Priced-out: Rent Control, Wages, and Inequality*

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We show that after a quasi-exogenous loss of rent control, low-income workers' earnings decline significantly, while high-income workers remain unaffected, even as both are equally likely to migrate outside the city. The wage decline stems from transitions to lower-quality jobs. Additional tests suggest that, in contrast to high-income, low-income workers face high commuting costs. For example, the negative impact on wages is uniquely concentrated among individuals who do not own a personal vehicle and rely on public transit for commuting. We introduce a general equilibrium model of residential and employment location choices in the presence of rent control. A means-tested transfer could achieve the same earnings gains as rent control but with significantly lower taxation on landlords. Housing vouchers must however be targeted toward neighborhoods with low commuting costs to high-productivity areas; otherwise, vouchers could fail to improve labor outcomes and increase income inequality.

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1 INTRODUCTION

Most major cities worldwide are facing a housing affordability crisis.[1] In the United States, over 50 percent of residents report that affordability is a significant challenge in their city.[2] Attempting to address this crisis, many politicians have implemented or strengthened rent control policies. For example, Berlin, Hong Kong, Minneapolis, New York, and Portland have recently introduced or expanded rent control policies, while London, Boston, Florida, and Scotland are considering the introduction of rent control laws. This widespread adoption suggests large economic benefits from rent control. Yet, a vast literature demonstrates several negative implications of rent control policies. They can reduce housing supply and quality, lead to negative house price spillovers, fail to transfer wealth, and limit migration (Ahern and Giacoletti, 2022; Autor et al., 2014; Diamond et al., 2019; Gyourko and Linneman, 1990; Hahn et al., 2023; Mense et al., 2023; Moon and Stotsky, 1993).

Rent control also causes spatial misallocation of individuals (Favilukis et al., 2022; Glaeser and Luttmer, 2003), potentially resulting in misallocation of workers in the labor market due to commuting frictions. On the one hand, rent control may offer job proximity to those in controlled units, helping alleviate commuting-related frictions and leading to higher wages. On the other hand, as access to price-controlled homes reduces the incentive to relocate, those in rent-controlled homes are likely to decline job opportunities far from their residence, leading to lower wages. As a result, it is unclear how access to rent control affects the wages of an average worker. Whether rent control leads to distributional effects depends on the distribution of amenities, commuting costs, and rent control itself. For example, if high-income workers have a strong preference to live in neighborhoods with high amenities but large commuting costs to productive areas, access to rent control might increase their wages.

[1] See for example this recent news article "The Housing Affordability Crisis Is Going Global". (2024, December 16). *Wall Street Journal*.

[2] "A growing share of Americans say affordable housing is a major problem where they live", *Pew Research*, 2022

It is non-trivial to identify the reduced form effect of access to rent control on labor outcomes. Individuals who choose rent-controlled homes are systematically different from those who do not. To address this challenge, we explore a rent control law change in Portugal. In August 2012, in an effort to reform the rental market, the newly elected Portuguese government abolished almost all existing rent control policies. Prior to 2012, rental contracts that had been signed before 1990 were under rent control. However, post-2012, only renters aged 65 or older with contracts dating from before 1990—hereafter, *legacy renters*—were subject to rent control. This regime prevents landlords from renegotiating rental values to market prices.[3] Legacy renters can retain their rent-controlled units until they pass away. We exploit this change in rent control laws in two different ways.

First, we examine the quasi-exogenous death of a legacy renter to study the career paths of residents younger than 64 sharing the same dwelling.[4] More concretely, we estimate a difference-in-differences model comparing the career trajectories of workers living with a legacy renter who passes away, to those residing with a surviving legacy renter with same gender, similar age and health condition, and located in the same neighborhood. When a legacy renter passes away, co-residents lose access to rent control.[5] They can negotiate with the landlord, but rents may be updated to market prices. A simple balancing test shows that workers living with a legacy renter that dies are identical on a wide range of observables (e.g., age, education attainment, pre-shock rent values, or pre-shock income) to those living with a surviving legacy renter.

