

# Poverty Alleviation by a Large-Scale Homeownership Program

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## Abstract

This paper evaluates whether homeownership alleviates poverty by exploiting a large-scale affordable housing program in Brazil. Linking applicants to administrative data on formal employment, we investigate the impact homeownership has on labor supply, earnings, mobility, occupation, and formalization. Becoming a homeowner increases hours worked and formal employment, suggesting wealth effects in this setting do not, on net, dampen labor supply. However, wages and earnings of new homeowners fall. The likely mechanism is homeownership programs accelerate formalization. New homeowners transition to public sector employment, and, consistent with the homevoter hypothesis, increase political participation.

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# 1 Introduction

Policies expanding homeownership are widespread and constitute large government expenditures.<sup>1</sup> Their economic rationale, particularly in developed economies, is that homeowners generate external benefits.<sup>2</sup> Yet another rationale is the private benefits of homeownership alone might justify policy intervention. In development, pre-existing market imperfections<sup>3</sup> generate multiple equilibria that trap individuals in poverty. Even if homeownership provides clear benefits, individuals cannot independently attain them. In this second-best environment, policies that expand homeownership may be the large-scale intervention needed to permanently alleviate poverty.

This paper studies the impact of a large-scale homeownership program on individual labor market outcomes. We confront two major empirical challenges. Beneficiaries of homeownership programs differ from non-participants on many characteristics. They differ in levels of human capital, consumption expectations, and life-cycle considerations. These differences shape individual labor market outcomes. So observed differences between homeowners and non-homeowners cannot be attributed to housing. Moreover, where people work and where they live are decided jointly, complicating empirical analysis. The second challenge is the lack of comprehensive data on housing tenure and labor market outcomes. The ideal setting to evaluate the impact of homeownership is via randomized experiment. One group of individuals is treated with owning a home while those in another group are not. Randomization ensures they are similar in all other aspects. Detailed data would track their subsequent labor market outcomes.

This paper approximates the ideal experiment by evaluating a large-scale program in Brazil that gave individuals a newly built home. We collect new data on applicants to the lottery-based program and link them to ultimate beneficiaries. Using matched employee-employer data, we closely track the labor market trajectories of beneficiaries to non-beneficiaries. Our rich, administrative data offer underlying mechanisms for how homeownership affects labor market outcomes.

Estimates from a differences-in-differences design show that becoming a homeowner affects labor market outcomes. We first study transitions to formal labor market participation. Individuals that earned a house from the program increased formal labor market participation by 2 percentage points. Relative to a baseline participation rate of 34 percent, this is a 5.8 percent increase. For individuals already in formal employment, we document effects on hours worked and wages. New

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<sup>1</sup>For example, the U.S. federal government spent \$196 billion in tax incentives for homeowners in 2019 ([Tax Policy Center, 2020](#)). Similar policies are pervasive around the world ([Cerutti et al., 2017](#)).

<sup>2</sup>Homeowners may have greater incentives to maintain local amenities, engage in civic activities, and provide residential stability for others in the household ([DiPasquale and Glaeser, 1999](#); [Sodini et al., 2016](#)).

<sup>3</sup>e.g., financial frictions, increasing returns, and behavioral biases may lead individuals to forgo productive investments ([Kraay and McKenzie, 2014](#)).

homeowners worked 47.1 more hours after six years, corresponding to a 3.4 percent increase. This suggests, at least for formal employment, that wealth effects of a large asset windfall does not dominate and reduce labor supply on net. However, wages fall by 1.6 BRL per hour, corresponding to a 22 percent decline.

We evaluate mechanisms driving these labor market effects. Homeownership extends formalization beyond labor markets; consistent with the home voter hypothesis, individuals increase civic engagement after becoming a homeowner. Beneficiaries increase their likelihood of registering with a political party by 0.1 percentage point. Relative to a baseline of 1.8 percent participation, this represents a 5.6 percent increase. Moreover, we find that beneficiaries of the program are more likely to be appointed to a public sector job. Finally, we do not find evidence that new homeowners have lower labor mobility.

These findings are enabled by a large-scale affordable housing program called *Minha Casa Minha Vida* (MCMV). Starting in 2009, the Brazilian government partnered with municipal governments to award newly built homes to low-income individuals. Our setting offers three keys advantages for empirical analysis.

First, we exploit the program's lottery-based allocation and across-city variation to estimate causal effects. Whenever housing for the program became available, the local government collected applicants who satisfied an income limit. Applicants from disadvantaged groups gained priority, while all others entered a lottery. We compare applicants to beneficiaries exploiting this lottery-based design. Moreover, we use the timing of new housing units as they become available across different cities. This intuition is crystallized in a differences-in-differences design. Since each municipality undertook multiple treatment events, we adopt recent advances in differences-in-differences designs ([Goodman-Bacon, 2021](#); [De Chaisemartin and d'Haultfoeuille, 2020](#); [Callaway and Sant'Anna, 2021](#)). Our quasi-experimental design generates causal estimates of the impact of winning a home on subsequent labor market outcomes.

Second, the the scope and scale of the program are uniquely suited for our analysis. The program directly awarded a new house to almost a million households across Brazil. Beneficiaries had modest financial obligations to the house. This helps cleanly separate the effect of new homeownership. If, for example, residents had to marshal financing, it would confound subsequent labor market outcomes. In addition, the program affected municipalities across Brazil and ultimately awarded almost a million housing units to beneficiaries. This enables us to examine 457,805 applicants and 53,885 beneficiaries across 16 municipalities in Brazil. The scale of the program lead to a large population of affected individuals for analysis.

Finally, detailed micro-data provide a full picture of labor market outcomes and underlying mechanisms. We obtained applicant lists for the program by contacting Brazilian municipalities. Matched to the universe of MCMV lottery winners, our sample enables us to compare winners to non-winners. This sample is then linked via unique taxpayer identifiers to matched employee-employer data on the universe of formal workers in Brazil. The matched panel details the evolution of earnings, employers, occupations, and employee demographics.

This paper contributes to a literature on the private and public benefits of homeownership. Building on observational studies, a small but growing work exploits policy experiments that increased access to individual homeownership. A subset of the literature focuses on field experiments or policies held in a single city (Shlay, 1985, 1986; Engelhardt et al., 2010). Our paper examines a national program in different municipalities across Brazil. Moreover, many previously studied policies gave tenants of public housing the opportunity to buy (Sodini et al., 2016; Hausman et al., 2021). In those settings, the impact of homeownership is entangled by the need to finance a purchase. The lottery we examine instead gave residents a brand new home. Our setting avoids confounding, alternate channels inherent in public housing privatizations.

Finally, our paper contributes to research on policies to reduce poverty a development context. An influential literature posits that initial poverty is a cause of underdevelopment. Dynamic feedback between fitness for labor and individual productivity can generate multiple equilibria (Kraay and McKenzie, 2014; Banerjee et al., 2021; Balboni et al., 2021). A large-scale policy intervention can conceivably make individuals permanently better off. This paper investigates how transferring a house can improve labor market outcomes among the poor. We propose and evaluate how homeownership can be a large-scale policy solution to alleviate poverty.

This paper is organized as follows. Section 2 reviews the conceptual background and existing empirical evidence on homeownership. We review the institutional setting of the MCMV program in Brazil in Section 3. In Section 4, we present data sources enabling our empirical analysis. Section 5 presents the empirical strategy and results, and Section 6 concludes.

## 2 Conceptual Background

The literature has focused on two external benefits to homeownership.<sup>4</sup> They motivate the policy interventions that promote homeownership. The first is homeowners are exposed to the asset value of their home and thus are induced to pro-social investments. The second is homeowners are residentially more stable. In this paper, we investigate the effects of homeownership on individual labor market outcomes.

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<sup>4</sup>For an extended coverage of the literature, we refer the reader to the discussion in Hausman et al. (2021).

The first social benefit is homeowners are more likely to undertake investments that benefit others (DiPasquale and Glaeser, 1999; Henderson and Ioannides, 1983; Hoff and Sen, 2005). As residual claimants on their home, homeowners are incentivized to make investments to enhance property values. These investments, like better local public goods, also benefit others. Owner-occupied dwellings are better maintained (Harding et al., 2000), which impacts neighboring property values (Campbell et al., 2011; Diamond and McQuade, 2019). Homeowners are also more civically and politically engaged (Fischel, 2005; DiPasquale and Glaeser, 1999; Hilber, 2010; Ahlfeldt and Maennig, 2015), though causal evidence remains mixed (Engelhardt et al., 2010).

The second external benefit is residential stability of homeowners. Owning a home increases the transaction cost of moving (Hausman et al., 2021). Long-term residents are therefore inclined toward long-run investments affecting others. The literature has particularly emphasized its positive impact on children. Children in owner-occupied homes exhibit better cognitive and educational outcomes (Green and White, 1997; Haurin et al., 2002), which are attributed to residential stability (Aaronson, 2000; Galster et al., 2007).

But can policy intervention ever be justified on the grounds of private benefits alone? We investigate whether granting a house is a large capital transfer that overcomes poverty trap dynamics. A large literature in development hypothesizes that initial poverty is itself a cause of underdevelopment (Kraay and McKenzie, 2014; Balboni et al., 2021). When individual productivity is a function of initial capital ownership, there may be multiple equilibria. Coupled with financial or behavioral frictions, low income individuals stay low income absent a "big-push" policy intervention.

For homeownership to affect poverty trap dynamics, it there must be a positive feedback on individual productivity. The first channel is granting a home is a large asset windfall. If the property serves as collateral and eases credit access, then individuals can better insure against shocks (Sodini et al., 2016; Hurst and Stafford, 2004; Leth-Petersen, 2010), pursue entrepreneurial opportunities (Schmalz et al., 2017), or invest in human capital (Lochner and Monge-Naranjo, 2011). The second channel is that residential stability promotes physical fitness for work. Stable access to shelter improves health outcomes (Meltzer and Schwartz, 2016; Burgard et al., 2012; Brennan et al., 2011) enabling greater labor supply or physically demanding work. In this case, housing effectively serves as a substitute for leisure. Finally, exposure to government programs promotes formalization. This may be due to better knowledge of formal institutions and compliance with program requirements (Mills et al., 2006). Homeowners may become more politically engaged to preserve property values (Fischel, 2005).

