

Do Governments Make Dreams Come True?

An Analysis of the *Minha Casa Minha Vida* Housing Program*

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Abstract

What are the attitudinal effects of being awarded homeownership by the government? In this paper we examine whether being selected as the beneficiary of a large public housing program affects beneficiaries-to-be political preferences as well as several other attitudes that have been known to vary with homeownership. We exploit the fact that Brazil's *Minha Casa Minha Vida* program selects beneficiaries for extremely subsidized mortgages through public lotteries to compare the preferences and attitudes of beneficiaries and non-beneficiaries. We conducted a survey of both groups *before* any homes had actually been delivered, and as such we are able to isolate effects from the award from any effects of welfare changes experienced by the beneficiaries once they actually move into the new home. Contrary to our expectations, we find that selection into the program generated anti-incumbent (i.e. *negative*) political effects. Although surprising, the result is consistent across several outcome variables, and compatible with program implementation difficulties that we identified while conducting the study.

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That voters respond to government policies that affect their welfare is a cornerstone of several models of political competition, from the economic voting to the distributive politics literature. But do voters reward (or punish) incumbents solely based on the welfare effects of government policies, or do less tangible factors, such as expectations about future welfare effects or even the sheer “feel good” effects derived from government actions also have an effect? The interplay between welfare and expectations is further complicated by the fact that, on the one hand, government programs might fulfill an aspiration of the beneficiaries but amount to little or even negative welfare changes and, on the other, by the fact that the flow of actual welfare effects of the same program or initiative might vary over time.

In this paper, we shed light on these issues by examining political effects of being selected to participate in a government housing program. We examine how preferences, perceptions, and aspirations of beneficiaries-to-be differ from those of a comparison group of otherwise similar non-beneficiaries *after* beneficiaries have been selected, but *before* any material advantage has been provided. At this point in time, all that beneficiaries-to-be have is the promise of receiving a good. They have not yet experienced any changes in economic welfare in terms of access to credit, liquidity, or the disruption of moving to a new neighborhood. With this unique setup, we are able to isolate the effects of expectations from selection into the program that promises to fulfill what is arguably the highest material aspiration for most families from actual welfare effects that might eventually follow from actually fulfilling it.

Our study focuses on a large, visible, programmatic housing program funded by the central government in Brazil called *Minha Casa Minha Vida* (roughly “My home my life,” henceforth MCMV). Lower-income individuals sign up for the program and beneficiaries are selected by a public lottery. Lottery winners who fulfill the eligibility requirements eventually receive a concrete, unconditional, (*de facto*) irreversible,¹ durable good: they move to a newly built home that is so generously subsidized that it resembles a outright handout.

We expect to find that voters reward incumbents for selection into the program, even though no transfers and actual welfare changes have taken place. Such a finding would sug-

¹Beneficiaries may lose their benefits if they stop making payment or violate their agreement. However, in practice, it is our understanding that beneficiaries rarely lose their benefits.

gest that voters reward governments not only based on transfers but also on the expectations about such policies. We measure beneficiaries' expectations about future welfare changes to understand the extent to which the main effects might derive from a rational analysis of future benefits or from more behavioral reactions to the fulfillment of an aspiration, irrespective of expected or actual welfare benefits. We do not expect to find effects of selection to participate in the program on broader social and political attitudes, such as subjects' attitudes towards markets and the state, redistribution, and self-reliance. We expect that these attitudes should change only once beneficiaries experience welfare changes associated with *de facto* homeownership, irrespective of the fact that homeownership was achieved through a heavily subsidized government program. In short, our expectation is that these broader social attitudes are slower-changing than electoral preferences.

Our results are clear. Beneficiaries-to-be have different evaluations of incumbents compared to lottery non-winners. However, they penalize incumbents associated with MCMV: lottery winners have lower evaluations of former presidents Lula and Dilma than lottery non-winners. This finding is the opposite of what we predicted. Lottery winners also reported lower life satisfaction than non-winners, and were also less likely to think of homeownership "as a dream," and much less likely to say that they would move into a MCMV home if they were awarded one. These results are striking but they are consistent with each other and with the additional analyses we conducted with administrative data. Furthermore, consistent with our predictions, being selected to participate in the program does not influence beneficiaries-to-be broader social attitudes about redistribution, the role of the state, and self-reliance. Although preliminary, our results suggest that voters only reward incumbents for public policies that deliver on their promises, even for policies that promise the dream of homeownership.

We proceed as follows. In the next section we review the literature on the political effects of public policies in general and housing programs in particular, emphasizing the possible tensions between short and long-term welfare effects and between symbolic, expectation, and welfare effects. We then discuss the institutional setting in Section 2, and describe the study design and data collection in Section 3. The analysis is discussed in Section 4, main results and an examination of the mechanisms generating them are presented in Sections 5 and 6, and Section 7 concludes.

1 Political Effects of Housing Programs

The MCMV provides provides a highly subsidized home, in a new housing project. Therefore, it provides homeownership,² but is also a subsidy that can be considered a large lump-sum transfer of wealth, and implies relocation. How might such bundle affect beneficiaries?

Dietz and Haurin (2003) reviewed early micro-level evidence on the effects of homeownership, mostly based on the United States. Despite econometric, theoretical, and empirical limitations, they found “substantial evidence that homeownership has important effects on some household behaviors and outcomes” (p.401). A subset of the literature has focused on the effects of home and property ownership more generally on political attitudes and preferences, and different theories have been proposed.

Perhaps the earliest conjecture about such impacts can be traced back to Engels and Dutt (1935), who argued that once a worker purchased a home, he entered the investor class and could not longer be considered part of the proletariat. As de Janvry, Gonzalez-Navarro and Sadoulet (2014) puts it quite clearly when discussing this “investor class” theory, the expectation is that “asset-owning individuals prefer politicians that are more pro-market, championing low taxes on capital and labor income and relying less on state intervention” (p. 216).

In this respect, previous studies found that becoming a property owner affects individuals’ self-placement on an inequality ladder and influences their attitudes about redistribution, inequality, and pro-state vs. pro-market policies (Di Tella, Galliani and Schargrodsky, 2007; de Janvry, Gonzalez-Navarro and Sadoulet, 2014; Ansell, 2014; Hankinson, 2018).³ These results suggest that Engel’s premonitions were correct. An interesting implication of this result for electoral behavior is that beneficiaries of these policies might “fail to reciprocate through votes for the benefactor party” (de Janvry, Gonzalez-Navarro and Sadoulet, 2014, p.216). If this is correct, it does not make political sense for left-leaning governments to engage in such policies.

In observational studies families typically chose to become property owners. Not only this creates a strong selection effect, but the intention and anticipation of becoming a homeowner

²We discuss the precise terms of the MCMV program later in the paper.

³See Prato (2018) for a general argument on how policy feedbacks for housing policies operate through the acquisition of economic information that could lead to voters becoming systematically less favorable of redistributive taxation.

can lead to changes in preferences and attitude even before ownership (Engelhardt, 1994; Dietz and Haurin, 2003). In the case of the MCMV, however, beneficiaries receive the good by chance, and do not receive full property rights upfront. Selection into the program precedes receiving the asset, and the asset cannot be freely disposed of for a relatively long period. Hence, it is unlikely that in the immediate aftermath of being selected into the program beneficiaries will feel they are now in the “investor class.”

A related, but different, story emerges from the literature in urban homeownership. Becoming an asset holder, in this context, means that households acquire a financial stake in local and political affairs. Moreover, a home is a fixed-asset and transaction costs are high, which imply a reduction in mobility for most households (Haurin and Gill, 2002; Rohe and Stewart, 1996), and high concentration in a single asset (Brueckner, 1997; Meyer and Wieand, 1996). One would not necessarily become more fiscally conservative in this context, as aversion to taxation would probably be weighted against the property value effect of government policies. Homeowners, here, might be prompted to express more voice over exit (Hirschman, 1970), which would suggest that individuals become more politically engaged (Cox, 1982).

Several observational studies have found evidence that homeownership increases awareness of local affairs Sykes (1950), involvement in grassroots organizations Perkins, Brown and Taylor (1996), and the voicing of demands for local government services Guterbock (1980). Along these same lines, Hall and Yoder (2018) finds that subsidized mortgage programs could make individuals more likely to participate in Democratic primaries. Studies with stronger research designs, however, are more circumspect. DiPasquale and Glaeser (1999) employ an instrumental variable approach and find a fairly small effect of ownership on households’ social capital, and one that is attributable in large part to tenure length. Relocating beneficiaries through a rent-subsidy program, in contrast, may lead to less interest and engagement in politics (Gay, 2012, 2014). Results from field experiments on a subsidized savings program indicates “no impact of homeownership on political involvement” and “did not spur households to provide more social capital or local amenities” (Engelhardt et al., 2010). These last two results make sense because in both cases, because beneficiaries moved to new neighborhoods which disrupt local ties that typically foster engagement.

Most MCMV beneficiaries would be leaving a situation of informal housing, and many be-

come owners for the first time. This should lead to reduced mobility, which is the mechanism through which which homeownership affects political engagement. Hence, it is reasonable to expect that in the medium- to long-run those that became homeowners through the MCMV program might become more politically engaged. In the short run, however, we expect no effects prior to moving and probably negative effects on political engagement soon after the move.

A third approach is to focus on the material/welfare impacts of the transfer. The idea that voters' political and electoral preferences follow changes in their welfare is possibly one of the most widespread in the discipline. In a very broad sense, the vast economic voting literature is built on this foundation (Meltzer and Richard, 1981; Persson and Tabellini, 2000), even if there is some disagreement about the extent to which one's personal economic conditions or more general economic performance more generally drive the decision or whether voters primarily sanction incumbents for past welfare changes or select leaders based on prospects for welfare enhancement.

The literature on distributive politics follows a similar logic but focuses on how voters respond to concrete material benefits provided by governments to specific individuals or groups. The literature on clientelism examines why and how voters reward politicians for goods that are tied to an electoral request (Stokes et al., 2013), but many studies show that voters also reward incumbent governments for benefits delivered through programmatic policies in which there is no explicit electoral quid-pro-quo. Government transfers that provide small cash payments for extended periods of time seem to elicit political support from beneficiaries (Manacorda, Miguel and Vigorito, 2011; De La O, 2013; Zucco Jr., 2013; Diaz-Cayeros, Estevez and Magaloni, 2016). Some question whether a one-time transfer could generate similar responses (de Janvry, Gonzalez-Navarro and Sadoulet, 2014) and Bardhan et al. (2008), for instance, find less response to one-time benefits such as receiving a land title than to short-term continued benefit programs in West Bengal. However, there is also evidence that one-time transfers such as government aid provided after natural disasters (Bechtel and Hainmueller, 2011), cash grants for specific uses (Pop-Eleches and Pop-Eleches, 2012), infra-structure spending (Schady, 2000), and even the creation of social insurance policies (Clinton and Sances, 2018) can affect political preferences and behavior.⁴

⁴Most of the literature explicitly or implicitly assumes a "welfare" effect drives voters' support following

By this logic, the MCMV should generate increased support for incumbents associated with the program. However, differently than titling programs, the MCMV requires moving homes and, for most beneficiaries, changing neighborhoods. Moving is disruptive, and the net effect of such programs on the welfare of beneficiaries is controversial, as the literature shows. On the one hand, a study on rent subsidies that implied relocation to in lower-poverty neighborhoods in the United States found that individuals showed substantial improvements in mental health, in income and educational achievement in the lives of individuals who moved to these neighborhoods when young, but little to no effect on the adult's income and labor prospects (Jeffrey R Kling, 2007; Chetty, Hendren and Katz, 2015) and Ludwig et al. (2013) found negative, null, and positive welfare effects of the same "Moving to Opportunity" program, depending on the outcome analyzed.

On the other hand, a study on upgrading slum dwellings conducted in El Salvador, Mexico, and Uruguay found that satisfaction with life improves after they receive a new house, but, after eight months, no gains in overall satisfaction can be detected (Galiani, Gertler and Undurraga, 2015). Barnhardt, Field and Pande (2015) examine a housing scheme in India, in which the policy's design was very similar to Brazil's MCMV; the authors found that lottery winners were as economically well-off as lottery non-winners, but lottery winners were actually worse off in access to access to public transportation and public health facilities, job opportunities for adult children and deterioration of social support from caste-based connections and family — all of these presumably related to the long distances from economic centers of the housing projects.

On balance, we expect the MCMV program to increase political support for the incumbents associated with program. Selection into the program itself, even if providing simply the promise of future benefits, should be enough to generate this support because of the credible expectations regarding the household welfare improvement. Although we recognize that beneficiaries-to-be are as cognizant as we are about the possible trade-offs in becoming a homeowner in a program that implies relocation possibly to a worse neighborhood, we believe that the expectations for future benefits from homeownership should overcome the anticipated downsides, especially so for individuals who would plausibly never achieve homeownership without heavily subsidized mortgages. Furthermore, the expectations about the transfers, but behavior might be also be driven by reciprocity (Finan and Schechter, 2012).

the program led individuals to actively sign up in the first place which we interpret to mean that, at least to some extent and at some point in time, individuals placed their hopes on this program.

Thus, the electoral preferences of beneficiaries as well as their evaluations of politicians' job performance and of the program itself are more likely to be shaped from the moment in which they are selected into the program. The symbolic effect of "being chosen" (even if by lottery) and most importantly the *promise* of a durable good and the expectations from homeownership should create support for the incumbents, but also the subsequent process of meeting the formal requirements of the program and actually signing the agreement and receiving the key to the new home require some level of interaction between the beneficiary-to-be with government (broadly considered) can also influence beneficiaries-to-be's views on governmental officials in general.

In contrast, we suspect that material effects of homeownership in the context of the MCMV program mainly take place as beneficiaries move to their new homes and start making heavily subsidized payments, typically much lower than their rent payments (see discussion in the next section). Although it is possible that beneficiaries anticipate changes in permanent income as soon as they learn about their selection into the program and thus adjust consumption accordingly, there is still uncertainty as to whether they fulfill the programs' requirements, whether the home will be delivered, and when exactly beneficiaries will move to their new home. In this sense, we expect that any effects of welfare changes will only be felt after the move.

Importantly, the costs and benefits of the homeownership accrue at different points in time which implies that the net welfare effects of the program might be different depending when they are measured, and therefore might have shifting effects on beneficiaries political preferences and attitudes. Adding to the complexity of the situation, given the already mentioned centrality of housing in peoples' lives, ownership could also generate substantial symbolic effects that can affect, on their own and independently of any welfare effects, beneficiaries' preferences and attitudes.