Second, we employ the 2012 rent control law change in an analysis that leverages on the age discontinuity thresholds established in the law. We compare working individuals in rent-controlled units living with a renter just below 65 to those living with a renter just above 65, before and after 2012.[6] Those living with a renter slightly younger than 65 lost rent control after 2012. In this analy-

[3]This special treatment of elder renters has remained in place even after the most recent law changes in 2017.

[4]In Lisbon and Porto, approximately 20 percent of workers younger than 64 live with a renter older than 65. Culturally, it is common for children and grandchildren to live with their parents until a later age, especially when it provides access to homes in the city center.

[5]There is an exception to this rule if the legacy is survived by a spouse older than 65. Due to this exception, we only analyze deaths of legacy renters that are not survived by a spouse older than 65.

[6]The age of the elder is measured in November 2012, when this law was enacted.

sis, we evaluate the main results from the first specification. However, the limited sample size near the age discontinuity restricts us from conducting many sub-sample analyses.

Our dataset merges housing and employer-employee linked data from Portugal. This data allows us to measure residence and workplace locations, salaries, hours worked, hourly wages, employer characteristics, rental contract information, and demographic data. We also complement this data with death records. We focus on the two major cities in Portugal, Lisbon and Porto.[7] Our panel starts in 2010 and ends in 2020, and we only examine the career paths of working individuals with ages between 18 and 64.

We first document that, following the death of a legacy renter, households are more likely to move to the outskirts of Lisbon and Porto. The likelihood of moving to the outskirts increases by over 12 percentage points in the 4 years after losing access to rent control. This is an increase over 80 percent relative to the unconditional mean. An event plot analysis shows that these effects are persistent over time, indicating that workers in our sample do not reallocate back to the city center. Next, we show that workers who lose access to rent-controlled homes are more likely to experience a decline in earnings of almost 2 percent. This effect is concentrated among individuals aged between 35 and 50, ruling out that surviving spouses drive the results. We document similar findings using the alternative specification that exploits the age-based discontinuity in access to rent control. Post-2012, families with a renter aged 62-64 or 60-64 experienced a 2 percent wage decline compared to families with renters aged 65-67 or 65-69, respectively.[8] The point estimates are almost identical between the two specifications, suggesting that the impact of the death of a legacy renter must only affect wages through the loss of rent control.

The wage results conceal substantial heterogeneity. Individuals with above-median income in rent-controlled homes experience no decline in earnings following the loss of rent control. In stark contrast, individuals with below-median income in rent-controlled units experience a significant and persistent drop in earnings after losing access to rent control. Additional results reveal that

[7] We detail the history of rental laws in Portugal in section 2.1.

[8] Post-2012, renters younger than 65 did not retain access to rent control.

this negative effect stems uniquely from transitions to new jobs. These findings suggest that low-income workers cannot afford commuting costs to higher-paying jobs in the city center. Supporting this mechanism, we show three key results. First, low-income individuals rely more on public transportation for commuting, whereas those with higher income workers are significantly more likely to use personal vehicles. Second, we show that commuting by car is significantly faster, taking less than half the time required for public transportation. Third, we observe that the wage losses following the elimination of rent control are concentrated among individuals who do not own a vehicle. These results help lend support to the role of commuting frictions in explaining our main findings.

We next examine alternative counterfactual policies to rent control. We introduce an urban commuting model following Monte et al. (2018) and Redding and Sturm (2024), where workers simultaneously choose their place of residence and employment. We embed access to rent control in the model as a lottery following Favilukis et al. (2022). We leverage the granularity of our data to estimate our model for the city of Lisbon. Since we observe commuting decisions and rental market outcomes at the worker level, we are able to estimate structural parameters for low- and high-income workers separately, leading to a high degree of accuracy when matching targeted moments. Notably, we also succeed in matching several untargeted moments, such as the distribution of market rental rates across neighborhoods.

