \*\*\* $p < 0.001$ ; \*\* $p < 0.01$ ; \* $p < 0.05$

Table D3: Differential Attrition Rates: Survey with Recent Lotteries

	Resp. Lin Model	Resp. Fixed-Effects Model	Resp.   Picking up Lin Model	Resp.   Picking up Fixed-Effects Model
(Intercept)	0.10*** (0.00)		0.66*** (0.02)	
Winning Lottery	-0.02 (0.01)	-0.02 (0.01)	-0.12 (0.08)	-0.17 (0.13)
Lottery ID	-0.00 (0.00)		-0.02** (0.01)	
Winning Lottery × Lottery ID	-0.02 (0.02)		0.17 (0.16)	
R <sup>2</sup>	0.00	0.00	0.00	0.00
Adj. R <sup>2</sup>	-0.00	-0.00	-0.00	-0.00
Num. obs.	12594	12594	1772	1772
RMSE	3.06	3.06	4.81	4.81
N Clusters	7,764	7,764	1,096	1,096

\*\*\* $p < 0.001$ ; \*\* $p < 0.01$ ; \* $p < 0.05$

Table D4: Differential Attrition Rates: Survey with Early Lotteries

	Resp. Lin Model	Resp. Fixed-Effects Model	Resp.   Picking up Lin Model	Resp.   Picking up Fixed-Effects Model
(Intercept)	0.12*** (0.00)		0.56*** (0.01)	
Winning Lottery	-0.00 (0.00)	-0.00 (0.00)	0.00 (0.01)	0.01 (0.01)
Lottery Id	0.00 (0.00)		-0.01 (0.01)	
Winning Lottery × Lottery ID	0.01 (0.01)		0.05 (0.03)	
R <sup>2</sup>	0.00	0.00	0.00	0.00
Adj. R <sup>2</sup>	-0.00	-0.00	0.00	-0.00
Num. obs.	31732	31732	6947	6947
RMSE	0.33	0.33	0.50	0.50
N Clusters	17, 117	17, 117	3, 771	3, 771

\*\*\* $p < 0.001$ ; \*\* $p < 0.01$ ; \* $p < 0.05$

## Appendix E Results for Individual Survey Items

For simplicity and parsimony, Figure F1, in the main body of the paper, only reports results for the summary indices that were produced by combining individual survey items, as described in Table 1. We report, in Figure E1 results for each of these items separately. The substantive message is clear. We find no effects for any of the individual items analyzed. Results are all but identical if we do not include pre-treatment controls, as reported in Figure E2.

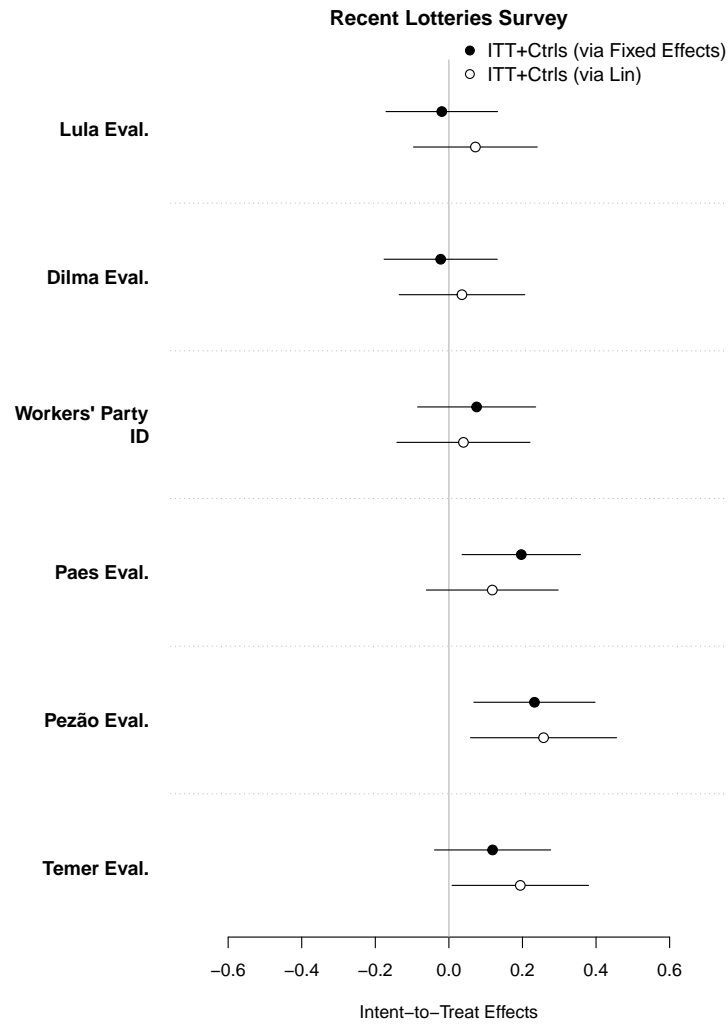


Figure E1: Effects of MCMV on Incumbent Evaluation and Mobilization (individual outcome items)