On the other hand, homeownership can reduce labor supply and even wages. Participating in homeownership programs typically entail wealth effects. If leisure is a normal good, then labor

supply would decline (Blundell and MaCurdy, 1999; Imbens et al., 2001). Moreover, if housing is a complement to leisure, labor supply would fall further (Jacob and Ludwig, 2012). In addition, if homeownership increases the transaction costs of moving, it restricts labor mobility (Dietz and Haurin, 2003; Coulson and Fisher, 2009). Coupled with labor market frictions, homeowners as a result would experience falling earnings (Munch et al., 2006, 2008; Newman et al., 2009; Svarer et al., 2005). Finally, beneficiaries of affordable housing program are often obligated to pay a fraction of their income. What is effectively a tax on earned income leads individuals to substitute toward less work.

### 3 Institutional Setting

The MCMV program began in 2009 to combat a housing shortage among the low-income population in Brazil. In eight years, the program helped build around 4.4 million housing units at a cost of more than 290 billion Brazilian Reals.<sup>5</sup> This was the first federal housing program in the country since 1986.<sup>6</sup>

The MCMV program was designed with three different arms tailored to income groups called *Faixas*, each with different levels of government subsidies. *Faixa 1*, which is the focus of this paper, directly awarded beneficiaries a newly built house. This arm of the program served the lowest income individuals. It benefited around 1.7 million people with subsidies that could be as high as 90% of the value of the house. This value could be no higher than around 50,000 Brazilian Reals in the first years of the program, increasing to about 100,000 Brazilian Reals in later years. *Faixas 2* and *3* subsidized financing to purchase a home. We focus on *Faixa 1* mainly because it is the only one of the three that ran a lottery to select beneficiary. Moreover, *Faixa 1* has publicly available information on applicants and beneficiaries.

*Faixa 1* primarily operated in the following manner: first, municipalities enroll in the program; second, the federal government allocates funding based on how many municipalities enrolled in each region; third, the federal public bank in charge of the program, Caixa Econômica Federal (CEF), releases a call to construction companies for project proposals; fourth, the CEF chooses the best project based on cost, risk, and quality parameters such as whether the project will be located in suitable site with urban infrastructure. After that, the CEF is responsible for enforcing deadlines and ensuring the units are delivered appropriately.

Municipal governments, on their end, are responsible for organizing a list of eligible applicants for receiving a house. To be an applicant, a person must not own a house and must have a monthly

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<sup>5</sup>Roughly 100 billion dollars at that time.

<sup>6</sup>Before that, between 1964 and 1986, a public bank called Banco Nacional de Habitação (BNH) financed the construction of 4.3 million units for low-to-middle-income families (Bonduki, 2019).

household income no higher than 1,600 Brazilian Reals.<sup>7</sup> All eligible applicants are then sent for additional screening and approval by CEF.

Then, six months prior to the conclusion of any given housing project in a city, the municipal government typically runs a public lottery<sup>8</sup> to select beneficiaries among the approved applicants. Importantly, the lottery is designed with preferential treatment to individuals in vulnerable conditions. The idea is to prioritize certain groups, such as families living in areas of geological risk, unhealthy conditions, and families whose main provider was a woman. Hence, all applicants are split in two groups: one with people that satisfied a “vulnerability” score; and another with people that did not. The municipality then runs a lottery for each group separately<sup>9</sup>, which guarantees that the vulnerable group does not compete for houses with the other candidates. Therefore, the lottery is not entirely random, because people in vulnerable conditions are more likely to get a house overall.

Winners of the lottery receive the keys to the house shortly after signing a contract with CEF, in which they agree to pay a monthly installment to compensate for part of the house value. These installments can be no higher than 10% of the beneficiaries’ household income — with a minimum of 50 Brazilian Reals per month — and they are due for a 10-year period. The value of the subsidy is essentially the difference between the house value and what the beneficiary paid off in that period. After 10 years, CEF transfers ownership of the house to the beneficiary.

## 4 Data

### 4.1 MCMV

Information about beneficiaries who received houses from the government as part of the MCMV program was provided by CEF. The database contains names, unique taxpayer identification numbers called the CPF, reported income, the date when they signed the contract to receive the house, and their new residential address. In total, 957,801 people were selected to receive a subsidized home in *Faixa I* between 2009 and 2016.

Besides data on the beneficiaries, we collect information on people who applied to the program, but were not selected. Obtaining this is rather challenging, because CEF is not required to keep a record

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<sup>7</sup>Roughly 900 US dollars in 2009.

<sup>8</sup>The rules for selecting beneficiaries were updated regularly, but they generally maintained need for running lotteries when selecting candidates. Executive actions regulating the procedure include: Portaria do Ministério das Cidades 140 de 05/04/2010; Portaria do Ministério das Cidades 610 de 26/12/2011; Portaria do Ministério das Cidades 595 de 18/12/2013.

<sup>9</sup>For example, according to the regulation in Portaria do Ministério das Cidades 595 de 18/12/2013, the vulnerable group competed for 75% of the houses, whereas the other candidates competed for the remainder.

of applicant data in their systems.<sup>10</sup> Some local governments do provide this information online, and we retrieved applicant lists for 16 municipalities. This gives us a total of 457,814 applicants, out of which 55,905 people were selected to receive a house. Hence, the sample of applicants and beneficiaries we use in this paper is a subset of all participants in the MCMV program.

## 4.2 Matched Employee-Employer Data

To analyze employment and earning outcomes of MCMV beneficiaries, we gather information from *Relação Anual de Informações Sociais* (RAIS), an administrative matched employer-employee data set collected annually by the Brazilian Ministry of Labor and Employment (*Ministério do Trabalho e Emprego*, MTE). RAIS is recognized as a high quality census of the Brazilian labor market (Dix-Carneiro and Kovak, 2017). It consists of job entries with both individuals' and employers' tax identifiers, allowing us to track MCMV beneficiaries work records over time. For each job record, it records the employee name, taxpayer identifier, employer, information on hiring and separation dates, working hours, earnings, occupation, and some demographics, such as age, gender, and race.

## 4.3 Other Datasets: Political Participation

We also examine political participation of new homeowners. To do this, we use data on party affiliation in Brazil, which is publicly available and provided by Tribunal Superior Eleitoral (TSE), the highest electoral authority in the country. Our data comprises all party members from 2008 to 2017, with basic information such as names, dates of affiliation, name of party, municipality, and any reason for cancellation of membership.

## 4.4 Merging Datasets

To merge the datasets described above, we must overcome the challenge of not having a unique identifier that matches applicants, beneficiaries, employees, and political party members for all cities in our sample. In some cases, we have the unique taxpayer ID (CPF) to match applicants, beneficiaries, and employees. In other cases, we only have applicants' names or partial digits of the CPF. In other cases, like for party registration, we do not have individuals' tax IDs.

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<sup>10</sup>To gain access to this, we would need to contact each of the 5,570 municipalities in Brazil individually. Even if we managed to do so, however, the list of applicants enrolled for the lottery is seldom readily available, because municipal governments are not required to publish them online. The vast majority of these lists were simply displayed physically at the local government building during the period of selection, making it very hard for outsiders to access them.



Hence, we adopt a mix of approaches to merge all datasets, using the best identifier available in each case. The process is trivial for when we have the tax ID, as it is unique for each individual. In the other cases, we use participants' full names and the municipalities where they live, which creates pairs that are not necessarily unique. In the event of duplicate names, we randomize the match.<sup>11</sup>

In this exercise, we first match our beneficiary data to our applicant data using tax ID — when available — or name and city of residence as keys. Appendix Table 8 summarizes the merger between our applicant and beneficiary lists broken down by each of the 16 municipalities. Then, we match our sample of beneficiaries/applicants to our list of employees and party members, again using either tax ID or the combination of name and city of residence.<sup>12</sup> Appendix Table 9 summarizes the merger between the participants of MCMV to RAIS and political registrations data. Once we merge these datasets, we have a panel with observations at the individual-year level. Specifically, all MCMV participants, their labor-market characteristics and political participation are observed in all years between 2008 and 2017, such that each individual is observed only once each year.

## 4.5 Initial Descriptives

Our sample contains 16 Brazilian municipalities<sup>13</sup> for which we retrieved data on both losers and winners in the MCMV program. Table 1 compares our sample of municipalities to those in the rest of Brazil. We see that the municipalities in our sample are richer and larger than the average Brazilian municipality. Moreover, the municipalities in the sample are more urbanized, based on the fraction of manufacturing and services to GDP. This makes sense, as municipalities with higher demand for affordable housing are likely to have a larger urban population.