We use our estimated model to conduct several counterfactuals. First, we eliminate rent control city-wide and evaluate the effects on workers. Surplus flows almost entirely from low-income workers (-3.16% on average) to landlords (+13.69%). High-income workers who lose their rent control option are modestly better off on average (+0.20%) since they are less likely to exercise the option in the first place and since rent control leads to inflated rental prices in the free market. Similar to the reduced-form results, low-income workers with prior access to rent control in the city center are more likely to migrate to and switch to a job in the outskirts, leading to lower average wages. High-income workers in contrast only experience a minimal wage decrease.

The substantial gains in landlord surplus motivates our remaining counterfactuals. We consider a means-tested transfer policy in which workers are given a voucher depending on their skill type. The amount of the voucher also depends on the neighborhood. We first consider a counterfactual policy in which neighborhoods with the highest marginal expected utility receive the largest vouchers. We then consider an alternative policy in which neighborhoods with the lowest commuting costs to high productivity areas receive the largest transfers.

Means-tested transfers have nuanced effects on welfare and labor outcomes. Voucher policies designed to maximize the utility of low-income workers leave average wages unchanged relative to the rent control equilibrium but increase wage inequality across the city: even though they improve the welfare of all constituents, including high-income workers and landlords, the average wage differential between low-income workers in the outskirts and the city center increases by 1.4%. In contrast, voucher policies targeted at neighborhoods with low commuting costs to high-productivity labor markets produce similar labor market outcomes as the rent control equilibrium while still improving average welfare to all constituents.

Our work highlights that rent control provides low-income workers with proximity to jobs. One might think this evidence strongly supports rent control. However, our counterfactual analysis shows that alternative policies with much lower distortions can achieve similar labor market benefits as rent control. In March 2023, the Portuguese government introduced a means-tested housing voucher policy to support households whose rent payments exceed 35% of their income. Our findings support the effectiveness of this policy relative to rent control; however, our paper also emphasizes the importance of targeting neighborhoods with better access to high-productivity areas.

Related Literature. This paper contributes to several different literatures. First, it advances the literature that examines the misallocation and externalities associated with rent control laws.[9] Glaeser and Luttmer (2003) find that, in New York City, a significant fraction of rent-controlled

[9] Kholodilin (2022) provides a recent review of the literature.

apartments are likely misallocated across demographic subgroups. Favilukis et al. (2022) employ a rich theoretical framework calibrated in New York, and show that rent control policies carry significant insurance value for low-income households despite the misallocation in labor and housing markets. Autor et al. (2014) find that, in Massachusetts, rent decontrol lead to large capital gains in decontrolled units and nearby never-controlled units.[10] We demonstrate that rent control results in misallocation in the labor market, benefiting low-income workers, but not high-income workers.

Second, our paper contributes to the literature on the labor market effects of housing affordability policies.[11] Prior research highlights diverse impacts of such policies. Rent control, for example, increases local job-finding probabilities (Svarer et al., 2005) but reduces mobility while limiting displacement (Diamond et al., 2019). Introduction of market-rate housing in low-income areas increase in-migration from low-income areas (Asquith et al., 2023). Public housing has mixed effects, often negatively influencing labor outcomes unless it helps moving into high-income neighborhoods (Van Dijk, 2018). Housing vouchers may improve earnings (Pollakowski et al., 2022) and reduce rent burdens, overcrowding, and homelessness, though they are less effective in promoting re-locations to high-opportunity areas (Ellen, 2020). Additionally, rent control policies can increase accumulation of human capital by increasing college attendance among young adults (Öst and Johansson, 2023). We add to this literature by showing that housing vouchers targeted to neighborhoods with low commuting costs to high-productivity areas offer labor market benefits comparable to rent control, suggesting that geographically targeted policies can effectively balance affordability and access to economic opportunity.

Lastly, our paper contributes to the literature on spatial frictions and worker welfare. Bilal and Rossi-Hansberg (2021) introduce the concept of a "location asset," where rent costs are offset by future employment opportunities. Reductions in commuting costs generate welfare gains (Monte et al., 2018), while job attractiveness declines sharply with distance (Manning and Petrongolo,

[10] Rent stabilization may also disproportionately benefited white tenants, who are more likely to occupy rent-stabilized units than non-white tenants and also receive higher rent discounts (Chen et al., 2022).