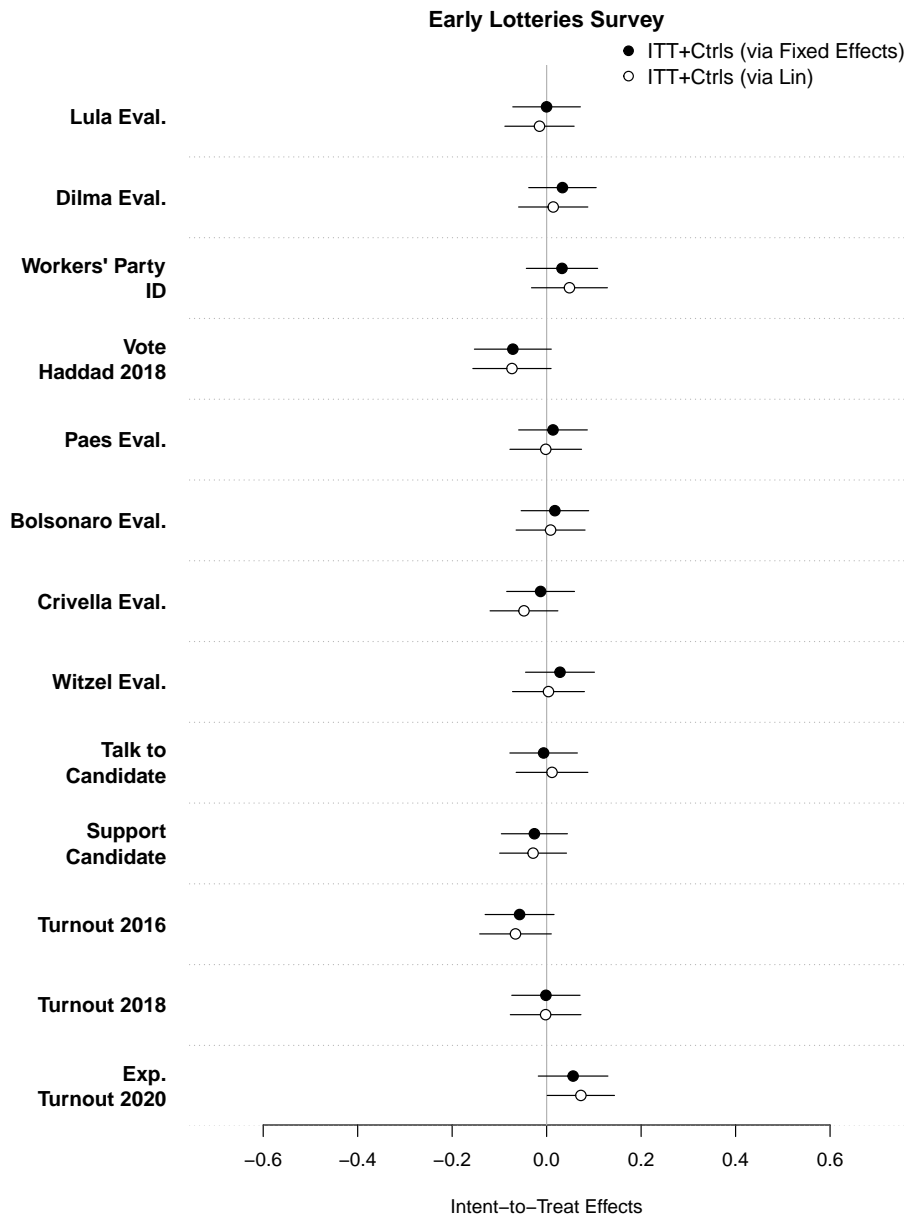


Figure E2: Effects of MCMV on Incumbent Evaluation and Mobilization (individual items, without controls)

## Appendix F Complier Average Causal Effects

In the main body of the paper we concentrate on ITT effects because non-compliance is a feature of the MCMV program. But not all lottery winners, however, decided to become program recipients or even learned about being lottery winners. All lotteries are public; however, only lottery winners are notified directly, and they are often not found by city officials. In the recent lotteries survey, we define treatment as “knowing about winning,” which we measured that by coding whether the telegrams sent to lottery winners were received by them or a family member<sup>3</sup> as well as by asking, in our survey, whether the subject learned about winning an MCMV lottery. When we combine both measures, we find that 199 out of 286 individual-lottery winners learned about winning the lottery, and 69 out of 981 individual-lottery non-winners believed they were informed that they won the lottery.<sup>4</sup> In our second survey, we define treatment as becoming a program participant based on whether the lottery participant or their spouse signed a MCMV contract with the CEF;<sup>5</sup> 221 out of 912 individual-lottery winners became MCMV recipients and 249 out of 3,012 individual-lottery non-winners signed MCMV agreements.<sup>6</sup> Noncompliance is a feature of the program, and we are interested in learning about effects of the program on political behavior – and the fact that individuals do not learn about winning, choose not to take up or are deemed ineligible and cannot become recipients is of interest. Therefore, we focus our main analyses on the intent-to-treatment parameter and only report the complier average causal effects in this Appendix.