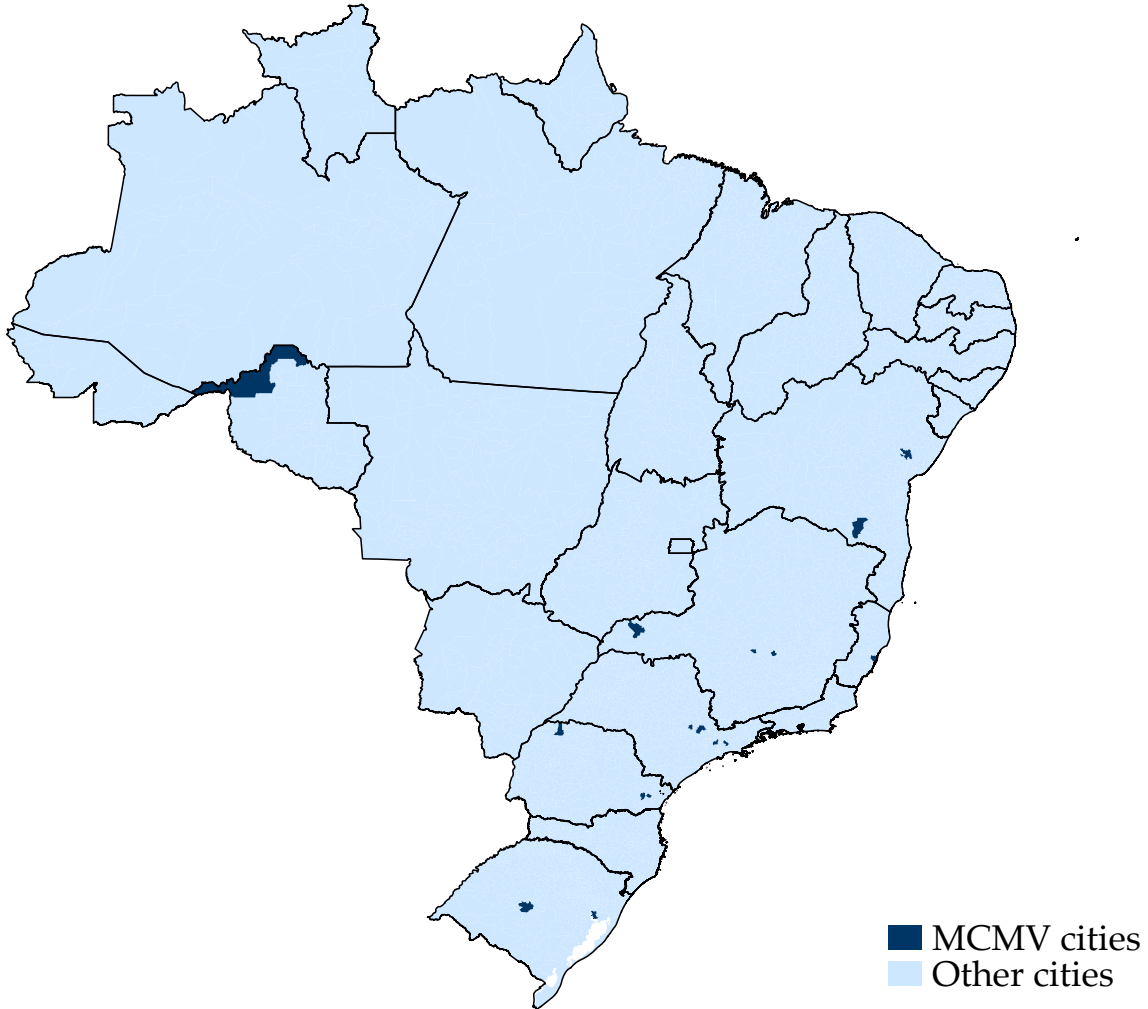
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<sup>11</sup>Although this might sound far from ideal at first, we rely on the fact that Brazilians have quite diverse first names and very often more than one surname. This way, people with the exact same name are much less frequent than in countries like the US, especially when we limit ourselves to matches within the same municipality

<sup>12</sup>To be precise, we use city of affiliation for the political participation data, since city of residence is not available.

<sup>13</sup>The following municipalities are in our sample, with state codes in parentheses: Betim (MG), Campinas (SP), Curitiba (PR), Feira de Santana (BA), Guararema (SP), Guarulhos (SP), Ituiutaba (MG), Nova Serrana (MG), Paranavaí (PR), Piraquara (PR), Porto Alegre (RS), Porto Velho (RO), Santa Bárbara d'Oeste (SP), Santa Maria (RS), Serra (ES), and Vitória da Conquista (BA).

Figure 1: Sample Municipalities across Brazil



Notes: Displayed are municipalities in Brazil. Municipalities in dark blue correspond to the 16 considered for analysis.

Tables 2 and 3 summarize applicants and beneficiaries in our sample. Participants tend to be female, informally employed, and low-income, consistent with the income limits in the program. Since the program prioritized disadvantaged groups, like females heads of household, beneficiaries are more likely to be female, less connected to the formal sector, and lower-income. These ex-ante differences between applicants and beneficiaries preclude direct comparison to estimate the effect of the program. Therefore, we adopt a differences-in-differences design. The design exploits the differential roll-out of housing across municipalities.

Table 1: Summary Statistics for Municipalities in Sample vs. Other Brazilian Municipalities, 2010

	Sample	Other Municipalities
GDP (in million BRL)	15.2 [18.5]	0.7 [7.3]
Population (in '000)	522.2 [540.5]	32.9 [199.7]
Share of Agriculture in GDP (%)	1.9 [2.8]	21.2 [15.4]
Share of Industry in GDP (%)	29.1 [12.1]	14.6 [15.1]
Share of Services in GDP (%)	54.2 [12.2]	30.2 [12.3]
Number of Municipalities	16	5,554

*Notes:* Displayed are summary statistics of municipalities considered for analysis. Standard deviation in brackets; All statistics correspond to values in 2010.

*Sources:* IBGE, Caixa Econômica Federal, and Municipal MCMV lists.

Table 2: Descriptives on MCMV Applicants and Beneficiaries

	Mean	Std. Dev.	Min.	Median	Max.	Obs.
Formally employed (%)	28.71	45.24	0.00	0.00	100.00	511,690
Female (%)	59.37	49.11	0.00	100.00	100.00	146,898
Age	31.02	9.65	10.00	29.00	108.00	146,898
Secondary schooling (%)	16.05	36.71	0.00	0.00	100.00	146,898
Primary schooling (%)	53.40	49.88	0.00	100.00	100.00	146,898
Tenure	2.10	3.72	0.00	1.00	50.00	146,898
Annual Working Hours	1,316.24	756.34	0.00	1,440.00	2,112.00	146,898
Hourly wage	4.75	5.03	0.00	3.83	625.61	146,898
Total earnings	6,282.20	6,556.57	0.00	5,387.45	316,775.16	146,898
Started first formal job (%)	7.61	26.52	0.00	0.00	100.00	146,898
Switched to a new job (%)	33.54	47.21	0.00	0.00	100.00	146,898
Fired with a cause (%)	0.38	6.19	0.00	0.00	100.00	146,898
Left job position (%)	5.83	23.44	0.00	0.00	100.00	146,898
Public employees (%)	5.80	23.37	0.00	0.00	100.00	146,898
Politically appointed employees (%)	0.48	6.93	0.00	0.00	100.00	146,898

*Notes:* This table provides summary statistics of the MCMV applicants and beneficiaries in 2008, the year before the start of the program. All variables are computed at the individual level, with hourly wage and total earning variables in current BRL price levels.

Table 3: Means Test for MCMV Applicants and Beneficiaries

	Treated		Control		Diff.
	Mean	Obs.	Mean	Obs.	Treated-Control
Formally employed (%)	14.60	53,885	30.37	457,805	-15.77***
Female (%)	84.21	7,866	57.97	139,032	26.24***
Age	31.03	7,866	31.02	139,032	0.01
Secondary schooling (%)	19.15	7,866	15.88	139,032	3.27***
Primary schooling (%)	46.20	7,866	53.81	139,032	-7.61***
Tenure	1.69	7,866	2.12	139,032	-0.43***
Annual Working Hours	1,215.99	7,866	1,321.91	139,032	-105.92***
Hourly wage	3.69	7,866	4.81	139,032	-1.12***
Total earnings	4,376.41	7,866	6,390.03	139,032	-2,013.62***
Started first formal job (%)	11.07	7,866	7.42	139,032	3.66***
Switched to a new job (%)	34.60	7,866	33.48	139,032	1.12**
Fired with a cause (%)	0.50	7,866	0.38	139,032	0.12
Left job position (%)	6.65	7,866	5.79	139,032	0.86***
Public employees (%)	6.15	7,866	5.78	139,032	0.38
Politically appointed employees (%)	0.57	7,866	0.48	139,032	0.10

Notes: This table provides summary statistics comparing the MCMV applicants to beneficiaries in 2008, the year before the start of the program. All variables are computed at the individual level, with hourly wage and total earning variables current BRL price levels.

## 5 Results

In this section, we estimate the impact of winning a home on formal labor participation, wages, and labor supply. We then investigate plausible mechanisms for our findings.

### 5.1 Empirical Design

In our empirical strategy, we take individuals who applied for but did not win a house as control. To do that, we follow the procedure from [Deshpande and Li \(2019\)](#), and construct our sample as follows. First, we stack all applicants for each pair of city and year of the MCMV projects (1,930 in total). Second, we label as treated the beneficiaries who received a house in that year, and as control those who did not. Third, we restrict to six years before and after the year beneficiaries signed to obtain a house.

To estimate the effect of earning a home from the MCMV program on labor market outcomes, we estimate the following regression:

$$y_{icpt} = \alpha_i + \gamma_t + \kappa_{ct} + Treated_{ip} + D_{pt} + \delta (Treated_{ip} \times D_{pt}) + \varepsilon_{icpt} \quad (1)$$

where  $y_{icpt}$  is an outcome (e.g., hourly earnings) for individual  $i$  for project  $p$  in city  $c$  and year  $t$ .  $\alpha_i$  are individual fixed effects,  $\gamma_t$  are calendar year fixed effects, and  $\kappa_{ct}$  are city-year fixed effects.  $Treated_{ip}$  is an indicator variable equal to 1 if  $i$  is treated in project  $p$ .  $D_{pt}$  is an indicator that equals 1 if year  $t$  is after the inauguration of the MCMV housing project  $p$ . Our coefficient of interest is  $\delta$ , which captures the difference in outcome  $y_{icpt}$  between lottery winners and losers after beneficiaries sign a contract to obtain a house.

To analyze dynamic effects, we estimate:

$$y_{icpt} = \alpha_i + \gamma_t + \kappa_{ct} + Treated_{ip} + \sum_{\tau} D_{pt}^{\tau} + \sum_{\tau} \delta^{\tau} (Treated_{ip} \times D_{pt}^{\tau}) + \eta_{icpt} \quad (2)$$

$D_{pt}^{\tau}$  is a set of indicators that equal 1 if year  $t$  is  $\tau$  years after the year of inauguration of the MCMV housing project  $p$ , or 0 otherwise. The coefficients of interest are  $\delta^{\tau}$ , which capture the difference in outcome  $y_{icpt}$  between treated and control individuals  $\tau$  years relative to the inauguration of the project.<sup>14</sup> In both specifications, standard errors are clustered at the individual-year level.

Equations 1 and 2 are two-way fixed effects specifications to estimate the impact of winning a house on labor outcomes. Since a municipality runs a lottery independently and potentially multiple times, there are multiple treatment events staggered across time and municipality. A growing econometrics literature shows that the two-way fixed effects specification for staggered designs generates biased estimates (Athey and Imbens, 2022; Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021; Sun and Abraham, 2021; De Chaisemartin and d’Haultfoeuille, 2020). The estimate  $\delta$  in Equation 1 is a weighted aggregate of treatment effects across treatment events, i.e., projects. With multiple treatment events and dynamic treatment effects, the weights may be negative and an estimate for  $\delta$  is biased. To circumvent this issue, we adopt the estimation and inference procedure proposed by Callaway and Sant’Anna (2021).