In this study, we examine an alternative mechanism about the political consequences of homeownership through a government program. We shift the focus from changes in social status ("investor class"), changes in interest and stakes in local and political affairs, and

responses to government transfers that are all driven by changes in material well-being caused by ownership. Instead, we examine the consequences of the promise of homeownership and the expectations about government program in shaping beneficiaries-to-be. Therefore, we focus on *selection* as a beneficiary in order to examine whether *selection* creates political effects even before any welfare effect has happened.

In summary, we expect to find selection into the program affects voters' electoral preferences, even though no actual welfare changes have taken place. Thus, we expect that the mere notification about being selected by the housing lottery to affect subjects incumbent evaluation even though they have not yet been awarded any good or became *de facto* homeowners. Any such effects, of course, should only happen relative to incumbents that are perceived as directly associated with the MCMV program. At any rate, such a finding would suggest that voters reward governments not only based on the economic, material rewards but also on the expectations created by such policies. This is a particularly relevant phenomenon to examine. If these effects do exist, it might be in the interest of politicians to promise to deliver goods and create expectations even if with significant delay to deliver them.

Furthermore, we also measure beneficiaries' expectations about future welfare changes to determine the extent to which these expectations, as opposed to other purely symbolic factors, drive the main effects. As today's poor may be tomorrow's wealthy, and vice versa, expectations about future income and mobility may shape demand for redistribution and attitudes about the role of government in social policy. Thus, such a distinction allows us to understand the extent to which the main effects derive from a rational analysis of future benefits or from more behavioral reactions to the fulfillment of an aspiration, irrespective of expected or actual welfare benefits.

We do not expect to find effects of selection to participate in the program on social and political attitudes, such as subjects' attitudes towards market, redistribution, and self-reliance. We expect that these attitudes should change only once beneficiaries become *de facto* homeowners, even though their status as homeowners would be due to a heavily subsidized government program. Not only social attitudes are probably slower-changing, but the triggers for any such changes (i.e. individuals' social status and economic position) can only change after welfare effects kick in, which only happens after the move. We thus define elec-

toral preferences as those directly related to incumbent evaluations, credit attribution, and partisanship whereas we social and political attitudes as those broader beliefs on someone's place in society and societal themes such as inequality and redistribution. In summary, we argue that the *expectation* of becoming a policy beneficiary shapes electoral attitudes, while the *actuality* of becoming a policy beneficiary affects social beliefs and political attitudes. Our main general hypotheses can be generically stated as:

Hypothesis 1. *Lottery winners' will hold more pro-incumbent electoral preferences and job evaluations than lottery non-winners, even before winners move to a new home.*

Hypothesis 2. *Lottery winners expectations about future welfare benefits will be more positive than that of non-winners*

Hypothesis 3. *Lottery winners life satisfaction (happiness) will be more positive than that of non-winners*

Hypothesis 4. *Lottery winners' social and political attitudes will not differ from those of lottery non-winners even before winners move to a new home*

Electoral preferences, expectations, life satisfaction, and social and political attitudes, which are the outcome variables in this study, are assessed through a survey of beneficiaries-to-be and non-beneficiaries of the MCMV program in the city of Rio de Janeiro, as discussed in detail starting in Section 3. Before this, however, we turn to the description of the program, research design, data collection and analysis.

2 The *Minha Casa Minha Vida* Program

The MCMV program is one of the largest government-run housing programs in the world. The program was initiated in 2009 and by 2017 it had contracted 4,692,623 housing units, in 3,663 of Brazil's municipalities. The program involves multiple levels of government and different eligibility tiers. As a preliminary point, the program finances homes, it does not really transfer them to beneficiaries. Therefore, program beneficiaries are technically "borrowers" (*mutatários*) and they are not homeowners until they finish paying off the installments. Eligibility, enrollment and the terms of the loans vary depending on the household income, but for the the lowest income participants in the program these terms are so generous that they approach an outright handout.

The lowest income tier known as Tier 1 (*Faixa 1*) is geared towards families with monthly income of up to R\$ 1,800. Families must enroll and apply with their local government and actual beneficiaries are also selected by the local government, either through a lottery run by a state-run bank (*Caixa Econômica Federal*) or any another method determined by the municipality. If selected, eligible beneficiaries are offered a 120 month contract with installments that vary from R\$ 80 to R\$ 270, depending *solely* on their income. The value of the home and beneficiaries' credit worthiness do not affect the selection process or the terms of the loan, and subsidies can reach 90% of the value of the home. By 2017, 1.5 million units had been contracted and 1 million delivered by the end of in this tier of the MCMV program.⁵

For the higher-income tiers, the subsidies are considerably lower. Families apply directly to one of the participating public banks and are, in principle, subject to credit analysis. Families with incomes of up to R\$ 2,600, for example, qualify for Tier 1.5⁶ and receive an interest rate of 5% on a 30-year mortgage, with a maximum subsidy of just under R\$ 50,000. Those in Tier 2 (up to R\$ 4,000) can receive a maximum subsidy of just under R\$ 30,000, while those in Tier 3 (income of up to R\$ 7,000) receive a discounted interest rate, but no direct subsidy.

The program's institutional architecture is also quite complex. Private developers typically buy the land, plan and seek approval for the project, and build the units, though the program also allows for civil society organizations (i.e. *MCMV Entidades*) to apply for funds and for smaller projects to be carried out by individuals and/or co-operatives. The local government (usually municipalities, but sometimes states) approves the site, and for Tier 1 projects also organizes the roll of applicants, and conducts the selection method for beneficiaries. The local government sends the list of selected families to the bank administering the project. This is typically *Caixa Econômica Federal*, the Federal Government's Mortgage lender, but *Banco do Brasil*, the main public retail bank and other smaller banks are in charge of some projects. The financial institution checks eligibility, administers the loans, and follows the construction. The federal government provides the funding through the

⁵MCMV Tier 1 also includes initiatives to provide housing to families in particularly vulnerable situations, and a number of units in Tier 1 lotteries are reserved for elderly and/or disabled beneficiaries. We do not include these units/beneficiaries in our analysis, focusing strictly on the "regular" Tier 1.

⁶This tier was created when the program was revised under president Temer.

Urban Affairs Ministry (*Ministério das Cidades*), and oversees its implementation by local governments.

Due to the country's heterogeneity, the program's decentralized implementation, and an active trial-and-error process at the local level, actual procedures on the ground pertaining to selection of beneficiaries vary widely across different municipalities. We focus on the program as implemented in the municipality of Rio de Janeiro. Rio is the the second largest municipality in Brazil, with a population of approximately 6.5 million (in 2017). Rio de Janeiro was also one of the first municipalities to take part in the MCMV program, and has continued to do so over the years. Since the program's inception, we identified 23 lotteries for Tier 1 units in Rio between 2009 and 2017.⁷ Administrative records of the roll of participants in each lottery, published prior to each drawing, indicate that a total of 712,885 individuals participated in at least one, but most often several, of these lotteries. As of August 2017, a total of 44,711 individuals had been selected as beneficiaries and as of April, 2017, 28,562 housing units had been built and delivered.

Many more individuals were selected than homes were delivered for several reasons. One preliminary reason for this discrepancy is that loan contracts for some projects had not yet been awarded at the time we collected the data due to construction delays. However, we identified several other implementation difficulties that lead to the relatively low take-up rate. The city government has trouble finding selected beneficiaries. Individuals could easily sign up for the lotteries, and once this was done they remained enrolled in subsequent drawings.⁸ Individuals eligible for Tier 1, however, often live in informal housing with addresses that are hard to reach or not served by regular mail, and switch mobile phone numbers often. By the time individuals were selected, their contact information was outdated and the city government could not easily reach them. In addition to this, many individuals that were reached by City Hall were no longer eligible for the program or could not provide the documentation necessary to for the loan approval. Finally, there was also the issue that some beneficiaries simply turned down units in housing projects for several reasons, but

⁷This number is after discarding lotteries for special units reserved to elderly, disabled, resettlements and neighborhood/regional lotteries. Table A.10, in the Supplemental Information, provides the list of Tier 1 lotteries.

⁸This is true for most lotteries but, in 2016, the city eliminated from the lottery pool a significant number of individuals that it was able to determine were no longer eligible, something that had never been done before.

mainly because the projects were in areas of the city far removed from their original homes.

The city’s first reaction to these difficulties was simply to increase the number of “reserve” individuals drawn in each lottery. This, however, was insufficient to meet the supply of units that were being built and several “supplemental” lotteries had to held to allocate units in some projects. A few years later, the city instituted “neighborhood” lotteries to distribute units that remained unallocated after at least three lotteries.⁹

3 Sampling and Data Collection

We collected data through a survey with winners and non-winners who participated in five Tier 1 lotteries conducted in Rio de Janeiro between October 2016 and March 2017. These were the most recent lotteries at the time of design of the study, which meant that beneficiaries had not yet actually received the home (or a contract, for that matter).¹⁰

Table 1 describes the lotteries that defined the population of our study. Most individuals reported in this table participated in several of the five lotteries. The population for the study consists of the unique number of participants in the set of five lotteries, and is reported in the last row of the table. Our original sample consisted of *all* unique lottery winners and a randomly selected subset of the non-winners. The size of the subset of non-winners was chosen to be larger than the treated sample and also to allow for more replacements.¹¹

We were able to obtain addresses and/or phone numbers for almost all the individuals we selected to be in our sample. We refer to this group as the “identified” sample,¹² in Table 1. Interviews were conducted between May 2017 and January 2018, both by phone and face-to-face, and enumerators were blind to treatment assignment. The fieldwork took longer than planned partially because of the deteriorating security conditions in Rio’s suburbs, but mostly derived from the same difficulties in finding the individuals in the sample that afflict

⁹At the time of writing, there were plans to implement a system to actively eliminate non-eligible individuals from the roll, thus increasing the “efficiency” of each lottery.

¹⁰Each lottery is announced by an official call (*edital*) that is published in the municipality’s gazette. These are lotteries 17/2016, 20/2016, 05/2017, 06/2017, and 07/2017, as described in Table A.10. Lottery winners are also publicly announced in official media. Our first design, described in the our registered Pre-Analysis Plan, included only lotteries 17 and 20, but due to difficulties in the field we had to expand the sample due to difficulties in contacting the sample.

¹¹Our planned sample was 2,000 individuals. However, due to difficulties in contacting subjects, we could not achieve that number. In our power calculations registered in our pre-analysis plans, we would achieve 0.80 power with less than 1,000 respondents but our pilot suggested that there could be compliance issues (as we later confirmed) – thus we decided on attempting to contact 2,000 respondents.

¹²We obtained contact information from commercial providers for all of the lottery winners and for a random sample of lottery non-winners.

Table 1: Population and Sample the Study

Population				
Edital	Lottery Date	Housing Project Name	Non-Winners	Winners
17.2016	2016-10-19	Vila Carioca, Safira, Sabóia	580,983	2,299
20.2016	2016-11-05	Porto Fino, Ametista	484,151	612
05.2017	2017-02-18	Onix, Porto Fino, Ametista	51,879	395
06.2017	2017-03-04	Vila Carioca	579,709	875
07.2017	2017-03-18	Safira	579,809	338
Samples				
Unique Lottery Participants			586,455	4,514
Identified Sample			8,039	4,420
Contacted			1,126	655
Interviewed			681	360

Notes: One of the individuals in the survey was selected as a lottery winner in a call different from the ones included in our survey. We considered that this individual to be treated because she was selected as lottery winner before our interview date. “Contacted” is defined as whether an someone answered the phone/door and whether the selected person was at home or available to talk on the phone.

City Hall, described above. Despite having employed more resources that City Hall typically does in finding each individual in the sample,¹³ and having made more than 45 thousand attempts to contact the 12.5 thousand individuals we identified for our sample, we were only able to find 1,555 individuals, and to ultimately interview 1,041 of them.¹⁴

4 Analysis

In this study, we are interested in knowing the effect of being selected as a lottery winner on electoral preferences, social, and political attitudes prior to moving to a new home. The existence of the lottery allows us to treat selection into the program as independent of any characteristics of the selected individuals, which is extremely helpful in parsing out the effect of being selected as lottery winner from all other observed and unobserved factors that typically influence both attitudes and whether an individual is a beneficiary of a social program. However, the complexity of the lottery and the MCMV create several challenges to the consistent estimation of causal effects of being a lottery winner, chief among them are differing patterns of participation in the lotteries, non-compliance, and attrition.

¹³We merged the sample with an administrative database and purchased telephone and addresses from two different commercial providers.

¹⁴Four individuals were interviewed twice (due to calls dropping and clerical error) and we excluded them.

Treatment and Compliance: As our intention in the present study is to identify the effect of having been selected to participate in the program, our sample was constructed not to include any beneficiary that had already received the home.¹⁵

This posed a conceptual difficulty in determining which individuals could, in fact, be considered treated. Results of the lottery are public, but in practice, individuals might not have learned about their selection. City Hall is not able to contact all the winners, and non-winners are not contacted, so for many individuals who signed up for the lottery can learn if the result of the lottery if they follow-up on their own. In this study, therefore, being selected in the lottery is the assignment to the treatment, and knowing about the result of the lottery is our indicator of compliance. Compliance, it should be mentioned, does not enter our main analysis, which focuses on the intent-to-treat effects. Compliance matters for our instrumental variable analysis, in which we replicate the main analysis but use winning the lottery as the (randomized) instrument for knowing the results of the lottery, and therefore estimate the effect of treatment for compliers. In our sample, 81% of respondents the correct information about the outcome of the lottery (56% of treated and 96% of control). More concretely, out of 359 subjects who were assigned to treatment, 202 knew they had won the lottery and 33 out of 682 assigned to control reported that they had won the lottery, which makes our compliance rate at 51%.

Patterns of Lottery Participation: The patterns of participation in the lotteries pose an important aspect of the analysis. All individuals in our sample have different probabilities of assignment to treatment depending on their patterns of participation in the lotteries because sampled individuals might have entered the roll of applicants at any point since 2009. Most never left the rolls,¹⁶ but some did, for several different reasons. Moreover, each past lottery had a different probability of winning (because the number of participants and the number of winners also varied). Thus, it is necessary to account for the pattern

¹⁵In fact, five treated respondents had already moved to the new home by the time of the interview. They were assigned to the “Sabóia” project, which had been completed a few months earlier and for which Edital 16.2017 selected beneficiaries for a few remaining units. This creates a challenge as their treatment is different than the treatment received by the other individuals who won the lottery but had not yet moved. We decided to keep these individuals in our main analysis because they were assigned to treatment by our instrument even though their inclusion might lead to SUTVA violations. However, they had only very recently moved by the time we interviewed them which means that the long-term welfare changes associated with becoming a homeowner had probably not taken place yet. Removing these individuals does not affect our main results.