[11] Luque et al. (2019) offers a thorough review of the literature on different housing affordability policies.

2017). Transportation infrastructure also impacts spatial dynamics: Heblich et al. (2020) estimate that reducing London's railway network decreases population density, land value, and commuting into the city center. Rising top incomes drive spatial sorting, benefiting wealthier households (Couture et al., 2019), while Los Angeles Metro Rail connections reduce commuting frictions but show limited productivity effects (Severen, 2019). Our paper extends this literature by showing that earnings losses from the removal of rent control stem from reduced job proximity and that means-tested housing vouchers targeting low-commuting-cost neighborhoods can alleviate the impact of commuting frictions on labor outcomes.

2 HOUSING MARKET IN PORTUGAL

This section provides a concise overview of the housing market in Portugal. Throughout our sample period, rent-controlled homes offered access to rent prices significantly below market rates. However, as highlighted below, the advantage of rent-controlled homes is more pronounced in the later part of the sample period.

2.1 Rent Control Laws in Portugal

We offer a brief summary of rental laws in Portugal, emphasizing the most recent law changes, especially those affecting renters older than 65.[12] The first evidence of rent stabilization policies in Portugal dates back to 1910, when any price updates for rents below certain pre-determined thresholds were prohibited.[13] A few years later, during the World War I, landlords could not terminate rental contracts at will, and contract renewals were automatic unless a tenant wished to terminate a rental agreement.[14]

Additional legislation was introduced to improve tenant protection in the following years. For

[12] For a detailed summary of rental laws in Portugal between 1910 and 1990, we direct readers to *Decreto-Lei nº 321-B/90, de 15 de Outubro de 1990*.

[13] See for example *Decreto de 12 de Novembro de 1910*.

[14] Landlords were also obligated to rent any empty apartment or building.

example, in 1922, the method to determine rent prices was indexed to a building's initial value and age; while in 1948, a tax-assessed property value was used to determine rent price updates.[15] Almost two decades later, in 1966, the ability for a landlord to terminate a rental contract was revoked. A landlord could not terminate a rental contract, and the contract would renew automatically until terminated by a tenant.[16] This was a major change in the Portuguese rental market with repercussions for many years to come.

Additional pro-tenant reforms were signed into law after the establishment of the first democratic government following the 1974 Carnation Revolution.[17] In 1979, eviction protections for renters older than 65 were signed into law. This is the first time renters older than 65 were mentioned in rental laws. After 1981, only two different rental regimes were allowed. The first, *renda livre*, let landlords and tenants negotiate freely a starting rent value, but had stringent limitations on any subsequent price updates. The second regime, *renda condicionada*, limited the maximum initial rent value based on a pre-determined percentage of the tax-assessed property value, but allowed annual rent price updates that were determined by a government agency.

Fifteen years post the 1974 Carnation Revolution, Portuguese government officials acknowledged that the rental market was inefficient and unable to offer a solution to the existing housing needs. Rental units and buildings lacked maintenance, and construction of new housing units had declined substantially. Several politicians claimed that this stagnant and unresponsive rental market stemmed from distorted low rent values and an inability to terminate rental contracts by landlords.[18] With this backdrop, new legislation aiming to revitalize the rental market, was introduced in 1990. This was the first attempt to relax some pro-tenant protections and liberalize the rental market. One major change was introduced in 1990. Lease contracts with a limited term were allowed again, giving the right to a landlord to refuse a contract renewal. But to avoid a po-

[15] For more detail, see *Decreto-Lei nº 9118, de 10 de Setembro de 1923* and *Decreto-Lei nº 2030, de 22 de Junho de 1948*.

[16] Rent values for new contracts were unrestricted, but future rent updates were capped by the value of the building determined by a tax-assessed property value. See article 1095 in *Decreto-Lei nº 47334, de 25 de Novembro de 1966*.