To evaluate the welfare of these beneficiaries we investigate wages and hours worked. RAIS only covers formal sector workers. To study the impact on wages, hours worked, and occupations, we present two sets of estimation strategies. The first strategy subsets to individuals who are always formally employed, i.e., appear in RAIS, throughout our sample. The second strategy employs a two-stage estimation procedure to account for the selection into formal employment.

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<sup>14</sup>Each beneficiary sign the contract to receive the MCMV house in a specific date. As the outcome variables are at the beneficiary-year level, we consider for the analysis only the year of the aforementioned date.

## 5.2 Baseline Results

### 5.2.1 Labor Market Participation

We report the impact of winning a home on formal labor market participation. To do so, we estimate Equation 1 with  $y_{icpt}$  indicating whether individual  $i$  appears in RAIS in year  $t$ . Figure 2a plots the dynamic response of formal employment over time. Before each lottery draw, ultimate winners and non-winners are alike in probability of appearing in RAIS. After earning a home, however, beneficiaries become more likely to engage in formal employment. This effect persists even five years after the initial win. At that point, winners of the lottery are 2 percentage points more likely to be employed in the formal sector. Relative to a baseline formal employment rate of 34 percent, owning a home causes a 5.8 percent increase in the probability of formal employment. Table 5 displays the resulting differences-in-differences estimates.

### 5.2.2 Formal Wages and Labor Supply

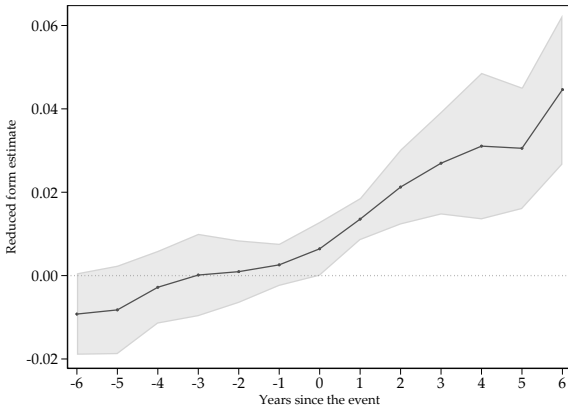
To evaluate the welfare of these beneficiaries we investigate wages and hours worked. RAIS only covers formal sector workers. Therefore wages and hours worked are unavailable for informal workers.<sup>15</sup> We estimate these formal employment outcomes by subsetting to individuals who were always formally employed.

Figure 2c plots the impact of winning a house on hourly wages. Since RAIS covers the formal sector, the estimate is on individuals who were formally employed over the study period. Starting in the year the winner wins the lottery, average wages fall by 0.57 BRL. Wages fall consistently every year, and by year five, new homeowners earn 1.6 BRL less than losers. These results show that the program reduced wages. Figure 2b displays the impact on formal hours worked. After an immediate dip in hours worked the year beneficiaries sign, beneficiaries work almost 60 hours more by year five. Toward the end of the sample, beneficiaries still work more, though our estimates are significantly different from 0 at 10% significance. We now investigate underlying mechanisms, whether beneficiaries responded to program payment requirements or experienced worse labor market opportunities.

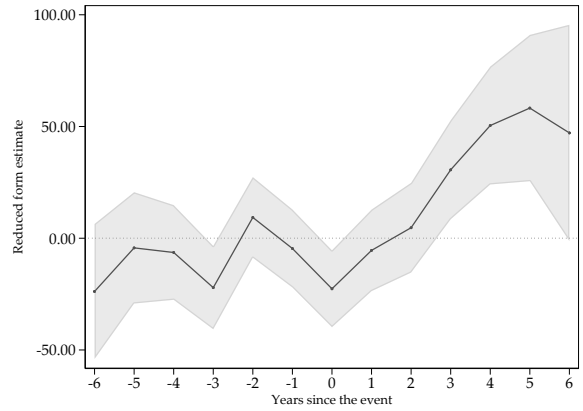
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<sup>15</sup>Conventional approaches to censored outcomes are incompatible with the staggered differences-in-differences design using Callaway and Sant'Anna (2021).

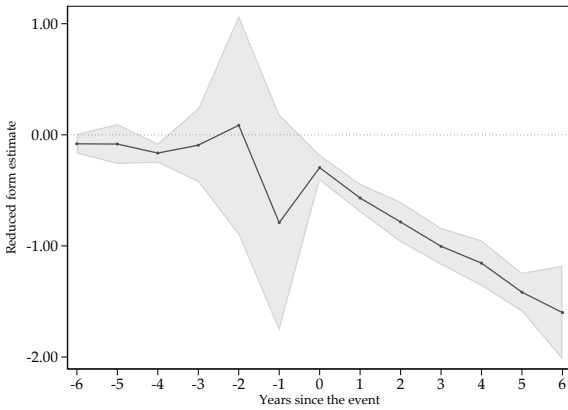
Figure 2: Labor Market Effects of Homeownership Lottery



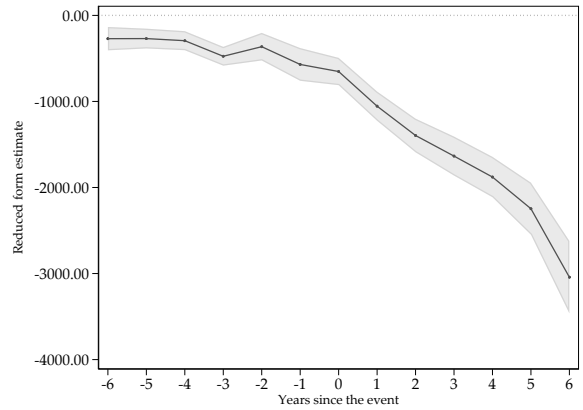
(a) Formal Employment



(b) Annual Hours Worked



(c) Hourly Wages



(d) Annual Earnings

Notes: Displayed are the impacts of winning a house on labor market outcomes over time. X-axis displays the year relative to a beneficiary signing a contract granting a house. (a) Formal Employment is defined as appearing in RAIS and is measured in percentage points. (b) Annual Hours Worked is total hours worked in the formal sector. (c) Hourly wages is total earnings divided by hours worked in the formal sector. (d) Annual earnings is the sum of all formal labor income in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

Table 4: Labor Market Effects of Homeownership Lottery

	(1)	(2)	(3)	(4)
$Treat_i \times Post_t$	0.020*** (0.005)	10.572 (7.904)	-0.777*** (0.056)	-1,363.431*** (72.834)
Pre-treat. Avg.	0.34	1396.69	7.21	10123.42
Cities	16	16	16	16
Individuals	511,690	292,269	292,269	292,269
Observations	5,628,590	1,778,416	1,778,416	1,778,416

Notes: Displayed are the impacts of winning a house on labor market outcomes. (1) Formal Employment is defined as appearing in RAIS and is measured in percentage points. (2) Annual Hours Worked is total hours worked in the formal sector. (3) Hourly wages is total earnings divided by hours worked in the formal sector. (4) Annual earnings is the sum of all formal labor income in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.



Table 5: Labor Market Effects of Homeownership Lottery

	(1)	(2)	(3)	(4)
Relative Year: -6	-0.009* (0.005)	-23.827 (15.409)	-0.081* (0.045)	-270.591*** (69.341)
Relative Year: -5	-0.008 (0.005)	-4.306 (12.710)	-0.083 (0.092)	-269.440*** (58.809)
Relative Year: -4	-0.003 (0.004)	-6.372 (10.815)	-0.164*** (0.045)	-294.002*** (56.571)
Relative Year: -3	0.000 (0.005)	-22.102** (9.509)	-0.093 (0.170)	-475.198*** (55.826)
Relative Year: -2	0.001 (0.004)	9.308 (9.210)	0.086 (0.504)	-363.718*** (81.805)
Relative Year: -1	0.003 (0.003)	-4.586 (8.911)	-0.791 (0.498)	-570.091*** (97.111)
Relative Year: 0	0.006* (0.003)	-22.624*** (8.774)	-0.295*** (0.059)	-652.261*** (81.010)
Relative Year: +1	0.014*** (0.003)	-5.440 (9.307)	-0.569*** (0.065)	-1,054.884*** (86.791)
Relative Year: +2	0.021*** (0.005)	4.699 (10.247)	-0.784*** (0.093)	-1,395.545*** (99.836)
Relative Year: +3	0.027*** (0.006)	30.619*** (11.307)	-1.003*** (0.085)	-1,634.905*** (,115.655)
Relative Year: +5	0.031*** (0.007)	58.244*** (16.704)	-1.416*** (0.089)	-2,246.412*** (,154.482)
Relative Year: +6	0.045*** (0.009)	47.152* (24.695)	-1.600*** (0.216)	-3,042.414*** (,214.985)
Pre Avg. Effect	-0.006* (0.004)	-12.642*** (3.747)	-0.167*** (0.026)	-315.183*** (18.090)
Post Avg. Effect	0.028*** (0.006)	15.406 (18.470)	-1.181*** (0.082)	-2,040.136*** (,123.157)
Pre-treat. Avg.	0.34	1,396.69	7.21	10,123.42
Cities	16	16	16	16
Individuals	511,690	292,269	292,269	292,269
Observations	5,628,590	1,778,416	1,778,416	1,778,416

Notes: Displayed are the impacts of winning a house on labor market outcomes. (1) Formal Employment is defined as appearing in RAIS and is measured in percentage points. (2) Annual Hours Worked is total hours worked in the formal sector. (3) Hourly wages is total earnings divided by hours worked in the formal sector. (4) Annual earnings is the sum of all formal labor income in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

### 5.3 Mechanisms

Becoming a homeowner increased formal labor market participation, decreased wages, and increased hours worked in the formal sector. We assess underlying mechanisms.

### 5.3.1 Labor Mobility

We find that wages and earnings fall for beneficiaries of a new home. One possible mechanism is homeownership reduces labor mobility. Owning a home increases the transaction cost of moving [Haurin et al. \(2002\)](#). Coupled with frictional labor markets, individuals locked into a labor market experience worse labor market outcomes. We test for reduced labor mobility by examining job switching and separation rates. We re-estimate Equations 1 and 2 but examining outcomes on job transition. In addition, we examine whether workers are fired for cause. Specifically, we set  $y_{icpt}$  as an indicator for whether individual  $i$  ever changed employers or voluntarily leave their employer, or was fired for cause in year  $t$ . Reducing labor mobility increases the cost of involuntary separation. To the extent workers can affect that likelihood, we expect workers are less likely to be fired if they are locked-in to a location.