¹⁶A few subjects stayed on even after winning the lottery.

of participation in lotteries such as not to create imbalances between lottery winners and non-winners due to different probabilities of assignment to treatment that stem from when subjects entered the lotteries and in which lotteries they participated. In this way, we created a variable that assigned “0” if the subject did not participate in a given lottery and “1” if the subject participated in a given lottery, for all lotteries. For the analyses in the paper, we include indicators for patterns of participation in the five lotteries included in the survey. Among individuals in our interviewed sample there exist 10 different patterns of participation in the MCMV lotteries included in our survey. As these patterns are probably not independent from other individual characteristics, we use these patterns as “fixed effects” both in the assessment of balance and in the analysis. These fixed effects can be thought of as akin to blocks fixed effects in block randomization schemes and they account for different probabilities of assignment to treatment based on the patterns of participation in the lotteries.¹⁷

Attrition: If response to our survey is related to treatment assignment, the comparison between lottery winners and non-winners would be compromised. We conduct two types of tests to assess whether attrition due to non-response rates are different across winners and non-winners: (1) a two-tailed unequal-variances test of the hypothesis that treatment does not affect our response rate (non-response indicator); (2) a linear regression of non-response indicator on treatment, baseline covariates, and treatment-covariates interactions (and we report the F-test of the hypothesis that all the interaction coefficients are zero).¹⁸

Table 2: Tests of Attrition

	Lottery non-winners	Lottery winners
Not interviewed	0.91 (7,358)	0.92 (4,060)
Interviewed	0.08 (681)	0.08 (360)

Notes: The unit is sampled subject.

The results from the t-test indicate that we are unable to reject the null hypothesis that

¹⁷See Appendix for a more detailed discussion of these patterns of lottery participation and for alternative specifications of patterns of participation.

¹⁸Furthermore, our interviewed sample is not very distinct from individuals in our sample that we were unable to interview: they have similar shares of participation in the formal labor market, income from formal employment, although males are a bit underrepresented in our interviewed sample. Table A.12 compares our interviewed and non-interviewed samples in terms of income from formal employment, share of formal employment, and sex.

treatment does not affect our response rate (mean in control = 0.085, mean in treatment = 0.081, $df = 9240$, $p\text{-value} = 0.5266$). We also regressed non-response indicator on treatment, baseline covariates (formal labor and income from formal labor) and treatment-covariate interactions in the sample of subjects. The $p\text{-value}$ of F-test of the hypothesis that all the interaction coefficients are zero is 0.41.¹⁹

Balance: We checked balance in the interviewed sample for several pre-treatment covariates. Table 3 reports regressions results of treatment on a single pre-treatment covariate at a time. This regressions included the above-mentioned fixed effects. We see statistically significant differences between treated and non-treated groups on age ($p\text{-value} < .05$), but no difference in the other five demographics. We also merged our sample data with administrative data on wages and formal employment to examine pre-treatment differences on these variables in eight years prior to the lottery. We found significant differences in the wages formal employment between 2004 and 2007 ($p\text{-value} < .10$), but not for levels of formal employment. It is somewhat concerning that we find statistically significant differences in 5 out of 22 covariates. However, we believe these differences to be acceptable, given that the $p\text{-value}$ of the F-statistic of the regression of treatment on variables of sex, religion, age, race, schooling, living with children, average annual nominal income from labor between 2003 and 2010 and indicator variables for formal labor between 2003 and 2010 is 0.26 (full model includes all pre-treatment variables and patterns of participation indicators and reduced model only includes pattern of participation indicators) and four of the five variables that are highly correlated (the average correlation between these is 0.7). Furthermore, models that include all of these covariates as controls yield very similar results.

¹⁹The $p\text{-value}$ of the F-test the hypothesis that all the interaction coefficients are zero for the regression including patterns of participation indicators is 0.45

Table 3: Balance for survey pre-treatment covariates, interviewed survey sample

Outcome	Effect	S.E.	n	p-val	Standardized Difference
Religion (pentecostal)	-0.01	0.06	1030	0.85	-0.017
Sex (male)	-0.01	0.06	1041	0.90	-0.011
Age (old)	0.18	0.06	1041	0.00	0.257
Race (white)	0.05	0.06	1032	0.38	0.079
Schooling (high)	-0.02	0.06	1037	0.79	-0.023
Children (number)	-0.02	0.14	1041	0.89	-0.010
Wages from formal employment (in each year)					
2003	-139.95	337.89	1041	0.68	-0.083
2004	100.19	49.66	1041	0.04	0.185
2005	104.25	55.81	1041	0.06	0.131
2006	116.38	55.59	1041	0.04	0.177
2007	122.10	65.24	1041	0.06	0.159
2008	149.50	92.84	1041	0.11	0.148
2009	301.17	240.50	1041	0.21	0.233
2010	53.36	73.65	1041	0.47	0.052
Formal employment (in each year)					
2003	0.09	0.06	1041	0.17	0.123
2004	0.07	0.06	1041	0.29	0.098
2005	0.06	0.07	1041	0.36	0.086
2006	0.08	0.06	1041	0.21	0.114
2007	0.06	0.06	1041	0.32	0.089
2008	0.03	0.06	1041	0.63	0.042
2009	0.03	0.07	1041	0.62	0.045
2010	-0.04	0.07	1041	0.59	-0.04

Notes: Estimates from regressions of treatment on a single pre-treatment covariate and participation patterns. Bootstrapped standard errors (1,000 replicates). Pattern indicators omitted. Religion: 1 = pentecostal, 0 = any other religion or no religion; Sex: 1 = male, 0 = female; Age: 1 = above the median (45.71), 0 = below the median (45.71); Schooling: 1 = completed high school or more, 0 = incomplete high school or less; all wages are in nominal values and those who do not receive wages from a formal job are assigned zero wages; formal labor is an indicator variable: 1 = any formal job in that year, 0 = no formal job in that year. Standardized difference is calculated by dividing the effect size by the pooled standard deviation of the outcome variable.

Estimation: In our main analyses, we estimate the average intent-to-treat effect using a regression that includes fixed effects for the patterns of participation in the full set of lotteries, as discussed above. In the regression model (1), d_i is the treatment assignment indicator, which was defined by the lotteries. This makes β_1 an estimate of the average intent-to-treat

effect (ITT).²⁰ Moreover, ω_i represents the fixed effects for lottery participation patterns.

$$Y_i = \beta_0 + \beta_1 d_i + \omega_i + u_i \quad (1)$$

We also estimate the complier average treatment effect (CACE) through a two-stage least squares approach. In this setup, treatment assignment d_i is used as an instrument for knowing about the result of the lottery, which we represent by l_i . The first stage of the analysis, therefore, is defined by:

$$l_i = \alpha_0 + \alpha_1 d_i + \omega_i + u_i \quad (2)$$

in which all other variables are defined as before. The second stage is identical to Eq. 1, except that the predicted values from the first stage (\widehat{l}_i) are used in place of d_i .

Operationalization of the Variables: Even though we work with only four general hypothesis, each of them implies the measurement and analysis of several outcome variables. All outcome variables in the study were operationalized based on respondents' answers to survey questions.

We measure electoral preferences and incumbent evaluations, which according to Hypothesis 1, we expect to be positively affected by selection into the program, in a number of different ways. These include job performance evaluations for former presidents Lula and Dilma, former mayor Eduardo Paes, and current President Temer and Governor Pezão. We also inquired about pro-incumbent vote in 2014 presidential and 2016 mayoral elections, though the link here is much more tenuous as these elections were held before the lotteries and, as such, we expect no effect on these outcomes.

Future expectations, the outcome variable for Hypothesis 2 is operationalized as the Future Expectations Index, which is a mean effects index²¹ of four variables that assess respondents long-term intergenerational mobility assessments, including items about whether they expect their children to finish high school; to attend college; whether they expect their

²⁰In the appendix, we also include additional regression models to include pre-treatment covariates (religion, race, sex, age, years of schooling, annual income from 2003 to 2010, and formal employment from 2003 to 2010) to increase precision of our estimates.

²¹We recoded outcomes so that positive effects are scored higher across all outcomes. We calculate a z-score by subtracting off the control group mean and dividing by the control group standard deviation.

children to have a better financial situation than the family currently has; and whether they expected the family to have better financial situation in five years time. As most of these items were only applied to respondents with children, we also report separate results for the last question, which was asked to all respondents.

We operationalized subjective wellbeing and happiness in four different ways. The first three, based on Kahneman and Deaton (2010) were life-satisfaction, measured through the Cantril Self-Anchoring Scale for life-satisfaction, a positive affect score, measured by averaging reports of enjoyment, happiness, and laughter in the previous day, and a “blue affect” score, created by averaging reports of worry and sadness. We also combined the three measurements in to an overall happiness score.

Finally, social attitudes, which are the outcome variable for Hypothesis 4 were operationalized in several different ways. One was through a market belief index, which following Di Tella, Galliani and Schargrotsky (2007) was constructed by combining answers to questions on trust, effort, materialism and personal success. We also examined respondents’ responses to items on tax-and-transfers, redistribution, individual self-reliance, as well as respondents’ perceptions of where they stood in the national distribution of income.²²

5 Results

The main results from the study are presented in Table 4, and we proceed from the expected to the unexpected results.²³

As expected by Hypothesis 4, we found no effect on social and political attitudes. Views on taxation, self-reliance, and redistribution, as well as respondents self-placement in the income distribution in society did not differ between selected and non-selected respondents. Complier average treatment estimates for the effects are somewhat larger than ITT estimates, as expected, but still too noisy to be distinguished from zero.²⁴

Results for Hypotheses 3 were moderately surprising. Not only we did not find the expected improvement in subjective wellbeing among lottery winners, but we actually found a decrease. Results are statistically significant for the life satisfaction (“Cantril ladder”)

²²Tax-and-transfers, redistribution, and self reliance are the combination of two survey items each.

²³We laid out our expected results in a pre-analysis plan prior to the collection of the outcome data.

²⁴Unsurprisingly, given the prevalence of compliers, assignment to treatment is a very strong instrument for knowing about the result (i.e compliance). The F-statistic on the instrument is 554.33, with a p-value $\ll 0.001$.

item, and the effect is sizable (just under one standard deviation of the distribution of the variable). For the emotional wellbeing items, results are small and not statistically significant, but we do see a reduction in positive affect and an increase in the “blue” affect, which further corroborate the overall negative effects of the program. Our overall happiness index is negative, but falls short of statistical significance.

The test of Hypotheses 2 also yielded results that were unexpected to us, but quite consistent with what we found for 3. Lottery winners’ expectations about the future did not differ from non-winners, regardless of the statistical specification for our composite future expectations index. The problem, here, is that this index included questions only applied to those that had young children and, therefore our N was much reduced. For this reason we also analyzed separately one of the item in the index that for which we had the most responses. Results are also not statistically distinguishable from zero, but the coefficient points to negative effect.

The most striking and surprising results, however, were obtained in the analysis of Hypothesis 1. We found a strong effect on approval of former presidents Lula and Dilma (for Dilma, the effect was borderline statistically significant) but this effect was *negative*. The average complier effects correspond to almost one standard deviation of the distribution of approval for Lula, and two-thirds of a standard deviation for Dilma.

Although surprising, we have reasons to believe it is not spurious. First, the baseline levels of approval for the five politicians seem to correspond to expectations (see Table A.11), with Lula’s being by far the highest, Dilma and former mayor Eduardo Paes’ at an intermediary level, and President Temer and Governor Pezão’s being close to the absolute bottom of the rating scale. Second, results for four of the five politicians we included in the survey were negative, and for the three politicians that might be more closely attached to the program (the two former presidents and the former mayor), they were relatively large. In fact, the only politician for which we did not obtain a negative estimate was President Temer, but the estimates are probably subject to a floor effect.

Additional baseline statistics and effects observed on other “unplanned” outcomes are consistent with these surprising results for Hypothesis 1. Of the whole interviewed sample, 36% attribute the MCMV to Lula and 11% to Dilma. These percentages are far higher than any other politician, as is revealed by the fact that the third place is former mayor Eduardo

Paes, with less than 3% of respondents. Therefore, it should not be that surprising that the we found stronger effects for the two former presidents. A higher share of winners also attribute the program to Lula and Dilma, which suggests that winning is also correlated with “learning” about the program.

Three of the most striking results, however, come come in the last rows of Table 4. The first measures agreement with the statement that the MCMV program “brings hope of a better life” and the second is a direct evaluation of the MCMV program, in which we asked whether respondents (both lottery winners and losers) thought the program had improved the lives of those that participated in it. Here results are not statistically significant, but they are negative for both variables in both specifications. The next item assessed respondent’s agreement with the adage that “homeownership is a dream” that transforms people’s lives and the last item inquired whether the respondent intended to move to the new house or would move to a new house if selected in the lottery. Again, selection into the program had a negative and statistically significant effect for both items. The change for agreeing that homeownership is a dream was just under one standard deviation, and for intention to move was almost 1.4 standard deviation, the single strongest effect found across all outcomes we analyzed. It is important to stress, from the outset, that a large majority of survey respondents (72%) would move or planned on moving to the MCMV home. Still, the treatment effects on this outcome are strong and negative across the range of specifications and heterogeneous effects we examined.

