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do Rio de Janeiro

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Two Essays on Housing Programs and the Labor Market

Dissertação de Mestrado

Dissertation presented to the Programa de Pós–graduação em Economia da PUC-Rio in partial fulfillment of the requirements for the degree of Mestre em Economia.

Advisor : Prof. Gabriel Ulyssea
Co-advisor: Prof. Gustavo Gonzaga

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Abstract

Belchior, Carlos Alberto; Ulyssea, Gabriel (Advisor); Gonzaga, Gustavo (Co-Advisor). **Two Essays on Housing Programs and the Labor Market**. Rio de Janeiro, 2019. 94p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

This dissertation is comprised of two chapters. In the first chapter, we assess how a large public housing program in Brazil affected short and medium-run employment probability and other labor market outcomes. We use data from lotteries in Rio de Janeiro to identify these impacts. We concluded that the program increased formal employment by about two percentage points and had no effect on informal employment. Moreover, we also find evidence that receiving a house increased wages and the quality of jobs held for the treated individuals and reduced participation in other social programs. Additionally, we used reduced-form models to test the mechanisms that might explain the observed increase in employment probability. We found evidence that the mobility costs from the individual job to the provided houses is an important determinant of the impacts of the program. On the other hand, neighborhood effects, relocation from the individuals' house, migration and the distance from individuals' previous homes do not seem to be important mechanisms in explaining the effect of the program on employment. In the second chapter, we complement the previous analysis by building and estimating a static labor supply structural model. We incorporate in the model the simultaneous decision to participate in the labor market and a housing program. We use data for lotteries to help identify the parameters of the model. The lotteries data is also used to out-of-sample validation. Our estimated model is able to reproduce well both the behavior of individuals in the data used for estimation and in the experimental hold-up sample. Then, we use this model to perform policy experiments and evaluate counterfactuals.

Keywords

Public housing; Labor supply; Neighborhood effects.

Resumo

Belchior, Carlos Alberto; Ulyssea, Gabriel; Gonzaga, Gustavo. **Dois Ensaios Sobre Programas Habitacionais e o Mercado de Trabalho**. Rio de Janeiro, 2019. 94p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Esta dissertação é composta por dois capítulos. No primeiro capítulo, avalia-se o impacto de curto e médio-prazo de um amplo programa habitacional brasileiro sobre a probabilidade de emprego e outros resultados dos beneficiários no mercado de trabalho. Nós usamos dados de sorteios realizados no Rio de Janeiro para identificar estes efeitos. Concluímos que o programa aumentou a probabilidade de emprego formal em cerca de dois pontos percentuais e não teve efeito sobre a probabilidade de emprego informal. Nós também encontramos evidências de que o programa aumentou salários e a qualidade dos empregos ocupados pelos beneficiários. Adicionalmente, usamos métodos de forma-reduzida para testar a importância de mecanismos que possam explicar o efeito do programa sobre a probabilidade de emprego dos indivíduos. Encontramos evidências de que o aumento dos custos de mobilidade do trabalho dos indivíduos aos projetos habitacionais construídos é um importante determinante dos impactos do programa. Em contrapartida, efeitos de vizinhança, fricções pela realocação dos indivíduos, migração e a distância para a sua residência anterior não parecem ser mecanismos importantes para explicar os impactos do programa. No segundo capítulo, complementamos a análise anterior construindo um modelo estrutural estático de oferta de trabalho. Nós incorporamos no modelo a decisão simultânea de participar do mercado de trabalho e do programa habitacional. Os dados do sorteio são utilizados para ajudar a identificar os parâmetros do modelo e para validá-lo fora da amostra. O modelo estimado é capaz de reproduzir de forma adequada o comportamento dos indivíduos que participam dos sorteios, tanto os utilizados na estimação quanto os sorteioes mantidos apenas para sua validação. Então, utilizamos o modelo previsto para realizar experimentos de política pública.

Palavras-chave

Moradias públicas; Oferta de trabalho; Efeito vizinhança.

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List of Abreviations

ANIE – *Average Natural Indirect Effect*
BF – Bolsa Família
Cadúnico – Cadastro Único de Programas Sociais
CBO – Classificação Brasileira de Ocupações
CPF – Cadastro de Pessoa Física
FAR – Fundo de Arrendamento Residencial
IBGE – Instituto Brasileiro de Geografia e Estatística
i.i.d – *independent and identically distributed*
IPCA – Índice de Preços ao Consumidor Amplo
ITT – *Intention-to-Treat*
IV – *Instrumental Variable*
MCMV – Minha Casa, Minha Vida
MREWSL – *Maximum Relevance Weighted Simulated Likelihood*
MTE – Ministério do Trabalho e Emprego
MTO – *Moving to Opportunity*
OLS – *Ordinary Least Squares*
RAIS – Relação Anual de Informações Sociais
TOT – *Treatment on the Treated*
SEBRAE – Serviço de Apoio às Micro e Pequenas Empresas
SECEX – Secretaria de Comércio Exterior

1
Housing Programs and Labor Supply: Evidence from Lotteries in Brazil

1.1
Introduction

Public housing is one of the welfare programs which most extensively influences the life of beneficiaries. Usually, when individuals receive subsidized housing, they not only receive a wealth shock but they also move from home. Thus, the program also changes the neighborhood where individuals live, how far these individuals live from job opportunities and whom these individuals interact with.

Due to the extensive use of this kind of program by governments, the impacts of housing programs on beneficiaries, especially on their labor market outcomes, has drawn lots of attention. Considerably less attention, however, has been given to distinguishing the several potential mechanisms through which this kind of program can affect labor market outcomes.

A deeper comprehension of which mechanisms are important in determining the recipients' labor market response can not only enhance our knowledge of the employment determinants of individuals but also help to design optimal program rules and incentives.

This chapter analyzes the impacts of a large housing program in Rio de Janeiro, the *Minha Casa, Minha Vida* (MCMV) program, which provided subsidized houses for individuals in peripheral regions of the city, on short and medium run employment probabilities and other labor market outcomes of the beneficiaries. A relevant fraction of the publicly built houses subsidized by the program was allocated to individuals through lotteries. We assembled a database of these lotteries for the municipality of Rio de Janeiro from the years 2011 to 2015, which allowed us to evaluate the program consequences without first-order concerns with endogeneity problems.

We link this database for lotteries with rich administrative data - for the universe of formal labor market workers (*Relação Anual de Informações Sociais*) and with data for the Brazilian Single Registry of social programs (*Cadastro Único*) for both pre- and post-treatment periods. Thus, we were able to analyze the program effects on employment probability. We found that receiving the program increased formal employment probability by two percentage points three years after the lottery. We found no effects before this time-horizon as well as no effects on informal employment probability. Given the baseline distribution of formal and informal employment, this effect represents a 0.8 percentage point increase in employment probability.

We show that the estimated treatment effect is highly heterogeneous across individuals. The previous estimate is mainly driven by low-income men. Additionally, we show that the estimated treatment effect decreases rapidly with baseline income and flips sign and turns negative for richer men. All estimated treatment effects for women are negligible. Thus, the positive treatment effects estimated are highly dependent on the composition of the sample.

Furthermore, we tested the effect of the program on other outcomes. Receiving a house from *MCMV* also increases the mean quality of the job held, conditional on participation in the labor market. Receiving the house increases wages by 12% and generates a significant upgrade on the skill percentile of the occupation. Finally, consistent with the wealth shock and upgrade of labor market conditions, we find that the program generates a sizable reduction in the fraction of families which received *Bolsa Família* (BF) benefits, which we interpret as a decrease in individuals' social vulnerability.

The main contribution of this chapter, however, is to estimate reduced-form models to test which mechanisms are important to explain the relationship between public housing and employment. We use heterogeneities in the program, in the baseline characteristics of the individuals, the timing of treatment effects and the effects of the program on mediating variables in order to distinguish the relative importance of different potential mechanisms.

First, we test the existence of neighborhood effects in the determination of employment, which has been recently pointed out as the most important mechanism to explain the effect of housing programs' labor market outocomes (CHETTY, HENDREN and KATZ, 2016; van DJIK, 2019). The institutional framework of the program provided a randomization not only in which individuals received the treatment but also in which housing project they received the house. This allows us to test the importance of the quality of the neighborhood in employment determination.

Contrary to these previous papers, we find no evidence of neighborhood effects. Treatment effects of individuals drafted to bad neighborhoods were not significantly different from the treatment effects of individuals drafted to better neighborhoods. Since we find no evidence of neighborhood effects, we suggest and test other potential mechanisms.

Next, we test whether the distance from the housing project which was drafted in each lottery to important places in the individuals' life before the lottery is important in determining future employment probability. Specifically, we test whether estimated treatment effects vary conditionally on the distance from the housing project to the individuals' previous job and conditionally on the distance from his previous home.

The former might be important to determine employment probability because it might be difficult to hold a job that is far from your residence. The latter, on the other hand, might also be important because of mobility costs and also because being separated from their home might disrupt social networks (BARNHARDT, FIELD, and PANDE, 2016).

Then, we test whether migration is an important mechanism to explain employment probability. This might be the case because, as argued by Munch, Rosholm, and Svarer (2006, 2007), house owners are much less mobile than renters and, therefore, the *MCMV* program might make beneficiaries of the program less willing to search for jobs in other job markets. In order to test this mechanism, we evaluated the direct impact of the program on the mediating variable, migration, and then provided bounds for the subsequent effects on employment.

Finally, we test a disruption mechanism. It was previously suggested in the literature that the moving process might disrupt individuals' job routines and provoke unemployment. If this is an important phenomenon, we expect employment probability of beneficiaries to decrease relative to the control group just after the lottery and gradually return to previous levels afterward. In order to test this mechanism, we relied on the timing of the treatment effects. Specifically, we estimate monthly treatment effects just after the lottery.

Thus, we tested four alternative mechanisms to neighborhood effects through which the program might influence employment probability. Only two of them seem to be important. The first is the relative distance from the housing project to the previous job of the beneficiaries. We find that treatment effects are positive only for those individuals working closer to the housing project prior to the lottery. For those who worked far from the housing project, the treatment effect is negligible or negative and statistically significant. The program also seems to affect employment through migration since recipients migrate less than non-recipients. However, we show that the effect on employment explained by migration is limited.

We contribute to three strands of literature. First, this study relates to the previous papers that evaluate the effect of housing programs in short and medium run labor outcomes. We can divide this literature into three distinct phases. First, some articles naively compared the labor market outcomes of the beneficiaries and non-beneficiaries. These estimates were subject to endogeneity and selection problems. These papers, revised by Shroder (2002), tend to show mixed labor supply outcomes. Shroder (2002) argues that this mixed evidence is consistent with a null effect of housing programs on labor supply.

The second phase of this literature attempted to estimate the effect of housing programs on labor supply through the inter-temporal or cross-area variation in housing benefits on labor outcomes (YELOWITZ, 2001; SUSIN, 2005; OLSEN et al., 2005; NEWMAN, HOLUPKA and HARKNESS, 2009 and FU, LIAO and ZHANG, 2016). Moreover, these estimates also showed mixed results of the housing program's impact on the labor supply. As indicated by Ludwig and Jacob (2012), these estimations are subject to the bias of omitted variables correlated with both variations in housing programs participation and labor supply.

Finally, the third phase of this literature used lotteries, similar to the ones analyzed in this paper, to overcome the previous concerns with endogeneity. Mills et al. (2006) and Jacob and Ludwig (2012) both used randomizations in the supply of housing vouchers in the United States to evaluate the impact of housing programs in labor supply. They concluded that these programs reduced the labor supply of the beneficiaries. While the former estimated that the effect of the program dissipated after a few years, the latter estimated that the treated individuals showed a consistently smaller labor supply than the control group.

Simultaneous to this dissertation, Da Mata and Mation (2018) also explore the *MCMV* lotteries to estimate the causal effect of the public housing programs on formal employment probability in the context of a developing country. Furthermore, they also explore the impact of the program on other outcomes, such as the consumption of vehicles.

We contribute to this strand of the literature by not only estimating the treatment effects of the housing programs on employment probability but also extensively analyzing the relative importance of different mechanisms that might drive these treatment effects. In this context, the paper most close to ours is van Djik (2019). She also uses housing lotteries to estimate the effects of a housing program on the socio-economic consequences of beneficiaries. Consistently with the previously cited literature, she also estimates that the program has negative average treatment effects on income and employment.

However, van Djik (2019) also explores the considerable heterogeneity in the program. She shows that the negative effects of the program are exclusively driven by individuals who were drafted to worst neighborhoods than they use to live in, which she interprets as suggestive evidence that neighborhood effects are mediating the effect of the program. However, since individuals self-select into lotteries in her framework, her reduced-form analysis cannot distinguish whether this heterogeneity is due to differences in treatment or due to differences in beneficiaries.

We test the importance of neighborhood effects by taking advantage of the fact that the *MCMV* program not only randomized which individuals received treatment but also in which neighborhood the house was received. Since we find evidence that the importance of neighborhood effects is negligible, we also suggest and test other alternative mechanisms.

The second strand this chapter relates to is the recent literature which specifically studies the impact of neighborhoods on long-run outcomes of individuals. Sanbonmatsu et al. (2011), Ludwig et al. (2013) and Chetty, Hendren and Katz (2016) all analyzed the effects of the Moving to Opportunity (MTO) program on the long-run labor market outcomes of beneficiaries. Generally, these papers show that individuals who had received housing subsidies moved to better quality neighborhoods. This move had positive effects on the children, slightly negative effects on the adolescents and no effect on the adults. Furthermore, Chay (2018) analyzed the importance of neighborhood quality using demolitions of public housing in Chicago and reached similar conclusions as of the papers above. Barnhardt, Field, and Pande (2016), on the other hand, evaluated the impact of a program that offered slum dwellers in India the chance to move to better-quality residences. The authors concluded that the isolation of these new houses ruptured social networks and, in the long run, did not alter the economic outcomes of the benefited individuals.

This chapter relates to this literature because we test whether these neighborhood effects explain the relation between housing programs and employment. We analyzed how the randomizations of individuals over neighborhoods differently affected the labor market outcomes of the beneficiaries.

Finally, we make a slight contribution to the theoretical literature which investigates the impact of in-kind transfers on labor supply and how they differ from direct cash transfers. We incorporate two characteristics of MCMV, which are common to other public housing programs, in a static labor supply model: non-linearity in the budget constraint and restrictions on the consumption of the transferred good. Similar to Leonesio (1988a, 1988b) and Schone (1992), we show that we cannot predict the labor supply response of the program beneficiaries, even if we prescind from other potentially relevant effects of housing programs on labor supply, such as peer effects and life-cycle considerations.

This chapter is organized in six sections besides this introduction. In section 1.3 we provide a description of the MCMV program and the respective lotteries. Subsequently, in section 1.3 we develop a simple theoretical framework and show that we cannot predict the household labor supply effect of a program such as MCMV. Next, in section 1.4, we present our database and some descriptive statistics. In section 1.5, we present our econometric model and main results. In section 1.6, we present the mechanism analysis. Finally, in section 1.7, we conclude the chapter.

1.2
Description of the program

In this section, we provide a brief description of the *Minha Casa, Minha Vida* program. For a comprehensive description of the program, see Saporito (2015). The first phase of the *MCMV* was launched in 2009, which aimed to reduce the housing deficit through the construction and financing of one million houses across the country. Later, in 2011, the second phase of the program was launched - aiming to provide two million more houses.

The potential beneficiaries were divided into three groups according to their income: (i) individuals with no capability of affording a house; (ii) individuals with partial paying capability and (iii) individuals with full capability of paying for a house.

All three groups received housing subsidies. In this dissertation, we shall focus on the individuals in the poorest income group - those who received most of the benefits. To be eligible for the poorest group, individuals should have up to R$ 1.600,00 of monthly income (approximately 950 US$ in 2011).

The larger fraction of subsidies of the program was conceded to the poorer income group through the *Fundo de Arrendamento Residencial* (FAR). The Brazilian federal government supplied funds for the building of houses in specific locations according to the measured housing deficit in each region, while the local governments provided necessary public infrastructure. Then, civil construction enterprises presented projects to financial intermediaries (public banks) - subject to minimum criteria established at the national level. The average cost per house was restricted to approximately R$ 63.000 in Rio de Janeiro in 2011. In large cities such as Rio de Janeiro, apartments were usually provided, instead of houses and they built in peripheral regions to minimize the cost of the land.

To be eligible for the program, individuals should meet the income threshold level mentioned earlier, be registered in the Single Registry for social programs, be Brazilian, be older than 18 and should not own a home or have had access to home financing. After the lottery, the self-reported information of the drawn individuals was cross-checked against several government datasets to evaluate its accuracy.

Once the housing projects were completed, after 2011, the local governments could allocate up to half of the available houses to individuals under extreme vulnerable conditions[1]. After this initial concession, local governments would then allocate the other houses to individuals from the first income group. Six percent of these houses were reserved for the elderly and disabled individuals. If these groups exceeded the housing reservations, specific lotteries for those groups were conducted. Finally, the remaining houses were then allocated through lotteries to other individuals from the first income group[2]. Each lottery provided houses to one or a few housing projects.

Benefited individuals received between 90% and 95% of the house value as subsidies. The benefited family paid the remaining value of the house during the following ten years through monthly payments, which could not exceed 10% of their total household income. Generally, these individuals provided monthly payments of approximately R$ 50 (approximately US$ 30)[3].

Until 2015, the individuals drawn in lotteries, which involved subsidized houses in different locations, could choose which housing project to go by order of arrival. This method led to a considerable burden on the municipal bureaucracy and, after 2015, the individuals drawn in lotteries were further randomized into different regions to avoid the cost of allocation.

Thus, for these lotteries, we have an exogenous and heterogeneous treatment available. We focused on the first lottery in 2015, which was conducted in January of that year and randomly assigned individuals to six different housing projects across three different neighborhoods.

1.3 Theoretical model

In this section, we show that even under strong assumptions, we cannot generally predict the effect of a housing transfer on labor supply in a static model. We assume that both leisure and housing are normal goods

[1]Individuals were considered under this condition if they lived in dangerous areas or were reallocated due to ambient or government necessities.

[2]Elderly and disabled individuals who did not win in the specific lotteries could participate again in the general lottery.

[3]Later the maximum monthly payment was reduced to 5%.

and disregard several mechanisms that might otherwise affect the labor supply as a consequence of a housing program, such as peer effects, distance from jobs and neighborhood characteristics. Moreover, we also neglected any life-cycle effect associated with the labor supply decision.