Figure 3b plots the likelihood of switching to a new job, 3c plots whether one is fired for cause, and 3d plots whether one voluntarily leaves a job. In the year after earning a home, beneficiaries switch to new work. This is likely an effect of relocating to a new home. Subsequently, however, beneficiaries switch jobs at the same rate as non-beneficiaries. Similarly, they are not more likely to be fired for cause or voluntarily leave a job. These findings do not suggest homeownership reduces labor mobility. Other channels likely rationalize our finding of falling wages and earnings.

### 5.3.2 Formalization

New homeowners increase formal labor market participation. We investigate formalization as a mechanism for our finding. Exposure to one government program —MCMV— potentially encourages further participation in formal institutions. We test this formalization channel by examining entry into the public sector and political registration.

We examine if beneficiaries are being employed in the public sector. RAIS indicates whether an employee works in the public sector and whether the job is based on a political appointment. Setting  $y_{icpt}$  as an indicator for employment in the public sector, we re-estimate Equations 1 and 2 on how earning a home impacted promotion or access to public sector jobs.

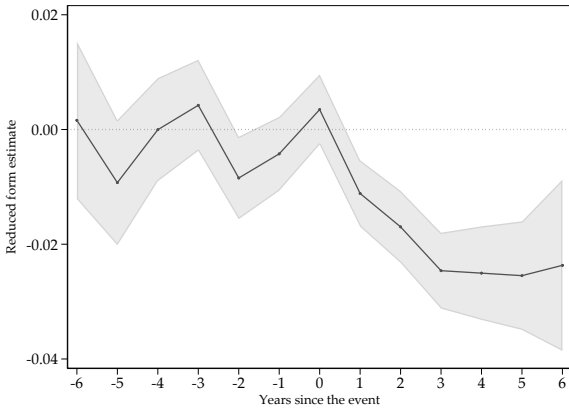
Figure 3e plots the likelihood of earning public sector employment and 3f whether the beneficiary earned a political appointment. Beneficiaries and non-beneficiaries are similar in their likelihood of joining the public sector. However, beneficiaries become more likely to earn employment via political appointment. From a baseline likelihood of 1 percent, the effect is a 1.1 percentage point increase. This implies that becoming a beneficiary of MCMV more than doubled the likelihood of

earning a political appointment. This suggests exposure to one program run by the local government familiarizes individuals with officials and programs in formal institutions.

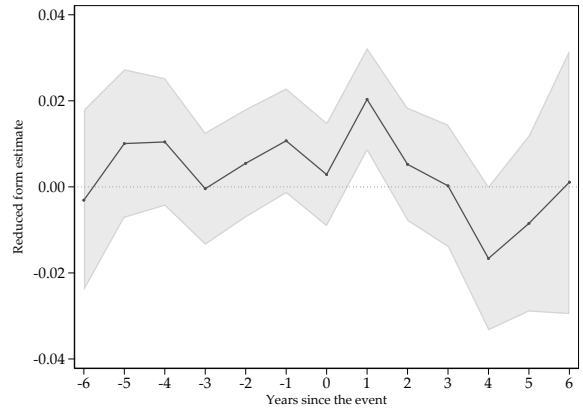
Our final test for formalization is political engagement. We examine the impact of winning a home on political participation. To do this, we re-estimate Equations 1 and 2 with an indicator for whether the individual is registered with a political party.

As Table ?? and Figure 4 show, winners of the lottery increased their political participation. By year six, winners are 0.0013 percentage points more likely to be registered with a party. Given the baseline of 1.8% participation, this represents a 5.6% increase. Higher political engagement is non-ideological. Figures 4 and 4 show that individuals are not more likely to register for right-wing or left-wing parties in Brazil. This further provides evidence for the formalization effect of homeownership. Beneficiaries not only increase engagement with the public sector but also more politically active. This is consistent with the home voter hypothesis (Fischel, 2005). New homeowners, who are residentially stable and have incentives to preserve property values, increase civic engagement.

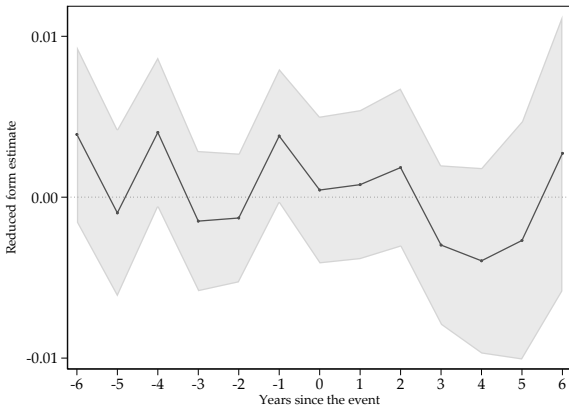
Figure 3: Mechanisms for Labor Market Changes



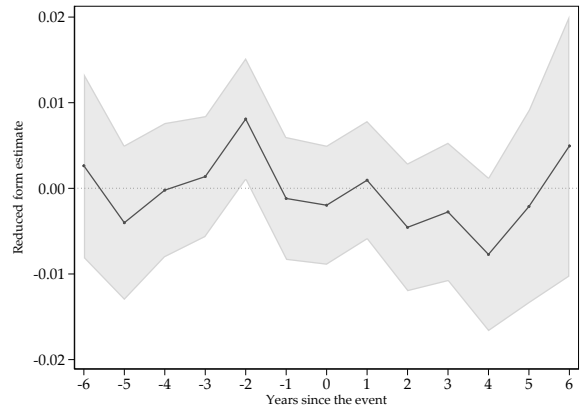
(a) Started the First Formal Job



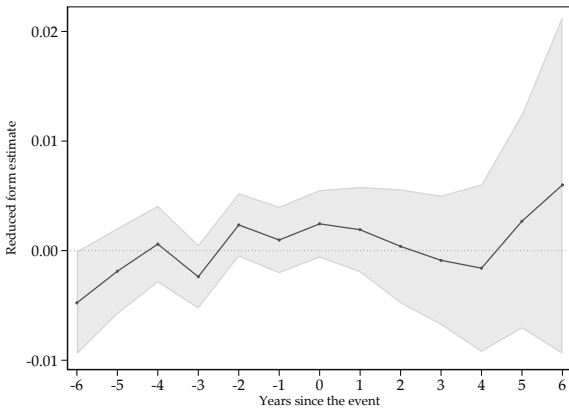
(b) Switch to a New Job



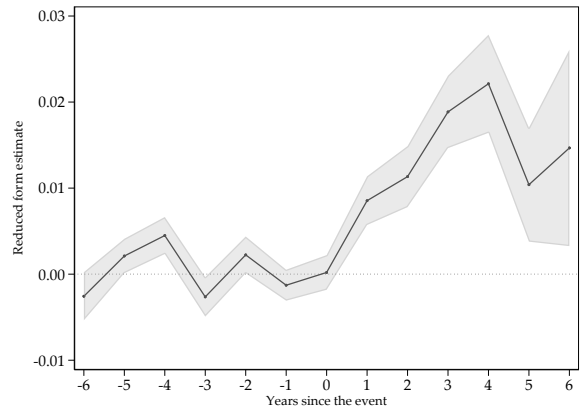
(c) Fired for Cause



(d) Left the job position



(e) Public Sector Employment



(f) Public Sector Politically Appointed Positions

Notes: Displayed are the impacts of winning a house on plausible mechanisms underlying labor market outcomes. X-axis displays the year relative to a beneficiary signing a contract granting a house. (a) Dependent variable is equal to 1 if individual started first job in a given year; (b) Dependent variable is equal to 1 if individual switched jobs in a given year; (c) Dependent variable is equal to 1 if individual was fired in a given year; (d) Dependent variable is equal to 1 if individual quit job in a given year; (e) Dependent variable is equal to 1 if individual was working in a public sector job in a given year; (f) Dependent variable is equal to 1 if individual was working in a public sector job with discretionary appointment in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

Table 6: Mechanisms for Labor Market Changes

	Job Switch		Separation		Public Sector	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i \times Post_t$	-0.014*** (0.003)	0.004 (0.005)	0.000 (0.001)	-0.002 (0.003)	0.001 (0.002)	0.011*** (0.001)
Pre-treat. Avg.	0.04	0.31	0.01	0.07	0.07	0.01
Cities	16	16	16	16	16	16
Individuals	292,269	292,269	292,269	292,269	292,269	292,269
Observations	1,778,416	1,778,416	1,778,416	1,778,416	1,778,416	1,778,416

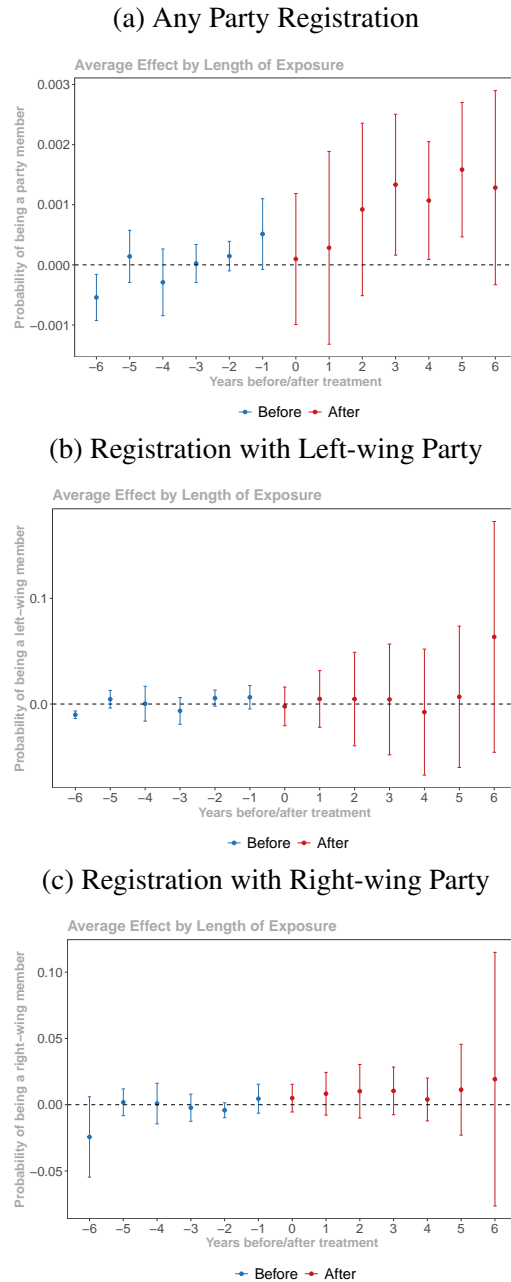
Notes: Displayed are the impacts of winning a house on plausible mechanisms underlying labor market outcomes. (1) Dependent variable is equal to 1 if individual started first job in a given year; (2) Dependent variable is equal to 1 if individual switched jobs in a given year; (3) Dependent variable is equal to 1 if individual was fired in a given year; (4) Dependent variable is equal to 1 if individual quit job in a given year; (5) Dependent variable is equal to 1 if individual was working in a public sector job in a given year; (6) Dependent variable is equal to 1 if individual was working in a public sector job with discretionary appointment in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