Table 4: Effects of MCMV on Main Outcomes

Outcome	ITT Estimates			CACE Estimates			N
	Effect	S.E.	p-val	Effect	S.E.	p-val	
Lula Eval.	-0.403	0.157	0.010	-1.220	0.480	0.011	980
Dilma Eval.	-0.241	0.156	0.121	-0.744	0.525	0.156	978
Paes Eval.	-0.172	0.158	0.275	-0.541	0.495	0.274	974
Pezão Eval.	-0.005	0.121	0.967	-0.016	0.410	0.968	977
Hyp. 1 Temer Eval.	0.026	0.126	0.836	0.080	0.401	0.842	957
Inc. Vote 2014	-0.011	0.079	0.887	-0.031	0.232	0.893	641
Inc. Vote 2016	0.003	0.035	0.928	0.008	0.100	0.936	617
Some Party ID	0.037	0.031	0.233	0.120	0.099	0.226	1002
PT ID	0.005	0.016	0.759	0.017	0.056	0.764	1041
Antiparty ID†	-0.002	0.048	0.975	-0.005	0.168	0.977	975
Hyp. 2 Fut. Expectations	0.033	0.192	0.862	0.129	0.807	0.873	505
Fut. Expect. (All)†	-0.148	0.182	0.418	-0.492	0.621	0.428	961
Happiness Index	-0.107	0.117	0.357	-0.354	0.419	0.399	1007
Hyp. 3 Life Satisfaction†	-0.575	0.290	0.048	-1.907	1.076	0.076	1035
Positive Affect†	0.010	0.043	0.817	0.033	0.138	0.809	1018
Blue Affect†	-0.013	0.043	0.766	-0.043	0.156	0.783	1028
Market Beliefs	0.076	0.122	0.532	0.257	0.433	0.553	1008
Tax-and-transfers	-0.148	0.122	0.225	-0.487	0.435	0.262	999
Hyp. 4 Self-reliance	-0.010	0.082	0.900	-0.036	0.297	0.905	1013
Redistribution	-0.265	0.250	0.288	-0.938	1.037	0.366	945
Placement	0.326	0.257	0.205	1.101	0.866	0.204	1033
Attribute to Lula/Dilma†	0.051	0.067	0.448	0.189	0.270	0.484	892
MCMV Brings Hope†	-0.007	0.035	0.841	-0.023	0.126	0.853	1033
Other MCMV is Positive†	-0.202	0.124	0.104	-0.671	0.475	0.158	991
Dream†	-0.216	0.085	0.011	-0.728	0.336	0.030	1039
Would Move†	-0.259	0.099	0.009	-0.857	0.284	0.003	1036

Table reports the effects of being selected into the program on several outcomes that operationalize the hypotheses presented earlier. The first set of estimates correspond to the intention to treat estimates, as defined in Eq. 1, and the second set are treatment effects on compliers, as defined in Eq. 2. † indicates that outcome variables were not in the registered Pre-Analysis Plan. Sample size varies due to item non-response. Standard errors were computed through non-parametric bootstrapping.

6 Making sense of unexpected findings

What drives these consistent “negative” results in the opposite direction of what we expected? We conducted in-depth follow-up interviews with 10 beneficiaries-to-be and 5 non-beneficiaries in our original sample to generate hypotheses about the mechanisms, and then returned to the data. In this section we review the qualitative and quantitative evidence.

Almost all individuals we interviewed were prompted to enroll in MCMV through work colleagues or acquaintances, even though many recalled seeing or hearing about the program through the media. Part enrolled in person, usually at the City Hall’s MCMV main office, but many enrolled over the Internet, usually with the help of a younger relative or friend. No interviewee mentioned any intermediary by politicians or political agents.²⁵ All mentioned that enrollment was easy and hardly any of the respondents sought additional information about the program at that point. Very few even followed the lottery results. For beneficiaries, the post-lottery experience with gathering the paperwork and other bureaucratic eligibility requirements were also generally regarded as simple and positive, even among those who ultimately declined the unit.

Although most of our interviewees seemed to connect the program with Lula and Dilma, most also had some (accurate) sense that the program was a shared responsibility of different branches of government.²⁶ In the interviews, there was no clear association between experience with the program and assessment of Dilma and Lula. As one interviewee put very candidly: “Look, I believe the program is good, it could be... Excellent. See what I mean? But this program doesn’t make me say that Dilma is an excellent president” (I3).

Evaluations of the MCMV program, however, did follow some clearer patterns. One very strong regularity is that *all* non-beneficiaries that we interviewed said they would move to the new home, if awarded one. When pressed to reflect on the question, and to think about possible location of the MCMV unit, crime-levels, and access to services, most conceded

²⁵This is consistent with the survey data, in which 80% of respondents indicated learning about the MCMV through family, friends, or media, and close to 90% declared not having received any type of help or intermediation. Still, the qualitative interviews bolster our belief in the quantitative results because during the qualitative interviews we are able to establish a better rapport (as suggested by the personal content and the admission of some socially frowned upon behavior in many interviews) between interviewees and interviewers that probably reduce the risk of social desirability bias that could lead to an underestimation of reporting *quid pro quo* in the survey.

²⁶In the survey, 42% said Lula was the main responsible for the MCMV, followed by Dilma with 13.1%. Close to 30% of respondents did not know how to answer this question.

that under some circumstances they would, perhaps, have second thoughts on accepting the unit (see interviews I5, I8, I9, I10, I12 for examples). But, on balance, the net effect of the hope of winning and the frustration of not winning is clearly positive (I8, I12).

For many beneficiaries-to-be, in contrast, this balance is tilted in the negative direction. Expectations rise dramatically as soon as they are contacted and they have high expectations both in terms of how quickly after the lottery they would be able to move into their new homes, and of how desirable the new home would be. From there on, however, there seems to be considerable heterogeneity: for one group, these expectations are not met from the outset. These lottery winners find out that they are no longer eligible (I2), decline the MCMV offer usually due to location of the home and return to the pool of lottery participants (I3, I14), or even decide to take a Faixa 2 or a MCMV loan in a preferable location (I4). For others, even though expectations are not quite met, they continue with the process and grow frustrated with the wait or with the arrangements of the new home (I1, I13). For a third group expectations are mostly met, and they are satisfied with the outcome (I6, I7, I15). The fact that a portion of the beneficiaries-to-be are frustrated with the outcome while non-beneficiaries have uniformly high expectation explains why the average levels of satisfaction among lottery winners and relatively higher levels among non-winners.

Our interviews, therefore, suggest that heterogeneity is a function of wait-time and two sets of pre-treatment characteristics: living arrangements and, perhaps more importantly, characteristics of the current neighborhood and neighborhoods in which the housing projects are located. With regards to living arrangements, interviewees that shared a home with relatives or lived in “borrowed” homes were particularly eager to have a place to call their own, and so were those who lived in low quality housing. Crime, both in the original and the MCMV neighborhood, and distance to daily life responsibilities (work, school, etc. . .) were cited by interviewees as reasons both for accepting and declining the offered unit. For example, one interviewee mentioned that the MCMV location was “a thousand times better” than her previous home because she lives in “a *favela* and her granddaughter can no longer stand the noise from gunshots” (I7). On the other hand, even some of the beneficiaries that were happy with the new unit consider their previous location better (I15).

Our interviews also indicated that most beneficiaries-to-be have a good understanding of

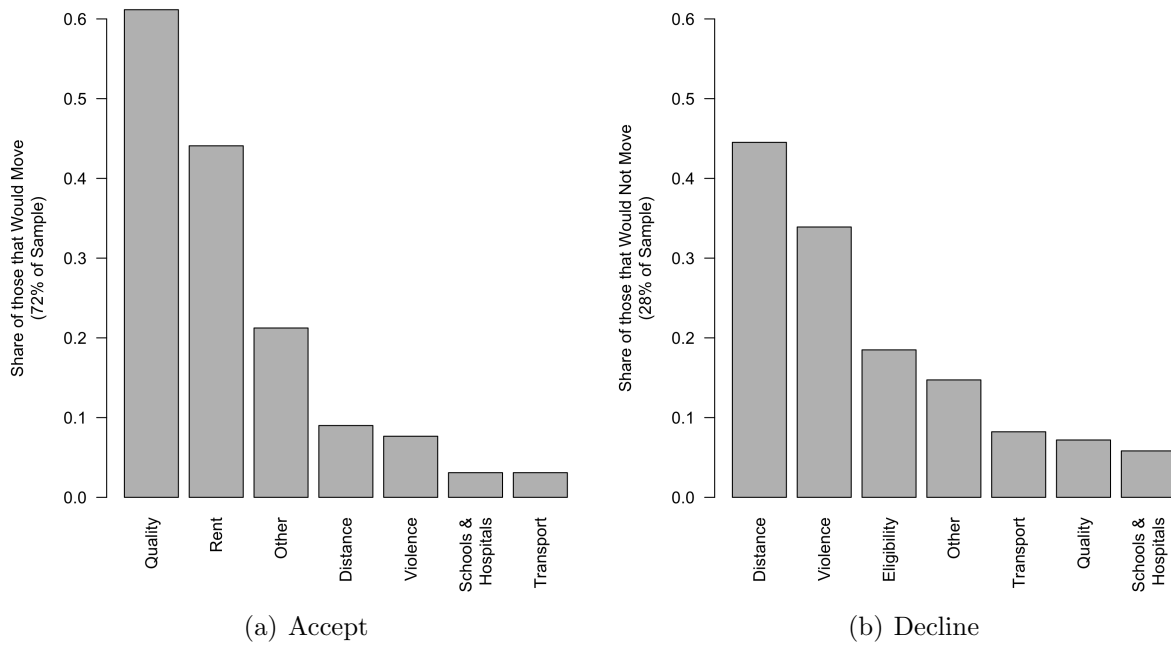


Figure reports frequency of reasons given by survey respondents to accept or decline MCMV units. Beneficiaries-to-be that knew about their status were asked about actual plans, non-beneficiaries and those that did not know about their status were asked what would they do if selected. Multiple answers were allowed, so figure reports shares of those accepting or declining that mentioned each of the reasons. Respondents that planned on declining and those that were undecided were merged in this analysis.

Figure 1: Reasons for Accepting or Declining MCMV Units

the costs (financing, utilities, association fees, etc...) associated with the new home. Even if costs were higher than what they had before, beneficiaries considered it worthwhile (I15). Those that declined the home, it seems, were not driven away by these costs. In fact, the cost of the financing does not seem to be a problem for any respondent. One interviewee, describing the situation of acquaintances who had received a MCMV unit, stated: “they are paying [the monthly installments] regularly, it is not an exorbitant amount because it is based on people’s income... I have never heard of people who lost their home because they couldn’t pay them.” Another interviewee corroborated the same idea. The driver for refusal, when it happened, seemed much more associated with location than with cost (I14).

Survey responses pertaining to the reasons for accepting or declining a MCMV unit corroborate many of these insights from the qualitative interviews.²⁷ Figure 1 presents simple frequencies in which each reason was given.

²⁷In our survey, respondents who reported having won a lottery were asked whether they planned on accepting or not the unit. Those that reported not having won the lottery were asked whether they would accept if the unit if they were selected in the lottery.

On the one hand, on the acceptance side, a majority of the respondents mentioned that the MCMV unit was of better quality than their current home and being able to stop paying rent was the second most frequent reason, mentioned by more than 40% of the subsample. Other answers were far less common. On the other hand, the reasons for declining a MCMV unit were much more varied. Concerns with distance (relative to social network, school, and work) as well as violence in the neighborhood of the projects were the most common complaints, followed by not meeting eligibility requirements.

To sum up, the in-depth interviews revealed uniformly positive expectations about the program for non-beneficiaries, and quite a lot of heterogeneity among beneficiaries-to-be. Being selected as a beneficiary focuses one's attention on concrete aspects of the program, prompting beneficiaries-to-be to reassess their expectations, some of which are not met. In this context, heterogeneity seems mostly driven by characteristics of the neighborhoods (current and MCMV neighborhood) but also, to a lesser extent, by previous living arrangements. It is also the case that some beneficiaries-to-be experience long wait-times after the lottery, which leads to a different set of frustrations. While the first mechanism is linked to "what" is being delivered, the second focuses on "how" it is provided. These insights suggest possible heterogeneous effects on outcomes pertaining to respondents evaluation of the MCMV program.

However, it is hard to determine how these assessments materialize in gain or loss of electoral support based on the interviews. Nevertheless, it is plausible that beneficiaries-to-be assign blame and credit to politicians about "what" and "how" the program delivers its benefits. Therefore, we also examine possible heterogeneous effects of selection into the program on our main political variables.

6.1 The "What" Mechanism

The qualitative interviews and the survey responses suggest that MCMV projects' locations and neighborhood crime levels are the concerns that drive beneficiaries-to-be to decline a MCMV unit and lottery non-winners to consider declining them. None of these characteristics, however, are absolutes. For several beneficiaries we interviewed, the district of Santa Cruz was "too far", and "controlled by militias." However, for others, the same location was desirable and crime in their previous neighborhood was considered much worse.

Therefore, in order to assess whether the heterogeneity we observed in the interviews is borne out in the data, we first examined whether treatment effects varied with (i) distance between (pre-treatment) home to assigned MCMV unit, and (ii) satisfaction with current (pre-treatment) neighborhood. Thus, in Table 5 we show how treatment effects of being selected as a lottery winner vary whether the beneficiaries lived near or far from the MCMV unit they were assigned to and in Table 6 we present heterogeneous treatment effects by satisfaction with neighborhood, measured through a composite index of satisfaction with different neighborhood attributes.

Table 5: Moderation by Distance (ITT Estimates, Selected Outcomes)

Outcome	Lived Near			Lived Far			Diff.	N
	Effect	S.E.	p-val	Effect	S.E.	p-val	p-val	
Lula Eval.	-0.490	0.176	0.005	-0.347	0.174	0.046	0.327	980
Dilma Eval.	-0.291	0.176	0.097	-0.209	0.169	0.216	0.537	978
Paes Eval.	-0.080	0.187	0.670	-0.231	0.168	0.169	0.274	974
Pezão Eval.	-0.022	0.139	0.874	0.006	0.128	0.965	0.803	977
Temer Eval.	0.130	0.143	0.363	-0.038	0.131	0.770	0.095	957
Fut. Expect. (All)	-0.405	0.227	0.074	0.022	0.192	0.911	0.011	961
Happiness	-0.086	0.137	0.530	-0.121	0.127	0.340	0.747	1007
Life Satisfaction	-0.844	0.325	0.009	-0.404	0.302	0.181	0.065	1035
Market Beliefs	0.050	0.138	0.714	0.092	0.122	0.450	0.700	1008
MCMV Positive	-0.204	0.145	0.159	-0.201	0.132	0.128	0.974	991
Dream	-0.158	0.096	0.099	-0.252	0.090	0.005	0.246	1039
Would Move	-0.083	0.101	0.408	-0.370	0.104	0.000	0.001	1036

Table reports the effects of being selected into the program computed by comparing outcomes for two subgroups of treatment individuals defined based on the distance from their homes to the MCMV units with average outcomes in the control group. Sample size varies due to missing responses. Standard errors were bootstrapped. “Diff. p-val” refers to the p-value of an F-test of the difference in the effects across groups.