This conclusion is contradictory to the well-established result of the static labor supply literature that the receipt of an unconditional cash transfer should reduce labor supply (BLUNDELL and MACURDY, 1999). This effect happens because an increment in income increases leisure (assuming it is a normal good), which mechanically generates a decrease in labor supply.

A housing transfer, on the other hand, not only affects the individuals' budget constraint but also increases the consumption of specific goods. Thus, the impact on labor supply depends on the complementarity relation between housing consumption and leisure. Since this relation is not *a priori* defined, the effect is ambiguous.

Thus, the program's impact on labor supply is an empirical question. Although we include several characteristics in the model which are specific to the MCMV program, we obtain results similar to Leonesio's (1988) and Schone's (1992). Furthermore, we also strongly rely on the results of the conditional demand systems provided by Neary and Roberts (1980).

Suppose an individual consumes leisure (l), houses (H) and a composite of other goods (x). Additionally, suppose that these individual preferences can be represented by a continuous, quasi-concave and twice differentiable utility function:

$$u = u(l, H, x) \tag{1-1}$$

Let T be the total available time for the individual, so that the labor supply may be represented by $h = T - l$, and V is the non-market income. We can represent the budget constraint as follows:

$$xp = V + wh \tag{1-2}$$

where p is the price of the composite good, and w is the market wage.

Now, consider a program such as the MCMV. As previously described, the beneficiaries receive a house and must dedicate a fraction of their monthly earnings to pay the non-subsidized fraction of the transfer. Let $\overline{H}$ be the amount of the house transferred and t the fraction of lost earnings. Therefore, the transfer generated by the program is as follows:

$$H^* = \overline{H}q - twh \tag{1-3}$$

where q is the price of the house, which alters the budget constraint to the following:

$$xp = V + \overline{H}q + (1 - t)wh \tag{1-4}$$

This transfer is not equivalent to an unconditional cash transfer for two reasons. First, an individual cannot sell the transferred house. Thus, the program alters the budget constraint but also restricts the consumption of the housing good. Second, there is an upper limit to the income for an individual to participate in the program, which creates a non-linearity in the budget constraint.

Hence, the individual problem can be expressed as follows:

$$\begin{aligned} &\max_{x,l} u(x,l,H) \\ s.t \quad & xp = V + q\overline{H} + (1-t)wh \\ & H = \overline{H} \\ & h \leq \overline{h} \end{aligned}$$

where $\overline{h}$ is the upper limit for the labor supply that is consistent with the eligibility in the program. We will first assume that the third restriction is not binding and consider the opposite case later.

To analyze the previous problem, it is useful to rely on the results of the rationing theory. In this context, the definition of virtual price is fundamental. The virtual price (q^*) is the one which would lead the individual to choose $\overline{H}$ in the absence of any restriction imposed by the program.

Theorem 1: *Let $(\tilde{x}, \tilde{l})$ be the vector of chosen goods in the presence of the restriction $H = \overline{H}$ and prices (p,w). Thus, (p, w, q^*) supports the consumption bundle $(\tilde{x}, \tilde{l}, \overline{H})$.*

Proof. See Appendix 3.1.

□

Next, it is useful to consider the conditional expenditure function $\overline{e}(p, w, q, \overline{H}, U)$. This function shows the minimum expenditure necessary to attain a utility level U under the prices p, w and q when the individual is restricted to consume $\overline{H}$. Note that:

$$\overline{e}(p, w, q, \overline{H}, U) = p\overline{x}^c(p, w, q, \overline{H}, U) + w\overline{l}^c(p, w, q, \overline{H}, U) + q\overline{H}$$

where $\overline{x}^c(.)$ and $\overline{l}^c(.)$ are the conditional Hicksian demand functions for the composite good and leisure. Using the definition of virtual price and Theorem

1 we can write the following:

$$\overline{e}(p,w,q,\overline{H},U) = px^c(p,w,q^*,U) + wl^c(p,w,q^*,U) + qH^c(p,w,q^*,U) = \\ = e(p,w,q^*,U) + (q-q^*)\overline{H} \qquad (1\text{-}5)$$

Now, consider the conditional Hicksian function of the labor supply:

$$\overline{h} = \overline{h}^c(p,w,q,\overline{H},U) \qquad (1\text{-}6)$$

Totally differentiating equation (1-6) and setting $dp = 0$, we have

$$d\overline{h} = \overline{h}^c_w dw + \overline{h}^c_{\overline{H}} d\overline{H} + \overline{h}^c_U dU \qquad (1\text{-}7)$$

Using the envelope theorem we can re-write equation (1-7) as follows:

$$d\overline{h} = -\overline{e}_{ww} dw - \overline{e}_{w\overline{H}} d\overline{H} - \overline{e}_{wU} dU \qquad (1\text{-}8)$$

Similarly, totally differentiating the conditional expenditure function and using both the envelope theorem and equation (1-5), we obtain the following:

$$d\overline{e} = \overline{e}_w dw + \overline{e}_{\overline{H}} d\overline{H} + \overline{e}_U dU \implies \overline{e}_U dU = d\overline{e} + \overline{H} dw - (q-q^*) dH \qquad (1\text{-}9)$$

Using duality relations and replacing equation (1-9) in equation (1-8), we reach a conditional version of the Slutsky equation:

$$d\overline{h} = -\overline{e}_{ww} dw - \overline{e}_{w\overline{H}} d\overline{H} + \overline{h}_V \left[dV + \overline{H} dw - (q-q^*) d\overline{H} \right] \qquad (1\text{-}10)$$

The previous equation provides a decomposition of the labor supply changes in terms of conditional income, a substitution effect and a direct effect of the restriction on labor supply. Despite that, it is difficult to analyze equation (1-10) and is useful to model this equation in terms of the unconditional expenditure and demand functions. Consider the following theorem.

Theorem 2: *We may write the conditional version of the Slutsky equation as:*

$$d\overline{h} = -\left[e_{ww} - \frac{e_{wq}^2}{e_{qq}} \right] dw - \frac{e_{wq}}{e_{qq}} d\overline{H} + \left(h_v + \frac{e_{wq}}{e_{qq}} H_V \right) \left[dV + \overline{H} dw - (q - q^* d\overline{H}) \right]$$

Proof. See Appendix 3.1. □

Next, we can use Theorem 2 to analyze the effect of a program such as MCMV on labor supply. Set $dw = -tw$, $dV = \overline{H}q$ and $d\overline{H} = \overline{H}$, so that:

$$d\overline{h} = \underbrace{\left[e_{ww} - \frac{e_{wq}^2}{e_{qq}}\right] tw}_{(a)} \underbrace{-\frac{e_{wq}}{e_{qq}}\overline{H}}_{(b)} + \underbrace{\left(h_v + \frac{e_{wq}}{e_{qq}} H_V\right)\left[\overline{H}(q^* - tw)\right]}_{(c)} \tag{1-11}$$

The term (a) is the pure substitution effect generated by the program. Since the market wage is discounted once the individual chooses to participate in the program, this generates a disincentive for the labor supply. Therefore, this term must be negative[4].

The term (b) represents the direct effect of the restriction on the labor supply, which sign is ambiguous, and it depends on the relationship between housing consumption and leisure. If these goods are Hicks-Allen (H-A) complements, e_{wq} will be negative and so will be the term (b). Conversely, if leisure and houses are H-A substitutes, the term will be positive.

Lastly, the term (c) is the income effect of the program, the sign for which is also ambiguous. Suppose that leisure and housing are both normal goods. Then, if these goods are H-A substitutes, the term (c) will also be negative. Contrastingly, if they are H-A complements, then this term's sign is ambiguous.

Therefore, if housing and leisure are H-A substitutes we will then observe two negative terms and a positive one. Consequently, the net effect on the labor supply is ambiguous. On the other hand, if housing and leisure are H-A complements, we will observe two negative terms and an ambiguous one. Nonetheless, even if the term (c) is positive, its magnitude cannot surpass the other two. So, the net effect on the labor supply would certainly be negative (Leonesio, 1988).

Thus far, we saw that we could not theoretically predict the impact of MCMV program on labor supply. Until now, we assumed that the upper restriction on the labor supply is not binding. Now, let's briefly consider this additional restriction. First, suppose the program negatively affects labor supply. Then, a beneficiary would not be affected by the restriction.

Next, suppose that the program would have a positive effect on the individual labor supply. Then, if the restriction is binding after the transfer, the effect of the program on the labor supply would be smaller than the effect in the absence of this restriction. In the limit, if the restriction were already binding before receiving the program, then the program would not affect labor supply. Therefore, considering this restriction does not significantly affect our previous conclusion.

[4]Deaton and Muellbauer (1980) depicted this formally using the envelope theorem.

1.4 Data

We draw information from four different Brazilian sources: a database from the lotteries of the *Minha Casa, Minha Vida* program, the Single Registry of Brazilian Social Programs (Cadúnico), Brazil's Annual Social Information Report (RAIS) and the Census data. In this section, we describe each source and provide some descriptive statistics and balancing tests.

1.4.1 Lotteries

We assembled data from lotteries for *Minha Casa, Minha Vida* program. We compiled data for the Rio de Janeiro municipality from 2011 to 2015. In this period, there were seven lotteries: three in 2011, one in 2012, two in 2013 and one in 2015.

This data was obtained from the municipal housing secretary. For each lottery, we have the names and a time-invariant identifier - the *Cadastro de Pessoa Física* (CPF) - for all individuals who participated and those who won. Furthermore, for each lottery, we have information on where the housing project was built.

Some of these individuals who won the lottery did not receive the benefits. This might have happened because these individuals missed important deadlines, self-reported certain information incorrectly or were no longer interested in receiving a house. The rules of the program stipulated that the individuals who won the lottery but did not receive a house immediately return to the set of potential beneficiaries and participate in the next lotteries. Therefore, we can easily infer the individuals who received treatment as the individuals drawn in a given lottery who did not participate in the next one.

1.4.2 Single Registry (CadÚnico)

We also use confidential information from the Single Registry of Social Programs[5]. We obtained information for the years 2012 to 2017. This database is composed of updates provided by the individuals to this government registry. In each year, the Single Registry data is the stock of the most recent updates for all individuals who registered since 2003.

The CPF identifying information is also available in this database. Unfortunately, this information is not reported (or reported with error) for

[5]We thank the Ministry for Social Development for the concession of this database, according to the process 71000.000372/2018-11.

a significant fraction of the individuals who participate in the lotteries. Hence, we cannot find all individuals in the Single Registry data. We keep in our final sample only individuals whom we could find in the Single Registry and from whom we have at least one available update before and one after the lottery. Details of the matching of the Single Registry data with the information from lotteries is given in Appendix 3.2.1. In this appendix, we also show a more detailed description of the several stages of the matching process and their impact on this study sample size.

Matching these databases generates a considerable problem of attrition. We discard a significant portion of the individuals who originally participated in these lotteries due to misreporting in the Single Registry. This attrition may generate three important problems. First, it is possible that the attrition is different for the treatment and control groups, which would introduce selection problems into our analysis. However, we show later in this section that this is not the case.

Second, the reduction in the sample size decreases the power of our estimates. We consider this to be a minor drawback since the original number of participants in the lotteries is very high. Thus, even with the high attrition in the matching of these databases, we still maintain a very broad final sample. As we will see in the subsequent sections, we have the power to detect several important significant results.

Third, since we are restricting our analysis to a subsample of the original data, all results presented below are valid only for this subsample and not for the totality of individuals who participated in the lottery. Note, however, that the original data was not representative of eligible individuals or the participants of the *MCMV* program in other cities. Thus, the results for this restricted sample do not seem to be intrinsically less interesting than the results for the original sample[6].

Despite these drawbacks, matching these datasets allows us to obtain very rich information for the lotteries' participants and winners. We draw several individual socio-demographic and labor market characteristics as well as household characteristics. Specifically, we use the following variables at the individual level: gender, race, education, wages, whether the individual worked the previous month, number of months worked in the last year and whether the main work of the individual is informal. We use the following variables at the household level: average monthly income, number of rooms, rent, number of components in the family and the home address. A detailed description of

[6]In fact, some characteristics of the restricted sample, such as formal employment, are closer to the Brazilian average than the original sample of lottery participants.

the variable definition can be found in Appendix 3.2.2.

Furthermore, the Single Registry provides information of the family members of each individual. Thus, we could identify the family members of the participants in the lottery, who may or may not also have participated in the lottery. Thus, we are able to form a panel of families who have at least one member participating in the lotteries.

1.4.3 Linked employer-employee data (RAIS)

We also use information from Brazil's Annual Social Information Report (RAIS) from 2009 until 2016. All formal firms in Brazil are required to fill information regarding their workers annually, and the Brazilian ministries use this information for unemployment insurance and other social programs, including those conceded by the MCMV program. The Labor Ministry (MTE), which administrates this information, applies fines on firms that deliver incomplete or incorrect reports[7].

We use a restricted access version of the database - where the CPF identifier is also available. This variable allows us to merge this information with the data from the housing lotteries and the Single Registry. Since RAIS gathers data from all formal contracts in one year, some individuals appear multiple times in the same year. When this is the case, we keep only the contract with the higher total annual wages.

From this source, we draw precise information on which individuals participated in the formal labor market as well as contract characteristics. We use this data as complementary information on the individuals who participated in the formal labor market. We use the following variables that are available only in this data: address of the firm where the individual worked and occupation held, which we use to create a rank based on the skill of each occupation, similar to Autor and Dorn (2013). We present a detailed description of the variables used in Appendix 3.2.2. This data, unlike the Single Registry, provides high-frequency information on the individuals. In the Single Registry, we have information only for the time of the update. RAIS, on the other hand, provides monthly information on the individuals' contracts.

Some variables were also available in both RAIS and the Single Registry, such as participation and wages in the formal labor market. Whenever this is the case, we opt for utilizing the information from RAIS which is more likely to be precise.

[7] We also merge this data with information from the Secretary of Foreign Trade (SECEX). From this alternative source, we draw information on exports and imports of firms, which we also use to test the balance of treatment and control groups.

Moreover, RAIS also provides information on the firms where the individuals worked. Particularly important to us is the availability of the firm's address. We georeferenced all these addresses and calculated the time of travel for each through the housing project. We proceeded in a similar way with information from the individual's home, which comes from the Single Registry. Details on this process are given in Appendix 3.2.3.

1.4.4 Census

Finally, we use data from the Brazilian Census of 2010. This data is produced once every ten years by the Brazilian Institute for Geography and Statistics (IBGE). This data provides basic information on every household in the country and more detailed information on a representative sample of households.

We are interested in the information on the census ponderation areas. IBGE defines these regions as the sum of a few census tracts with approximately 5.000 households. This is the smallest area where we have representative information in the sample questionnaire. In very dense areas, such as Rio de Janeiro, the census ponderation areas are usually relatively small and encompass lesser area than a neighborhood and follow its geographical delimitations.

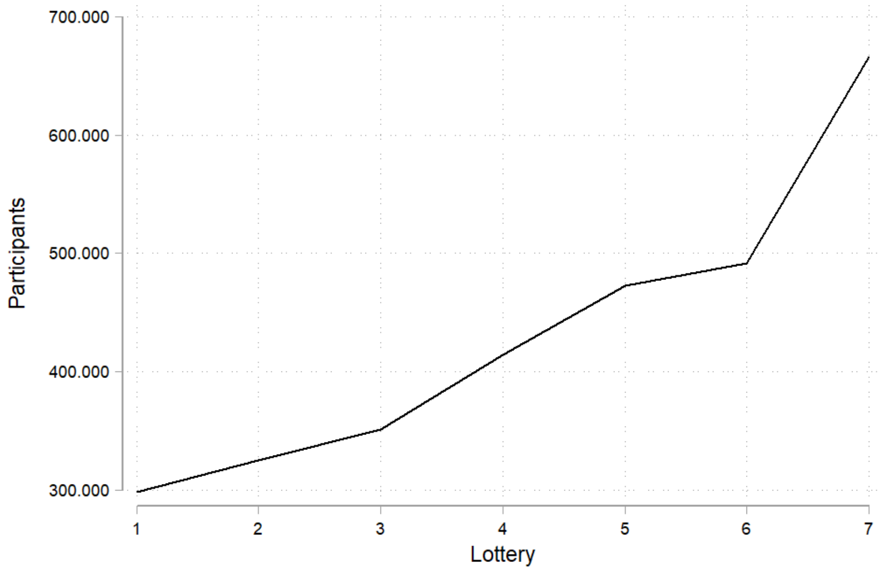
We gathered baseline information on the regions where the housing projects were built. Following Chetty, Hendren, and Katz (2016), we collected information on mean per capita income, mean education level, the share of single-parent households and the share of adequate households. These variables are used to characterize the neighborhood characteristics where the individuals were sent. A detailed description of the variables can also be found in Appendix 3.2.2.

1.4.5 Descriptive statistics

In this subsection, we first describe some general characteristics of the lotteries before describing the characteristics of the individuals who participated in them. Furthermore, we also present balancing tests that support the hypothesis that lotteries were successful in balancing the characteristics of the treatment and control groups.

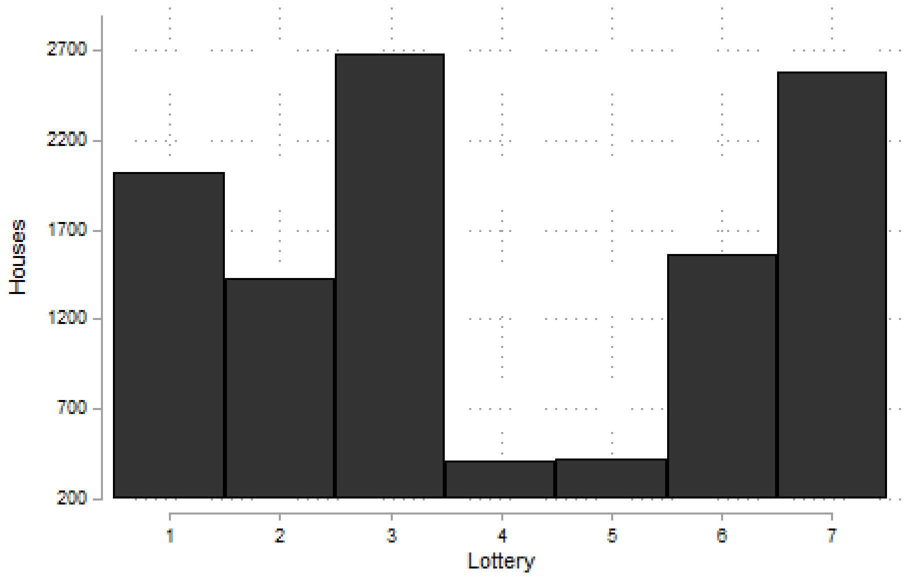
As previously mentioned, we assembled data for seven lotteries. Figure 1.1 presents the number of participants in each lottery. Figure 1.2 presents the number of houses available for each lottery.