[17] The Carnation Revolution (also known as *25 de Abril*) was a coup led by left-leaning military officers that toppled the authoritarian right-wing regime on April 25,1974. The revolution resulted in the transition to a democracy.

[18] See the motivation and introduction of *Decreto-Lei nº 321-B/90, de 15 de Outubro de 1990*.

litical turmoil, this reform did not apply to lease agreements signed prior to this date, effectively exempting older tenants, and creating the existence of legacy contracts.

The next major reform occurred in August 2012, shortly after the election of a pro-business government in June 2011.[19] This reform aimed to fully liberalize rental markets as it transitioned out most rent control contracts. It allowed landlords to renegotiate rent values and even to terminate rental contracts (with 2 years' notice). However, this reform did not apply to tenants older than 65 with a contract signed prior to 1990.[20] Consequently, these old lease contracts effectively maintained the original protective privileges—i.e., low rents protected by the impossibility of renegotiation or termination of the agreement. This status still prevails until today.

Landlords are then bonded to these legacy lease agreements until an elderly tenants dies. If a tenant dies, a lease might be transferred to a surviving spouse or descendants, provided they shared residency with the deceased. However, the terms of an original lease are only maintained if the succeeding tenant is older than 65 or has a severe disability. If a lease agreement is transferred to a younger and working-able tenant, a landlord can renegotiate contract terms, rent values, or terminate it. The 2 year notice does not apply in this case.

These legacy leases have extremely low rents in current euros. According to the 2011 Census, 71 percent lease agreements signed before 1990 have rents under 100 euros, and 44 percent involve rents lower than 50 euros. These are rents well below market prices—in 2011, the average rent in Lisbon was 185 euros, while in Porto was 150 euros.

3 DATA

Our data is provided by the *Statistics Portugal*. Statistics Portugal combines data from several sources: social security, death records, employer-employee matched data. Access to these datasets was granted through the secure infrastructure of Statistics Portugal's safe center, where all data

[19] More details can be found in the *Lei n.º 31/2012, de 14 de Agosto de 2012* .

[20] Individuals with at least 60 percent disability with rental contracts signed prior to 1990 were also exempted.

processing was conducted. We describe in this section each dataset, the construction of the final dataset, and key summary statistics of these data.

3.1 BPR

Our primary dataset is the Portuguese Administrative Census dataset (BPR).[21] This dataset encompasses all individuals residing in Portugal from 2010 through 2020. This data is gathered annually by the Statistics Portugal. From this dataset, we extract information on individuals living in rented houses in 2010 in the two largest municipalities in Portugal: Lisbon and Porto. We further filter our sample based on household composition. Specifically, a household must have an elderly individual (aged 65 or older), reside in a rent-controlled home, and have at least one working-age adult (between 18 and 64 years old).

The dataset includes demographic data such as age, education, gender, and marital status, as well as home addresses and an identifier that tracks individuals over time. Additionally, we gather information on the rental properties, including size, date of the rental agreement, and the rent amount. We only include privately-owned rental properties, excluding any properties under government-sponsored rental assistance programs. The BPR also enables us to identify deaths through the yearly change in the number of people who are alive. We proxy for health conditions based on whether an elder receives social benefits for disability.

3.2 Employer-employee linked data

Labor market information is obtained from *Quadros de Pessoal*. This is a longitudinal employee database that covers all workers in the private sector and was merged to the BPR by Statistics Portugal.[22] We observe this data for every year between 2010 and 2020. From this database we obtain monthly salaries (defined as the sum of the base wage plus the regular benefits) and number of

[21] A detailed explanation of the BPR can be found in this link: https://www.ine.pt/xportal/xmain?xpid=INE&xpgid=dia_europ_est

[22] For detailed information about *Quadros de Pessoal*, see for example Blanchard and Wolfers (2000).

hours worked per month. We define wages per hour as the ratio between monthly salary and total number of hours worked. We also observe the identity of an employer, which allows us to assess whether workers switch jobs. We supplement this information with data from the annual personal income tax declarations (IRS) and data on family relationships from the population registry (BPR). Once we restrict the sample to individuals with wage information in *Quadros de Pessoal*, we obtain a final dataset containing almost 35,000 working-age adults. Our final dataset does not include individuals working in the public sector and those who are self-employed.