In Table 5, we define individuals who live “near” and “far” from the MCMV unit as those below and above the median distance (among treated individuals) of 18.1 km. Results suggest that greater distance to MCMV units negatively affects individuals’ decision to move, and this effect is quite strong. We also find that for those who live farther away from MCMV units being selected into the program generates smaller (statistically significant) reductions in life satisfaction and future expectations. As distance is not measured for the control group, these results are driven by positive associations between distance and expectations and distance and life satisfaction in the treatment group. It is possible that those who

live farther away actually live in more centrally located neighborhoods.²⁸ Anyhow, these characteristics do not appear to translate into more support for incumbents, as the negative effects on approval of most politicians is only marginally smaller among those who live farther away from the MCMV projects.

Table 6 we find similar results beneficiaries-to-be who are satisfied with their neighborhoods are less likely to say they would move to a MCMV unit. This is consistent with our descriptive findings showing that intention to move declines strongly with satisfaction with current neighborhood for those in the treatment group, but not for those in the control group — see Figure A.8 in Appendix. Interestingly, intention to move declines strongly with satisfaction with living conditions, but this is true both for those in the treatment and in the control group (Figure A.9 in Appendix). Hence, we do not see heterogeneous treatment effects on this variable as we see with neighborhood. Overall, these findings suggest that assessments of the current neighborhood are made in comparison to that of the MCMV unit.

Table 6: Moderation by Neighborhood Satisfaction (ITT Estimates, Selected Outcomes)

Outcome	Low Satisfaction			High Satisfaction			Diff.	N
	Effect	S.E.	p-val	Effect	S.E.	p-val	p-val	
Lula Eval.	-0.327	0.182	0.072	-0.464	0.180	0.010	0.409	957
Dilma Eval.	-0.210	0.177	0.237	-0.275	0.171	0.107	0.684	955
Paes Eval.	-0.286	0.180	0.113	-0.110	0.176	0.533	0.278	952
Pezão Eval.	-0.091	0.139	0.513	0.075	0.131	0.569	0.178	955
Temer Eval.	0.033	0.148	0.824	0.007	0.140	0.962	0.833	935
Fut. Expect. (All)	-0.173	0.216	0.424	-0.150	0.206	0.467	0.907	939
Happiness	-0.043	0.136	0.753	-0.129	0.140	0.359	0.494	984
Life Satisfaction	-0.555	0.312	0.076	-0.563	0.332	0.090	0.976	1010
Market Beliefs	0.063	0.135	0.640	0.053	0.136	0.695	0.935	986
MCMV Positive	-0.250	0.148	0.091	-0.124	0.137	0.364	0.309	967
Dream	-0.181	0.096	0.061	-0.207	0.097	0.033	0.766	1013
Would Move	-0.150	0.105	0.156	-0.407	0.111	0.000	0.009	1011

Table reports the effects of being selected into the program for subgroups of treatment and control individuals that reported high and low levels of satisfaction with the neighborhood. Sample size varies due to missing responses. Standard errors were bootstrapped. “Diff. p-val” refers to the p-value of an F-test of the difference in the effects across groups.

Again, these variations do not appear to translate into lower or higher support for the incumbent — individuals’ support for incumbent does not appear to vary by satisfaction with the current neighborhood or with the location of the neighborhood in terms of distance

²⁸We would need a more systematic analysis using objective indicators of neighborhood’s quality based on subjects’ addresses to more rigorously examine this claim.

to current home. In summary, although “what” the policy offers does seem to be judged against what beneficiaries-to-be have, politicians in general do not appear to be penalized (or rewarded) by it.

6.2 The “How” Mechanism

A common criticism of MCMV that came up during interviews are the delays in building and delivering new homes.²⁹ How the policy is implemented, and especially the delays in getting those selected by the lotteries to move into their new homes, could be partially responsible for the a negative views on the program and, possibly, incumbents associated with them.

We then examined whether treatment effects vary with the length of time that had passed between winning the lottery and being interviewed. For this analysis we leverage two sources of variation. First, our sample for the survey is composed of participants in five lotteries, held up to five months apart. Second, interviews were conducted in a span of seven months. As a result, the time between winning the lottery to our survey interview varied from 61 to 466 days, with a median of 272.

Granted, the second component of wait-time is, strictly speaking, non-random and realized after the treatment. Many reasons determined the moment in which the interviews were conducted, and we believe these reasons are independent of individuals having been selected or not in the lottery. Enumerators and field-staff were blind to treatment status of potential interviewees and several interviewees that were contacted early in the field research requested to be interviewed in a later date. Moreover, we only released the contact information of interviewees to the polling firm in batches of selected and non-selected individuals. Neither treated or untreated individuals had yet moved, therefore they were equally geographically dispersed around the city, and were equally hard to reach. In fact, we checked at several moments while the survey was in the field, and found that the proportion of beneficiaries-to-be and non-beneficiaries were stable. For all these reasons, we believe that if we had randomized the date or order in which the interviews were to be conducted, results would not have been different than what we actually find.

In the analysis that follows, we divided the treated individuals into those waiting for up

²⁹One of the projects (Vila Carioca) was requisitioned by the National Guard in order hose troops deployed in the city. The operation was originally planned for the Olympics, but was extended after the Games, causing delays in the release of the units to their beneficiaries.

to or more than 10 months,³⁰ and computed both the difference between the average for each of these groups for those assigned to treatment and those who complied with treatment relative to the overall average of the control group. For the reasons discussed in the previous paragraph, this analysis does not reveal any sort of causal estimate because randomization does not dictate which individuals wait for longer and shorter periods and control group participants cannot be divided into these two groups, as almost all participated in several of the lotteries. Nevertheless, we believe they illuminate a possible reason why we observe such surprising “negative” results.

Results of this analysis are striking. Table presents the results on selected variables. For the key political outcomes, the negative effects found earlier are driven primarily by the effects on those that have been waiting for a longer time. Consider the case of Lula’s and Dilma’s job evaluation. For Dilma, these results are -0.17 and -0.47 (p-value of 0.01). For Lula, the shorter wait group the estimate of assignment to treatment on Lula’s evaluation is -0.429 while it is -0.515 for the longer waiting group (p-value of 0.553) — the difference between the point estimates for evaluation of the other leaders is of the same direction, though estimates are noisy. Consistent with that, we find that expectations about the future (p-value of 0.022) and happiness (p-value of 0.064) and life satisfaction (p-value of 0.040) decline with wait-time, which suggest an overall dismay associated with delays in actually moving to their new homes.³¹ A final point to note is that we do see an unexpected change in the market index for participants in the long-wait group showing that those who having been waiting for a longer time for fulfillment of the benefit move in the “pro-market” direction on this index, something that did not happen with the average effects reported earlier. We do not want to make too much of this isolated result, but it could indicate that bad interactions with government turn respondents more towards pro-market opinions.

These exploratory analyses underscore two dimensions that drive the surprising effects we found. The decision to move appears to be strongly influenced by distance from home to MCMV unit, the relative quality of the MCMV neighborhood (or the “what mechanism”) and the process of getting to the MCMV units (or the “how mechanism”). However, the

³⁰This amounts to splitting treated individuals into a group composed of those in the first three quintiles of wait-time and another in the higher two quintiles.

³¹In the appendix, we report more detailed results for the moderation of wait time on expectations about the future (Figure A.7).

Table 7: Moderation by Wait-time (ITT Estimates, Selected Outcomes)

Outcome	Shorter Waits			Longer Waits			Diff. p-val	N
	Effect	S.E.	p-val	Effect	S.E.	p-val		
Lula Eval.	-0.429	0.165	0.010	-0.515	0.168	0.002	0.553	980
Dilma Eval.	-0.178	0.161	0.269	-0.409	0.161	0.011	0.086	978
Paes Eval.	-0.179	0.161	0.266	-0.175	0.177	0.322	0.978	974
Pezão Eval.	0.057	0.122	0.638	-0.094	0.134	0.481	0.167	977
Temer Eval.	0.090	0.131	0.492	-0.005	0.142	0.974	0.391	957
Fut. Expect. (All)	-0.075	0.184	0.683	-0.463	0.206	0.025	0.022	961
Happiness	0.018	0.119	0.883	-0.194	0.143	0.174	0.064	1007
Life Satisfaction	-0.377	0.296	0.203	-0.714	0.348	0.040	0.170	1035
Market Beliefs	-0.110	0.124	0.376	0.326	0.135	0.016	0.000	1008
MCMV Positive	-0.224	0.127	0.077	-0.201	0.162	0.213	0.841	991
Dream	-0.198	0.084	0.018	-0.182	0.090	0.045	0.836	1039
Would Move	-0.348	0.101	0.001	-0.195	0.099	0.050	0.065	1036

Table reports the effects of being selected into the program computed by comparing outcomes for two subgroups of treatment individuals defined based on the wait-time since selection with average outcomes in the control group. Sample size varies due to missing responses. Standard errors were bootstrapped. “Diff. p-val” refers to the p-value of an F-test of the difference in the effects across groups.

process of getting to the MCMV units is more closely associated with individuals’ growing frustration with incumbents and overall dismay about life and expectations. Although related, these dimensions do not match perfectly, the decision to move or not seems to happen quickly and the process of receiving the unit is associated with changes attitudes over time. In summary, these results are suggestive — although far from conclusive — that “what” a policy provides and “how” a government delivers these goods influence voters through different paths.

These two mechanisms, however, are not extensive. Previous literature emphatically underscored that effects of homeownership take place because of the changes in wealth, financial stakes, or social status. Our analyses so far do not directly examine these alternative mechanisms which could also partially account for changes in attitudes about incumbents and views on MCMV and homeownership in general.

6.3 The “pocketbook” mechanism

Are these negative effects of selection into the program consistent with the program’s welfare effects? Do beneficiaries-to-be penalize incumbents associated with the program and have a more negative view of homeownership and MCMV in general because the program, in fact, does not make beneficiaries better off? Participants in the lotteries we selected for

the present study had not yet moved into their homes, so presumably had experienced no changes in their actual welfare. The research design we employed, therefore, cannot really answer this question.³² However, beneficiaries-to-be may have expectations that are not fully captured in our survey based on what they observe and learn from others who have already become beneficiaries. Thus, we turn to a different set of beneficiaries that have already moved to their new housing units to (partially) assess the program's impact on voters' welfare.

For this task, we compiled the lists of individuals who enrolled and were selected in the first general MCMV lottery in Rio de Janeiro (*Editais 03-2011*), which was conducted in June, 2011. This was a particularly large lottery, in which 2,983 individuals were selected from a pool of almost 300 thousand enrolled. The housing units were delivered to the qualified winners in 2011 and 2012, depending on the housing project of the unit. We also compiled the names of all individuals who were effectively awarded contracts for Tier 1 MCMV units, which is a subset of lottery winners.³³

We were then able to assess the program effects on (log of) formal employment and wages from formal employment in the years following the lottery and delivery of the housing unit (i.e. 2012, 2013, and 2014). Data on these outcome variables were obtained from a government database of all formal employees in the country (*Relação Anual de Informações Sociais*, known as RAIS) covering 2003–2014. We computed both ITT effects and also effects on compliers for formal employment and wages in each year. As before, the ITT was defined by the comparison between lottery winners and non-winners. In contrast, we operationalized compliance with the MCMC program in a different way than we had previously done. Whereas in the survey compliance was defined as having learned about the result of the lottery, in the analysis of administrative data we define compliance as having been awarded a MCMV Tier I contract.

Table 8 reports results for formal employment, which is simply a dichotomous indicator for whether individuals were found in the RAIS. The conclusion is that MCMV beneficiaries, if anything, are *less* likely to be formal workers than non-beneficiaries. In each of the post-treatment years the difference ranges from 1.2 to 1.6 percentage points. When we focus

³²We expect to reinterview our subjects in 2019 to assess this question directly.

³³These data were obtained through separate queries to the Urban Affairs Ministers and to *Caixa Econômica Federal*, and reconciled by us.

only on compliers, the effects are about four times larger. Although none of the effects are statistically significant, they are quite stable and always in the direction of a “negative” welfare change for beneficiaries.

Table 8: Effects on Formal Employment for Participants in First Lottery

Var	Ind	ITT			CACE		
		2012	2013	2014	2012	2013	2014
Const.	Estimate	0.577	0.569	0.560	0.579	0.570	0.561
	Rob. SE	0.001	0.001	0.001	0.001	0.001	0.001
	p-value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Winner	Estimate	-0.016	-0.012	-0.013	-0.062	-0.047	-0.051
	Rob. SE	0.009	0.009	0.009	0.035	0.035	0.036
	p-value	0.082	0.186	0.155	0.081	0.185	0.154

The table reports the effects of being selected into the program on the share of individuals with formal employment. ITT estimates correspond to the intention to treat estimates, and CACE are effects on compliers, defined as those that received a contract. Robust standard error.

Table 9: Effects on (log) of Formal Wages for Participants in First Lottery

		ITT			CACE		
		2012	2013	2014	2012	2013	2014
Const	Estimate	6.93	7.06	7.17	6.94	7.06	7.17
	Rob. SE	0.00	0.00	0.00	0.00	0.00	0.00
	p-value	<0.01	<0.01	<0.01	<0.01	<0.01	<0.01
Winner	Estimate	-0.06	-0.04	-0.06	-0.25	-0.17	-0.24
	Rob. SE	0.03	0.03	0.03	0.11	0.11	0.12
	p-value	0.05	0.17	0.08	0.02	0.14	0.05

The table reports the effects of being selected into the program on wages from formal employment. ITT estimates correspond to the intention to treat estimates, and CACE are effects on compliers, defined as those that received a contract. Robust standard error.

Results for (log of) formal wages, reported in Table 9, are similarly stable and negative. Beneficiaries seem to make 6% less than non-beneficiaries from formal employment (relative to a baseline of about R\$1,500), and the effect is about four times larger for compliers.

It might very well be that beneficiaries *choose* informality as a way to avoid future complications with the program administration. It might also be that the MCMV effects are, in fact, null with respect to formal employment and wages. Or it might as well be that voters are better off in other dimensions of welfare we are not measuring here, such as access to health services, investments in children’s education, access to credit, etc. Either way, at the very least, these data support the idea that there were no positive welfare effects from

the program. This lack of positive welfare effects are compatible with the negative political and electoral effects we encountered in the survey study, particularly if one considers that beneficiaries probably had some expectation of welfare improvements.

7 Discussion and Concluding Remarks

Our theory predicted that being selected to become a *de facto* homeowner would make beneficiaries-to-be view incumbents associated to the program in a more positive light than other politicians. Beneficiaries expectations about the future and levels of life satisfaction should also have improved, as well as their evaluations of the MCMV program. In contrast, we predicted no effects of selection into the program on what we call broader social and political attitudes.