Figure 1.1: Number of candidates per lottery



Note: Number of individuals registered in the Single Registry that participated in each lottery.

Figure 1.2: Number of houses per lottery



Note: Number of houses provided in each lottery.

Next, we present visual evidence on the geographical distribution of the offered housing projects in Figure 1.3. As previously mentioned, the houses offered by the program were predominantly built in poorer regions of Rio de Janeiro, away from the center of the city.

Figure 1.3: Geographical distribution of housing projects

Note: Map for the Rio de Janeiro city. Each red dot represents a housing project built by the *MCMV* program in Rio de Janeiro.

We present some characteristics of the individuals who took part in the lotteries and were found in the Single Registry. We also perform some balancing tests. Specifically, we estimate the following equation:

$$y_{il}^{b} = \beta_0 + \beta_1 * T_{il} + \alpha_l + \epsilon_{il} \quad (1\text{-}12)$$

where y_{il}^{b} is the variable of interest of individual i in lottery l in the baseline period, T_{il} is a dummy for individuals who won the lottery l and α_l is a fixed-effect for lotteries. If the lotteries were successful in randomizing the treatment, we would expect that the coefficient of the treatment effect (β_1), which measures the differences between the treatment and control groups within each lottery, not to be statistically different from zero. Throughout the chapter, we will present clustered standard-errors at the lottery-level and will use 95% confidence interval to conduct inference.

Table 1.1 presents the means of several variables of interest for our control group and our coefficient of interest estimated from the equation above. The first panel of variables presents information on whether the individuals are present in our database. The second-panel focuses on individual-level variables and the third panel of Table 1.1 focuses on household level variables.

Table 1.1: Balancing test and descriptive statistics

	Control mean	Difference
I. Attrition		
Individuals found	0.160	0.010
	(0.365)	(0.012)
Number of updates	3.057	0.028
	(1.032)	(0.041)
Months after the lottery	26.391	-1.378
	(15.981)	(1.231)
II. Individual Characteristics		
Male	0.120	0.018
	(0.325)	(0.011)
White	0.258	0.015
	(0.438)	(0.015)
Age	40.215	0.100
	(11.139)	(0.278)
Education	4.987	0.136
	(1.737)	(0.058)
Months worked	4.043	0.649
	(5.199)	(0.362)
Informal	0.302	0.001
	(0.460)	(0.015)
Informal wages	273.218	58.845
	(0.202)	(34.845)
III. Family Characteristics		
Family income	137.129	43.744
	(2939.292)	(28.755)
Number of rooms	3.855	0.104
	(1.454)	(0.051)
Rent	76.653	2.350
	(157.517)	(2.325)
Number of components	3.305	-0.208
	(1.834)	(0.135)
Months before the lottery	-15.715	0.830
	(14.668)	(2.410)
Distance to the housing project	96.923	-13.872
	(379.044)	(8.538)

Note: Means of variables for our control group. Treatment effects were estimated regressing by OLS, each variable in a dummy for treatment and a lottery fixed-effect. Clustered standard errors at the lottery level in parenthesis. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

First, we observe that the fraction of individuals found in the Single Registry is not significantly different in the treatment and control groups. The number of updates and the time after the lottery when the updates occur are also not significantly different between groups. These results suggest no issues with attrition. Moreover, Table 1.1 also shows that the treatment effect is not significantly different from zero for any of our variables of interest.

All variables provided come from the Single Registry data. Furthermore, we also analyzed the baseline characteristics of the contracts of those individuals who held jobs in the formal labor market and their firms using data from the RAIS. Additionally, the lottery was also successful in balancing these characteristics. These additional descriptive statistics are shown in Appendix 3.4.1.

1.5 Empirical assessment

In this section, we describe our empirical strategy and present our main results.

1.5.1 Empirical strategy

First, we estimate "intent-to-treat" (ITT) effects using the following regression:

$$y_{ilt} = \beta_0 + \sum_{t=1}^{3} \beta_t^{ITT} * T_{ilt} + \boldsymbol{X}_{il}\boldsymbol{\beta} + \alpha_l + \alpha_t + \epsilon_{ilt} \tag{1-13}$$

where y_{ilt} is our variable of interest for individual i, who participated in lottery l and t years after the lottery happened. As expressed in equation (1-12), T_{ilt} is a dummy that indicates individual i won the lottery l, t years after the lottery, and furthermore, $\boldsymbol{X}_{il}$ is a vector of control variables for individual i before the lottery l.

Thus, β_t^{ITT} identifies the causal effect of being offered a house through the MCMV program t years after the lottery. We estimate this parameter for the first three years after the lottery. As discussed earlier, not all individuals who win a lottery end up actually receiving the treatment. Hence, the ITT effect attenuates the causal effect of actually receiving a house.

Next, we estimate the effect of the "treatment on the treated" (TOT). We cannot simply estimate the impact of receiving treatment on our variable of interest because, conditional on being drafted, receiving treatment is no longer an exogenous variable.

Therefore, we estimate the following equation using two-stage least squares:

$$y_{ilt} = \beta_0 + \sum_{t=1}^{3} \beta_t^{TOT} * Treat_{ilt} + \boldsymbol{X}_{il}\boldsymbol{\beta} + \alpha_l + \alpha_t + \epsilon_{ilt} \tag{1-14}$$

where $Treat_{ilt}$, which indicates that the individual has received a house is instrumented by being drafted in lottery l (T_{ilt}). Under the hypothesis that being drafted only affects the variables of interest through the house receipt and monotonicity, β_t^{TOT} identifies the causal effect of receiving the program t years after receiving the lottery (ANGRIST, IMBENS and RUBIN, 1996).

We will estimate the ITT and TOT effects for three time-horizons: one, two and three years after the lottery. Our variables of interest are formal employment, informal employment, received wages, skill percentile of the occupation and participation in the *Bolsa Família* program.

We have fifteen different hypothesis tests in our main results. Therefore, the probability of rejecting at least one true null hypothesis by chance (the family-wise error rate) is much greater than the established confidence level. In order to deal with this problem of multiple hypothesis and clustering simultaneously, we use the method proposed by Jones, Molitor, and Reif (2018). Details for the inference method are provided in Appendix 3.3.

1.5.2 Effects of the program on employment probability

We present the main estimates for our primary variables of interest. In Table 1.2, we show our ITT and TOT estimates of the impact of the program on employment probability. We present results for three time-horizons.

Table 1.2: Employment probability impacts of the program

	ITT	TOT
	(1)	(2)
Panel A: Formal employment probability		
First year	0.010	0.015
	(0.007)	(0.013)
Second year	0.020	0.034
	(0.008)	(0.023)
Third year	0.012*	0.020*
	(0.006)	(0.010)
Panel B: Informal employment probability		
First year	-0.019	-0.023
	(0.011)	(0.012)
Second year	0.021	0.031
	(0.015)	(0.017)
Third year	-0.013	-0.021
	(0.011)	(0.022)

Note: Column (1) presents ITT estimates of the MCMV program on formal and informal labor supply by OLS. Column (2) presents TOT estimates of the program impact, obtained estimating equation (1-14) via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates include individual covariates at the baseline period. Clustered standard-errors in parenthesis. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

We found positive treatment effects of the program on formal employment three years after the program. Treated individuals increase their formal employment by 2.0 percentage points. Given the baseline meployment distribution, these results suggest an increase in aggregate employment probability of approximately 0.8 percentage points. We do not find significant treatment effects before this time-horizon. Additionally, we do not find any significant treatment effects on informal employment on any time-horizon.

The dependent variable in panel A of Table 1.2 comes from RAIS since it is more likely to be measured accurately. This variable, however, is also available at the Single Registry data. We also estimated the previous model using the alternative as a dependent variable and obtained similar results. Additionally, we re-estimated these previous regressions using alternative definitions of informal employment and also obtained similar results. These additional results are shown in Appendix 3.4.2.

Furthermore, since a house is a public good within each family, it is possible that the effects of the program are not restricted to the individuals who participate in the lottery. The effects of the program may spillover to other family members. We provide additional estimates on these effects in Appendix 3.4.3.

1.5.3 Heterogeneous effects on employment probability

Next, we try to uncover heterogeneity patterns in the previous results. Since we found statistically significant treatment effects only for formal employment after the third year, we decompose treatment effects only for this variable and time-horizon.

First, we estimate our ITT and TOT parameters separately for men and women. These results are displayed in Table 1.3:

Table 1.3: Heterogeneous effects on employment probability by gender

	ITT	TOT
Panel A: Men		
Treatment effect	0.043**	0.078*
	(0.011)	(0.038)
Panel B: Women		
Treatment effect	-0.0001	-0.0002
	(0.005)	(0.009)

Note: Each column represents the impacts of the housing program on formal employment three years after the lottery decomposed by gender. Clustered standard-errors in parenthesis. * $p<0.05$. ** $p<0.01$, *** $p<0.001$.

We can see from Table 1.3 that the relatively modest treatment effect shown in Table 1.2 is very different for men and women. While we estimated large increases in the program on men's formal employment probabilities, between 4.3 and 7.8 percentage points, we found no such effect on women's formal employment probabilities.

Next, we estimate treatment effects for each quintile of baseline family income. Results are displayed in Table 1.4:

Table 1.4: Heterogeneous effects on employment probability by income

Quintiles	ITT	TOT
First	0.046*	0.084
	(0.014)	(0.049)
Second	0.032	0.059**
	(0.013)	(0.021)
Third	0.035	0.043*
	(0.016)	(0.021)
Fourth	0.006	0.011
	(0.030)	(0.052)
Fifth	-0.015	-0.025
	(0.010)	(0.025)

Note: Each column represents the impacts of the housing program on formal employment three years after the lottery decomposed by gender. Clustered standard-errors in parenthesis. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

We can see that the effect is larger for poorer individuals and decreases with baseline income. In fact, for the highest quintile, the estimated treatment effects actually flip sign, despite not being statistically significant. This section suggests that the impacts of the program are highly heterogeneous and that the local average treatment effect estimated in Table 1.2 is likely to be dependent on the initial composition of the sample.

1.5.4 Other labor market outcomes

Next, in Table 1.5, we show the impacts of the program on the quality of jobs held by individuals, measured by the log of wages and the skill percentile of the occupation held. Furthermore, we also show the results for the impact of the program on the receipt of the *Bolsa Família* program, which we take as a proxy of individual vulnerability, such as in Ludwig and Jacob (2012).

Table 1.5: Impacts of *MCMV* on other outcomes

	ITT	TOT
	(1)	(2)
Dependent Variable: ln(Wages)		
First year	0.080	0.121
	(0.041)	(0.073)
Second year	-0.029	-0.042
	(0.015)	(0.023)
Third year	0.080**	0.125*
	(0.017)	(0.058)
Dependent Variable: Skill percentile		
First year	0.930	1.401
	(0.530)	(0.940)
Second year	0.358	0.615
	(0.474)	(0.776)
Third year	0.540	0.937*
	(0.332)	(0.447)
Dependent Variable: Participation BF		
First year	-0.007*	-0.010***
	(0.002)	(0.0001)
Second year	-0.030*	-0.044*
	(0.012)	(0.019)
Third year	-0.061*	-0.099*
	(0.020)	(0.041)

Note: Column (1) presents ITT estimates of the MCMV program on wages, skill percentile and participation on *Bolsa Família* program, via OLS. Column (2) presents TOT estimates of the program impact, obtained estimating equation (1-14) via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates include individual covariates at the baseline period. Clustered standard-errors in parenthesis. * p<0.05, ** p<0.01, *** p<0.001.

Next, we evaluate the effect of the program on the quality of jobs acquired by the individuals. We observe no effects on the first two years after the lottery, just as the employment probability. In the third year, we observe a significant increase in wages and the skill percentile of individuals.

Our results suggest an increase of 12.5% in wages, once we account for the fact that not all individuals actually receive treatment. Furthermore, we also estimate that three years after the lottery the individuals held more qualified jobs, with a one percentile higher skill index. These results suggest that the receipt of the *MCMV* program increases the quality of jobs.

It should be noted that we only observe the quality of jobs for formally employed individuals. Thus, we aggregate two effects - a change on the quality of jobs of already employed individuals and a selection effect, which is driven by individuals who enter the labor market as a result of the program. We consider it is reasonable to assume that individuals who enter the labor market as a result of the program (and who would not work otherwise) have smaller shadow-wages. Therefore, if the employment statuses were kept constant, we would estimate an even higher effect of the treatment on job quality.

The effect of the *MCMV* program on the receipt of the BF program is markedly different. We observe a small decrease in participation of about 1 percentage point in the first year after the lottery. This effect intensifies over time, reaching 4.5 percentage points after two years and about 10 percentage points after three years. The reduction is consistent with the wealth shock provided by the program as well as the higher participation rates and the higher quality of jobs held.

1.6 Mechanism analysis

In the last section, we provided evidence that the housing program affected the employment probability of individuals. In this section, we investigate which mechanisms are important to explain this effect.

In order to disentangle the relative importance of different mechanisms, we explore treatment heterogeneities, ex-ante heterogeneities of individuals and the effects on mediating variables. We will discuss the particular methodology used to test each mechanism below. In Appendix 3.4.4, we also use the timing of the treatment effects to evaluate one other mechanism - a disruption effect.

Intending to sum-up our analysis, we will focus on only one dependent variable. We will focus on employment since this is our main variable of interest. We combine information from the RAIS and the Single Registry and create a variable of total employment (formal or informal) three years after each lottery[8]. All results depicted use this dependent variable.

[8]For one of our lotteries, the last one, which was conducted in January 2015, we do not have available information three years after the lottery. In this case, we only use information from the Single Registry for the last three months of 2017. Then, we were able to analyze individuals two years and nine months to two years and eleven months after the lottery.

1.6.1 Neighborhood effects

In this subsection, we will test whether neighborhood quality affects the individual's employment probability. For this, we leverage random treatment heterogeneity provided by the program. We focus only on the first lottery of 2015 since, as previously described, this lottery randomly assigned which individuals would be treated and, then, further randomized drawn individuals into six different housing projects over three different neighborhoods.

Aiming to test the importance of the neighborhood quality mechanism, we first rank the three neighborhoods where the individuals were offered houses (details of this ordering are presented in Appendix 3.2.4). These six housing projects were offered in a peripheral region of the city but there is still considerable heterogeneity among them. Considering the average of adequate households, our broadest measure of neighborhood quality, moving from the worst to the best of these neighborhoods implies moving to the eighth to the thirtieth percentile of neighborhood quality.

Then, we estimate the following regression:

$$y_i = \beta_0 + \sum_{j=1}^{3} \beta_j * T_{ij} + \epsilon_i \tag{1-15}$$

where T_{ij} indicates that individual i was assigned to neighborhood j. Let neighborhood 1 be the one with the best quality and neighborhood 3 be the one with the worst quality. Therefore, our research hypothesis is that the treatment effect for individuals randomized to neighborhood 1 is greater than the treatment effect for individuals randomized to neighborhood 2 and that the latter is greater than the treatment effect of individuals randomized to neighborhood 3.

We model our null hypothesis as the opposite of our research hypothesis, that is:

$$H_0 : \{\beta_2 > \beta_1\} \cup \{\beta_3 > \beta_2\}$$

This can be characterized as an intersection-union test. We estimate equation (1-15) using OLS and, after that, use one-sided t-tests to evaluate each part of the null hypothesis separately. Then, we will reject the null hypothesis if, and only if, we reject the two separate hypothesis. Note that if we evaluate both separate hypotheses with a size α test, then our global test will be a level α test (BERGER and HSU, 1996). Here, as in all other estimates of this chapter, we employ a 5% size test.

Let $\boldsymbol{\beta}$ be the vector of the estimated parameters and $p_a(\boldsymbol{\beta})$ and $p_b(\boldsymbol{\beta})$ the p-values of each of the separate hypothesis. We can state the p-value of the null hypothesis in the following manner:

$$p(\boldsymbol{\beta}) = \max\{p_a(\boldsymbol{\beta}), p_b(\boldsymbol{\beta})\} \tag{1-16}$$

Table 1.6 presents the estimation of equation (1-15) and p-values calculated according to equation (1-16):

Table 1.6: Labor supply impacts with heterogeneous treatment

Treatment 1	-0.012
	(0.023)
Treatment 2	0.036
	(0.033)
Treatment 3	0.0164
	(0.064)
Aggregate treatment	0.026*
	(0.013)
Global hypothesis (p-value)	0.764

Note: ITT estimates of equation (1-15) and treatment effects for different neighborhoods ordered for their general quality. p-value for our null hypothesis calculated according to equation (1-16). Robust standard errors in parenthesis. * p<0.05, ** p<0.01, *** p<0.001.

We can see from Table 1.6 that we cannot reject our null hypothesis. Therefore, we find no support for the hypothesis that neighborhood quality is an important mechanism behind the short and medium-run employment probability. The absence of significant neighborhood effects also does not seem to be due to the lack of statistical power. Once we aggregate all treatments, we estimate a statistically significant and positive treatment effects. Finally, these results does not seem to be driven by differences in the compliers with each particular treatment. We show descriptive statistics for each group of compliers in Appendix 3.2.5.

1.6.2 Location effects

In this subsection, we test whether the distance from the individuals' home and job to the housing project is important in determining employment probability. These distances might influence employment probability because of mobility costs. Barnhardt, Field, and Pande (2016) also argue that housing projects that take individuals far from home might also affect employment probability because it disrupts social and family networks.