4 EMPIRICAL DESIGN

It is challenging to identify the effect of access to rent-controlled homes on labor outcomes. For instance, households that apply to live in a rent-controlled home might have unobservable characteristics that impact their labor market outcomes. Similarly, households that opt to exit rent-controlled homes may possess unobservable characteristics that could likewise impact labor market outcomes. These conditions make it difficult to identify our hypothesis by merely comparing those entering, residing, or exiting endogenously rent-controlled homes with those who do not.

We leverage rental control laws in Portugal to address potential endogeneity issues. Renters older than 65 with rental contracts established prior to 1990—referred to as "legacy renters"—benefit from a rent control regime that prevents landlords from renegotiating rental values to match market prices.[23] This special provision for legacy renters remains effective to this day. Legacy renters only lose their access to rent-controlled homes upon death. We exploit this fact to test our hypothesis. Our first approach involves a difference-in-differences (DiD) model, and our second approach uses the age discontinuity in access to rent control. We describe both in this section.

[23] For more information on rental laws in Portugal, please refer to section 2.1.

4.1 The death of a *legacy* renter

Our baseline sample focuses on working individuals younger than 64 that live with a legacy renter. A treated worker meets two criteria: (i) the eldest family member in the household is older than 65 and passes away, and (ii) the rental contract was signed prior to 1990. These criteria lead to the termination of the legacy rental contract if the elder is not survived by a spouse older than 65. Rent control might still be maintained if there is a surviving spouse older than 65 and with the name on the lease.[24] Since we do not have this lease detail, our baseline comprises households with and without surviving spouses older than 65—-in robustness tests, we replicate our analysis for households without any surviving spouse older than 65. In our sample, over 74 percent only have one family member older than 65. Thus, when a legacy renter dies, family members living in the same apartment are likely to lose access to the rent-controlled home. They have an option to negotiate with the landlord, but rent values may be updated to market prices.

Our control group comprises matched living legacy renters. We match legacy renters in the control group to the treatment group based on four criteria: they have similar age, similar health conditions, same gender, and live in the same *freguesia* (neighborhood). Figures A9 and A10 depict all freguesias in Lisbon and Porto. We use whether a legacy renter receives a disability pension to proxy for their health conditions, and we create eight age classes above 65 to group legacy renters of similar age. Legacy renters in the control group that are matched to a treatment legacy renter belong to the same stratum. We assign a post period from each treatment unit to each control unit within the same stratum. We name the matched groups within the stratum a *cohort*. After applying this procedure, the age difference between a legacy renter that passes and one that survives is only 0.258 years. Thus, we assume that the passing is exogenous to labor outcomes of co-residents.

Table 1 compares workers in the treatment and control samples across several observable variables, including age, income, gender, educational attainment, rent values, and hours worked—all measured in 2011. The table reveals no significant economic or statistical differences between the

[24] See Section 2.1 for more details on this exception.

treatment and control workers in terms of age, gender, income, education level, or rent paid. Furthermore, this table provides insights into our sample. The average worker in our sample is 42 years old, with a probability of over 50 percent of being male, and a 22 percent likelihood of holding a college degree. Looking at the household composition, approximately 17 percent are aged between 18 and 35, over 19 percent are between 36 and 50, and 21 percent are between 51 and 64. This composition is slightly older than an average household in Portugal, which is understandable given that these families live with a legacy renter who is over 65. The average age of a legacy renter is 75 years old.