Table 7: Mechanisms for Labor Market Changes

	Job Switch		Separation		Public Sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Relative Year: -6	0.002 (0.007)	-0.003 (0.011)	0.002 (0.001)	0.003 (0.006)	-0.005** (0.002)	-0.003* (0.001)
Relative Year: -5	-0.009* (0.006)	0.010 (0.009)	0.000 (0.001)	-0.004 (0.005)	-0.002 (0.002)	0.002** (0.001)
Relative Year: -4	0.000 (0.005)	0.010 (0.008)	0.002* (0.001)	0.000 (0.004)	0.001 (0.002)	0.004*** (0.001)
Relative Year: -3	0.004 (0.004)	0.000 (0.007)	-0.001 (0.001)	0.001 (0.004)	-0.002 (0.001)	-0.003** (0.001)
Relative Year: -2	-0.008** (0.004)	0.005 (0.006)	-0.001 (0.001)	0.008** (0.004)	0.002 (0.001)	0.002** (0.001)
Relative Year: -1	-0.004 (0.003)	0.011* (0.006)	0.002* (0.001)	-0.001 (0.004)	0.001 (0.002)	-0.001 (0.001)
Relative Year: 0	0.003 (0.003)	0.003 (0.006)	0.000 (0.001)	-0.002 (0.004)	0.002 (0.002)	0.000 (0.001)
Relative Year: +1	-0.011*** (0.003)	0.020*** (0.006)	0.000 (0.001)	0.001 (0.004)	0.002 (0.002)	0.009*** (0.001)
Relative Year: +2	-0.017*** (0.003)	0.005 (0.007)	0.001 (0.001)	-0.005 (0.004)	0.000 (0.003)	0.011*** (0.002)
Relative Year: +3	-0.025*** (0.003)	0.000 (0.007)	-0.001 (0.001)	-0.003 (0.004)	-0.001 (0.003)	0.019*** (0.002)
Relative Year: +5	-0.025*** (0.005)	-0.008 (0.010)	-0.001 (0.002)	-0.002 (0.006)	0.003 (0.005)	0.010*** (0.003)
Relative Year: +6	-0.024*** (0.008)	0.001 (0.016)	0.001 (0.002)	0.005 (0.008)	0.006 (0.008)	0.015** (0.006)
Pre Avg. Effect	-0.003* (0.002)	0.008*** (0.002)	0.000 (0.000)	0.001 (0.001)	0.000 (0.001)	0.000 (0.000)
Post Avg. Effect	-0.022*** (0.008)	0.005 (0.011)	0.001 (0.002)	-0.001 (0.004)	-0.009 (0.006)	0.012*** (0.002)
Pre-treat. Avg.	0.04	0.31	0.01	0.07	0.07	0.01
Cities	16	16	16	16	16	16
Individuals	292,269	292,269	292,269	292,269	292,269	292,269
Observations	1,778,416	1,778,416	1,778,416	1,778,416	1,778,416	1,778,416

Notes: Displayed are the impacts of winning a house on plausible mechanisms underlying labor market outcomes. (1) Dependent variable is equal to 1 if individual started first job in a given year; (2) Dependent variable is equal to 1 if individual switched jobs in a given year; (3) Dependent variable is equal to 1 if individual was fired in a given year; (4) Dependent variable is equal to 1 if individual quit job in a given year; (5) Dependent variable is equal to 1 if individual was working in a public sector job in a given year; (6) Dependent variable is equal to 1 if individual was working in a public sector job with discretionary appointment in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

Figure 4: Effect on Political Participation



Notes: Displayed are the impacts of winning a house on political participation. X-axis displays the year relative to a beneficiary signing a contract granting a house. (a) outcome of interest is registration with an official political party in Brazil; (b) outcome is registration with a left-wing party; (c) outcome is registration with a right-wing party. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

## 6 Conclusion

Becoming a homeowner accelerates formalization, increases labor supply, but reduces earnings. These are the findings from a large-scale, lottery-based homeownership program in Brazil. Our

findings evaluate the rationale for policies promoting homeownership in developing economies. Residential stability encourages formal work in the extensive and intensive margins. However, homeownership reduces earnings by constraining labor mobility. This does not suggest that affordable housing sufficiently improves individual labor market outcomes.

However, we find that becoming a homeowner permanently increases formal sector employment. Exposure to formal institutions, via the homeownership program, leads individuals to further participate in the formal sector. Consistent with formalization, homeowners are more likely to register with a local political party. This is consistent with higher residential stability exposing beneficiaries to local fluctuations. They have a higher incentive for civic engagement to shape neighborhood or local amenities. This could act to expand the social networks of these individuals and thus increase their access to the labor market.

One particular channel through which these social networks could lead to formal jobs is access to public sector employment. Being more politically active, as well as participating in a public housing lottery, may expose winners to politicians who later hire them to work in the public administration. Indeed, we find that winners are more likely to be appointed to public sector positions. Importantly, we only find this effect in appointments that are under politicians' discretion, which corroborates the proposed mechanism.

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# Appendix

## A Data

Table 8: Matching MCMV Applicants to Beneficiaries

	<i>Match Status</i>							
	<i>Applicant</i>		<i>Matched Beneficiary</i>		<i>Only Beneficiary</i>		<i>Total</i>	
	No.	%	No.	%	No.	%	No.	%
<i>State: Municipality</i>								
BA: FEIRA DE SANTANA	305,220	95.1%	4	0.0%	15,660	4.9%	320,884	100.0%
BA: VITORIA DA CONQUISTA	3,753	31.2%	38	0.3%	8,256	68.5%	12,047	100.0%
ES: SERRA	40	6.3%	498	77.9%	101	15.8%	639	100.0%
MG: BETIM	18,452	86.7%	280	1.3%	2,544	12.0%	21,276	100.0%
MG: ITUIUTABA	31,624	90.9%	1,621	4.7%	1,539	4.4%	34,784	100.0%
MG: NOVA SERRANA	831	58.2%	64	4.5%	532	37.3%	1,427	100.0%
PR: CURITIBA	242,490	98.2%	1,113	0.5%	3,324	1.3%	246,927	100.0%
PR: PARANAVAI	13,279	94.6%	320	2.3%	437	3.1%	14,036	100.0%
PR: PIRAQUARA	1,193	78.0%	298	19.5%	39	2.5%	1,530	100.0%
RO: PORTO VELHO	33,611	96.6%	419	1.2%	759	2.2%	34,789	100.0%
RS: PORTO ALEGRE	102,580	97.9%	628	0.6%	1,616	1.5%	104,824	100.0%
RS: SANTA MARIA	11,216	88.0%	493	3.9%	1,043	8.2%	12,752	100.0%
SP: CAMPINAS	94,250	91.4%	2,230	2.2%	6,665	6.5%	103,145	100.0%
SP: GUARAREMA	822	64.6%	259	20.3%	192	15.1%	1,273	100.0%
SP: GUARULHOS	352,716	98.7%	1,938	0.5%	2,558	0.7%	357,212	100.0%
SP: SANTA BARBARA D'OESTE	10,900	96.1%	337	3.0%	100	0.9%	11,337	100.0%
Total	1,222,977	95.6%	10,540	0.8%	45,365	3.5%	1,278,882	100.0%

Table 9: Matching MCMV Participants to RAIS and Political Registration

	Individual Observations									
	<i>All</i>	<i>Matched to RAIS</i>					<i>Matched to Politics</i>			
		All Matched	CPF Match	Exact Name Match	Fuzzy Name Match	Mult. Matches	All Matched	Exact Name Match	Fuzzy Name Match	Mult. Matches
<i>Applicant Status</i>										
Applicant	442,765	281,190	175,198	271,977	9,213	114,149	33,970	33,523	447	78
Beneficiary	40,456	20,335	2,576	20,246	89	8,397	2,593	2,589	4	6
Total	483,221	301,525	177,774	292,223	9,302	122,546	36,563	36,112	451	84

## B Empirical Design

### B.1 Censored Regression and Staggered Differences-in-differences

This two-step procedure is based on Heckman (1976, 1979) [XXX CITE XXX]. Take as the operating example wages in RAIS. Setting  $\mathbf{X}_{icpt} = [\alpha_i, \gamma_t, \kappa_{ct}, Treated_{ip}, D_{pt}, Treated_{ip} \times D_{pt}]'$ , true wages are determined by:

$$y_{icpt}^* = \mathbf{X}'_{icpt} \mathbf{B} + \sigma \varepsilon_{icpt}$$

where  $\varepsilon_{icpt} \sim N(0, 1)$ . Since that data only covers formal employment, we have:

$$y_{icpt} = \begin{cases} y_{icpt}^* & \text{if } y_{icpt}^* > 0 \\ 0 & \text{if } y_{icpt}^* \leq 0 \end{cases}$$