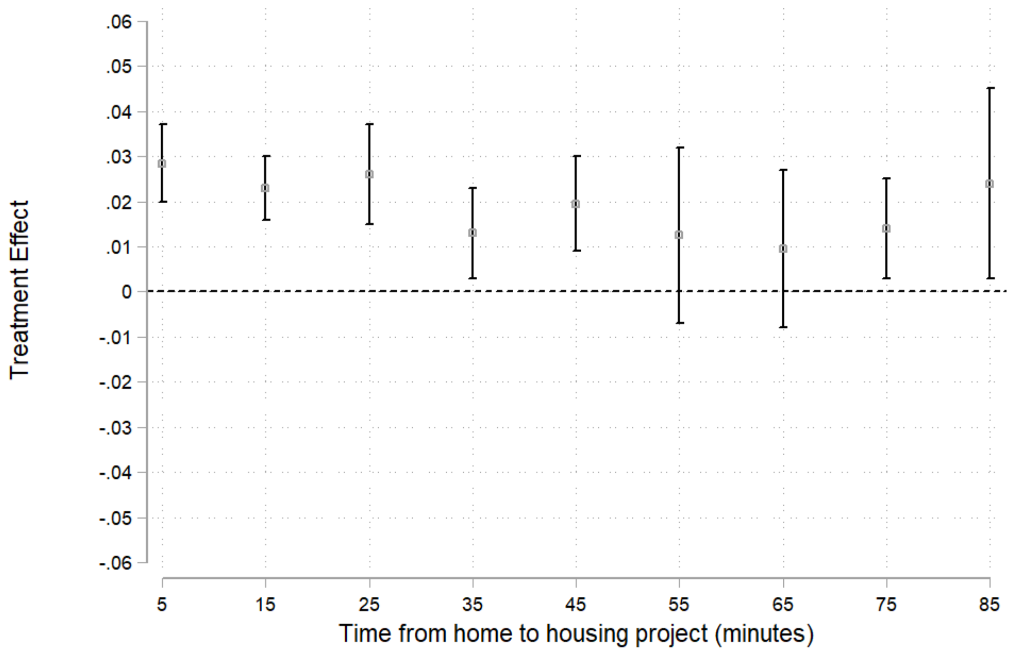
We first discretize each distance variable. Then, we split individuals into ten bins according to the distance from the housing project, which we call b_j. We opt for a semi-parametric approach and estimate the following:

$$y_{ijl} = \beta_0 + \sum_{j=1}^{10} \beta_j * Treat_{ijl} * b_j + \boldsymbol{X}_{ijl}\boldsymbol{\beta} + \alpha_l + \boldsymbol{b}_j + \epsilon_{ijl} \tag{1-17}$$

where we instrument $Treat_{ijl} * b_j$ with an interaction between T_{il} and b_j and $\boldsymbol{b}_j$ is a set of dummy variables for each bin. Note that the variation from equation (1-17) comes from the drafting of individuals who participate in the same lottery and lived or worked in a similar distance from the housing project.

Results for heterogeneous treatment effects conditional on the distance from home are shown in Figure 1.4:

Figure 1.4: Treatment effects conditional on ex-ante distance from home

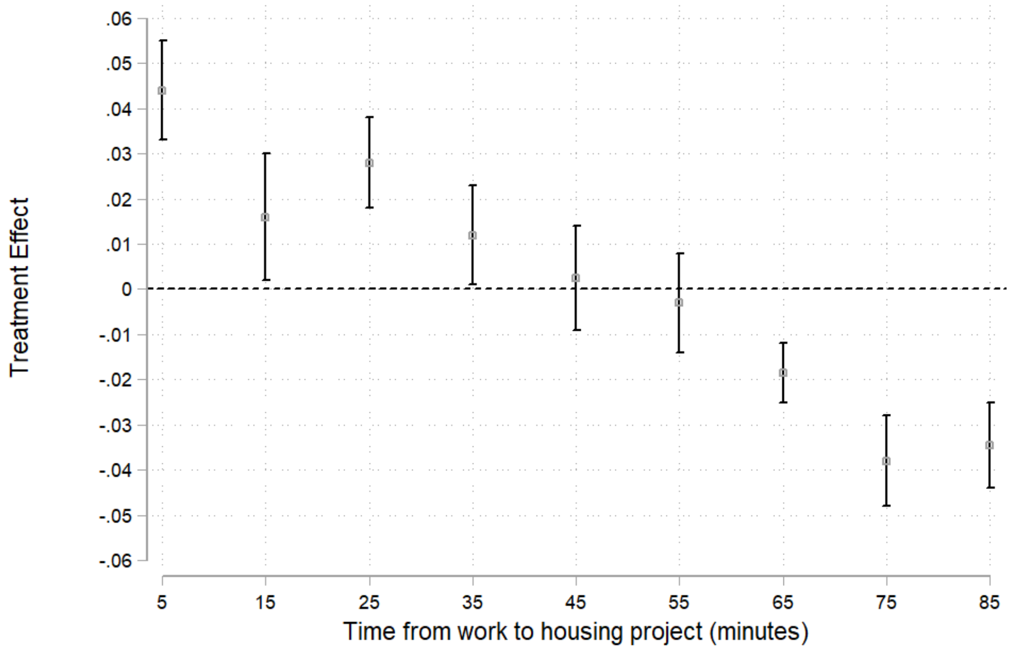


Note: Graphical representation of treatment effects conditional on the distance from home. 95% level confidence interval calculated from clustered standard-errors at the lottery-level.

Conditional treatment effects displayed in Figure 1.4 are almost homogeneous. Thus, the relative ex-ante distance from the home of the individuals to the housing projects does not seem to be an important determinant of treatment effects on employment probability.

Results for heterogeneous treatment effects conditional on the distance from job are shown in Figure 1.5. The treatment effects are estimated only for the subsample of individuals who participated in the formal labor market at any moment in the year before the lottery.

Figure 1.5: Treatment effects conditional on ex-ante distance from job



Note: Graphical representation of treatment effects conditional on the distance from the job. 95% level confidence interval calculated from clustered standard-errors at the lottery-level.

Contrary to the results displayed in Figure 1.4, the conditional treatment effects shown above are highly heterogeneous. We find positive, high and statistically significant treatment effects for individuals who worked close to the housing project. The estimated treatment effects decreases almost monotonically and we estimated negative and statistically significant treatment effects for the individuals who worked far away from the housing project. Thus, these results suggest that the relative distance from the previous job to the housing project seems to be an important mechanism through which the housing program affects employment probability.

1.6.3 Migration

In this subsection, we analyze one more mechanism: migration. Munch, Rosholm, and Svarer (2006, 2008) suggested that home-owners have larger costs of geographical mobility than renters. Thus, a housing project might reduce employment probability if it makes beneficiaries less willing to search for jobs in other regional labor markets.

In order to test this hypothesis, we estimated equations (1-13) and (1-14) using migration from Rio de Janeiro city as a dependent variable. Thus, we can evaluate the first stage of this relation, thereby estimating the effect of being drafted on the mediating variable.

Results are displayed in Table 1.7:

Table 1.7: Effect of the *MCMV* program on migration

	Reduced-form		IV	
Treatment	-0.020**	-0.018**	-0.027***	-0.025**
	(0.004)	(0.004)	(0.006)	(0.008)
Baseline covariates		✓		✓

Note: The first and second columns present ITT estimates of the MCMV program on migration, via OLS. The third and fourth columns present TOT estimates of the program impact, obtained estimating the baseline equation via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates lottery fixed-effects. Inference conducted with clustering at the lottery level. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

As expected, being drawn in the lottery reduces the probability that the individuals move from the Rio de Janeiro capital. Our IV estimate point to a reduction of migration probability between 2.5 and 2.7 percentage points. Estimates are significant at the 1% or the 0.1% level. Since we find a significant effect of the treatment on the mediating variable, it is a potentially important mechanism.

Despite the statistically significant effects of the program on migration, the magnitude of these estimates is not very large. We estimated the subsequent effect of migration on employment in order to calculate the effect of the program on employment probability that is mediated through migration. Details on the estimation of the second step of the process are given in Appendix 3.4.5.

We conclude that the decrease in employment probability which occurs due to migration is not greater than 0.035 percentage points. Therefore, it is unlikely that this mechanism explains a great portion of the impact of the housing program on employment probability.

1.7 Conclusions and implications

In this chapter, we examined the effects of a large housing program in Brazil on employment probability and other labor market outcomes. We used the lotteries from the *Minha Casa Minha Vida* program that took place within the Rio de Janeiro municipality to identify the causal impacts of the housing program on the recipients.

We showed that even in a very restrictive labor supply static model, we could not predict the program's effects on labor supply, which reinforces our research question as an empirical problem.

In the first two years after the lotteries, we observed no effect of the program on employment probability whatsoever. However, after this period, we observed a continuous and consistent increase in the formal labor market participation, reaching 2.3 percentage points. Furthermore, we found no effect of the program on informal employment probability, which suggests that the program increased total employment by 0.8 percentage points.

These previous results are highly heterogeneous. While we found no effect on women, we observe a positive impact on men, which ranges from 4.3 to 7.8 percentage points. The impacts of the program are also higher for poorer individuals and decrease rapidly with an increase in income.

We also analyze the effect of the program on wages, conditional on being formally employed, and participation in another large-scale social program, *Bolsa Família*. We find an increase of 12.5% on wages and an increase of one percentile on the skill rank of occupations, consistent with an increase in the quality of acquired jobs as well as a reduction of 10% in the participation in the BF program. We also observe significant effects of the program only three years the program.

If we interpret these results in a static labor supply setting, we must conclude that housing and leisure are H-A substitutes for the majority of our sample. Despite this, we also have several potential mechanisms that might explain the relationship between housing and employment probability other than income and substitution effects.

Then, we discuss several possible mechanisms which might explain this relation. First, we found no evidence regarding the fact that the quality of the neighborhood where the individuals were drafted is an important determinant of employment probability. We indicated that individuals randomized into better neighborhoods presented no better labor market outcomes than the individuals who were randomized to the worst neighborhoods.

Next, we analyzed the importance of the distance from the individuals' homes and jobs before the lottery to the housing project. We estimated semi-parametric treatment effects, conditional on the prior distance of the job and home addresses to the housing project. We found no significant heterogeneity in the treatment effects conditional on the distance from home.

On the other hand, the treatment effects conditional on the distance from the job to the housing project are very important in explaining the impact of the program on employment probability. The estimated treatment effects are positive and statistically significant for individuals who worked closer to the housing project that they were being drafted to, while they are negative and statistically significant for the individuals who lived further away from the housing project. This is consistent with the frequent critique that the labor market outcomes of the beneficiaries might be hurt by the fact that the houses of the program were offered in the peripheral regions of the cities (SIMPSON, 2013).

Furthermore, we also tested the effect of the program on migration. We showed that the program, in fact, reduced the mobility of individuals. Despite this, we showed that this mechanism is unlikely to explain a significant portion of the housing program's impact on employment probability.

Finally, we tested a potential disruption effect of the program. Some authors suggest that a housing program might decrease short-run employment probability due to the disrupting effect of changing homes. In order to test this, we estimated monthly treatment effects of the program on employment. The timing of the estimated treatment effects is also inconsistent with the disruptions.

Our results have three important implications. First, unlike the previous literature, we showed that the housing assistance program need not to have an adverse effect on employment probability or the quality of the jobs held, especially for poorer individuals.

Second, we showed that the quality of the neighborhoods where individuals lived, which has been repeatedly pointed as the main mechanism through which housing projects influences the labor market, does not seem an important determinant of medium-run labor market outcomes once we control for self-selection of beneficiaries into heterogeneous treatment.

Thus, it is important to consider important to consider alternative mechanisms that might explain this effect. The final implication of this chapter is that the increased mobility cost generated by the distance where the houses are offered relative to beneficiaries' previous job might explain a greater portion of the estimated treatment effects. In order to confirm all of these policy recommendations, however, more deep analysis of the program is required.

2
A Structural Model for Housing and Labor Supply with Experimental Estimation and Validation

2.1
Introduction

The housing deficit, the number of citizens without proper access to a residence, has been seen as a first order urban, economic and health issue in modern cities (SIMPSON, 2013). This concern, common to both developed and developing countries, has led to sizable government investments in housing programs (YAMAZAKI, 2017).

Contrasting with the large benefits provided by this kind of program, the take-up of potential beneficiaries is strikingly low. Chetty, Hendren, and Katz (2016), for instance, show that half of the eligible individuals choose not to participate in the Moving to Opportunity program in the United States. The take-up of the *Minha Casa, Minha Vida* (MCMV) program, which we analyze in this paper, and where individuals received a subsidized house is higher, but even still about 30% of eligible individuals opt for not participating in the program.

One of the potential reasons for this low take-up is the impact of the housing programs on labor market outcomes of beneficiaries. Several articles, including the first chapter of this thesis, used reduced-form methods to show that housing projects indeed can affect labor market outcomes. Despite being able to isolate the causal effects of the program on beneficiaries and distinguishing the importance of different potential mechanisms, these studies have limited power to directly suggest policy recommendations.

In this chapter, we build a static labor supply structural model where individuals choose not only whether to participate in the formal labor market but also whether to participate in the *MCMV* program. We incorporate in the modeling of the decision to participate in the program features which are specific to housing programs. We incorporate in the model moving costs, which vary according to where individuals lived and worked before receiving the program, and rationing costs associated with the nature of the welfare transfer.

This model allows us to complement the previous reduced-form literature by studying the inter-relationship between take-up and labor market outcomes and by doing policy experiments and evaluating the effect of alterations on the program on the decisions of beneficiaries.

In order to estimate this model, we use data for lotteries of the *MCMV* program that took place in Rio de Janeiro combined with rich administrative data. We suggest a two-step estimation strategy. First, we use a Heckman correction model to predict wages of individuals who do not participate in the labor market. Then, we apply a relevance weighted maximum likelihood estimation strategy in order to obtain the structural parameters of the model.

We also take our experimental framework into account when building the model and leverage it in order to help identification. Specifically, we model the lottery as a random expansion of the budget constraint. The random variation helps to identify the marginal utility of consumption. In this static labor supply framework, decision-makers balance the trade-off between consumption and the disutility of labor supply. Thus, the marginal utility of consumption is a crucial parameter of the model, and its correct estimation is important for the ability of the model to describe individuals' behavior and generate reliable counterfactuals.

Since we have several lotteries at our disposal, we hold one of those out of the estimation sample. Then, we use this alternative lottery to validate our model. The model is able to precisely predict the choices of individuals observed in the sample used for estimation. The model performs a little worst in the hold-up sample but is still able to accurately predict observed choices. The good performance of the model, both in the within-sample and out-of-sample validations, increases the confidence in the counterfactuals generated by the model.

Then, we suggest two policy experiments. First, we test what would happen to beneficiaries if the houses were built next to individuals' jobs and not on peripheral regions, as they were. We show that this change would increase the take-up of the program by four percentage points but would barely change labor market participation.

Second, we test what would be the program impact on beneficiaries if the government provided a cash transfer of the house value instead of the product benefit. We do this by increasing the value that individuals attribute to the housing transfer until it matches the value of a cash transfer. We show that this policy experiment greatly increases the take-up of the program, by twenty percentage points, and severely decreases formal labor market participation, by about twelve percentage points.

In this paper, we bridge two strands of literature. The first is the one on housing programs and neighborhood choice in the United States (BAYER, FERREIRA, and MCMILLAN, 2007; BAYER, KEOHANE, and TIMMINS, 2009; BAYER and MCMILLAN, 2012; GALIANI, MURPHY and PANTANO, 2015). These papers study how individuals sort through neighborhoods and how this decision is affected by housing programs, such as the Moving to Opportunity (MTO).

We incorporate several characteristics of neighborhood choice into our model but accommodate these features into a static labor supply framework. This allows us to study the joint decision of the labor market and welfare participation and shed new light on how these decisions mutually influence each other.

The second one is the static labor supply literature which incorporates endogenous participation in welfare programs (HOYNES, 1996; KEANE and MOFFITT, 1998; BINGLEY and WALKER, 2001; FLOOD, HANSEN and WAHLBERG, 2004). Besides bringing features of the neighborhood choice literature, which enriches the description of the participation in a housing program, we also contribute to this literature by using experimental variation to help identify the parameters of the model and validate it.

The experimental variation helps identify the parameters of the model, decreasing the need to rely on functional form identification. Also, the validation of the model on an experimental hold-up sample increases the confidence in the results suggested by the policy counterfactuals.

This paper is divided into seven sections, besides this introduction. Next, we describe the used data. In the third section, we describe the model. In the fourth section, we describe the estimation strategy. Next, in the fifth section, we show our results of the estimation. In the sixth section, we present in-sample and out-of-sample validation of the model. In the seventh section, we discuss our policy experiments. Finally, the eighth section concludes the chapter.

2.2 Data description

Here we use a subsample of the data used in the first chapter of this dissertation. As explained, our data combines information for *MCMV* lotteries in Rio de Janeiro with detailed administrative registries for the universe of formal labor market workers (RAIS) and family characteristics (Single Registry). As in the first chapter, we keep only individuals who participated in the lottery and could be found in the Single Registry.

This merge between the lotteries and the Single Registry allows us to have a rich set of baseline family and individual characteristics for lottery participants. We showed in the first chapter of this dissertation that the analyzed housing program affects labor market participation and other outcomes, but just three years after the lottery.

Given our previous results and, since we are interested in the interdependence between the take-up and labor supply decisions, we additionally merge our data with RAIS three years after the lottery. This allows us to obtain information on whether the individual worked in the formal labor market as well as hours of work and wages for those individuals who worked.

However, we make a few additional restrictions on the previous data. First, we drop individuals who do not report any individual baseline characteristic since they will be important for the estimation strategy. Second, we drop all data on the last lottery in our sample. We do this because this lottery happened in January of 2015 we do not have RAIS information three years after this lottery.

Then, we are left with six lotteries in our sample. We use five of these lotteries in the estimation process and keep one as a hold-up sample. This is done to allow us to validate our model in this hold-up lottery, where the individuals who participate are different and where the incentives of drawn individuals are also potentially different. We further discuss this validation strategy in section six.

We showed in the first chapter of this dissertation tests that corroborates the lotteries were successful in balancing the treatment and control groups. Nonetheless, since we are focusing on a subsample of our original data, it is important to show that the experimental design is still valid in this subsample.

We perform the same balancing test like the one used in the first chapter. That is, we estimate:

$$y_{il}^b = \beta_0 + \beta_1 * T_{il} + \alpha_l + \epsilon_{il} \tag{2-1}$$

where y_{il}^b is the variable of interest of individual i in lottery l in the baseline period, T_{il} is a dummy for individuals who won the lottery l and α_l is a fixed-effect for lotteries.