We estimate complier average causal effects (CACE) through a two-stage least squares procedure, employing the same pre-treatment controls and the fixed-effects framework defined in Equation ???. Results are presented in Figure F1.

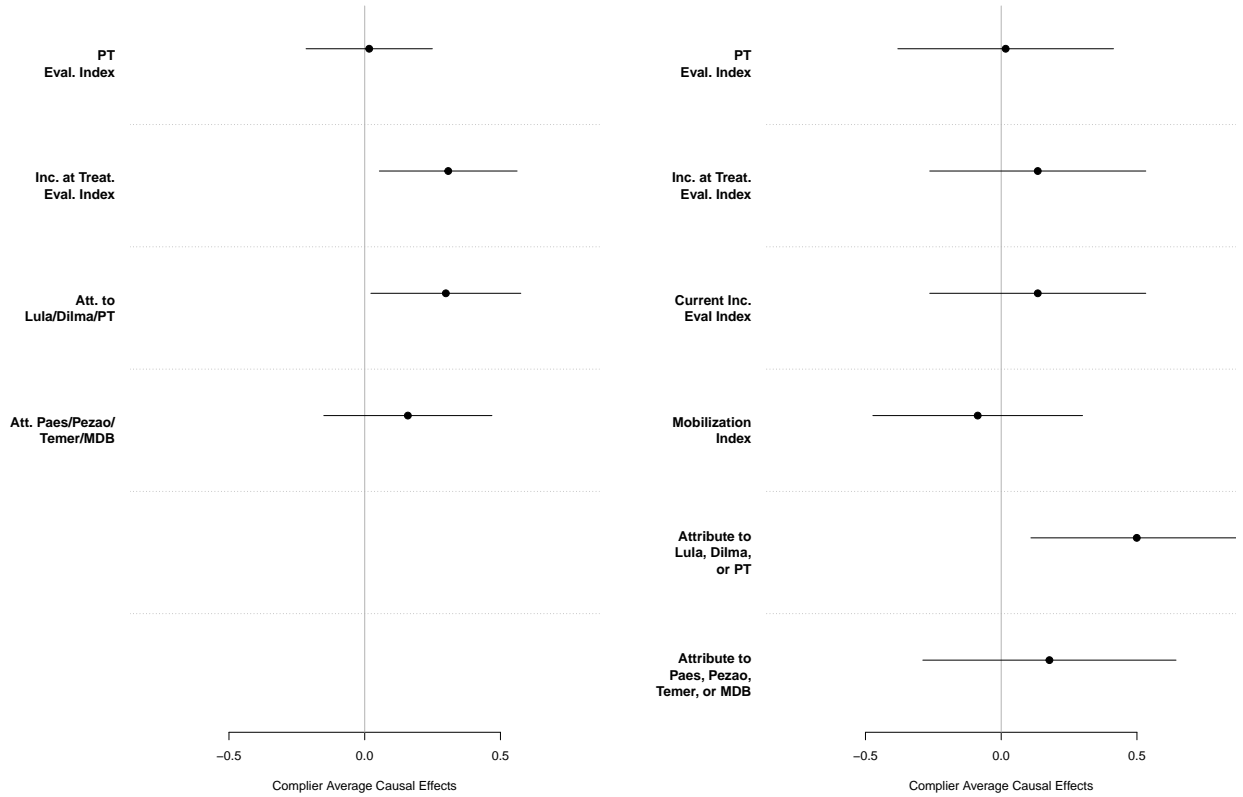
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<sup>3</sup>City hall shared these telegrams with us and a research assistant hand-coded them.

<sup>4</sup>Some non-winners sometimes participate in other social programs or are genuinely confused about government notifications, which leads them to declare being notified as lottery winners. More importantly, our survey data are measured at the individual level. Therefore, compliance ( $D_i$ ) is measured at the individual-level  $i$  and not individual-lottery  $ij$  level.

<sup>5</sup>We obtained on all signers of MCMV agreements through an Access to Information request and we used the national social registry (Cadunico) to determine spousal status with lottery winners.

<sup>6</sup>Our agreement data are not linked to the lottery identification so individuals might have become MCMV beneficiaries through winning other lotteries held before or – more likely – after the ones we used in our survey. Also, some individuals may have become beneficiaries because of forced relocation or elderly/special needs lotteries – lottery applicants, under some infrequent circumstances may become beneficiaries through other pathways which could cause noncompliance. Like in our first survey, compliance ( $D_i$ ) is measured at the individual  $i$  level.