Our results matched our predictions for the second part of our story and provided surprising results for the first part. Individuals who won the lottery were *less* likely to hold favorable views of politicians who are associated with the program: lottery winners consistently had more negative evaluations of former presidents Dilma and Lula. Beneficiaries also reported lower life satisfaction than non-beneficiaries and expectations of selected individuals, on average, were unchanged. What was even more striking was that individuals selected into the program were less likely to think of homeownership “as a dream,” less likely to evaluate the MCMV positively, and much less likely to say that they would move into a MCMV home if they were awarded one.

Although these results are counter to our expectations, they are logically consistent with each other, and suggest that the program did, in fact, have negative effects on political preferences and assessment of the program. Our qualitative follow-up study and examination of moderating variables suggest that the negative effects are driven by a combination of uniformly high expectations among the control group and heterogeneous effects on treated individuals. Even if many treated individuals are happy with their MCMV unit, some experience long waits, some discover they are not really eligible, while others are offered units in locations that are less appealing than where they live. The wait seems to be particularly consequential for expectations about the future, electoral preferences, and even pro-market (anti-state) attitudes, whereas location seems to affect only the decision to accept or not a unit. We found no evidence that costs of living or construction quality drive any effects.

These are, it should be stressed, effects of being selected into the program. It is possible that the mostly negative and null results we found become positive once selected individuals actually move and settle into their new lives. However, the preliminary analysis of administrative data from previous lotteries suggests that perhaps beneficiaries are correct in their bleak assessments of the consequences of the program, at least based on the formal employment outcomes we report.

There is a silver lining to these negative and null effects. Previous studies have shown that in many different contexts, delivering small but reliable benefits allows government to reap electoral and political rewards. What our results show that mere promises of goods, even if as highly symbolic and valuable as homeownership, have to be backed up by actual delivery. Unfulfilled promises, if our results are to be believed, can backfire.

That said, the results should be read in context. Lula and Dilma's approval ratings are relatively high among this population and the MCMV program, in general, is also very well-perceived by the overall public and potential beneficiaries alike, despite the negative treatment effects. Given that there are relatively fewer beneficiaries compared to policy applicants, it might very well be that the program still produces overall positive political effects despite negative effects. That is, it is possible that support for Lula and Dilma is higher than it would be if the MCMV did not exist. But assessing this conjecture would require a different research design.

Future analysis should investigate the different alternative explanations about the pathways in which policies influence voters' attitudes in general. In response to many of the surprising findings, we investigated dimensions related to the quality of the good and policy implementation that go beyond the usual suspects of generating political effects of public policies. These dimensions are closer to politicians' control than most policies' actual welfare effects and we have potentially much to gain in understanding about the politics of policies if we turn our attention to these issues.

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Do Governments Make Dreams Como True?

--Appendix--

A Descriptive Statistics

Table A.10 presents the complete list of lotteries of the MCMV program held in Rio de Janeiro up to the time of writing. The first lottery (03.2011) is used in the analysis of the effect of the program on formal employment and wages. Lotteries 17.2016, 20.2016, 05.2017, 06.2017 and 07.2017 were used as the population from which the survey sample was selected. One of our survey participants, however, was selected in lottery 09.2017, which was held while we were in the field, and was considered part of the treatment group.

Table A.11 reports the simple descriptive statistics for all variables used in our analyzes. Age, wages, and formal employment were obtained from administrative data. All other variables were collected through our survey of beneficiaries and non beneficiaries.

Table A.10: MCMV Lotteries in the City of Rio de Janeiro

	Winners	Participants	Prob. Winning
lottery03.2011†	2,983	298,218	0.0100
lottery06.2011	6,462	325,294	0.0199
lottery09.2011	14,027	351,406	0.0399
lottery12.2012	392	414,188	0.0009
lottery03.2013	550	472,468	0.0012
lottery06.2013	2,383	491,630	0.0048
lottery01.2014	472,468	384	0.00081
lottery01.2015	800	669,468	0.0012
lottery04.2015	1,840	669,979	0.0027
lottery07.2015	239	552,996	0.0004
lottery18.2015	1,218	556,410	0.0022
lottery19.2015	870	555,145	0.0016
lottery26.2015	1,374	562,157	0.0024
lottery27.2015	354	562,157	0.0006
lottery28.2015	707	562,157	0.0013
lottery29.2015	423	28,767	0.0147
lottery30.2015	329	13,567	0.0242
lottery31.2015	74	10,267	0.0072
lottery34.2015	494	569,169	0.0009
lottery36.2015	114	10,445	0.0109
lottery37.2015	58	134,638	0.00043
lottery03.2016	423	50,611	0.008
lottery04.2016	68	67,865	0.001
lottery05.2016	238	574,488	0.0004
lottery06.2016	828	574,914	0.0014
lottery07.2016	354	129,888	0.027
lottery10.2016	1,057	580,829	0.0018
lottery13.2016	461	51,560	0.0089
lottery14.2016	126	41,599	0.003
lottery17.2016‡	2,197	583,282	0.0038
lottery18.2016	187	51,863	0.0036
lottery19.2016	25	41,870	0.00059
lottery20.2016‡	587	484,763	0.0012
lottery05.2017‡	395	52,274	0.0075
lottery06.2017‡	875	580,584	0.0015
lottery07.2017‡	338	580,147	0.0005
lottery08.2017	573	53,612	0.0106
lottery09.2017	532	43,124	0.0123
lottery10.2018	287	53,785	0.0053
lottery11.2018	341	43,318	0.0078
lottery12.2018	147	43,235	0.0033
lottery13.2018	53	43,309	0.0012
lottery14.2018	13	43,780	0.00029
lottery15.2018	78	54,556	0.00142
lottery16.2018	39	54,676	0.00071

Notes: †Lotteries selected for the administrative data analysis. ‡Lotteries selected for the survey.

Table A.11: Sample Descriptive Statistics

Variable	N	Min.	Mean	Median	Std. Dev.	Max	Missing
Lula Eval.	980	-2.0	0.3	0.5	1.2	2.0	61
Dilma Eval.	978	-2.0	-0.2	0.0	1.2	2.0	63
Paes Eval.	974	-2.0	-0.3	0.0	1.2	2.0	67
Pezão Eval	977	-2.0	-1.3	-2.0	0.9	2.0	64
Temer Eval.	957	-2.0	-1.2	-1.0	0.9	2.0	84
Inc. Vote 2014	641	0.0	0.6	1.0	0.5	1.0	400
Inc. Vote 2016	617	0.0	0.0	0.0	0.2	1.0	424
Some Party ID	1002	0.0	0.1	0.0	0.3	1.0	39
PT ID	1041	0.0	0.0	0.0	0.2	1.0	0
Future Expectations	505	-4.1	0.0	0.5	1.0	0.7	536
Fut. Expect. (All)	961	-2.0	1.0	2.0	1.4	2.0	80
Happiness	1007	-2.9	-0.0	0.2	1.0	1.4	34
Market Beliefs	1008	0.0	1.7	2.0	0.9	4.0	33
Tax-and-transfers	999	-4.8	-0.0	0.1	1.0	0.9	42
Self-reliance	1013	0.0	1.3	1.0	0.7	2.0	28
Redistribution	945	1.0	10.0	11.0	1.9	12.0	96
Placement	1033	0.0	4.0	4.0	1.9	10.0	8
Attribute to Lula/Dilma	892	0.0	0.6	1.0	0.5	1.0	149
MCMV positive	991	-2.0	1.4	2.0	0.9	2.0	50
Dream	1039	-2.0	1.7	2.0	0.7	2.0	2
Would Move	1036	-1.0	0.6	1.0	0.7	1.0	5
Antiparty ID	975	0.0	0.2	0.0	0.4	1.0	66
Life Satisfaction	1035	0.0	6.9	7.0	2.2	10.0	6
Positive Affect	1018	0.0	0.8	1.0	0.4	1.0	23
Blue Affect	1028	0.0	0.5	0.5	0.4	1.0	13
Religion (pentecostal)	1030	0.0	0.4	0.0	0.5	1.0	11
Sex (male)	1041	0.0	0.3	0.0	0.5	1.0	0
Age (old)	1041	0.0	0.5	1.0	0.5	1.0	0
Race (white)	1032	0.0	0.2	0.0	0.4	1.0	9
Schooling (high)	1037	0.0	0.6	1.0	0.5	1.0	4
Children (number)	1041	0.0	0.9	1.0	1.2	11.0	0
Wages from Formal Employment							
Wages 2003	1041	0.0	258.9	0.0	1194.8	31107.4	0
Wages 2004	1041	0.0	224.8	0.0	378.1	2617.5	0
Wages 2005	1041	0.0	258.9	0.0	522.2	10355.6	0
Wages 2006	1041	0.0	277.0	0.0	462.5	5725.7	0
Wages 2007	1041	0.0	320.8	0.0	543.7	5929.3	0
Wages 2008	1041	0.0	378.4	0.0	672.7	12538.1	0
Wages 2009	1041	0.0	441.2	0.0	860.0	16169.8	0
Wages 2010	1041	0.0	496.4	0.0	714.5	7376.2	0
Formal Employment							
Formal 2003	1041	0.0	0.4	0.0	0.5	1.0	0
Formal 2004	1041	0.0	0.4	0.0	0.5	1.0	0
Formal 2005	1041	0.0	0.4	0.0	0.5	1.0	0
Formal 2006	1041	0.0	0.4	0.0	0.5	1.0	0
Formal 2007	1041	0.0	0.4	0.0	0.5	1.0	0
Formal 2008	1041	0.0	0.5	0.0	0.5	1.0	0
Formal 2009	1041	0.0	0.5	0.0	0.5	1.0	0
Formal 2010	1041	0.0	0.5	1.0	0.5	1.0	0

This table compares among all sampled individuals, those we were able to interview and those we were unable to interview (for any reason, including incorrect contact information, declined to participate, etc.). We use administrative data on levels of formal employment and wages from formal employment and we code sex based on their names using the R package *genderBR*.

Table A.12: Attrition: Balance on Pre-Treatment Characteristics

	Not interviewed	Interviewed
Average wages in 2003	202.74	258.89
Average wages in 2004	233.43	224.83
Average wages in 2005	257.04	258.88
Average wages in 2006	292.67	277.01
Average wages in 2007	337.23	320.79
Average wages in 2008	395.62	378.37
Average wages in 2009	257.04	258.88
Average wages in 2010	520.99	496.39
Formal employment in 2003	0.35	0.37
Formal employment in 2004	0.37	0.37
Formal employment in 2005	0.38	0.38
Formal employment in 2006	0.41	0.40
Formal employment in 2007	0.41	0.40
Formal employment in 2008	0.47	0.46
Formal employment in 2009	0.49	0.46
Formal employment in 2010	0.52	0.50
Sex (male)	0.38	0.32

Table shows average values of pre-treatment formal employment characteristics for interviewed and not interviewed subsamples.

B Robustness Checks

We are using four specifications of patterns of participation and a specification that does not include any indicator for participation in lotteries. We created variables that assigned “0” if the subject did not participate in a given lottery and “1” if the subject participated in a given lottery, for all lotteries. For the analyses in the paper, we include indicators for patterns of participation in the five lotteries included in the survey (survey lotteries FE). The list below presents our alternative patterns and the following Figures show how results vary according to these different choices.

1. All general lotteries: pattern built on indicators for each general (city-wide) lottery in which each individual participated.
2. All lotteries FE: pattern built on indicators for each lottery (city-wide and neighborhood) in which each individual participated.

3. Survey lotteries FE: pattern built on indicators for each lottery included in the survey in which each individual participated.
4. Survey lotteries FE (reduced): pattern built on indicators for each lottery included in the survey in which each individual participated, excluding those after an individual after an individual won the lottery.
5. No FE: model without any adjustment for participation in lotteries