Results are shown in Table 2.1:

Table 2.1: Balancing test and descriptive statistics

	Control mean	Difference
I. Individual Characteristics		
Male	0.120	0.022
	(0.325)	(0.018)
White	0.250	0.008
	(0.433)	(0.020)
Age	37.437	-0.068
	(10.462)	(0.396)
Education	5.016	0.186
	(1.718)	(0.083)
Labor Market Participation	0.497	0.011
	(0.476)	(0.023)
Wages	176.407	85.963
	(470.586)	(46.236)
II. Family Characteristics		
Family income	102.902	79.245
	(138.188)	(39.766)
Number of rooms	3.864	0.168
	(0.086)	(0.085)
Rent	70.556	9.170
	(134.002)	(4.120)
Number of components	3.337	-0.272
	(1.839)	(0.161)
Distance to the housing project	75.375	-12.240
	(287.279)	(8.514)

Note: Means of variables for our control group. Treatment effects were estimated regressing. via OLS, each variable in a dummy for treatment and a fixed-effect for lottery. Clustered standard errors in the lottery level in parenthesis. * p<0.05, ** p<0.01, *** p<0.001.

Similarly to what we found in the first chapter, we do not observe any statistically significant difference between the treatment and control group in any of the individual or family baseline characteristics. This evidence supports that the lotteries were successful in balancing the two groups, even in the subsample used.

2.3 The model

2.3.1 Set-up

Consider that i indexes individuals and j indexes families. Let $d_{ij} \in \{0, 1\}$ be an indicator variable that individual i who belonged to family j was drawn in the lottery for the *MCMV* program. Also, let h_{ij} denote the individual labor supply and C_j the per capita consumption of family j.

Finally, let m_{ij} be an indicator variable that the individual chose to participate in the program. Drawn individuals choose simultaneously whether to participate in the program and the optimal combination of consumption and labor supply, while individuals who were not drawn choose only the optimal combination of these last two variables.

Individuals' utility depends on consumption, labor supply, welfare participation, observable individual ($\boldsymbol{X}_{ij}$) and family ($\boldsymbol{Z}_j$) characteristics and an individual-specific non-observed taste shock which varies for each combination of welfare participation and labor supply (ϵ_{ij}^{hm}). We denote the vector of parameters which indexes these several variables as $\boldsymbol{\theta}$.

We assume that the taste-shock and the direct effect of the participation on the program on utility are separable from the other components of the utility function:

$$U(C_j, h_{ij}, m_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j, \epsilon_{ij}^{hm}, \boldsymbol{\theta}) = \overline{U}_{ij}(C_j, h_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j) + \lambda * m_{ij} + \epsilon_{ij}^{hm} \quad (2\text{-}2)$$

In order to keep our model tractable, we discretize our labor supply variable. Specifically, if H_{ij} is a countinuous variable that indicate the weekly number of worked hours, we define:

$$h_{ij} = \begin{cases} 0, & \text{if} \quad H_{ij} = 0, \\ 0.5, & \text{if} \quad H_{ij} \in (0, 40), \\ 1, & \text{if} \quad H_{ij} \geq 40. \end{cases} \quad (2\text{-}3)$$

Thus, each individual has three possible labor supply choices. Drawn individuals, who decide simultaneously the optimal labor supply and welfare participation, have six potential discrete choices. Each individual also has the following budget constraint:

$$C_j = \frac{w_{ij} * h_{ij} + N_{ij} + \sum_{k \neq i \in j} r_{kj} + m_{ij} * (\gamma * B - p_{ij})}{n_j} \quad (2\text{-}4)$$

where

$$p_{ij} = \sigma * \sum_{k \in j} r_{kj}$$

We choose to divide this per-capita household consumption by one-hundred, in order to approximate the magnitude of consumption from other variables and ease numerical optimization of the model.

In equation (2-4), w_{ij} is the individuals' full-time formal wage, N_{ij} is his non-labor income, r_{kj} is the total income of individual k in family j, n_j is the total number of components of the family, B is the total transfer provided by the program and p_{ij} is the amount that the individual has to pay if she chooses to participate in the program, which is a fraction σ of total family income. Since the housing program does not assist individuals with an in-kind transfer, we allow it to be discounted by a factor γ, similarly to Keane and Moffitt (1998). This parameter will be estimated from the data.

Let δ_{ij}^{hm} be an indicator variable that the individual chose the combination of labor supply h and welfare participation m, that is:

$$\delta_{ij}^{hm} = \begin{cases} 1, & \text{if} \quad h_{ij} = h \quad \text{and} \quad m_{ij} = m, \\ 0, & \text{otherwise.} \end{cases} \tag{2-5}$$

Then, the individuals' problem is to choose the feasible[1] combination of labor supply, consumption and welfare participation that maximizes (2-1) subject to the budget constraint (2-3). We will observe:

$$\delta_{ij}^{hm} = 1 \iff U_{ij}(C_j, h_{ij}, m_{ij}, \epsilon_{ij}^{hm}) > U_{ij}(C_j, h'_{ij}, m'_{ij}, \epsilon_{ij}^{h'm'}), \quad \forall h', m'$$

2.3.2 Empirical specification

We assume a flexible specification for the utility function of individuals. We opt for specifying $\overline{U}_{ij}(.)$, similarly to Keane and Moffitt (1998), as following:

$$\begin{aligned} \overline{U}_{ij}(C_j, h_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j) = \alpha^h * h_{ij} + \beta^c * C_j + \alpha^{hh} * h_{ij}^2 + \beta^{cc} * C_j^2 + \\ + \alpha^{hhh} * h_{ij}^3 + \beta^{ccc} * C_j^3 + \beta^{ch} * C_j * h_{ij} + \beta^{cm} * C_j * m_{ij} + \\ + \beta^{hc} * h_{ij} * m_{ij} + \boldsymbol{X}_{ij} * \boldsymbol{\beta}^X + \boldsymbol{Z}_j * \boldsymbol{\beta}^Z \end{aligned} \tag{2-6}$$

Moreover, we assume that the error-term ϵ_{ij}^{hm} is distributed as an i.i.d extreme value random variable.

[1] Here, the feasibility of the choice is defined not only in reference to the budget constraint but also to choose a specific an available choice, given the random assignment d_{ij}.

Next, similarly to Galiani, Murphy and Pantano (2015), we assume that:

$$\lambda = \lambda_0 + \lambda_1 * Dist_{ij} + \lambda_2 * \overline{Dist}_{ij} \tag{2-7}$$

where $Dist_{ij}$ is the distance between the house which the individual was drawn and his job before the lottery and $\overline{Dist}_{ij}$ is the distance between the same house and his house before the lottery.

In the literature on welfare participation and labor supply, the direct cost of participating in the program (λ_0) is interpreted as a stigma. In our application, this can also be interpreted as a moving cost. We will allow some of the previous coefficients to vary in the population. Specifically, we assume that:

$$\alpha^h = \overline{\alpha}^h + \epsilon_{ij}^{\alpha} \tag{2-8}$$

and

$$\lambda_0 = \overline{\lambda}_0 + \epsilon_{ij}^{\lambda} \tag{2-9}$$

where $(\epsilon_{ij}^{\alpha}, \epsilon_{ij}^{\lambda}) \sim N(\mathbf{0}, \mathbf{W})$. Here, we allow arbitrary correlation between the disutility of labor supply and the cost of participating in the program. This is desirable because individuals who have high tastes of work might have low tastes for welfare participation, for instance.

As for the individuals' and families' characteristics, we opt for a parsimonious specification. We include gender and race in the first vector and amount paid in rent and average family income (both measured before the lottery) in the second vector of characteristics.

2.4 Estimation

2.4.1 Identification

In all static models of labor supply, individuals balance the trade-off between consumption and the disutility of work. Thus, the parameter β^c is central to the results since it measures the relative importance of consumption. Note that we model the lottery d_{ij} as a random expansion in consumption. Thus, like in Galiani, Murphy, and Pantano (2015), this coefficient is partly identified by this random variation.

The vectors of coefficients $\boldsymbol{\beta}^X$ and $\boldsymbol{\beta}^Z$ are identified by different choices of labor supply across different demographic groups. Since these variables do not vary at the choice level for the same individual, it is also necessary to normalize the coefficient for each of these coefficients to zero in one of the choices faced by individuals. We opt for normalizing these coefficients to zero in the choice of not participating in the labor market and also not participating in the program. Thus, these variables should be interpreted as the differential effect of these socioeconomic variables on the indirect utility relative to their effect on the choice of reference.

The elements of the Cholesky matrix ($\mathbf{C}$) of variance-covariance matrix ($\boldsymbol{W}$), which is its generalized squared-root, has three different elements, since it has dimension two and is symmetric, and these elements can be identified without normalizations and directly estimated together with the remaining parameters of the model. Finally, the parameters λ_0, λ_1, λ_2 and γ are identified by the take-up choices, only within the group of drawn individuals.

2.4.2 Calibration

We do not have data for two important variables that enter the individuals' budget constraint in our model; the cost of the house subsidized by the program (B) and the fraction of family income paid by beneficiaries to help to pay for the house (σ).

These variables are not the same for different individuals. As discussed in the program's description, in the first chapter of this dissertation, B depends on which housing project the subsidized house was built (primarily because of the cost of the land) and varied to a smaller extent within housing projects due to differences in the houses' structure. The fraction of income dedicated to the program (σ) also varies between 5% and 10% according to family income and the year of the lottery.

We opt to overcome this problem by calibrating these parameters to their average value, according to the perception of government officials who implemented the program. We set the fraction of income paid by beneficiaries to 7.5% and the value of the transfer to 60.000 R$.

2.4.3 Method of estimation

In order to estimate this model, we propose a two-step procedure. In the first step we predict individual wages, and in the second step, we use these predicted wages to estimate the parameters of the model.

First-Step. Note that one of the elements in the budget constraint is the individuals' wages. We, however, only observe wages for the individuals who formally work. Thus, in the first step of the estimation, we predict wages for all individuals. Following Hoynes (1996), we estimate a mincerian regression as:

$$ln(w_{ij}) = \eta_0 + \tilde{\boldsymbol{X}}_{ij} * \boldsymbol{\eta}^X + \tilde{\boldsymbol{Z}}_j * \boldsymbol{\eta}^Z + v_{ij} \tag{2-10}$$

and use Heckman's correction to account for self-selection into employment. The vector of individual and family characteristics we use this first-stage of the estimation is much larger than the vector included in the estimation of the structural model. Also, these variables are measured at the baseline period - before the lotteries.

In order to estimate the Heckman correction model, we need an exclusion restriction, that is, a variable that affects selection into the formal labor market without affecting wages. We use the number of public daycare facilities in the neighborhood[2] where individuals lived before the lottery as this instrument. It is possible that, even after controlling for income, number of children and several other variables, that the number of daycare facilities is correlated with wages. We think of this as a minor drawback since our goal in the first step is to predict wages and not to perform causal inference.

Second-step. Once we have the predicted wages, we can derive the choice probabilities. First, consider the case of drawn individuals ($d_{ij} = 1$). As a first approach, consider the choice of an individual taking the parameters α^h and λ_0 as given. Then:

$$P(\delta_{ij}^{hm}|d_{ij} = 1, \alpha^h, \lambda_0) = P(\overline{U}_{ij}^h + \lambda * m_{ij} + \epsilon_{ij}^{hm} > \overline{U}_{ij}^{h'} + \lambda * m_{ij}^{'} + \epsilon_{ij}^{h'm'}, \forall h', m') =$$
$$= P(\overline{U}_{ij}^h + \lambda * m_{ij} - \overline{U}_{ij}^{h'} - \lambda * m_{ij}^{'} > \epsilon_{ij}^{h'm'} - \epsilon_{ij}^{hm}, \forall h', m')$$

Due to the assumption on the distribution of ϵ_{ij}^{hm}, we can write the probability above as:

$$P(\delta_{ij}^{hm}|d_{ij} = 1, \alpha^h, \lambda_0) = \frac{\exp(\overline{U}_{ij}^h + \lambda * m_{ij})}{\sum\limits_{m\in\{0,1\}} \sum\limits_{h\in\{0,1,2\}} \exp(\overline{U}_{ij}^h + \lambda * m_{ij})}$$

Then, the probability of choice for drawn individuals unconditional on individual coefficients can be written as:

$$P(\delta_{ij}^{hm}|d_{ij} = 1) = \int\int \frac{\exp(\overline{U}_{ij}^h + \lambda * m_{ij})\phi(\alpha^h, \lambda_0)d\alpha^h d\lambda_0}{\sum\limits_{m\in\{0,1\}} \sum\limits_{h\in\{0,1,2\}} \exp(\overline{U}_{ij}^h + \lambda * m_{ij})} \tag{2-11}$$

where $\phi(.)$ is a multivariate normal distribution.

[2]We specifically delimit the number of municipal facilities in the Census ponderation area where the individual lived.

On the other hand, individuals who were not drawn ($d_{ij} = 0$) cannot choose to participate in the program. Thus:

$$P(\delta_{ij}^{h1}|d_{ij} = 0) = 0, \forall h$$

Therefore, the choice probability for these individuals reduces to[3]:

$$P(\delta_{ij}^{h0}|d_{ij} = 0) = \int \frac{\exp(\overline{U}_{ij}^{h})}{\sum\limits_{h\in\{0,1,2\}} \exp(\overline{U}_{ij}^{h})} \phi(\alpha^h, \lambda_0) d\alpha^h \tag{2-12}$$

The probabilities displayed in equations (2-7) and (2-8) do not have a closed form. Consequently, we will need to simulate these probabilities. Finally, once we have simulated these probabilities we can estimate our parameters with the maximum relevance weighted simulated likelihood (MREWSL).

In order to obtain estimates of the vector of coefficients of the model we maximize the simulated log-likelihood below:

$$\begin{aligned}\hat{\boldsymbol{\theta}} = \arg\max_{\boldsymbol{\theta}} \Bigg[\sum_{d_{ij}=1} \sum_{m\in\{0,1\}} \sum_{h\in\{0,1,2\}} w_{ij} * \delta_{ij}^{hm} * ln\Big[\check{P}(\delta_{ij}^{hm} = 1|d_{ij} = 1)\Big] + \\ + \sum_{d_{ij}=0} \sum_{m\in\{0,1\}} w_{ij} * \delta_{ij}^{h0} * ln\Big[\check{P}(\delta_{ij}^{h0} = 1|d_{ij} = 0)\Big]\Bigg]\end{aligned} \tag{2-13}$$

where $\check{P}(.)$ are the simulations of the previously displayed probabilities and the weights are defined by:

$$w_{ij} = \frac{1}{\sum_i (d_{ij} * \mathbf{1}[d_{ij} = 1] + (1 - d_{ij}) * \mathbf{1}[d_{ij} = 0])} \tag{2-14}$$

Note that, in the absence of weights, the estimated parameters will be mainly driven by the choices of the control group since the number of observations in this group is much higher than the observations in the treatment group. We choose to weight the observations so that the treatment and control groups each have the same weight in the estimation process[4].

The model depends on twenty parameters. Table 2.2 summarizes the meaning of each one of them:

[3]Note that not only the structure of probabilities of choice in the control group simplifies but also that the cost of participating in the program, which varies in the population, degenerates to zero. Then, for the individuals in the control group the population parameters vary in one dimension.

[4]See Hu (1997) for a discussion of the maximum likelihood estimator when we apply weights to the observations.

Table 2.2: Summary of parameters

Notation	Meaning
α^h	Disutility from work
α^{hh}	Disutility from work squared
α^{hhh}	Disutility from work cubed
β^c	Effect of consumption on utility
β^{cc}	Effect of consumption on utility squared
β^{ccc}	Effect of consumption on utility cubed
β^{ch}	Interaction between work and consumption
β^{cm}	Interaction between program participation and consumption
β^{hm}	Interaction between work and program participation
λ_0	Fixed-cost of participating in the program
λ_1	Variable cost as a function of distance from home
λ_2	Variable as a function of distance from home
γ	Discount of the program transfer
β^{X1}	Dummy for male
β^{X2}	Dummy for white
β^{Z1}	Average family income
β^{Z2}	Rent
C_{11}	Standard-error of the disutility from work
C_{11}	Cov. between disutility from work and cost of participation
C_{22}	Standard-error of the cost of program participation

Note: Notation for the variables of the model as well as a brief description for its meaning.

2.5 Estimation results

2.5.1 First stage

In Table 2.3, we present the results for the Heckman two-stage correction model:

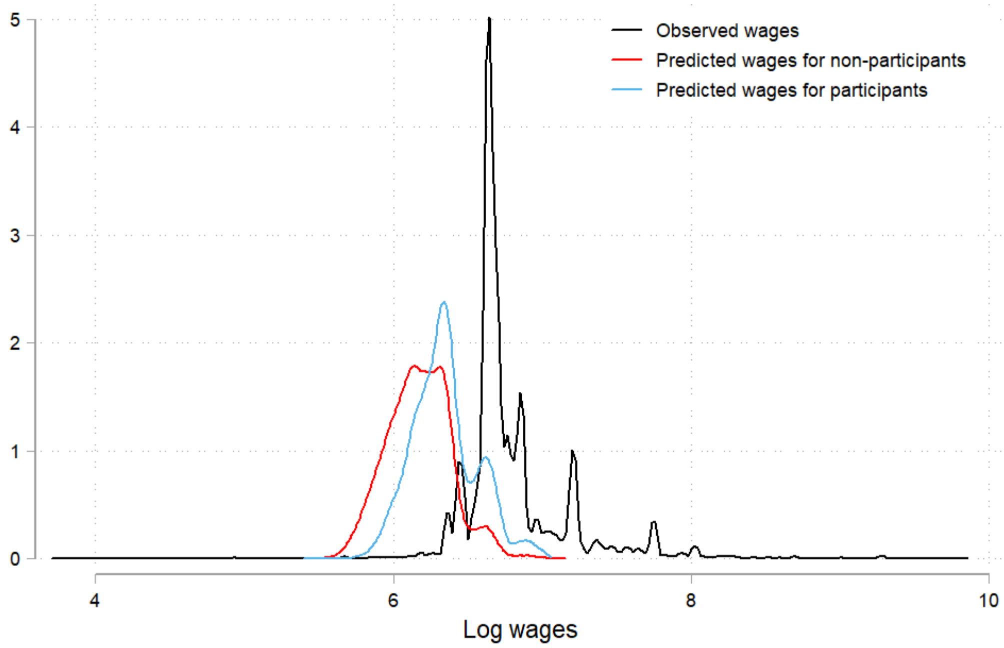
Table 2.3: Heckman correction model for wages

	Wages	Participation
Male	0.282***	0.311***
	(0.005)	(0.010)
White	-0.029***	-0.088***
	(0.004)	(0.008)
Age	-0.009***	-0.023***
	(0.001)	(0.001)
Education	0.067***	0.123***
	(0.001)	(0.002)
Formal	0.276***	0.751***
	(0.005)	(0.010)
Months Worked	0.004***	0.010***
	(0.004)	(0.001)
Family income	0.00003	-0.0001***
	(0.00001)	(0.00003)
Number of children	0.009	-0.001
	(0.005)	(0.001)
Daycare facilities		0.015***
		(0.006)

Note: Estimates of a two-stage Heckman correction model. Robust standard errors in parenthesis. * p<0.05, ** p<0.01, *** p<0.001.