(a) Recent Lotteries (Wave 1)

(b) Early Lotteries (Wave 2)

Figure F1: MCMV Effects on Incumbent Evaluation and Mobilization

Notes: Figure reports complier-average-causal effects on outcomes listed in Table 1, and for which we presented ITT estimates in Figure .



## Appendix G Additional Public Opinion Data on MCMV

None of the general population public opinion surveys distinguish between MCMV’s tiers in their question wording, but we believe that survey respondents have Tier 1 in their minds when answering questions because of MCMV Tier 1’s distinctive-looking housing projects, government ads, and politicians credit claiming activity, both of which are focused on Tier 1 (Bueno forthcoming).

Figure G2 reports the full breakdown across income categories of support for the main social policies in the Ibope survey we analyzed, which is also reported more synthetically in Table G1.<sup>7</sup>

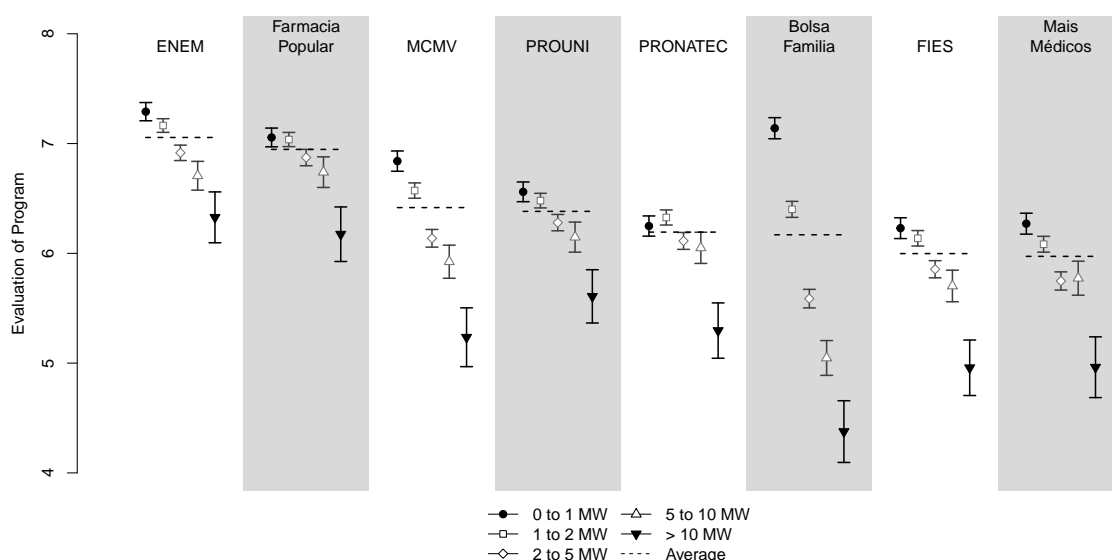


Figure G2: Evaluation of Programs and Initiatives by Family Income (2015)

Notes: Respondents were asked to rate programs or initiatives on a 0–10 scale. Family income is measured in multiples of monthly minimum wages, which at the time was R\$ 788 (then about US\$ 200). See text for summary descriptions of the programs.

In the general population, the MCMV is better evaluated among beneficiaries than non beneficiaries (evaluation is 1.21 higher than the baseline evaluation of 6.39 ( $p < 0.01$ )). However, this includes all MCMV tiers in all types of municipalities. If we restrict our analysis to individuals whose reported family income is below 2 minimum wages (potentially eligible MCMV beneficiaries), the positive effect is smaller (0.97,  $p < 0.01$ ) and even smaller if we restrict the analysis to cities that – as Rio de Janeiro – are their state capitals (0.89,  $p = 0.06$ ). Further restricting the analysis to the metropolitan area of Rio de Janeiro (-0.70,  $p = 0.64$ ) or to the city itself (-0.70,  $p = 0.64$ ) yields negative point estimates that are not statistically

<sup>7</sup>It is worth noting that the survey has plausible estimates of *Bolsa Família* and MCMV beneficiaries (23% and 1.5%, respectively).

significant. These results are compatible with the findings of our survey and suggest that, perhaps, the MCMV was less effective in capitals in general and in Rio in particular.

Table G1 presents more complete data from the the IBOPE survey, which was discussed in the main body of the paper. When asked about which government programs or initiatives they remembered (up to three), 12.8% cited *Bolsa Família*, 7.9% cited MCMV, and the next program was remembered by only 2.4% (ENEM, the unified college admissions exam). About 70% of respondents could not cite a single program. Both were substantially better known than *Farmácia Popular*, a program that distributes free medicines for selected chronic illnesses and is known by 28% of respondents.