Letting  $d_{icpt} = I\{y_{icpt}^* > 0\} = I\{\mathbf{X}'_{icpt} \mathbf{B} + \sigma \varepsilon_{icpt} > 0\} = I\left\{\varepsilon_{icpt} > -\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right\}$ , we have:

$$\begin{aligned} E[y_{icpt} | \mathbf{X}'_{icpt} \mathbf{B}] &= E_d \left[ E[y_{icpt} | \mathbf{X}'_{icpt} \mathbf{B}, d_{icpt}] \right] \\ &= E \left[ y_{icpt} | \mathbf{X}'_{icpt} \mathbf{B}, d_{icpt} = 1 \right] P\{d_{icpt} = 1\} + E \left[ y_{icpt} | \mathbf{X}'_{icpt} \mathbf{B}, d_{icpt} = 0 \right] P\{d_{icpt} = 0\} \\ &= E \left[ y_{icpt} | \mathbf{X}'_{icpt} \mathbf{B}, d_{icpt} = 1 \right] P\{d_{icpt} = 1\} \\ &= \left[ \mathbf{X}'_{icpt} \mathbf{B} + E \left[ \varepsilon_{icpt} | \mathbf{X}'_{icpt} \mathbf{B}, \varepsilon_{icpt} > -\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma} \right] \right] P \left\{ \varepsilon_{icpt} > -\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma} \right\} \end{aligned}$$

Focusing for now on effects on formally employed:

$$\begin{aligned}
 E \left[ y_{icpt} \mid \mathbf{X}'_{icpt} \mathbf{B}, d_{icpt} = 1 \right] &= \mathbf{X}'_{icpt} \mathbf{B} + E \left[ \varepsilon_{icpt} \mid \mathbf{X}'_{icpt} \mathbf{B}, \varepsilon_{icpt} > -\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma} \right] \\
 &= \mathbf{X}'_{icpt} \mathbf{B} + E \left[ \varepsilon_{icpt} \mid \varepsilon_{icpt} > -\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma} \right] \\
 &= \mathbf{X}'_{icpt} \mathbf{B} + \int_{-\mathbf{X}'_{icpt} \mathbf{B}}^{\infty} e \frac{\phi(e)}{\Phi\left(-\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right)} de \\
 &= \mathbf{X}'_{icpt} \mathbf{B} + \frac{1}{\Phi\left(-\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right)} \int_{-\mathbf{X}'_{icpt} \mathbf{B}}^{\infty} e \phi(e) de \\
 &= \mathbf{X}'_{icpt} \mathbf{B} + \frac{\phi\left(-\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right)}{\Phi\left(-\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right)}
 \end{aligned}$$

setting  $\lambda\left(-\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right) = \frac{\phi\left(-\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right)}{\Phi\left(-\frac{\mathbf{X}'_{icpt} \mathbf{B}}{\sigma}\right)}$  and  $\mathbf{A} = -\frac{\mathbf{B}}{\sigma}$  we have:

$$E \left[ y_{icpt} \mid \mathbf{X}'_{icpt} \mathbf{B}, d_{icpt} = 1 \right] = \mathbf{X}'_{icpt} \mathbf{B} + \sigma \lambda \left( \mathbf{X}'_{icpt} \mathbf{A} \right)$$

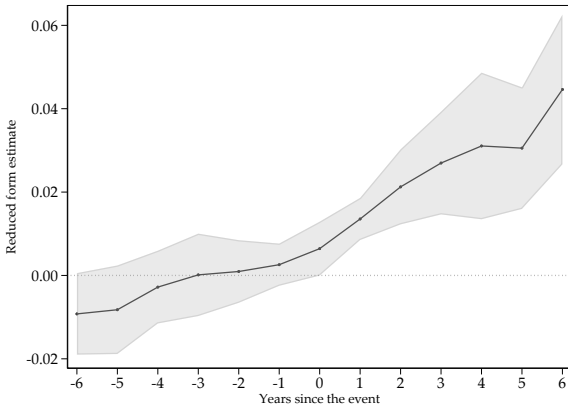
**Two-Step Estimator** To get consistent estimates of  $\mathbf{B}$ , estimate  $\mathbf{A}$  using the following Probit:

$$d_{icpt} = \mathbf{X}'_{icpt} \mathbf{A} + \eta_{icpt}$$

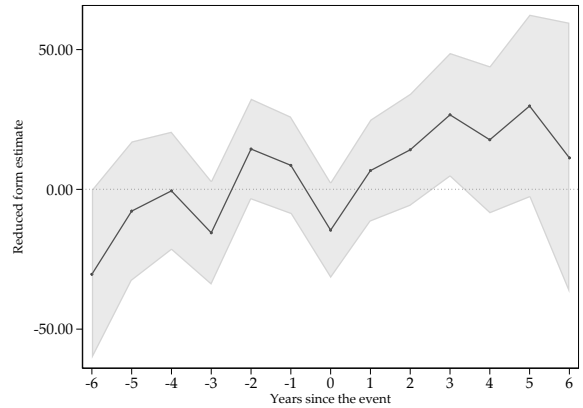
Then form  $\lambda\left(\mathbf{X}'_{icpt} \hat{\mathbf{A}}\right)$ . Include  $\lambda\left(\mathbf{X}'_{icpt} \hat{\mathbf{A}}\right)$  as control variables in the procedure in [Callaway and Sant'Anna \(2021\)](#).

## B.2 Alternative Specifications

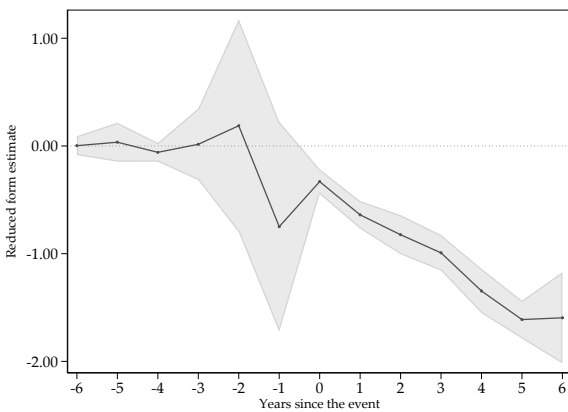
Figure 5: Labor Market Effects of Homeownership Lottery (Two-step Heckman correction)



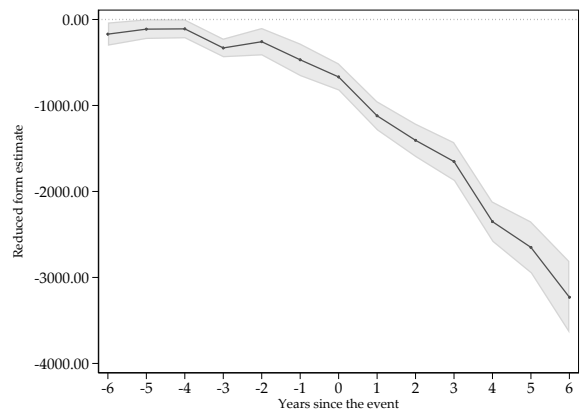
(a) Formal Employment



(b) Annual Hours Worked



(c) Hourly Wages



(d) Annual Earnings

Notes: Displayed are the impacts of winning a house on plausible mechanisms underlying labor market outcomes. X-axis displays the year relative to a beneficiary signing a contract granting a house. (a) Dependent variable is equal to 1 if individual started first job in a given year; (b) Dependent variable is equal to 1 if individual switched jobs in a given year; (c) Dependent variable is equal to 1 if individual was fired in a given year; (d) Dependent variable is equal to 1 if individual quit job in a given year; (e) Dependent variable is equal to 1 if individual was working in a public sector job in a given year; (f) Dependent variable is equal to 1 if individual was working in a public sector job with discretionary appointment in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

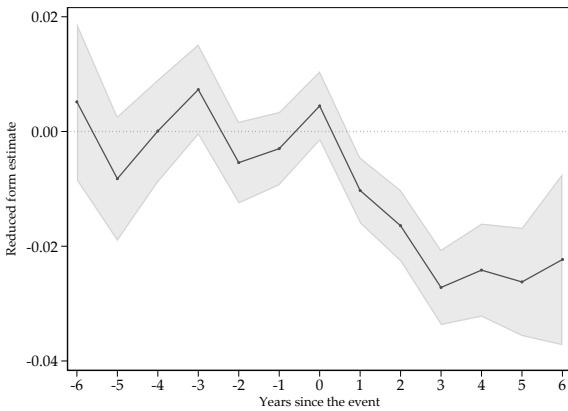
Table 10: Labor Market Effects of Homeownership Lottery (Two-step Heckman correction)

	(1)	(2)	(3)	(4)
Relative Year: -6	-0.009* (0.005)	-30.369** (15.432)	0.003 (0.045)	-170.320** (69.229)
Relative Year: -5	-0.008 (0.005)	-7.774 (12.718)	0.035 (0.092)	-112.295* (58.786)
Relative Year: -4	-0.003 (0.004)	-0.529 (10.805)	-0.059 (0.045)	-108.963* (56.461)
Relative Year: -3	0.000 (0.005)	-15.536 (9.505)	0.015 (0.170)	-330.895*** (55.751)
Relative Year: -2	0.001 (0.004)	14.426 (9.208)	0.188 (0.504)	-258.359*** (81.732)
Relative Year: -1	0.003 (0.003)	8.604 (8.898)	-0.750 (0.498)	-468.745*** (97.074)
Relative Year: 0	0.006* (0.003)	-14.588* (8.773)	-0.331*** (0.059)	-668.176*** (80.911)
Relative Year: +1	0.014*** (0.003)	6.709 (9.302)	-0.640*** (0.066)	-1,119.947*** (86.750)
Relative Year: +2	0.021*** (0.005)	14.156 (10.232)	-0.824*** (0.093)	-1,405.149*** (99.722)
Relative Year: +3	0.027*** (0.006)	26.672** (11.299)	-0.992*** (0.085)	-1,652.110*** (,115.232)
Relative Year: +5	0.031*** (0.007)	29.826* (16.680)	-1.612*** (0.090)	-2,650.038*** (,154.146)
Relative Year: +6	0.045*** (0.009)	11.272 (24.674)	-1.595*** (0.216)	-3,229.866*** (,214.187)
Pre Avg. Effect	-0.006* (0.004)	-11.325*** (3.767)	-0.050** (0.026)	-154.140*** (18.092)
Post Avg. Effect	0.028*** (0.006)	-4.237 (18.652)	-1.166*** (0.084)	-2,080.740*** (,123.704)
Pre-treat. Avg.	0.34	1396.69	7.21	10123.42
Cities	16	16	16	16
Individuals	511,690	292,269	292,269	292,269
Observations	5,628,590	1,778,416	1,778,416	1,778,416

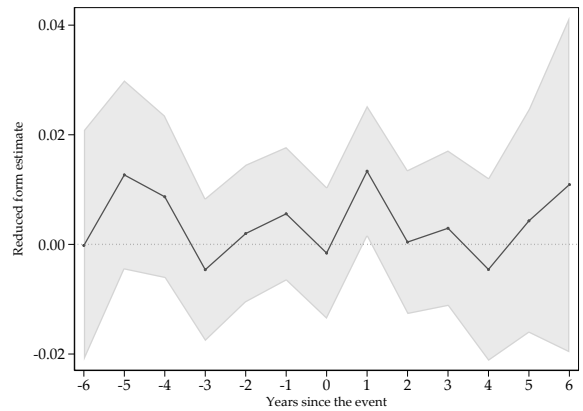
Notes: Displayed are the impacts of winning a house on labor market outcomes. (1) Formal Employment is defined as appearing in RAIS and is measured in percentage points. (2) Annual Hours Worked is total hours worked in the formal sector. (3) Hourly wages is total earnings divided by hours worked in the formal sector. (4) Annual earnings is the sum of all formal labor income in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.