TABLE 1: BALANCING TESTS IN 2011

	Treatment	Control	Treatment vs Control	
	Mean	Mean	Difference	t-stat
Age (in years)	41.64	41.52	0.125	0.26
Male	0.565	0.537	0.028	1.27
College	0.218	0.235	-0.017	-0.95
Percentage of 18-35 (%)	17.10	17.37	-0.263	-0.31
Percentage of 36-50 (%)	18.60	19.23	-0.632	-0.70
Percentage of 51-64 (%)	22.07	21.31	0.759	0.75
Log(Total Salary/Hours worked)	1.549	1.574	-0.025	-1.11
Hours worked (monthly)	165.9	165.9	-0.054	-0.08
Rent category	5.119	5.094	0.025	0.25

Notes: Our Internet Appendix details the definitions of our main variables.

4.1.1 Regression specification

Our sample is restricted to individuals who live in rent control homes in Lisbon and Porto and those participating in the labor force with ages between 18 and 64. We estimate the following regression

model for worker i in year t restricted to the sample of individuals who live with a *legacy renter*:

$$\begin{aligned} \text{Y}_{i,t} &= \beta_1 \times \text{Post}_t \times \text{Legacy death}_{f(i),t} + \beta_2 \times \text{Post}_t \\ &+ \text{Individual FE} + \text{Year FE} + \text{Cohort FE} + \epsilon_{i,t}, \end{aligned} \tag{1}$$

where and *Legacy death*$_{f(i),t}$ equals one after a *legacy renter* in family f passes away. $\text{Y}_{i,f,t}$ represents the labor outcomes of individual i in family f at time t. Errors are robust and clustered at the individual level. In most regressions, we split the *Post* variable into period-by-period dummies that trace the dynamic adjustment of our variables of interest. We also include several fixed effects. We include Cohort FE to compared workers within the same stratum and death year. We also include individual and year fixed-effects to absorb any remaining individual time-invariant variation that is not accounted by our empirical design.

We note that our empirical only compares a treatment unit with never-treated units due to the Cohort fixed-effect. This is methodology helps avoid the classic problems associated with staggered differences-in-differences models (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

4.2 Age discontinuity

The death of an elder may impact the career outcomes of surviving family members through different alternative channels. For example, if the deceased elder helped with childcare, family members might need to adjust their labor supply or may experience lower productivity. Alternatively, grief associated with a family member's death could adversely affect productivity at work. We further employ the 2012 rent control law change in an additional analysis.

We leverage on the age thresholds established in the law. We compare families living with a renter slightly younger than 65 to those with a renter older than 65, pre- and post-2012. In both scenarios, the rental contracts were initiated before 1990. Age of the elder is measured on November 2012, when the new rent control law was enacted. In this second design, we anticipate findings similar to our primary specification, despite the smaller sample size around the discontinuity. We

then estimate the following differences-in-differences:

$$\begin{aligned} \text{Y}_{i,t} &= \beta_1 \times \text{Post}_t \times \text{Above 65}_{f(i)} + \beta_2 \times \text{Post}_t \\ &+ \text{Individual FE} + \text{Year FE} + CohortFE + \epsilon_{i,t}, \end{aligned} \tag{2}$$

The underlying assumption for identification is that workers living with an elder slightly younger than 65 are similar to those living with an elder slightly older than 65. This assumption becomes more plausible as the elder's age approaches 65. However, narrowing the age range of the elder to be closer to 65 reduces the sample size. We manage this trade-off by employing various age windows of 2, 5, and 10 years around the cut-off and comparing the various point estimates. While these age windows enable us to estimate average treatment effects, they pose limitations for sub-sample analyses.

5 RESULTS

This section examines the impact of rent control on wages and inequality. Section 5.1 documents the effect of rent control on migration. Sections 5.2 through 5.4 introduce the impact on wages of those who lose access to rent-controlled apartments. Section 5.5 discusses the role of commuting costs in explaining our results. Section 5.6 offers additional results.