Table G1: Knowledge and Evaluation of Social Programs and Initiatives (2015)

Program	Knowledge (%)	Evaluation (0–10)		
		Average	Std. Dev.	Poorest-Richest
Bolsa Família	59.1	6.2	3.4	-2.8
MCMV	49.8	6.4	3.2	-1.6
Farmácia Popular	27.8	6.9	2.9	-0.9
ENEM	27.4	7.1	2.8	-1.0
PRONATEC	24.8	6.2	2.9	-1.0
Mais Médicos	22.9	6.0	3.3	-1.3
FIES	19.1	6.0	3.0	-1.3
PROUNI	18.9	6.4	2.8	-1.0
None	16.6			

Notes: Knowledge was assessed by asking respondents whether they knew about 27 selected government programs or initiatives. Respondents were also asked to rate 8 of those programs or initiatives on a 0–10 scale. The last column reports the difference in average evaluations between those earning up to 2 and those earning more than 10 minimum wages. ENEM is a national exam for high-school students used as entrance exam for many universities; PRONATEC provides technical education; FIES is a private college financing program; PROUNI is a stipend for low-income students in public universities. See text for the other programs.

Data from a survey close to the 2014 election (Ames et al. 2015) also shows that MCMV is less polarizing than other social policies. Not only was the MCMV less polarizing across income levels (Figure G2), but it was also less polarizing along partisan lines. As Figure G3 shows, even among antipetistas (anti-partisans of the PT), MCMV’s approval rating was about 85%, which is much higher than antipetistas’ approval of *Bolsa Família* and *Mais Médicos*, which was about 50%.

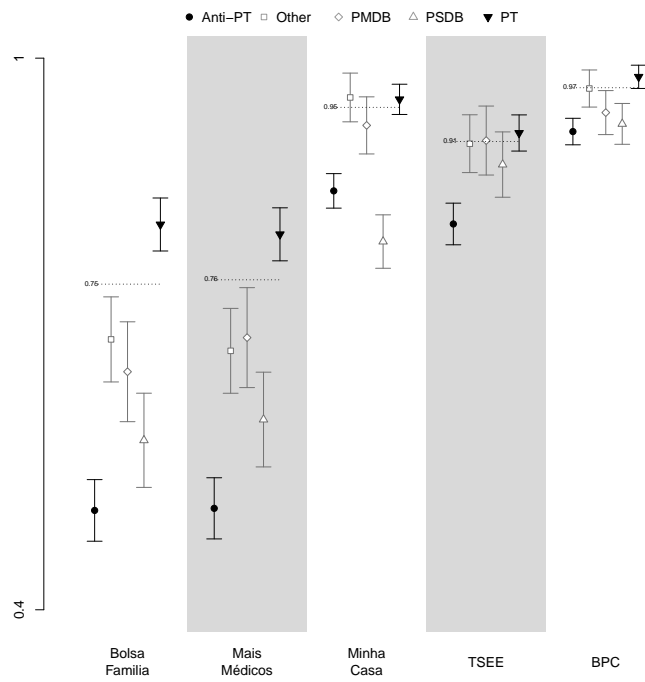


Figure G3: Support for Selected Social Programs by Partisanship (2014)

Notes: Data are from Ames et al. (2015). Horizontal lines represent program ratings among non-partisans; see Samuels and Zucco (2018) for definitions of partisans and anti-partisans.

## Appendix H Extended Analysis of Electoral Results

In the main body of the paper we presented results from a DiD analysis in which we operationalized MCMV as a binary variable that took on the value of one if the municipality delivered any number of homes in the electoral period. In Table H1 we report the same analysis employing two different continuous operationalizations of MCMV presence in the municipality, namely the number of homes delivered by 100 inhabitants and the number of homes delivered relative to the housing deficit, as estimated by the *Ministério das Cidades* for 2009.

The results are very similar to what we reported in the main body of the paper. For the presidential elections, we see statistically significant results for the measure relative to the population in the first two cycles and in the pooled analysis. When we operationalize MCMV relative to the original housing deficit, results fall short of statistical significance in the second cycle. Still, the two operationalizations tell a similar substantive story. In the pooled analysis, the incumbent candidate received approximately half a percentage point more votes in a treated municipality with the median intensity of MCMV, and this effect increases to 3 p.p. and 4 p.p. in municipalities at the 95<sup>th</sup> percentile of MCMV intensity relative to the housing deficit and relative to the population, respectively.

For municipal elections, as in the main body of the paper, we do not see statistically significant effects in the second cycle, but the pooled estimates for each operationalization of MCMV presence imply a boost to the incumbent of 0.3 and 0.4 p.p. in median intensity municipalities and 2.2 and 2.4 p.p. in high intensity ones.

Table H2 reports results for the same binary operationalization shown in Table 2, in the main body of the paper, but estimated on data that were pre-processed by matching treated and untreated municipalities on income per capita (and on its log), on population, and on the housing deficit (and the square of the housing deficit). Results change very little with respect to those previously reported.