We summarize our predicted wages in Figure 2.1. We show the non-parametric distribution of observed wages, conditional on participation, and the distribution of predicted wages for individuals who participate and who do not participate in the formal labor market.

Figure 2.1: Non-parametric distribution of observed and predicted wages



Note: Non-parametric estimates of the distribution of observed and predicted wages. Standard bandwidth of 0.8 chosen.

Several features stand-out in Figure 1. First, as expected, the shadow wages of individuals who do not participate in the labor market have a lower average than those of the individuals who participate in the labor market. Next, we can see that we underestimate the average wages, even when we restrict our predictions to individuals who were employed.

Finally, the main drawback of this kind of procedure is that it usually underestimates the variance of wages. This is the case because observable characteristics explain a limited amount of the total wage variance. In our application, this problem is less severe once we have a large vector of individual and family characteristics available[5].

2.5.2 Second stage

Now, once we have predicted wages, we can estimate our structural model. The estimated parameters are only identified up to a ratio of the coefficient and the standard error of the unobservable component of utility. Note, however, that the ratio between two structural parameters is identified.

Thus, in order to ease interpretation, we will divide all estimated parameters by β^c. All results below can be interpreted as a proportion of the marginal

[5]In fact, the standard-error of predicted wages is 0.23 and the standard-error of observed wages is 0.37. This gap is considerably smaller than in other applications (HOYNES, 1996).

effect of consumption on the utility when individuals have no income[6]. Results are displayed in Table 2.4:

Table 2.4: Structural estimates with 500 random draws

Notation	Estimate	Notation	Estimate
α^{h}	-10.148***	λ_1	-0.004**
	(0.146)		(0.000)
α^{hh}	1.540	λ_2	0.038**
	(4.360)		(0.016)
α^{hhh}	7.384	γ	0.105***
	(29.696)		(0.003)
β^{c}	1.000***	β^{X1}	0.382***
	(0.045)		(0.058)
β^{cc}	-0.081***	β^{X2}	-0.217***
	(0.009)		(0.044)
β^{ccc}	0.003***	β^{Z1}	0.004***
	(0.000)		(0.000)
β^{ch}	-0.523***	β^{Z2}	0.0004***
	(0.113)		(0.000)
β^{hm}	0.525	C_{11}	0.024
	(0.402)		
β^{hc}	0.180***	C_{12}	-0.269
	(0.028)		
λ_0	-0.075	C_{22}	-0.046
	(0.555)		

Note: Estimates of a the eighteen parameters of the model by simulated maximum likelihood. Inference conducted by the estimator asymptotic properties. * p<0.05, ** p<0.01, *** p<0.001.

Now, we discuss the results of the most interesting parameters. First, as expected, the effect of consumption on utility is positive, while the effect of labor market participation is negative. Also, both of these variables are concave for the relevant space of these variables. It is also noteworthy that individuals value the housing transfer approximately 10,5% of a direct cash transfer (when they have no income). This result is remarkably close to the

[6]Note that we divided the consumption variable by one hundred and, therefore, the specific interpretation of these coefficients is the marginal utility of an increase of one hundred R$ for individuals with zero per capita income.

one estimated by Keane and Moffitt (1998). Also, we can see that most of the estimated variables are statistically significant at least at the 5% level.

2.6 Validation

In this section, we evaluate whether the previously estimated model is able to accurately predicts lottery participants behavior.

2.6.1 Within-sample validation

We expect our model to match four empirical moments: full-time formal work, part-time formal work, no formal work and, for individuals drawn in a lottery, the take-up of the program. First, we present results of within-sample validation. That is, we assess how well our model predicts individuals' behavior in the sample we used to estimate the model. We compare the empirical frequencies observed in the data with the probabilities predicted by the model. In order to formally evaluate the model fit, we also calculate the Pearson statistic. Under the null hypothesis that predicted probabilities and observed frequencies come from the same probability distribution, we have:

$$\sum_{m=1}^{\overline{m}} \sum_{h=1}^{\overline{h}} \frac{(\check{P}_{hm} - q_{hm})^2}{\check{P}_{hm}} \overset{H_0}{\sim} \chi^2_{\overline{m}+\overline{h}-1}$$

where $\check{P}_{hm}$ is the probability of the combination of choices h and m predicted by the model in the sample and q_{hm} is the observed frequency of this combination of choices. Note that the statistic is asymptotically distributed as chi-square and that the number of freedom degrees depends on the number of available choices. Since the choices vary for treatment and control groups, we calculate this statistic separately for each group.

We show the results of the validation process in Table 2.5:

Table 2.5: Within-sample fit

	Control		Treatment	
	Data	Model	Data	Model
Take-up	0.000	0.000	0.693	0.709
Full-time employment	0.307	0.296	0.217	0.197
Part-time employment	0.041	0.033	0.200	0.194
No employment	0.652	0.670	0.583	0.601
Pearson test (p-value)	0.03 (0.99)		0.11 (0.99)	

Note: Comparison of frequencies observed in the data used for estimation and probabilities predicted by the model displayed separately for the treatment and control group. The variable coefficients were evaluated at the estimated mean in the population. For the treatment group, the combination of choice probabilities was aggregated in order to display the moments of interest. The last column displays the Pearson statistics as well as the corresponding p-value in parenthesis.

Our model generally predicts the observed share of choices in the control group. It slightly over-predicts the proportion of individuals who have no employment and we cannot reject the null hypothesis that the model accurately predicts these choices. The performance of the model in the treatment group is also quite good. It predicts well labor supply choices, and also accurately predicts the take-up choice of the program. Again, the Pearson test cannot reject the null hypothesis of model validity.

2.6.2 Out-of-sample validation

The results above are encouraging about the ability of the model to explain the behavior of individuals. Nonetheless, we can subject our model to a stricter test. We can test whether the model is able to replicate the behavior of individuals who participated in a different lottery which was not used in the estimation process. Individuals who participated in this hold-up lottery have potentially different characteristics and incentives. As pointed out by Keane and Wolpin (2007), an experimental hold-out sample is the most convincing form to validate a structural model.

In Table 2.6, we show the results for the out-of-sample validation:

Table 2.6: Out-of-sample fit

	Control		Treatment	
	Data	Model	Data	Model
Take-up	0.00	0.00	0.725	0.753
Full-time employment	0.337	0.295	0.209	0.178
Part-time employment	0.040	0.032	0.209	0.183
No employment	0.622	0.673	0.582	0.638
Pearson test (p-value)	0.012 (0.948)		0.42 (0.983)	

Note: Comparison of frequencies observed in the hold-up sample not used in estimation and probabilities predicted by the model displayed separately for the treatment and control group. The variable coefficients were evaluated at the estimated mean in the population. For the treatment group, the combination of choice probabilities was aggregated in order to display the moments of interest. The last column displays the Pearson statistics as well as the corresponding p-value in parenthesis.

As expected, the out-of-sample fit of the model is worse than the within-sample fit, but the performance of the model is still very good and we do not reject the validity of the model for either group. Thus, since the model seems to be relatively accurate, even outside the sample used for estimation, it is likely that the counterfactuals based on the model will be reliable.

2.7 Counterfactuals and policy experiments

In this section we use the previously estimated model to analyze counterfactuals. We study two policy experiments and their effect on labor market participation and the take-up of the program. We showed in the first chapter of this dissertation that mobility costs is an important mediator of the effect of the program on labor supply. Then, in the first counterfactual we analyze what would be the response to the program if the housing projects were built closer to individuals' jobs.

Next, we analyze what would be the impact of the program if its nature changed, that is, if instead of the subsidized house, the government provided individuals with a cash transfer in the value of the built houses. We also study how much money the government would need to provide individuals in order to keep welfare of beneficiaries constant.

2.7.1 Changes in location

We evaluate what the effect of the program would be if the subsidized house were provided by the government near the job of each individual. We do this by setting λ_1 to zero and evaluating the changes in the choice probabilities predicted by the model.

Note that this change might affect the labor supply through two channels: a decrease in the cost of keeping its formal job once it is decided to participate in the program and a decrease in the cost of participating in the program. If the increase in program participation is interpreted as an expansion of the budget constraint, then these two forces may affect labor supply in opposite directions.

Results are shown in Table 2.7:

Table 2.7: Alternative location choice

	Original Prediction	Counterfactual Prediction
Take-up	0.709	0.742
Full-time employment	0.197	0.201
Part-time employment	0.194	0.203
No employment	0.601	0.596

Note: Comparison of probabilities predicted by the original model and by the model with the distance from work to the housing project set to zero. The combination of choice probabilities was aggregated in order to display the moments of interest.

First, note from Table 2.7 that the counterfactual change increases the take-up of the program by four percentage points due to the reduction in the cost of taking up the program. Second, the first of the two forces depicted above stands out. That is, the cost of sustaining the formal job decreases and, thus, the formal labor supply increases. Nonetheless, this effect is very limited, and changes in labor supply do not change much.

2.7.2 Change in the nature of the benefit

We incorporate in the structural model some characteristics of the housing transfer that differ from a cash transfer. In particular, we allowed beneficiaries to discount the value of the transfer, relative to an income transfer. Now, we consider what would happen to beneficiaries if this transfer was valued as much as cash in the value of the cost of the built house. We do this by setting γ equal to one, which amounts to an almost tenfold increase in the benefit.

Results are shown in Table 2.8:

Table 2.8: Alternative nature of the benefit

	Original Prediction	Counterfactual Prediction
Take-up	0.709	0.892
Full-time employment	0.197	0.088
Part-time employment	0.194	0.086
No employment	0.601	0.826

Note: Comparison of probabilities predicted by the original model and by the model with the discount factor set to one. The combination of choice probabilities was aggregated in order to display the moments of interest.

We can see that this change in the nature of the transfer would, as expected, greatly increase the take-up of the program - by almost twenty percentage points. This counterfactual also shows that, for a fraction of individuals, the cost of participating in the program is very high and they would not take-up the program - even for a much higher transfer. Also, the effect of this alternative transfer on the labor market is quite substantial, decreasing employment by more than twelve percentage points.

Alternatively, we could ask: how much money governments would save if the government opted for a cash transfer, instead of a housing transfer, and opted for keeping the welfare of beneficiaries constant. We argue in Appendix 4.1 that we can easily derive this magnitude from our model. If we denote the cost of the program by G, then we can estimate the reduction in the cost of the program simply by:

$$\tilde{G} = G * \hat{\gamma} \tag{2-15}$$

where $\tilde{G}$ is the new cost of the program and $\hat{\gamma}$ is the estimate of the discount applied to individuals to the housing transfer, obtained in Table 2.4. Therefore, a transfer of 6.240 R$ for each beneficiary would be enough to keep the welfare of individuals constant.

It could be suggested that a change in the nature of the benefits of individuals would not only change how much individuals value it but also decrease the costs of taking-up the program. In Appendix 4.2, we suggest an alternative policy experiment that also takes into consideration this change in costs of participating in the *MCMV* program.

2.8 Conclusions and Discussion

In this paper, we build a simple static labor supply structural model which incorporates the decision to participate in a housing program. Several studies used reduced-form methods and provided causal evidence that housing programs affect labor market outcomes, such as wages and employment.

This structural approach allows us to analyze how the decisions to participate in the labor market and the housing program mutually influence each other. This framework is also appropriate to conduct policy experiments and consider how alternative program institutional designs would influence beneficiaries.

Two main features distinguish our model from the traditional labor supply literature. First, the decision to participate in the welfare program included in the model incorporates specific characteristics of a housing program - such as a discount on the value of the transfer and varying costs of participating in the program according to the distance from the built housing project to the individuals' job and previous home.

Second, due to the availability of lotteries data, we incorporate random assignment of eligibility to the housing program in the model. This not only adapts the framework of the model to the structure of the data but helps with the identification of the parameters of the model. Specifically, since we model the housing program as a random expansion of the budget constraint, our experimental framework helps to identify how much individuals value consumption relative to the disutility of labor supply and the cost of participating in the housing program.

We use a two-step procedure to estimate our model. In the first step, we predict wages of individuals, and in the second step we use these predicted wages to estimate our structural parameters using a relevance weighted maximum likelihood estimation. Our estimated parameters are generally aligned with the theoretical predictions and we have a high enough sample to estimate them with relative precision.

More important, our model is able to accurately predict the proportion of choices observed in the data, both in the treatment as in the control group. The availability of several lotteries allows a more stringent test of the capacity of the model to predict the individuals' behavior. We keep one of the available lotteries in our data as a hold-up sample, which we do not use for estimation. We show that the model is also able to accurately predict the observed choices, even in this hold-up sample.

This good performance of the model increases the confidence in our counterfactuals. We propose two policy experiments. First, we evaluate what would be the impact of the program on beneficiaries if the houses were offered close to individuals' previous jobs. We show that this alteration would mildly influence program take-up, increasing it by four percentage points, and would barely affect labor supply choices. Thus, since offering houses closer to job opportunities is likely to greatly increase the costs of the program, this alteration of the program is unlikely to be cost-effective.

Next, we consider what would be the effect on beneficiaries if, instead of providing houses, the government provided to the eligible individuals a cash transfer in the value of the built houses. We show that this change in the nature of the program would affect much more drastically the take-up and labor supply decisions. Our policy experiment suggests that the former would be increased by almost twenty percentage points and formal labor supply would be reduced by twelve percentage points.

Our model also allows us to analytically calculate how much cheaper the program would be if it provided a cash transfer and aimed to keep the welfare of beneficiaries constant. The value of a cash transfer to individuals could be greatly reduced, almost to 10% of the house value, without decreasing beneficiaries welfare. This last counterfactual strongly suggests that the nature of the benefit is very socially costly.

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3
Appendix to chapter 1

3.1
Mathematical proofs

Theorem 1: *Let $(\tilde{x}, \tilde{l})$ be the vector of chosen goods in the presence of the restriction $H = \overline{H}$ and prices (p,w). Thus, (p, w, q^*) supports the consumption bundle $(\tilde{x}, \tilde{l}, \overline{H})$.*

Proof. First, consider the following definition of the conditional indirect utility function:

$$V(H, p, w, V) = \max_{x,l} u(l, H, x) : px + wl = V \tag{3-1}$$

Since $(\tilde{x}, \tilde{l})$ is chosen when the individual is restricted to consume $\overline{H}$ under the prices (p, w), then:

$$V(\overline{H}, p, w, p\tilde{x} + w\tilde{l}) = u(\tilde{l}, \overline{H}, \tilde{x}) \tag{3-2}$$

Using the supporting hyperplane theorem, $\exists\ \mu$, q^* such that:

$$V(H, p, w, V) > V(\overline{H}, p, w, p\tilde{x} + w\tilde{l}) \implies \mu V + q^* H > \mu(p\tilde{x} + w\tilde{l}) + q^*\overline{H} \tag{3-3}$$

Now, normalize μ to one and suppose that (p, w, q^*) does not support $(\tilde{x}, \tilde{l}, \overline{H})$. Then, there exists another preferred consumption bundle that fits the consumer budget constraint. That is, $\exists\ (\hat{x}, \hat{l}, \hat{H})$ such that:

$$u(\hat{x}, \hat{l}, \hat{H}) > u(\tilde{x}, \tilde{l}, \overline{H}) \tag{3-4}$$

and

$$p\tilde{x} + w\tilde{l} + q^*\overline{H} \geq p\hat{x} + w\hat{l} + q^*\hat{H} \tag{3-5}$$

Using the conditional indirect utility function definition,

$$V(\hat{H}, p, w, p\hat{x} + w\hat{l}) > V(\overline{H}, p, w, p\tilde{x} + w\tilde{l}) \tag{3-6}$$

Combining equations (3-3) and (3-6), we can see that:

$$p\hat{x} + w\hat{l} + q^*\hat{H} > p\tilde{x} + w\hat{l} + q^*\overline{H} \tag{3-7}$$

Equation (3-7) contradicts equation (3-5) and therefore (p, w, q^*) supports $(\tilde{x}, \tilde{l}, \overline{H})$, which proves the theorem.

□

Theorem 2: *We may write the conditional version of the Slutsky equation as:*

$$d\overline{h} = -\left[e_{ww} - \frac{e_{wq}^2}{e_{qq}}\right]dw - \frac{e_{wq}}{e_{qq}}d\overline{H} + \left(h_v + \frac{e_{wq}}{e_{qq}}H_V\right)\left[dV + \overline{H}dw - (q - q^* d\overline{H})\right]$$

Proof. First, consider the following duality relations, which follow from the definition of the virtual price:

$$\overline{h}^c(\overline{H}, p, w, q, U) = h^c(p, w, q^*, U) \tag{3-8}$$

$$\overline{h}(\overline{H}, p, w, q, V) = h(p, w, q^*, V + (q - q^*)\overline{H}) \tag{3-9}$$

$$\overline{H} = H^c(p, w, q^*, U) \tag{3-10}$$

$$\overline{H} = H(p, w, q^*, V + (q - q^*) \tag{3-11}$$

Totally differentiating equation (3-8) in relation to $\overline{H}$, we obtain:

$$\overline{h}^c_{\overline{H}} = h^c_q \frac{\partial q^*}{\partial \overline{H}} = h^c_q (H^c_q)^{-1} \implies \overline{e}_{w\overline{H}} = \frac{e_{wq}}{e_{qq}} \tag{3-12}$$

Next, differentiating equation (3-8) in relation to w and using equations (3-10) and (3-12), we can see that:

$$\overline{h}^c_w + \overline{h}^c_H \overline{H}_w = h^c_w \implies \overline{e}_{ww} = e_{ww} - \frac{e_{wq}^2}{e_{qq}} \tag{3-13}$$

Then, differentiate equation (3-9) in relation to V,

$$\overline{h}_V + \overline{h}^c_H H_V = h_V \implies \overline{h}_V = h_V + \frac{e_{wq}}{e_{qq}} H_V \tag{3-14}$$

Finally, pluging equations (3-12), (3-13) and (3-14) into equation (10) in the main text, we conclude that

$$d\overline{h} = -\left[e_{ww} - \frac{e_{wq}^2}{e_{qq}}\right]dw - \frac{e_{wq}}{e_{qq}}d\overline{H} + \left(h_v + \frac{e_{wq}}{e_{qq}}H_V\right)\left[dV + \overline{H}dw - (q - q^* d\overline{H})\right]$$

which proves the theorem.