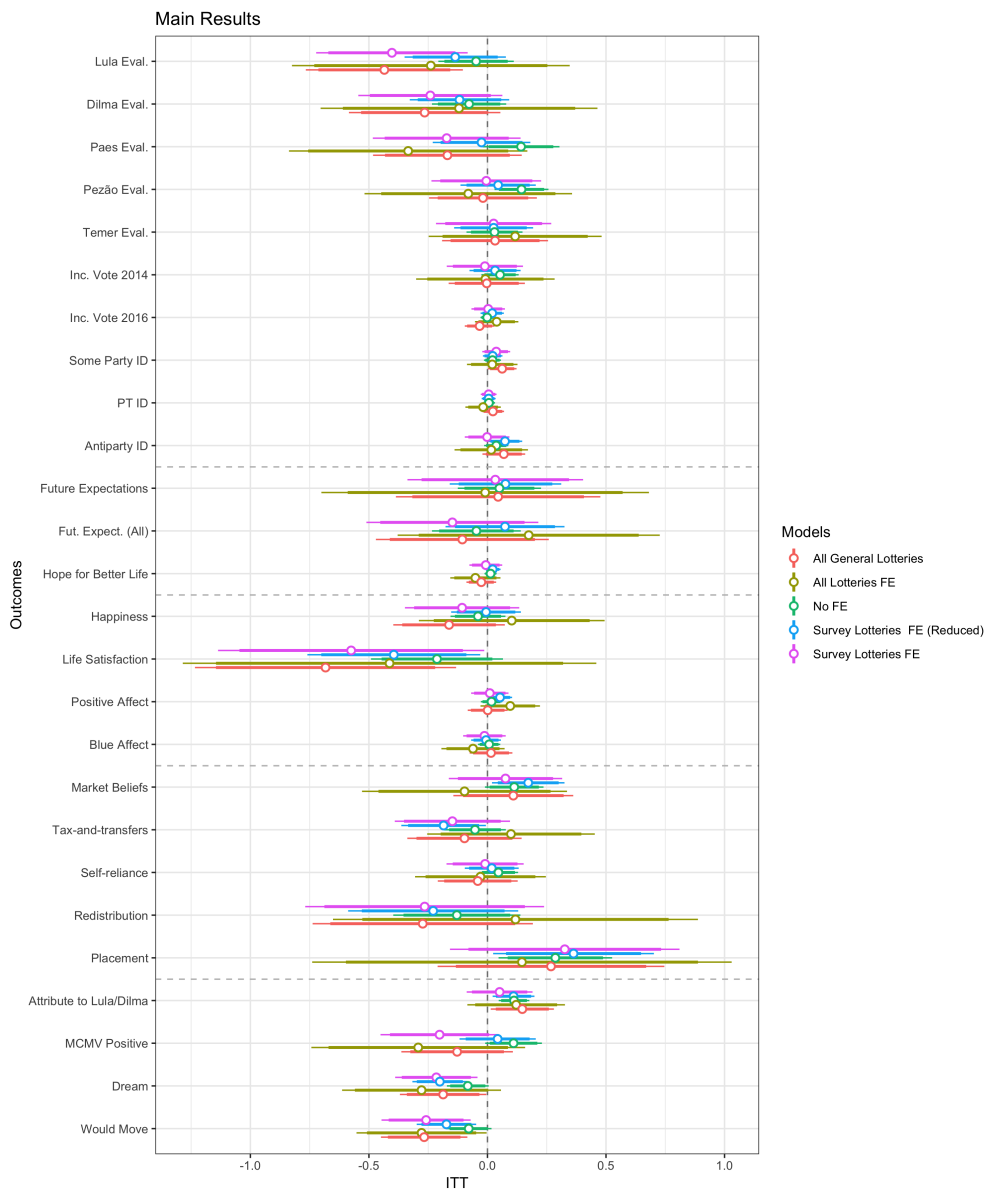


Figure A.2: Main ITT effects using different specifications

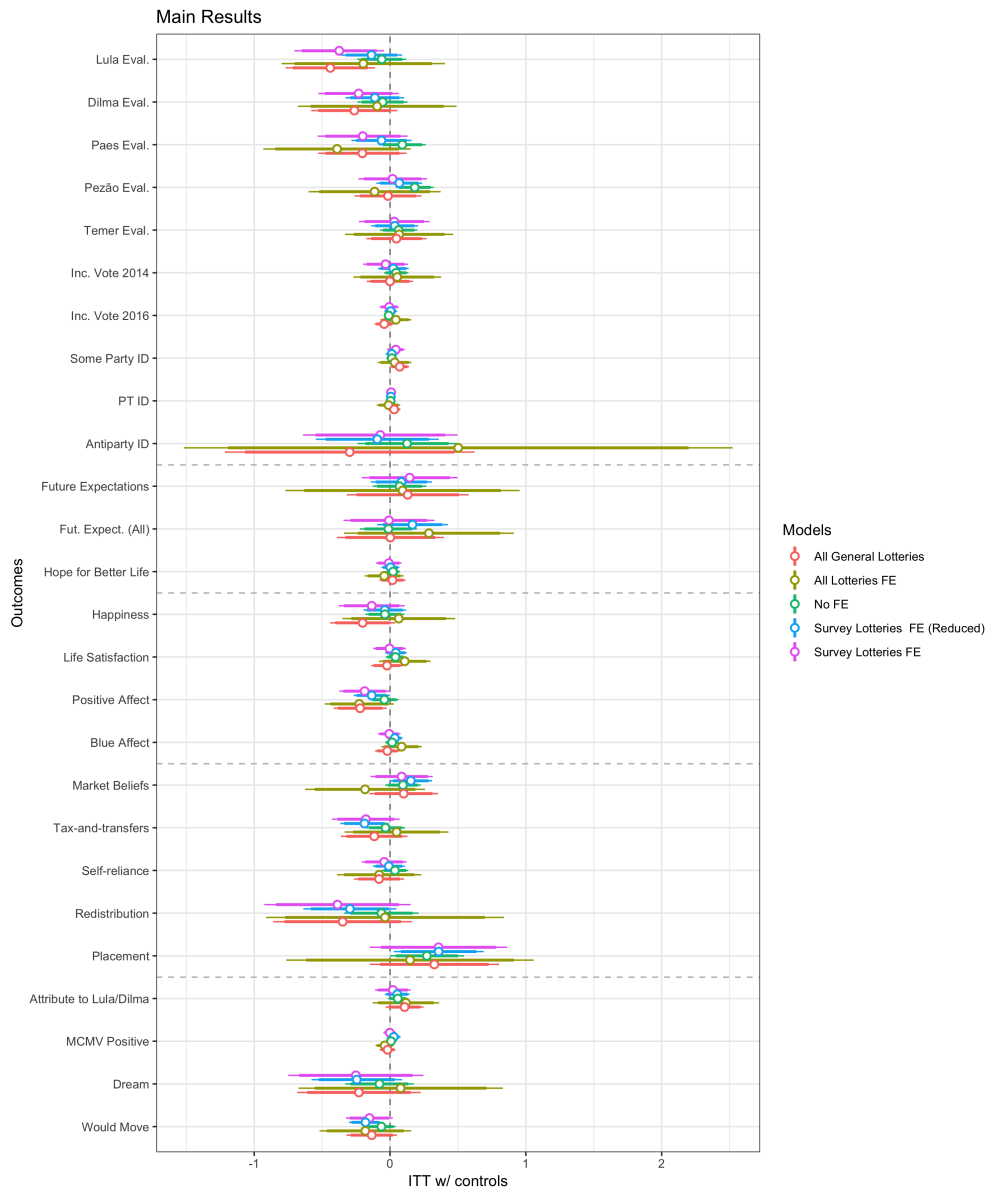


Figure A.3: Main ITT effects with controls using different specifications

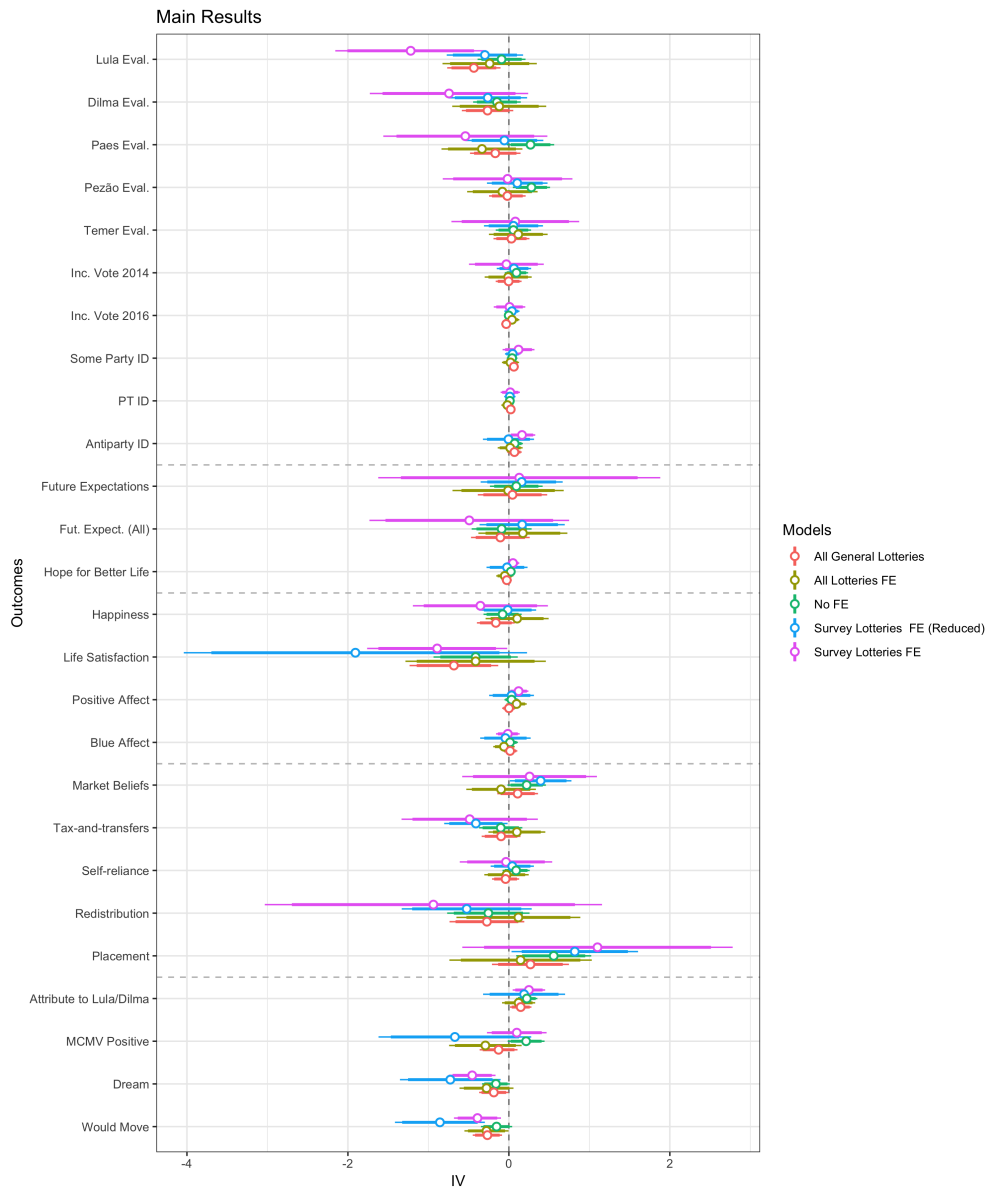


Figure A.4: Main IV effects using different specifications

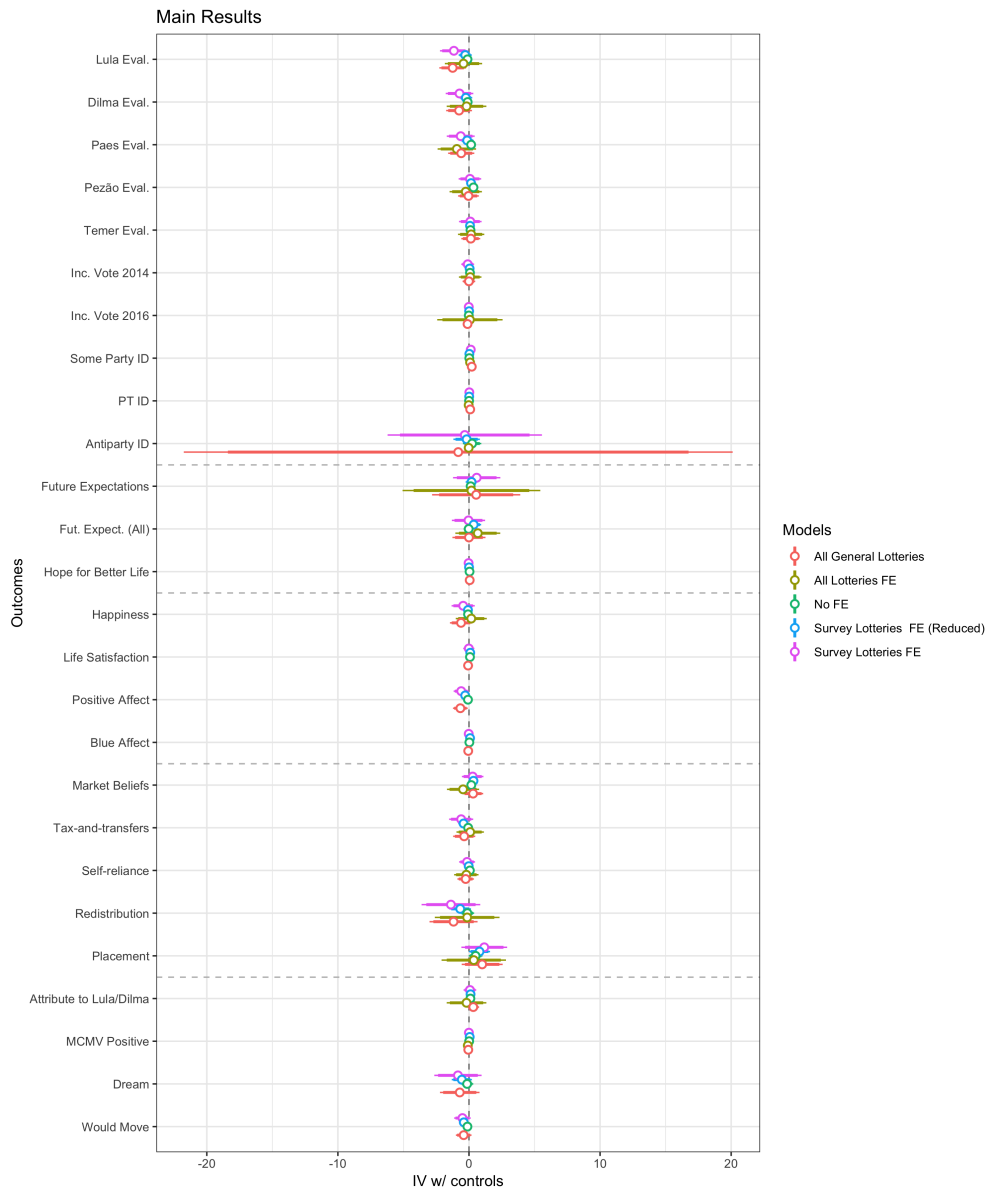


Figure A.5: Main IV effects with controls using different specifications

Table A.13 reports estimates for the same planned and unplanned outcomes that were reported in Table 4, in the main body of the paper, but estimated with the inclusion of control variables. The control variables that were included were whether individuals had formal employment and wages from formal employment in pre-treatment years 2003–2010, religion, sex, age, race, and years of schooling.

Table A.13: Effects of MCMV on Main Outcomes (with Controls)

Outcome	ITT Estimates			CACE Estimates			N
	Effect	S.E.	p-val	Effect	S.E.	p-val	
Lula Eval.	-0.374	0.172	0.030	-1.151	0.511	0.024	965
Dilma Eval.	-0.231	0.155	0.135	-0.723	0.518	0.163	962
Temer Eval.	-0.201	0.166	0.226	-0.631	0.536	0.239	957
Paes Eval.	0.019	0.125	0.878	0.071	0.433	0.870	962
Pezão Eval.	0.031	0.127	0.810	0.100	0.422	0.812	941
Inc. Vote 2014	-0.033	0.085	0.701	-0.096	0.259	0.712	635
Inc. Vote 2016	-0.006	0.037	0.870	-0.015	0.099	0.883	609
Some Party ID	0.042	0.033	0.205	0.135	0.107	0.210	984
PT ID	0.008	0.018	0.670	0.027	0.064	0.672	1019
Antiparty ID	0.005	0.050	0.927	0.009	0.167	0.955	959
Future Expectations	0.144	0.186	0.440	0.588	0.924	0.524	495
Fut. Expect. (All)	-0.008	0.200	0.970	-0.038	0.656	0.954	944
Happiness	-0.135	0.130	0.298	-0.444	0.468	0.342	989
Life Satisfaction	-0.623	0.297	0.036	-2.063	1.121	0.066	1015
Positive Affect	-0.006	0.044	0.891	-0.020	0.159	0.899	998
Blue Affect	-0.009	0.048	0.858	-0.030	0.171	0.862	1007
Market Beliefs	0.085	0.113	0.450	0.280	0.414	0.499	987
Tax-and-transfers	-0.178	0.133	0.182	-0.597	0.461	0.195	982
Self-reliance	-0.043	0.089	0.629	-0.145	0.296	0.623	993
Redistribution	-0.387	0.272	0.155	-1.378	1.108	0.213	929
Placement	0.357	0.249	0.151	1.165	0.914	0.203	1012
Attribute to Lula/Dilma	0.030	0.068	0.656	0.105	0.265	0.692	876
MCMV Positive	-0.099	0.139	0.475	-0.314	0.465	0.500	969
Dream	-0.152	0.091	0.096	-0.499	0.313	0.111	1017
Would Move	-0.187	0.095	0.048	-0.594	0.301	0.048	1014

Table reports the effects of being selected into the program on several outcomes that operationalize the hypotheses presented earlier. The first set of estimates correspond to the intention to treat estimates, as defined in Eq. 1, and the second set are treatment effects on compliers, as defined in Eq. 2. Standard errors were computed through non-parametric bootstrapping.