Table H1: Estimates with Continuous Operationalizations of MCMV

(a) Presidential Elections

	MCMV by 100 people				MCMV to housing deficit			
	2006–2010	2010–2014	2014–2018	Pooled	2006–2010	2010–2014	2014–2018	Pooled
MCMV	0.019*** (0.003)	0.007** (0.003)	−0.001 (0.005)	0.016*** (0.002)	0.044*** (0.004)	0.012 (0.009)	−0.006 (0.009)	0.044*** (0.004)
N	11128	5354	3150	19632	11130	5354	3140	19624
Mun	5564	2677	1575	5569	5565	2677	1570	5565

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

(b) Mayoral Elections

	MCMV by 100 people				MCMV to housing deficit			
	2008–2012	2012–2016	Pooled	Pooled	2008–2012	2012–2016	Pooled	Pooled
MCMV	0.003*** (0.001)	0.000 (0.001)	0.001*** (0.000)	0.053*** (0.016)	0.004 (0.028)	0.027*** (0.008)		
N	11128	5640	14338	11130	5630	14335		
Mun.	5564	2820	5455	5565	2815	5450		

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

Table H2: Estimates on Matched Datasets

(a) Presidential Elections

	m2006.2010	m2010.2014	m2014.2018	mpool
MCMV	0.013*** (0.003)	0.019*** (0.006)	-0.012 (0.011)	0.013*** (0.003)
N	11548	6642	632	16608
Mun.	4425	1791	302	5243

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

(b) Mayoral Elections

	2008–2012	2012–2016	Pooled
MCMV	0.021* (0.011)	0.016 (0.013)	0.005 (0.007)
N	10996	7224	13528
Mun.	4259	1925	5018

\*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.1$

## Appendix I MCMV Allocation

We analyze the effects of alignment with the presidency and with the minister’s parties on funds for MCMV using a close-race regression-discontinuity design. Municipalities where the president’s (or the minister’s) party won the local election by a narrow vote margin are considered aligned and places where the president’s party lost by a narrow vote margin are considered unaligned. Municipalities should, in expectation, differ only in the presence or absence of a mayor who belongs to the president’s (or minister’s) party. Table I4 shows no large or systematic partisan bias exists in the allocation of resources to fund MCMV. We also examine whether the central government provides aligned mayors with money earlier in their term to give them a head start. Table I3 shows that partisanship is not a predictor for the average number of years it takes a municipality to receive funding for MCMV. In sum, unlike discretionary transfers, we fail to find evidence suggestive of uneven distribution of funds across localities.

We fit different regression models, within the RDD bandwidth, to estimate this limit parameter ( $\tau_{lim}$ ), represented in equation 3. Let  $c$  denote the point of discontinuity and  $r$  denote the values of the running variable that define the set of units included in the RD study group. In regressions with kernel smoothers, bandwidth refers to the width of the kernel.

$$\tau_{lim} = \lim_{r \downarrow c} [\bar{Y}_i(1) | R_i = r] - \lim_{r \uparrow c} [\bar{Y}_i(0) | R_i = r] \quad (3)$$

We also use difference of means to estimate  $\tau_{local}$ . Let  $Y_{i,t+1}(1)$  and  $Y_{i,t+1}(0)$  denote transfers during the mayor’s term after election  $t$ , allocated to municipality  $i$  when the mayor belongs to the president’s party and when the mayor belongs to any other party, respectively. The study group ( $N$ ) is composed of municipalities in which there were close mayoral electoral races at time  $t$ .

$$\tau_{local} = \frac{1}{N} \sum_{i=0}^N [Y_{i,t+1}(1) - Y_{i,t+1}(0)] \quad (4)$$

Table I4 reports RDD using races in two-candidate races. We remove 7 agreements that were signed after May 2016, post President Rousseff’s removal from office – the results are very similar if these agreements are included. Units are total transfers in each municipality, during the mayors’ term, pooled across election cycles. Robust standard errors for difference of means and local linear regression, columns (8) and (9) use bias-corrected and robust standard errors for local linear regressions using approach by Calonico et al. (2014); all standard errors are clustered at the municipality level. P-values were calculated using a normal approximation and a one-tailed test statistic. Local linear regression at column (7) uses a triangular kernel and Imbens and Kalyanaraman (2012)’s method for estimating the optimal bandwidth (14.58% and 9.25%). Local linear regression at columns (8) and (9) use

Table I1: Balance Test (President’s Party Alignment)