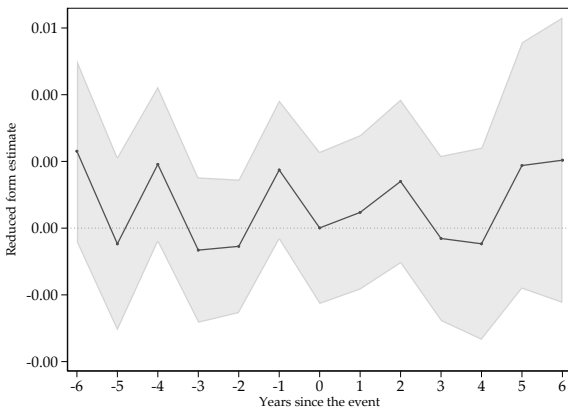
Figure 6: Mechanisms for Labor Market Changes (Two-step Heckman correction)



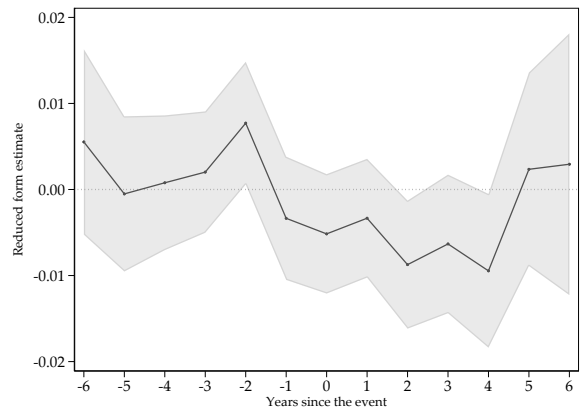
(a) Started the First Formal Job



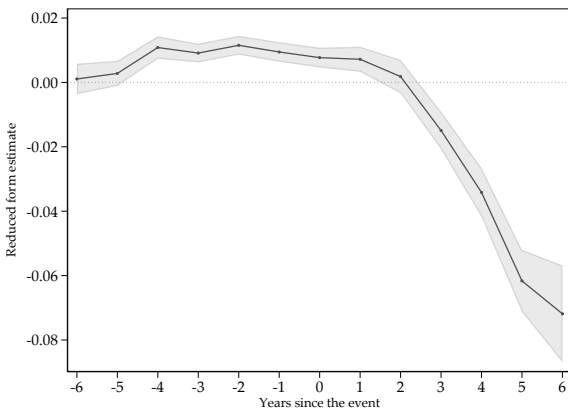
(b) Switch to a New Job



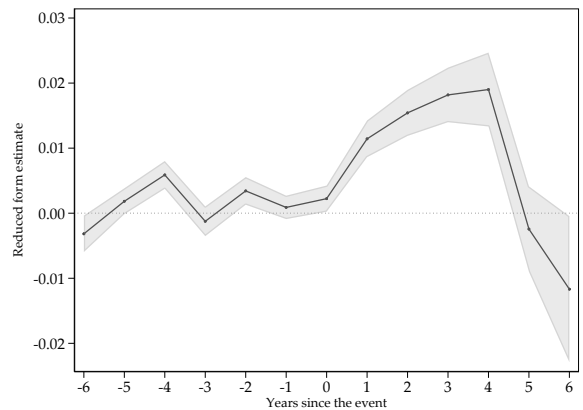
(c) Fired for Cause



(d) Left the job position



(e) Public Sector Employment



(f) Public Sector Politically Appointed Positions

Notes: Displayed are the impacts of winning a house on plausible mechanisms underlying labor market outcomes. (1) Dependent variable is equal to 1 if individual started first job in a given year; (2) Dependent variable is equal to 1 if individual switched jobs in a given year; (3) Dependent variable is equal to 1 if individual was fired in a given year; (4) Dependent variable is equal to 1 if individual quit job in a given year; (5) Dependent variable is equal to 1 if individual was working in a public sector job in a given year; (6) Dependent variable is equal to 1 if individual was working in a public sector job with discretionary appointment in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

Table 11: Mechanisms for Labor Market Changes (Two-step Heckman correction)

	Job Switch		Separation		Public Sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Relative Year: -6	0.005 (0.007)	0.000 (0.011)	0.002* (0.001)	0.006 (0.005)	0.001 (0.002)	-0.003** (0.001)
Relative Year: -5	-0.008 (0.006)	0.013 (0.009)	0.000 (0.001)	-0.001 (0.005)	0.003 (0.002)	0.002* (0.001)
Relative Year: -4	0.000 (0.005)	0.009 (0.008)	0.002 (0.001)	0.001 (0.004)	0.011*** (0.002)	0.006*** (0.001)
Relative Year: -3	0.007* (0.004)	-0.005 (0.007)	-0.001 (0.001)	0.002 (0.004)	0.009*** (0.001)	-0.001 (0.001)
Relative Year: -2	-0.005 (0.004)	0.002 (0.006)	-0.001 (0.001)	0.008** (0.004)	0.012*** (0.001)	0.003*** (0.001)
Relative Year: -1	-0.003 (0.003)	0.006 (0.006)	0.002 (0.001)	-0.003 (0.004)	0.009*** (0.002)	0.001 (0.001)
Relative Year: 0	0.004 (0.003)	-0.002 (0.006)	0.000 (0.001)	-0.005 (0.004)	0.008*** (0.002)	0.002** (0.001)
Relative Year: +1	-0.010*** (0.003)	0.013** (0.006)	0.000 (0.001)	-0.003 (0.004)	0.007*** (0.002)	0.011*** (0.001)
Relative Year: +2	-0.016*** (0.003)	0.000 (0.007)	0.001 (0.001)	-0.009** (0.004)	0.002 (0.003)	0.015*** (0.002)
Relative Year: +3	-0.027*** (0.003)	0.003 (0.007)	0.000 (0.001)	-0.006 (0.004)	-0.015*** (0.003)	0.018*** (0.002)
Relative Year: +5	-0.026*** (0.005)	0.004 (0.010)	0.002 (0.002)	0.002 (0.006)	-0.062*** (0.005)	-0.002 (0.003)
Relative Year: +6	-0.022*** (0.008)	0.011 (0.016)	0.002 (0.002)	0.003 (0.008)	-0.072*** (0.008)	-0.012** (0.006)
Pre Avg. Effect	-0.004*** (0.002)	0.011*** (0.002)	0.000 (0.000)	0.003*** (0.001)	0.005*** (0.001)	0.000 (0.000)
Post Avg. Effect	-0.019** (0.008)	0.016 (0.011)	0.002 (0.002)	-0.004 (0.004)	-0.030*** (0.006)	0.008*** (0.002)
Pre-treat. Avg.	0.04	0.31	0.01	0.07	0.07	0.01
Cities	16	16	16	16	16	16
Individuals	292,269	292,269	292,269	292,269	292,269	292,269
Observations	1,778,416	1,778,416	1,778,416	1,778,416	1,778,416	1,778,416

Notes: Displayed are the impacts of winning a house on plausible mechanisms underlying labor market outcomes. (1) Dependent variable is equal to 1 if individual started first job in a given year; (2) Dependent variable is equal to 1 if individual switched jobs in a given year; (3) Dependent variable is equal to 1 if individual was fired in a given year; (4) Dependent variable is equal to 1 if individual quit job in a given year; (5) Dependent variable is equal to 1 if individual was working in a public sector job in a given year; (6) Dependent variable is equal to 1 if individual was working in a public sector job with discretionary appointment in a given year. Standard errors are clustered at the individual level, and 95 percent confidence intervals are reported.

## C Conceptual Framework

Mechanism	Explanation	Sources	Outcomes				Further Tests
			Formal Participation	Wages	Hours Worked	Occupation	
<b>Labor Fitness</b>	Shelter and residential stability enables physical fitness for work	Meltzer and Schwartz (2016); Burgard et al. (2012); Brennan et al. (2011)		(+)	(+)	(+) physically challenging work	(-) firing
<b>Formalization</b>	Participation in public programs encourages formality	Mills et al. (2006)	(+)			(+) public sector jobs	(+) political registration (+) public programs
<b>Wealth Effects</b>	Large asset transfer affects labor-leisure trade-off	Blundell and MaCurdy (1999); Imbens et al. (2001)			(-)		
<b>Substitution Effects</b>	Housing substitute for leisure	Jacob and Ludwig (2012)			(+)		
	Housing complement for leisure	Jacob and Ludwig (2012)			(-)		

Table 12: Mechanisms for the Impact of Homeownership on Labor Market Outcomes

Mechanism	Explanation	Sources	Outcomes				Further Tests
			Formal Participation	Wages	Hours Worked	Occupation	
<b>Mobility</b>	Owning a home increases transaction cost of moving	Dietz and Haurin (2003); Coulson and Fisher (2009); Munch et al. (2006, 2008); Newman et al. (2009); Svarer et al. (2005)		(-)			(-) job switching (-) firing
<b>Relocation</b>	New neighborhood has different access to employment	Damm (2014)					
<b>Program Requirements</b>	New homeowners pay 10% of income every month	Dujardin and Goffette-Nagot (2009); Jacob and Ludwig (2012)	(-)	(-)	(-)		
<b>Credit Access</b>	A house serves as collateral for financing	Schmalz et al. (2017)	(+)			(+) new sole proprietorships	