We planned on interviewing individuals who participated in city-wide lotteries (and exclude neighborhood lotteries as well as lotteries for the elderly and those with special needs). However, we decided to include one neighborhood lottery because of the difficulty in contacting lottery winners and losers (*edital 05.2017*). We interviewed 60 individuals who were in this lottery (note that they were also in the city-wide lotteries). We conducted our main analyses after excluding these individuals and results are largely unchanged. As before, we see negative effects for the evaluation of all incumbents except Temer, with significant effects for Lula and borderline significant effects for Dilma. We also see the significant fall in life satisfaction, and increase in attribution to Lula and Dilma, and negative changes in evaluation of MCMV, on believing that homeownership is a dream, and in the intention to move to the new home.

Table A.14: Effects of MCMV on Main Outcomes (sample without neighborhood lotteries)

Outcome	ITT Estimates			CACE Estimates			N
	Effect	S.E.	p-val	Effect	S.E.	p-val	
Lula Eval.	-0.379	0.166	0.023	-1.054	0.465	0.023	881
Dilma Eval.	-0.255	0.169	0.132	-0.723	0.490	0.140	880
Paes Eval.	-0.204	0.170	0.229	-0.592	0.504	0.240	873
Pezão Eval.	-0.042	0.126	0.738	-0.126	0.389	0.745	878
Temer Eval.	-0.001	0.135	0.995	-0.002	0.406	0.995	859
Inc. Vote 2014	-0.051	0.083	0.539	-0.137	0.253	0.587	583
Inc. Vote 2016	0.009	0.038	0.810	0.022	0.102	0.825	555
Some Party ID	0.028	0.031	0.370	0.083	0.092	0.366	902
PT ID	0.007	0.019	0.708	0.022	0.061	0.720	938
Antiparty ID	0.006	0.053	0.904	0.019	0.165	0.908	877
Future Expectations	0.020	0.217	0.926	0.077	0.951	0.936	448
Fut. Expect. (All)	-0.138	0.191	0.468	-0.425	0.591	0.472	867
Hope for Better Life	-0.009	0.040	0.823	-0.028	0.131	0.830	931
Happiness	-0.083	0.131	0.529	-0.252	0.421	0.549	904
Life Satisfaction	-0.468	0.288	0.104	-1.437	0.978	0.142	932
Positive Affect	0.010	0.042	0.807	0.032	0.135	0.813	915
Blue Affect	-0.019	0.048	0.687	-0.060	0.156	0.699	925
Market Beliefs	0.075	0.130	0.562	0.235	0.428	0.583	909
Tax-and-transfers	-0.185	0.127	0.144	-0.565	0.435	0.194	901
Self-reliance	-0.038	0.089	0.671	-0.121	0.292	0.680	912
Redistribution	-0.324	0.288	0.261	-1.058	1.057	0.317	849
Placement	0.253	0.261	0.332	0.794	0.849	0.350	931
Attribute to Lula/Dilma	0.097	0.068	0.154	0.342	0.278	0.220	802
MCMV Positive	-0.212	0.141	0.132	-0.652	0.479	0.173	891
Dream	-0.208	0.094	0.027	-0.652	0.329	0.047	936
Would Move	-0.226	0.094	0.017	-0.692	0.263	0.008	933

Table reports the effects of being selected into the program on several outcomes that operationalize the hypotheses presented earlier. The first set of estimates correspond to the intention to treat estimates, as defined in Eq. 1, and the second set are treatment effects on compliers, as defined in Eq. 2. Standard errors were computed through non-parametric bootstrapping.

C Heterogeneous Effects

Figure A.6 reports visually a selection of the results that are reported in Table 7. It shows that treatment effects are stronger in almost all outcomes for those in the longer wait group than in the shorter wait group. Both groups are compared against the average of the non-treated individuals. These are estimated “in the spirit” of complier average causal effects, but are not strictly speaking causal effects, as wait time was not randomized and was realized after the treatment. See paper for discussion of these issues.

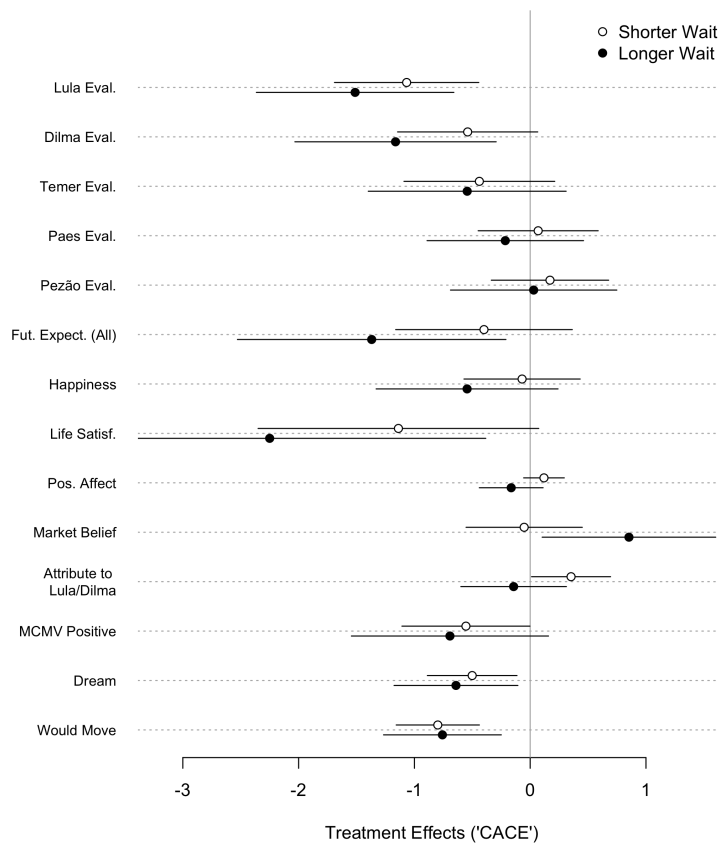


Figure reports the effects of being selected into the program on selected planned and unplanned outcomes that operationalize the hypotheses presented earlier, for two subgroups of treated compliers (as defined by Eq. 2). The complete set of estimates are reported in Table 7.

Figure A.6: Heterogeneous Treatment Effects on Compliers (Selected Outcomes)

In the figures that follow we report more detailed moderation effects for selected outcomes and moderators that were discussed in Section 6. Figure A.7 show show the probability of reporting a positive expectation about the future decreases with the wait-time. Wait-time, however, is not measured for the control group, so these results are compared with the average of the control group that is represented by the dashed horizontal line. For very short wait times, the treatment effect (i.e. difference between the blue line and the horizontal dashed line) is actually positive or null, but it becomes clearly negative as the wait time increases.

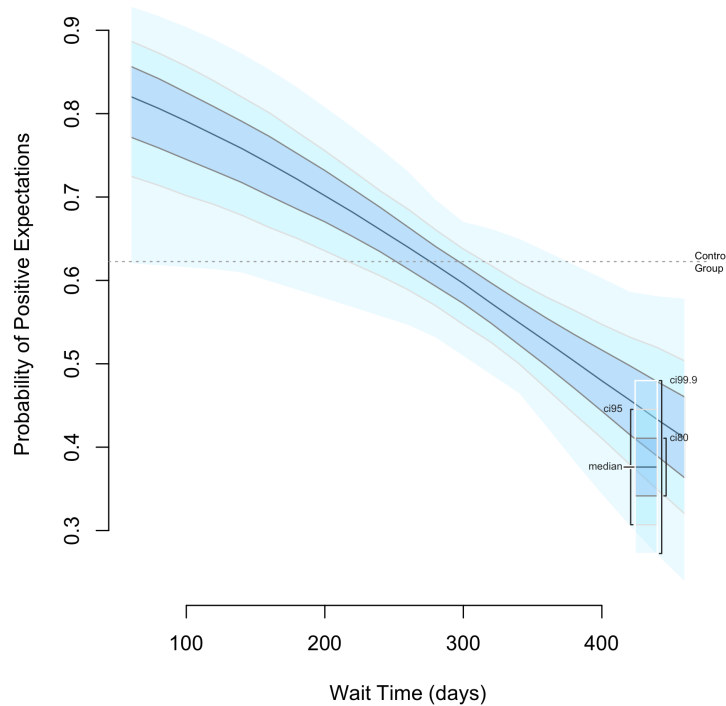


Figure reports probability of having positive expectations about the future by wait time for the treatment group. The control group, by definition, does not have a wait time.

Figure A.7: Expectations about the future by wait time and treatment condition

Figures A.8 and A.9 report results for the probability of saying that one would/will move to a MCMV unit as satisfaction with neighborhood and satisfaction with home vary. Differently than in the previous graph, here the moderating variable is measured for both control and treatment groups. For the treatment group, the probability of wanting to move declines both with satisfaction with neighborhood and with home. For the control group, the probability of wanting to move also declines with home satisfaction, but it increases with neighborhood satisfaction. To us, this suggests that those in the control who are more satisfied probably are probably thinking of “better” potential destinations than those who are less satisfied. Upon being offered a MCMV unit, however, the assessment is then made relative to a concrete neighborhood that is often worse than the one they currently live in. Evaluation of this hypothesis, however, would require analysis of objective indicators of neighborhood quality, and we leave it for future research.

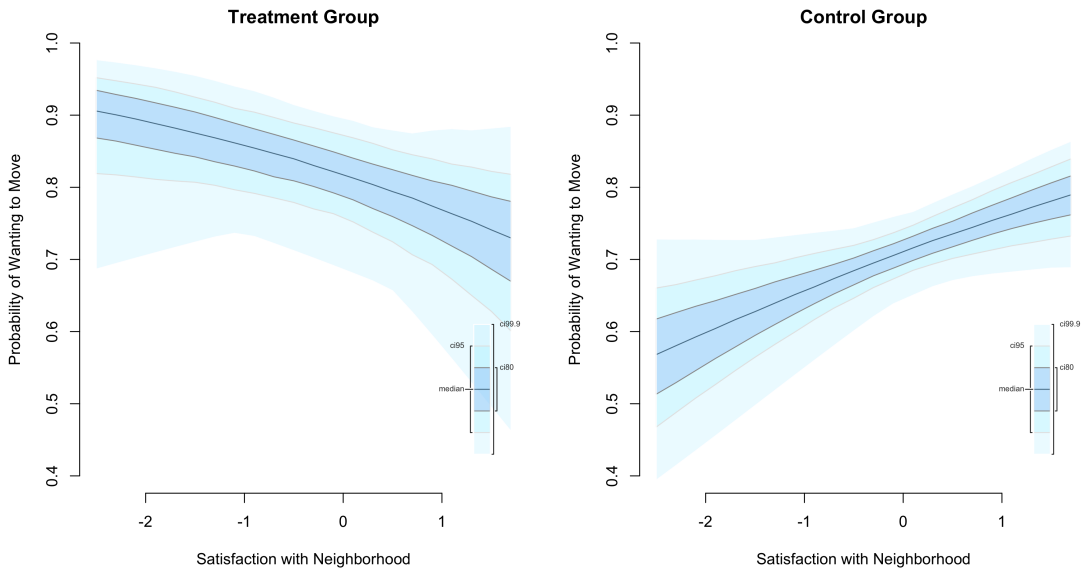


Figure reports probability of wanting to move by level of satisfaction with neighborhood and treatment condition.

Figure A.8: Wanting to move by neighborhood satisfaction and treatment condition

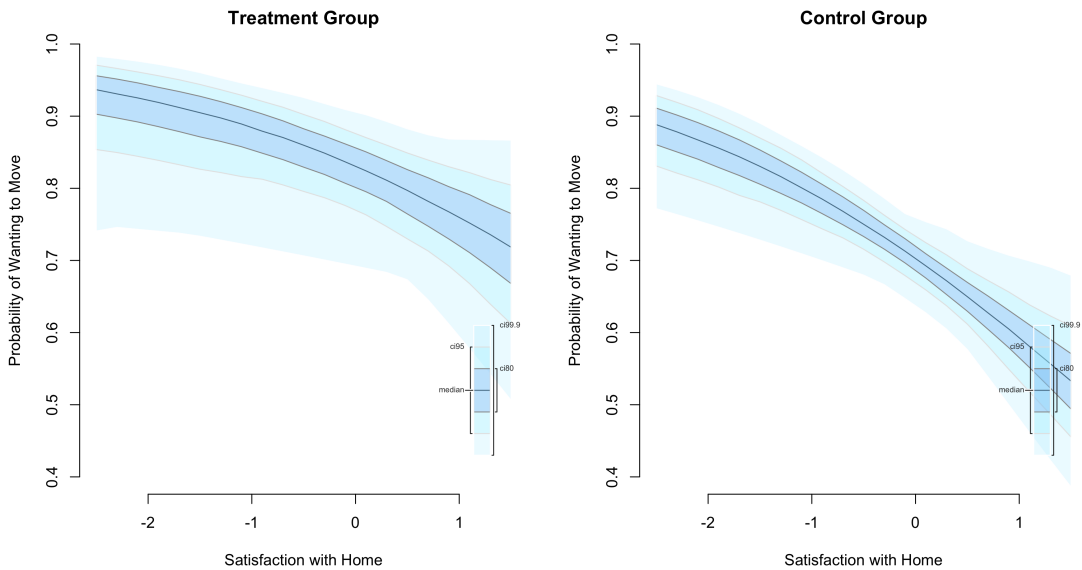


Figure reports probability of wanting to move by level of satisfaction with home and treatment condition.

Figure A.9: Wanting to move by home satisfaction and treatment condition