Covariate	Est.	Robust Std. Error	p-value
Income per capita	-26.88	14.95	0.07
Doctors per thousand pop.	-0.09	0.08	0.27
Education (IDH)	-0.02	0.02	0.25
Income (IDH)	-0.01	0.01	0.41
Longevity (IDH)	-0.02	0.02	0.23
Illiteracy rate	0.31	2.32	0.89
Infant mortality	4.64	3.41	0.17
Population	2217.96	7330.34	0.76
Poverty rate	5.53	3.71	0.14
Vote for Lula	0.01	0.03	0.78
Vote for FHC	-0.02	0.03	0.50
Vote for PT (fed. dep.)	0.02	0.02	0.30
Votes for PSDB (fed. dep.)	0.05	0.04	0.16
Vote for PT (governor)	-0.03	0.02	0.18
Vote for PSDB (governor)	-0.00	0.04	0.94
Votes for PT (state dep.)	0.00	0.02	0.84
Votes for PSBD (state dep.)	0.04	0.04	0.24
Turnout	-0.04	0.02	0.04

triangular kernel, bias estimated with quadratic polynomial, and optimal bandwidth (15.4% and 12.9%) selector developed by Calonico et al. (2014). We report the standard deviation of the outcome variable in the control group. The Progressive Party, which held the ministry of urban affairs between 2005 and 2015, and the Workers’ Party held the presidency between 2003 and 2016.



Table I2: Balance Test (Minister’s Party Alignment)

Covariate	Est.	Robust Std. Error	p-value
Income per capita	20.01	17.77	0.26
Doctors per thousand pop.	0.14	0.08	0.08
Education (IDH)	0.01	0.02	0.58
Income (IDH)	0.02	0.02	0.26
Longevity (IDH)	0.01	0.02	0.60
Illiteracy rate	-1.90	3.00	0.53
Infant mortality	-2.05	3.40	0.55
Population	-7574.01	8488.13	0.37
Poverty rate	-7.18	4.51	0.11
Vote for Lula	0.01	0.03	0.67
Vote for FHC	0.01	0.03	0.67
Vote for PT (fed. dep.)	-0.02	0.02	0.24
Votes for PSDB (fed. dep.)	-0.00	0.04	0.93
Vote for PT (governor)	0.02	0.03	0.48
Vote for PSDB (governor)	-0.03	0.05	0.52
Votes for PT (state dep.)	0.01	0.01	0.70
Votes for PSDB (state dep.)	0.04	0.03	0.29
Turnout	0.02	0.03	0.36

Table I3: Association between Year of Funding and Mayor’s Party

Mayor’s	estimate	Std. Error	Statistic	p-value
PT	-0.04	0.09	-0.43	0.66
PP	0.05	0.10	0.46	0.64
PSDB	0.02	0.07	0.32	0.75

Notes: Table I3 shows a regression of number of years it takes a municipality to receive MCMV funding on mayor’s partisanship (from the first year of the mayor’s term). Dependent variable is the average number of years it took a municipality to receive funding (from the first year of the mayor’s term) of agreements. Units are municipality-agreements. Municipality and term fixed effects included. Standard errors clustered at the municipal level.

Table I4: RDD President Party and Minister Estimates, *Minha Casa Minha Vida* Contracts (logged R\$ per capita), 2009–2016

Estimator	Difference of Means						Local Linear Regression (bias-corr.) (robust)		
	0.5	1	2	3	4	5	Opt. Bd.	Opt. Bd.	Opt. Bd.
	<b>President’s Party, MCMV Contracts (logged R\$ per capita)</b>								
Estimates	0.68	-0.24	0.34	0.25	0.07	0.13	0.16	0.25	0.25
Std. error	0.88	0.66	0.47	0.41	0.37	0.34	0.42	0.42	0.49
p-value	0.22	0.36	0.23	0.27	0.42	0.35	0.35	0.28	0.31
Mean dep. var.	1.94	1.95	2.09	2.25	2.42	2.43	2.52	2.53	2.53
Std. dev. dep. var.	2.40	2.70	2.57	2.65	2.75	2.72	2.76	2.75	2.75
non zero	13	27	54	77	101	124	333	350	350
n left	13	28	60	80	104	120	337	348	348
n right	20	43	75	98	117	149	362	382	382
clusters	33	69	128	170	212	254	601	627	627
hline height	<b>Minister’s Party, MCMV Contracts (logged reais per capita)</b>								
Estimates	-1.45	-1.58	-0.84	-0.11	0.22	0.04	-0.53	-0.29	-0.29
Std. error	1.09	0.73	0.50	0.39	0.35	0.32	0.51	0.42	0.49
p-value	0.09	0.02	0.05	0.39	0.26	0.46	0.15	0.24	0.28
Mean dep. var.	1.59	1.99	1.69	1.65	1.68	1.77	1.81	1.82	1.82
Std. dev. dep. var.	2.88	2.76	2.67	2.52	2.47	2.63	2.59	2.64	2.64
non zero	7	20	33	55	70	95	167	223	223
n left	9	22	50	92	108	140	231	313	313
n right	12	29	52	83	115	151	263	344	344
clusters	21	50	97	166	210	266	440	558	558

## Appendix J Pre-Analysis Plan Considerations

We registered hypotheses related to this project, which includes this manuscript and other manuscripts currently under review or in preparation. We focus our discussion regarding our PAP for the sections that directly relate to this manuscript.