$\square$

3.2 Additional data description

3.2.1 Matching of datasets

Here, we describe how we merged observations from the *MCMV* lotteries with the information from the Single Registry. We proceeded as following:

1. We dropped individuals with incorrect CPF's in both datasets in order to prevent incorrect matches. This is possible because these individual codes follow a particular algorithm that can be checked.

2. We aggregated all different updates of individuals in the Single Registry data. Then, we evaluated whether this individual provided valid CPF information in at least one of the updates. Then, if he did, we aggregated this CPF information for all other updates. This is necessary because some individuals provide correct identifying information in one update but not in the others.

3. We merged data for each lottery with Single Registry data using only valid CPF information. We kept the most recent update of each individual found before the lottery and all updates after it.

4. We validated the matching process by comparing the names of each matched individual in both datasets, which is used to prevent incorrect matches. Several individuals provide their names incorrectly or abbreviate them in one of the datasets. Thus, we rely on a fuzzy validation process. Specifically, let's write the vectorizations of names A and B as s_A and s_B. Then, we calculate the similarity between the names in the two databases using a modified version of the Jaccard index:

$$\tilde{S}_{AB} = \boldsymbol{w}\frac{< s_A, s_B >}{|s_A||s_B|} \tag{3-15}$$

which is just the intersections between the vectorizations divided by the total number of vectors in the names, weighted by the relative frequency which each vector appears in the data. The weight of the vector i is calculated as:

$$w_i = \frac{1}{log(f_i)} \tag{3-16}$$

where f_i is the relative frequency of the vector i in the dataset. We keep the matches formed in step 3 of the matching process only if the similarity score expressed in equation (3-15) is equal or greater to 0.6. In

Table 3.1 we present some examples of the matches in the database and the calculated similarity score.

Table 3.1: Examples of the validation process

Name in the Lottery Data	Name in the Single Registry	Similarity Score	Results of the validation
Leonardo Nascimento Monetiro	Cecilia de Souza Braga	0.035	Not validated
Marcos Dos Santos Francisco	Aline de Carvalho Francisco	0.366	Not validated
Sabrina Tereza de O. Pereira	Marcela de Oliveira Pereira	0.551	Not validated
Ana C G Brando	Ana Cristina Gustavo Brandão	0.611	Validated
Arlete Machado Napolitano	Arlete Ribeiro Machado	0.624	Validated
Liliane Macedo Uivano	Liliane Macedo Vivano	0.945	Validated
Naia Viana de Oliveira	Naia Viana de Oliveira	1.000	Validated

Note: Each entry of the Table is a match between the Single Registry and the Lotteries database using valid CPF information, as described in the steps 1-3 of the matching method above. The first two columns are the names of the matched individuals in the lottery database and the Single Registry, respectively. Next, in the third column, we present our similarity score using equations (3-15) and (3-16). Finally the fourth column, describes whether the match was validated according to the rule described in step 6 of the process above.

Now, we will describe successive filters applied to the database in order to obtain the sample of interest.

1. The original database is a combination of the lotteries described in section 1.4.1. Each observation is an individual who participated in one of the lotteries. At this level, we have 3.019.254 observations.

2. The original database is combined with the Single Registry. We match only individuals which we have information both before and after the lottery. As described in Table 1.1, approximately 16% of the observations of the previous dataset satisfy this criteria. Thus, we have 480.561 observations in the baseline. In this new dataset, each observation represents one update of an individual who participated in the lottery. Each one of them has at least one update after the lottery and some of them have more than one update after the lottery. Thus, we have 1.112.322 observations at this stage. This data is used in most of this study.

3. In section 1.6.1, we restrict the sample described in item 2 only to the matched individuals who participated in the last lottery. Thus, in this exercise the sample decreases to 259.363.

4. In Figure 1.5, displayed in section 1.6.2, we restrict the sample only to individuals who participated in the formal labor market one year before the lottery. Approximately 40% of individuals participate in the labor market and the sample size of 193.344 observations.

3.2.2
Variables description

In this section of the appendix, we provide further description of the variables used in the paper. First, we will describe variables from the Single Registry, and, then from RAIS and the Census.

Single Registry: We drew the following variables at the individual level from the Single Registry: race, gender, age, education level, participation in the formal and informal labor market, wages in the informal labor market and the number of months worked in last month. The race variable is a dummy for white individuals, the gender variable is a dummy for women and age is calculated in years from the information of the date of birth.

Our education variable is an index of the higher level of formal education reached by the individual. This index ranges from one to nine. Each category represents the following levels of education: 1) No formal education, 2) attendance to daycare or pre-school; 3) incomplete first part of fundamental level, 4) complete first part of fundamental level, 5) incomplete second part of fundamental level 6) complete fundamental level, 7) incomplete high school, 8) complete high school, 9) college or further education.

Formal participation in the labor market is a dummy for individuals who declared they have a *carteira assinada* for their main activity in the labor market. We include in the informal category all workers who work without *carteira assinada*, temporary workers in rural areas and self-employed workers. Informal wages are real wages, deflated by the IPCA for January of 2010, for informal workers. The number of months worked is the number of months in which the individual worked in both formal and informal labor market in the twelve months that preceded the update.

The following variables are drawn from the Single Registry at the family level: family income, rent, number of components and number of rooms. Family income is the sum of all sources of income for all members of each family divided by the number of components of the family. Rent is the monthly rent of the family. Both variables are also deflated by the IPCA from January of 2010. The number of components and rooms are the sum of each of those for each family at the last available update. We also use information on the address where these families lived.

RAIS: We draw the following variables from RAIS at the contract level: participation in the formal labor market, formal wages and occupations held. Participation in the formal labor market is a dummy for whether the individual appeared in the RAIS data in a certain period. Formal wages are the remuneration established in the contract for these formal workers. Finally, occupation is a six digit code which identifies the occupation of the individual in the Brazilian classification of occupations (CBO). From this occupation variable, we create a skill rank for the individual. We did this by ranking occupation by their mean wages in 2010. Then, we calculate the percentile of each occupation in this rank as a proxy for the skill associated with the occupation.

We also draw some information about the firms where the individuals worked. We gather information on their size, which is measured according to Sebrae's (2013) definition, and their address. We use uniquely identifying information of the firms to merge the information from RAIS with information from SECEX data. From this alternative dataset, we extract information on whether each firm is a direct importer or exporter.

Census: Finally, we gathered the following variables from the Census data: Family income, education, family composition and household adequacy. These variables are not available in the broadest questionnaire, which is applied to all individuals in the country but only on the more restricted sampling questionnaire, which is not so broadly applied. The sample in this restricted questionnaire is large enough, however, to be representative on ponderation areas, which are fractions of Rio de Janeiro's neighborhoods. Since we are not using the universal data, we apply sample weights in order to obtain an accurate aggregation of observations.

The variables are defined as follows. Family income is just the household monthly income divided by the number of household components. Education is an index particular for the Census data which ranges from one to four. The family composition is a dummy variable for single-parent households. Finally, the household adequacy variable is a dummy, which is defined in the census as houses where up to two individuals lived in each room, appropriate energy, and water supply, access to basic sanitation and daily garbage collection.

3.2.3 Details of georeferencing addresses

Here, we provide details on the georeferencing of addresses. Our goal is to calculate the distance from the individuals home and job before the lottery to the housing project which is being drafted in the lottery. We obtain information on individuals' home address from the Single Registry and for individuals' job address from RAIS. About 14% of our sample in the Single Registry does not provide information on home location or did not live in a house. Also, about 60% of the sample does not work in the formal labor market in the baseline period. Thus, we try to georeference just the remaining observations.

Using the process described below, we are able to georeference about 99,2% of the home addresses and about 97% of the job addresses in the available sample of interest. We follow:

1. We combine information for several variables in the Single Registry and RAIS in order to create a single variable for the address that includes street address, neighborhood, city, state, country, and postal code.

2. Since both of these variables are self-reported and do not follow a standard of reporting, we use Campos (2018) method to clean this address variable and standardize the reporting of street and neighborhood names.

3. We use Hess (2015) algorithm to obtain the latitudes and longitudes of all job and home addresses in the sample.

4. We manually obtained latitudes and longitudes for all housing projects which were drafted in each one of the seven lotteries in our sample.

5. We used the algorithm of Weber and Péclat (2016) to calculate the time it would take to travel from each of home and job addresses to each of the housing projects by car without traffic. Then, we created a variable of time which is the minimum amount of minutes it would take for the individual to go from his job or home to the closest housing project being drafted.

6. We manually checked 0.1% of the latitudes, longitudes and travel distances and observed that the process above provides a similar travel distance as the manually obtained (with less than a five-minute difference) for 98% of the checked sample.

3.2.4 Neighborhoods ordering

Here, we describe the process used to rank the locations, where the individuals were provided houses under the MCMV program. As mentioned in the main text, the first lottery of 2015 not only randomized the program's benefit but also which housing project each individual was sent.

This lottery randomly assigned individuals to six different residential projects within three different neighborhoods. These locations were classified into the following ponderation areas in the Census: Paciência I, Cosmos II and Santa Cruz I. We can spot these regions geographically in Figure 3.1:

Figure 3.1: Rio de Janeiro's ponderation areas

Note: Ponderation areas of Rio de Janeiro municipality. Regions targeted in the first lottery of 2015 highlighted in red.

We classified these ponderation areas (from best to worst) according to their general quality in the following way: 1) Cosmos II, 2) Paciência I and 3) Santa Cruz I. To do that, we gathered information on each ponderation area characteristics from 2010. Similarly to Chetty, Hendren, and Katz (2016), we chose the variables of per capita income, education, family composition, and household adequacy.

In Table 3.2, we can see the average of these four variables individuals living in each location:

Table 3.2: Average characteristics of each location

Variables	Cosmos II	Paciência I	Santa Cruz I
Income	535.773	490.00	415.680
Education	1.803	1.726	1.672
Family composition	0.176	0.190	0.223
Household adequacy	0.600	0.518	0.490

Note: Variables averages within ponderation areas using 2010 Census data. Observations weighted by the probability of appearing in the sample.

There is a clear order in neighborhood quality for each of the dimensions analyzed, which supports our previously described ranking.

3.2.5 Compliers with heterogeneous treatment

In this section of the appendix, we explore the compliers with each particular treatment discussed in section 7.1. Thus, we compare the characteristics of individuals who took-up each of the treatments. In Table 3.3 we show the average of several variables for compliers of treatment 1 and differences in average between the first column and the averages for compliers of treatments two and three as well as a F test for the joint null hypothesis that these differences are all equal to zero.

Table 3.3: Compliers' balancing test for the lottery of 2015

	Mean for treatment 1	Effect of treatment 2	Effect of treatment 2
Male	0.181	0.043	0.049
	(0.386)	(0.036)	(0.059)
White	0.250	0.040	0.057
	(0.430)	(0.040)	(0.067)
Age	37.524	0.453	-2.281
	(14.441)	(1.246)	(2.201)
Wages	446.723	22.541	61.718
	(317.661)	(41.487)	(70.033)
Informal	0.252	0.032	0.034
	(0.435)	(0.040)	(0.067)
Joint hypothesis (p-value)		0.590	

Note: Clustered standard-errors in parenthesis. * p<0.05, ** p<0.01, *** p<0.001.

3.3 Additional econometric details

In this Appendix, we provide additional description for the inference method used in our main results, in section 6. We use the method suggested by Jones, Molitor and Reif (2018). We opted for this procedure for a couple of reasons. First, this inference method, based on Westfall and Young (1993), allow us to correct for multiple hypothesis testing. Unlike other common methods, such as the one Bonferroni correction suggested by Dunne (1959), this procedure does not rely on the assumption that the alternative dependent variables are not correlated, which allows that this method does not under-reject the null hypothesis.

Second, the procedure suggested by Jones, Molitor and Reif (2018) and described below allows us to take into consideration arbitrary correlation in the error terms of individuals whithin clusters, which is desirable in our application due to the stratification on the composition of the sample.

In our application, we have five different variables of interest and three different time-horizons. Thus, we conduct fifteen different hypothesis tests. Then, we conduct inference in the following manner:

1. Consider estimating the following parameters of interest $\{\hat{\beta}_1, ..., \hat{\beta}_{15}\}$. Then, we calculate standard p-values for each separate hypothesis $\{p_1, ..., p_{15}\}$. Assume, without any loss of generality, that each hypothesis is increasingly ordered with respect to p-values.

2. Draw entire clusters from the sample (lotteries) 1000 times. Then, for each k resampling, we estimate $\{\hat{\beta}^*_{k1}, ..., \hat{\beta}^*_{k15}\}$ and conduct the following hypothesis testing $\hat{\beta}_i = \hat{\beta}^*_{ki}$ for each coefficient of interest. From this hypothesis, we draw the following p-values $\{p^*_{k1}, ..., p^*_{k15}\}$.

3. For each drawn of clusters, we set $q^*_{k15} = p^*_{k15}$ and, then, we calculate the following parameters iteratively:

$$q^*_{ki} = \min\{q^*_{ki+1}; p_i\}, \quad i = 1, ..., 14$$

4. We repeat the two previous steps 1000 times. Then, we calculate the following proportion for each parameter o interest:

$$r_i = \sum_{k=1}^{1000} \frac{I[q^*_{ik} \leq p_i]}{1000}$$

where $I[.]$ is an indicator function.

5. Finally, we define the adjusted p-value for the first hypothesis as $p_1^{adj} = r_1$. Then, we calculate the other adjusted p-values in the following way:

$$p_i^{adj} = \max\{r_i, r_{i-1}\}, \quad i = 2, .., 15$$

   This adjusted p-value is the one used for inference in Tables 1.2 and Table 1.4 in the main text. Note that it is possible that this adjusted p-values are higher than the traditional ones, but this is highly unlikely using 1000 replications in the procedure.

3.4
Additional results

3.4.1
Additional descriptive statistics

Now, we provide additional descriptive statistics and balancing tests. We estimate equation (1-12) again, but use variables from RAIS. All variables below, both at the individual and the firm level variables, are available only for individuals who participate in the formal labor market.

Results are displayed in Table 3.4:

Table 3.4: Balancing test and descriptive statistics for additional variables

	Control mean	Difference
I. Contracts		
Formal wages	745.053	20.780
	(0.157)	(22.467)
Hours	42.419	-0.135
	(0.001)	(0.157)
Tenure	21.502	2.643
	(0.010)	(1.566)
II. Firms		
Firm size	5.641	-0.084
	(0.001)	(0.070)
Importer	0.048	-0.004
	(0.001)	(0.008)
Exporter	0.018	-0.005
	(0.001)	(0.003)

Note: Means of variables for our control group. Treatment effects were estimated regressing. via OLS, each variable in a dummy for treatment and a fixed-effect for lottery. Clustered standard errors in the lottery level in parenthesis. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

The conclusions from Table 3.4 are similar from the conclusions of Table 1.1. We found no significant differences between any of the variables, both at the contract and the firm level. These results reinforces that the lotteries were successful balancing the treatment and control groups.

3.4.2 Robustness of the results on employment

We showed in section 6 that receiving a house from the *MCMV* lottery increased average participation in formal labor market by 2.3 percentage points. For this exercise we used participation in RAIS as dependent variable. Participation in formal labor market information is also available at the Single Registry. Here we show that our previous results are highly dependent on the choice of the source of the dependent variable.

We estimate equations (1-13) and (1-14) again using the alternative variable for formal employment as a dependent variable. Results are displayed in Table 3.5:

Table 3.5: Formal employment probability impacts of the program

	ITT	TOT
	(1)	(2)
Dependent Variable: Formal labor supply		
First year	0.012	0.018
	(0.012)	(0.013)
Second year	-0.015	-0.020
	(0.009)	(0.018)
Third year	0.023	0.036*
	(0.009)	(0.017)

Note: Column (1) presents ITT estimates of the MCMV program on formal and informal labor supply. via OLS. Column (2) presents TOT estimates of the program impact, obtained estimating equation (1-14) via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates include individual covariates at the baseline period. Clustered standard-errors in parenthesis. * p<0.05, ** p<0.01, *** p<0.001.

It can be seen that the results using this alternative dependent variable are slightly higher than the ones estimated in Table 1.2 of the main text. The magnitude and significance of this treatment effects, however, are remarkably close to the ones previously discussed.

Next, we analyze the robustness of the results of the program on informality. We showed that the program had no effect on informal employment, defined as the all employment without *carteira assinada*, rural temporary workers and self-employed workers. In Table 3.6, we show the results of the estimation of equations (1-13) and (1-14) for different definitions of informal employment.

Table 3.6: Informal employment probability impacts of the program

	ITT	TOT
	(1)	(2)
Panel A: Informal employment without rural workers		
First year	-0.037	-0.042**
	(0.016)	(0.013)
Second year	0.023	0.033
	(0.014)	(0.021)
Third year	-0.026	0.040
	(0.021)	(0.021)
Panel B: Informal employment without rural workers and self-employed		
First year	0.008	0.008
	(0.006)	(0.005)
Second year	0.002	0.003
	(0.006)	(0.008)
Third year	0.003	0.005
	(0.007)	(0.007)

Note: Column (1) presents ITT estimates of the MCMV program on formal and informal labor supply, via OLS. Column (2) presents TOT estimates of the program impact, obtained estimating equation (1-14) via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates include individual covariates at the baseline period. Clustered standard-errors in parenthesis. * p<0.05, ** p<0.01, *** p<0.001.