**Outcomes and Adjusting for multiple comparisons** We deviated from our pre-analysis plan in determining which variables we used as main outcome for the hypotheses analyzed here. In all of the instances in which we deviated from the PAP in determining the main outcome, we used an overall composite index containing all of the variables related to that hypothesis rather than choosing a single outcome related to that hypothesis. We took this step precisely as a strategy to reduce the number of comparisons we were making. Furthermore, we also report all of the single outcomes that composes our indices in the Appendix and all of the single components of the index were included in our PAP as either the main or secondary outcome for a given hypothesis. Our procedure for creating these indices was pre-registered. The table below describes our deviations:

Table J1: Comparing PAP and Manuscript’s Main Outcome

Hypothesis	Main Outcome	PAP Main Outcome
PT Evaluation	PT Eval. Index	Dilma Eval./PT ID.
Mobilization	Mobilization Index	Talked recently to candidate
Inc. Evaluation	Inc. at Treat. Eval. Index	Dilma Eval.
	Curr. Inc. Eval. Index at treatment.	Crivella Eval.

In our amendment to our pre-analysis plan, we stated that we would employ corrections to account for multiple comparisons within the same general hypothesis, according to the following rule: for hypotheses for which there is a single outcome, there would be no correction, and, for other hypotheses, we specified that the primary outcome is not subject corrections. We did not include corrections in this manuscript because they would inflate our p-values and we are showing null results.

**Attrition** We followed our pre-registered protocol for examining and evaluating attrition.

**Control Variables** We pre-registered age, sex, race, formal employment and wages as our control variables for the 2011 lotteries study. In any case, our adjusted and unadjusted results are quite similar.

## Hypotheses

Based on our PAP, we expectations about 2011 lotteries survey were:

Beneficiaries' preferences, voting behaviors, and job evaluations will be more favorable to those who were incumbents at the moment in which they became beneficiaries than nonbeneficiaries (gratitude)

On average, however, beneficiaries' partisan preferences will not differ from nonbeneficiaries (no partisan retrospection)

We also expect a more positive evaluation of current incumbents, even though they are not directly related to the benefits.

Beneficiaries' preferences, voting behaviors, and job evaluations will be more favorable to incumbents at the time of the survey than nonbeneficiaries (blind-retrospection)

Beneficiaries will be more engaged in politics than nonbeneficiaries

Overall, we expected greater direct electoral returns than what we found.

**Non-beneficiaries:** We did not registered a pre-analysis plan for the analyses regarding non-beneficiaries (DiD).

## Appendix K Ethics Discussion

This research project was submitted for review at Human Subjects Research Review Boards at *name omitted* (exempt, protocol *omitted*), *omitted* (approved, protocol *omitted*), and *name omitted* (approved, protocol omitted). Our survey with early lottery winners was partly conducted during the Covid-19 pandemic. All of our survey interviews in 2019 and 2020 were conducted via phone and enumerators were working from home.

We collected subjects' names and contact information from public sources and via private vendors, respectively. This data collection was in accordance with national laws regarding privacy of personal information.

We obtained voluntary and informed consent via phone. Our enumerators read our consent form to subjects who then agreed or declined to answer our questionnaire. In addition to reading our consent form, we also offered subjects the option to receive the consent form via WhatsApp (a messaging application widely used in Brazil). That way, subjects would have our consent form in writing too. The consent form explicitly stated this questionnaire was part of a research project conducted by universities and subjects were also provided with a local research institution review board's contact information to facilitate access (via phone, email, or in person). We also trained enumerators to answer questions related to the nature of the research project to make it as clear as possible to subjects that we were not involved with any political group or governmental agency. Our survey did not engage in deception and we do not anticipate having intervened in the political process because the intervention analyzed here was conducted by the government (without collaboration from the authors).

For our qualitative MCMV interviews, we obtained oral consent in person and individuals were compensated for their time and reimbursed for transportation costs.

## Appendix L Data Sources & Transparency

1. Survey data was collected via telephone surveys conducted with company *omitted*
2. Data on *Minha Casa, Minha Vida* enrollments through requests using the Law of Access to Information.
3. Qualitative interviews conducted in mid-2018 with support from company *omitted*
4. Data on formal jobs and income from formal labor obtained from RAIS (access via institution *omitted*)
5. Subjects' gender using R package `genderBR`.
6. Cadunico (national registry data) data obtained via request to the Ministry of Social Development, which was subsequently merged into what is now the Ministry of Women, Family and Human Rights
7. Brazilian Electoral Panel Studies (2014): <https://publications.iadb.org/en/publication/12807/brazilian-electoral-panel-studies-beps>
8. IBOPE (2015, Study 04683): [https://www.cesop.unicamp.br/eng/banco\\_de\\_dados/page:408?ext=html](https://www.cesop.unicamp.br/eng/banco_de_dados/page:408?ext=html)