In the first panel of Table 3.6, we exclude temporary rural workers from the definition of informal employment. Next, in the second panel, we also exclude self-employed individuals. We still do not find any effects of the program on informal employment probabilities using any of the alternative definitions and in any of the time-horizons.

3.4.3 Estimates on family members

In this section of the appendix, we test the effects of the *MCMV* program on the family members of the individuals who participated in the lotteries. Since a house is a public good for each family, it is possible that the program has effects on employment probabilities for all members of the family.

Note, however, that we do not expect the treatment effects to be the same as the ones estimated in section 6. We showed that the treatment effects are highly heterogeneous, conditional on baseline individual characteristics, and, since the characteristics of the family members are not equal to the individuals who participated in the lotteries, we do not expect the treatment effects to be the same.

We used the panel of families created using information from the Single Registry. In the estimates below we consider only individuals who had, at the moment of the update, more than sixteen-years-old and less than sixty-five. We also kept in the sample only individuals who belonged to the family in all the available updates.

We estimate the treatment effects of the program on all the family members who satisfy the conditions described above. Specifically, we estimate:

$$h_{ijl} = \beta_0 + \beta_1 * Treat_{jl} + \boldsymbol{X}_{ijl}\boldsymbol{\beta} + \alpha_l + \epsilon_{ijl} \tag{3-17}$$

where h_{ijl} is the formal labor supply of individual i in family j who has some member of the family participating in lottery l. We instrument $Treat_{il}$ with a dummy T_{il}. Thus, we compare family members of drawn individuals with family members of participants who were not drawn. We do not include any participants of the lotteries in the estimates below and report estimates using the participation in RAIS as a dependent variable. Regressions using data from the Single Registry were also estimated, and we obtained similar results.

Results are displayed in Table 3.7:

Table 3.7: Labor supply impacts of the program on family members

	ITT	TOT
First year	-0.034*	-0.052
	(0.013)	(0.016)
Second year	-0.011	-0.016
	(0.011)	(0.018)
Third year	-0.010	-0.016
	(0.013)	(0.024)

Note: Column (1) presents ITT estimates of the MCMV program on formal employment, via OLS. Column (2) presents TOT estimates of the program impact, obtained estimating equation (1-1) via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates include individual covariates at the baseline period. The inference is conducted with clustering at the lottery level. * p<0.05, ** p<0.01, *** p<0.001.

Contrary to participants of the lottery, we do not find consistent effects of the program on family members employment probability. All estimates are close to zero and statistically indifferent from zero, except ITT effect in an one-year horizon. Next, we present heterogeneous treatment effects of the program on family members. Results are presented in Table 3.8 the treatment effects separated by gender.

Table 3.8: Labor supply impacts of the program on family members separated by gender

	ITT	TOT
Panel A: Males		
First year	-0.024	-0.032
	(0.020)	(0.035)
Second year	-0.030	-0.050
	(0.020)	(0.041)
Third year	-0.025	-0.042
	(0.020)	(0.036)
Panel B: Females		
First year	-0.050*	-0.084
	(0.017)	(0.044)
Second year	-0.033	-0.072
	(0.029)	(0.047)
Third year	-0.006	-0.013
	(0.037)	(0.075)

Note: Column (1) presents ITT estimates of the MCMV program on formal employment, via OLS. Column (2) presents TOT estimates of the program impact, obtained estimating equation (1-1) via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates include individual covariates at the baseline period. The inference is conducted with clustering at the lottery level. Panel A includes only man and Panel B only woman. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

In Panel A of Table 3.8, we observe no significant effects on men in none of the specifications and none of the time horizons. In Panel B, on the other hand, we observe a statistically significant treatment effect for women in one of the estimates, but this effect vanishes in the other specifications, just like the estimates in Table 3.7.

Next, in Table 3.9 we present heterogeneity analysis by the position of the individual in the household.

Table 3.9: Labor supply impacts of the program on family members separated by position on the household

	ITT	TOT
Panel A: Heads of the household		
First year	-0.011	-0.011
	(0.012)	(0.018)
Second year	-0.030	-0.051*
	(0.015)	(0.024)
Third year	-0.018	-0.028
	(0.021)	(0.035)
Panel B: Other members		
First year	0.063	0.112
	(0.027)	(0.075)
Second year	0.067	0.137
	(0.045)	(0.151)
Third year	0.069	0.140
	(0.028)	(0.123)

Note: Column (1) presents ITT estimates of the MCMV program on formal employment, via OLS. Column (2) presents TOT estimates of the program impact, obtained estimating equation (1-1) via 2SLS and instrumenting the treatment variable with being drawn in the lottery. All estimates include individual covariates at the baseline period. The inference is conducted with clustering at the lottery level. Panel A includes only heads of the household and Panel B other members. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

In Panel A we include only the person of reference of the household and their spouse. Again, we find negative treatment effects, but these estimates are generally not significant. In Panel B we include sisters, brothers, sons in law, daughters in law and other parents of the person of reference. We do not include here sons, grandsons, fathers, and mothers of the person of reference. For these members, we found positive and, again, statistically insignificant treatment effects.

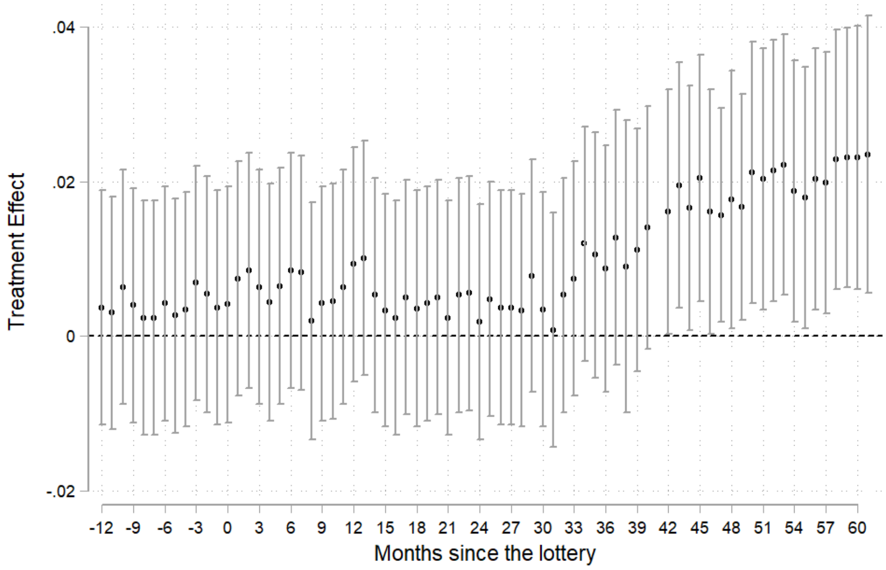
3.4.4
Disruption effect

In this section of the Appendix, we evaluate whether moving to a different location disturbs an individual's routine and negatively affects employment, which is called the disruption effect. This potential mechanism was emphasized by the previous literature (MILLS et al., 2006; JACOB and LUDWIG, 2012). Despite this, we think is unlikely that this disruption is particularly important in our framework for two reasons. First, the *MCMV* program provided help with the moving process, which is likely to mitigate the disruption effect. Second, formal employment in Brazil is much more rigid than in the United Stated. Then, is also less likely that this disruption affects employment.

We test this possible mechanism looking at the timing of the treatment effects and leveraging the high frequency of RAIS data. If this is an important phenomena, we expect to see a decrease in the employment probability of our treatment group, relative to the control group, just after the lottery and a recovery in the medium-run. Thus, we estimate monthly treatment effects of the program on formal employment.

We intend to capture more complex timing of the treatment effects that were not clear in our main results. Results are shown in Figure 3.2:

Figure 3.2: Treatment effects according to time since the lottery



Note: Graphical representation of seventy-two estimated coefficients and robust standard-errors according to equation (1-15).

We can draw three important conclusions from Figure 3.2. First, we do not observe any difference in the monthly employment probability for our treatment and control groups before the lottery using the monthly data. This reinforces conclusions from Table 1.1 of the main text that the lottery was successfully balancing outcomes of the treatment and control groups.

Second, we cannot reject that our estimated treatment effect is different from zero in the first three years after the lottery. Finally, after the third year, we observe a continuous and persistent increase in the employment probability of our treatment group relative to our control group. Hence, we found no evidence to support the disruption effect as depicted earlier.

The pattern of our previous results is notably different from those found in the previous literature. Not only our estimates contradict those of Mills et al. (2006), which concluded that the disruption effect is an important mechanism, but also those of Ludwig and Jacob (2012) since our estimates point to a positive effect of the housing program on employment. Our estimates also have smaller magnitude (closer to zero) than the ones of Ludwig and Jacob (2012).

3.4.5 Migration and employment

We showed in section 7.3 of the main text that the *MCMV* program significantly reduced migration for beneficiaries. Thus, the program might influence employment through the reduced mobility. We are interested in the average natural indirect effect (ANIE), that is, the impact on the program on employment that is mediated through migration.

We suggest estimating:

$$h_{il} = \gamma_0 + \gamma_1 * M_{il} + \gamma_2 * Draw_{il} + \boldsymbol{X}_{il}\boldsymbol{\gamma} + \boldsymbol{Z}_{il} * \boldsymbol{\delta} + \alpha_l + \varepsilon_{il} \tag{3-18}$$

where Z_{il} are intermediate confounders.

If we had a causal estimate ($\hat{\gamma}_1$) of the effect of the mediator on employment, controlling for the participation on the program, we could estimate the effect of the *MCMV* on employment that is mediated through migration just multiplying $\hat{\gamma}_1 * \hat{\beta}_1$.

Unfortunately, this requires that both the treatment and the mediating variable be randomly assigned. Of course, migration is not random - even conditioning on observables. Thus, we cannot directly estimate the ANIE with this procedure.

Note, however, that we $\hat{\gamma}_1 * \hat{\beta}_1$ might be informative even if it is not causal. We expect $\hat{\gamma}_1$ to be overestimated, once we are not controlling for auto-selection of migration - which is positively correlated with both unobservable ability and employment probability. If this is the case, $\hat{\gamma}_1 * \hat{\beta}_1$ provides an upper bound of the effect of the program mediated through migration.

In Table 3.10, we present the results obtained estimating equation (3-18). The dependent variable is a dummy for employment in both formal and informal labor markets.

Table 3.10: Effect of migration on employment

Migration	0.019***	0.014***
	(0.004)	(0.004)
Baseline covariates		✓
Intermediate Confounders	✓	✓

Note: Estimates of equation (1-12). All estimates lottery fixed-effects. Inference conducted with clustering at the lottery level. * p<0.05, ** p<0.01, *** p<0.001.

Estimates of Table 3.10 are consistent with our intuition. Individuals who migrate have a higher probability of being employed, between 1.4 and 1.9 percentage points. These coefficients are likely overstated. To confirm our intuition, we also estimated a Heckman two-step correction model to account for the auto-selection of migrants, similarly to Brotherhood, Ferreira, and Santos (2017). We do not have an instrumental variable for migration. Therefore, the identification of the estimator relies only on the functional form assumptions. Since these estimations tend to be very sensitive to those assumptions, we see these results as only illustrative. As we would expect, we obtain slightly smaller coefficients for migration than in the OLS estimates.

Multiplying the preferred treatment effects obtained in Tables 1.6 and 3.10, we estimate that the decrease of employment probability caused by the *MCMV* and mediated through migration is not greater than 0.035 percentage points. Using a likelihood ratio test, we are able to distinguish this magnitude from zero. Despite being statistically significant, this estimate is very small. We conclude that migration is, in fact, a relevant mediating variable in our housing program but unlikely to explain an important portion of the total effect of the program on employment.

4
Appendix to chapter 2

4.1
Analytical Expression for the cost of the program

In this appendix, we argue that equation (2-15), in fact, represents the value the government would need to transfer to beneficiaries through a cash transfer in order to keep their welfare constant. The total cost of the program can be expressed as:

$$G = \sum_{d_{ij}=1} m_{ij} * B \tag{4-1}$$

Since we have an estimate for how much individuals value the housing transfer, relative to cash, we argue that a cash transfer for each individual in the value of $\hat{\gamma} * B$ would keep the welfare of each beneficiary constant. Then, the total cost of this alternative program can be written as:

$$\tilde{G} = \hat{\gamma} \sum_{d_{ij}=1} m_{ij} * B \tag{4-2}$$

The validity of this argument is not obvious for two reasons. First, it is not obvious that a transfer of $\hat{\gamma} * B$ would in fact make all individuals indifferent between the cash transfer and the housing transfer since $\hat{\gamma}$ is the estimated valuing of the housing transfer when individuals have no income. Then, it is not true in general that alterations in the nature of the program would, in fact, make the welfare of beneficiaries constant. This point is made by McFadden (1995) in the context of the calculation of the equivalent variation with non-linear consumption entering utility.

Second, it is not true in general that a change in the nature of the program would keep the optimal decision of every individual constant, even if the beneficiaries' welfare is kept constant. Then, the change in the nature of the program may change the extensive margin of program participation and influence the cost of the program through this channel.

In order to analyze the first issue, consider the transfer $\underline{B}$ that would make beneficiaries indifferent. That is:

$$U(C_j, h_{ij}, m_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j, \epsilon_{ij}^{hm}, \boldsymbol{\theta}|\hat{\gamma}, B) = U(\tilde{C}_j, \tilde{h}_{ij}, \tilde{m}_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j, \epsilon_{ij}^{hm}, \boldsymbol{\theta}|\gamma = 1, \underline{B})$$

where $\tilde{C}_j$, $\tilde{h}_{ij}$, $\tilde{m}_{ij}$ are the beneficiaries' choices under the alternative nature of the program.

Suppose first that $\underline{B} = \hat{\gamma} * B$ makes all consumption terms equal in the above expression. Then:

$$U(C_j, h_{ij}, m_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j, \epsilon_{ij}^{hm}, \boldsymbol{\theta}|\hat{\gamma}, B) = U(C_j, \tilde{h}_{ij}, \tilde{m}_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j, \epsilon_{ij}^{hm}, \boldsymbol{\theta}|\gamma = 1, \hat{\gamma}*B)$$

Suppose, as usual, that the random error disturbance is invariant to the the change in the nature of the program. Since the remaining variables of the utility function are not affected by the change in the nature of the program, then, if δ_{ij}^{hm} was chosen under the original nature of the program, it must be that:

$$\{C_j, h_{ij}, m_{ij}\} = \underset{C_j, \tilde{h}_{ij}, \tilde{m}_{ij}}{\arg\max}\, U(C_j, \tilde{h}_{ij}, \tilde{m}_{ij}, \boldsymbol{X}_{ij}, \boldsymbol{Z}_j, \epsilon_{ij}^{hm}, \boldsymbol{\theta}|\gamma = 1, \hat{\gamma} * B) \quad \text{(4-3)}$$

Therefore, if the transfer $\underline{B}$ keeps the consumption terms constant, then all the remaining choices will be the same. This implies that there are no extensive margin effects on the cost of the program. Now, let's evaluate whether this transfer, in fact, keeps the consumption constant.

In order to simplify notation, write:

$$o_j = w_{ij} * h_{ij} + N_{ij} + \sum_{k \neq i \in j} r_{kj}$$

which are the sources of income of family j which are not influenced by program participation.

Note that:

$$C_j = \frac{o_j + m_{ij} * (\hat{\gamma} * B - p_{ij})}{n_j} = \frac{o_j + m_{ij} * (\underline{B} - p_{ij})}{n_j} = \tilde{C}_j$$

which implies that:

$$\underline{B} = \hat{\gamma} * B \quad \text{(4-4)}$$

Even more, note that a similar argument applies for non-linear terms of consumption:

$$C_j^2 = \frac{o_j^2 + 2 * o_j * m_{ij}(\hat{\gamma} * B - p_{ij}) + m_{ij} * (\hat{\gamma} * B - p_{ij})^2}{n_j^2}$$

and

$$\tilde{C}_j^2 = \frac{o_j^2 + 2 * o_j * m_{ij}(\underline{B} - p_{ij}) + m_{ij} * (\underline{B} - p_{ij})^2}{n_j^2}$$

Thus, if we impose the equality in the previous equations, we will have that:

$$2 * o_j * m_{ij}(\hat{\gamma} * B - \underline{B}) + m_{ij} * [(\hat{\gamma} * B - p_{ij})^2 - (\underline{B} - p_{ij})^2] = 0$$

which is clearly also solved by equation (4-4). A similar argument applies to the cubed consumption term. This also implies that the transfer $\hat{\gamma} * B$ keeps the welfare of each individuals constant.

Thus, we have shown that, since the change in the nature of the program affects utility only through consumption, then the non-linear marginal utility of consumption is not a restriction to calculate the equivalent variation. We also show that, once the consumption is kept constant by the equivalent variation, the optimal choice of each beneficiary is also not changes by the equivalent variation. It follows directly that equation (4-2), in fact, represents the alternative cost of the program.

4.2 Alternative Counterfactual

In section 7.2, we conduct a policy experiment where we evaluate how a change in the nature of the benefit would affect the behavior of beneficiaries. In particular, we change how much individuals value the housing transfer to the equivalent of a cash transfer.

However, it should be considered that the change in the nature of the benefit might not only affects the perceived benefit of the program but also the involved costs. In this appendix, we consider an alternative counterfactual where we also change the perceived costs.

We incorporate three different costs of participating in the program in the model: λ_0, λ_1 and λ_2. The first one is a fixed-cost of participating in the program. As we discussed in the main text, this fixed-cost may represent a perceived stigma to participate or a moving cost. The government provided help with the moving process, which tends to attenuate λ_0. Thus, we opt for keeping this cost as estimated in the main text.

However, λ_1 and λ_2 are varying moving costs. Then, in this alternative counterfactual, we also set these two cost parameters to zero, besides setting γ to one. We show the results in Table B1:

Table 4.1: Alternative nature of the benefit with no moving costs

	Original Prediction	Counterfactual Prediction
Take-up	0.709	0.918
Full-time employment	0.197	0.084
Part-time employment	0.194	0.087
No employment	0.601	0.828

Note: Comparison of probabilities predicted by the original model and by the model with the discount factor set to one and moving costs set to zero. The combination of choice probabilities was aggregated in order to display the moments of interest.

As expected, the take-up of the program would be even higher. If we compare Tables 8 and B1, we can see that setting the moving costs to zero increased participation in the program by three additional percentage points. The labor supply decisions are barely changed. We can see that the main conclusions of section 7.2 are not changed in this alternative experiment.