5.1 Does loss of rent control affect migration to outskirts away from workplace?

We begin by examining whether losing access to rent controlled homes leads households to relocate away from the city. Specifically, we examine whether the loss of rent control dislocates renters to the outskirts of Lisbon and Porto. We categorize a location as the outskirts if it is outside the municipality but within the greater metropolitan area.[25] Figures A7 and A8 depict the municipalities that belong to the outskirts of Lisbon and Porto, respectively. We use our empirical design (1)

[25]For example, Amadora or Almada municipalities are considered outskirts of Lisbon. See Figures A7 and A8 for further details.

and assign the outcome variable to one if an individual relocates to the outskirts in a given year, and zero otherwise. If an individual moves within the city center, even towards the city center's periphery (e.g., Benfica neighborhood in Lisbon municipality), the outcome variable is still zero.

TABLE 2: DOES THE LOSS OF RENT CONTROL PUSHES WORKERS TO THE *OUTSKIRTS*?

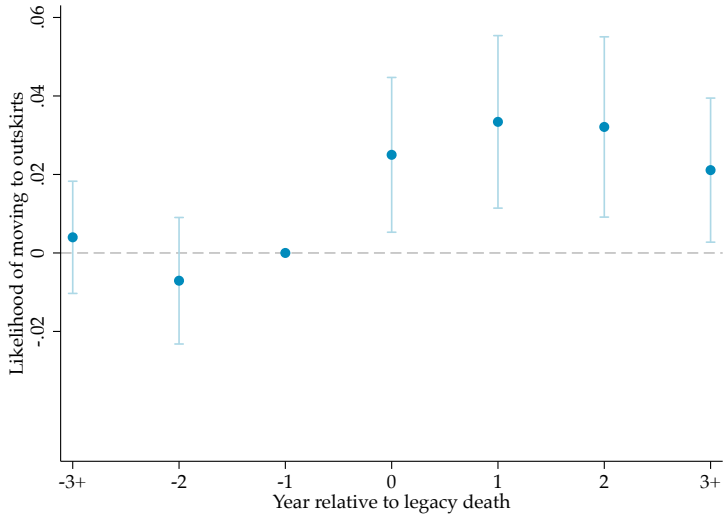
	y-var: Move to outskirts			
	Full sample		Inc<P50th	Inc>P50th
Treatment × Post	0.027***		0.0248**	0.0281***
	(0.006)		(0.010)	(0.010)
Treatment ×Post$_{t=\{0,1\}}$		0.0285***		
		(0.007)		
Treatment ×Post$_{t=\{2,+3\}}$		0.0249***		
		(0.007)		
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
N	94,986	94,986	29,731	34,980
R-squared	0.199	0.199	0.162	0.158

Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable only equals one in the year when the worker moves to the outskirts. Outskirts are municipalities in the metropolitan area of Lisbon and Porto, excluding the municipality of Lisbon and Porto. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table 2 presents the results of this estimation. Column (1) and (2) report a regression for the likelihood of moving to the outskirts after the passing of a legacy renter. A family member is 2.5 percentage points more likely to move to the outskirts in a given year. This effect is statistically significant at the one percent level. These coefficients imply that, after losing access to a rent control apartment, a family is 13.5 (=2.7×5) percentage points more likely to move to the outskirts in the four years after the passing of a legacy renter. This effect is unusually large, as it represents over 86 percent of the sample mean. Column (3) and (4) report the estimate of the same regression specification for the sub-sample of workers with incomes below and above the sample median. Any worker under rent control is equally likely to move to outside the city after losing access to

it. The point estimates are almost identical. We observe that the sample mean is lower than the population mean because our sample is limited to individuals residing in rent-controlled units. High-incomers in rent control units have incomes lower than high-incomers in the population.

FIGURE 1: EVENT PLOT ANALYSIS: MOVE TO OUTSKIRTS



Note: Our Internet Appendix details the definitions of the variables legacy death and moving to outskirts. The event plot reports confidence intervals at the 95 percent level. Standard errors are clustered at the individual level

We also offer an event plot analysis, which allow us to detail the breakdown of the effect by each period and, more importantly, test the parallel trends assumption. To estimate any event plot in our paper, we employ the following model:

$$\begin{aligned} \text{Y}_{i,t} &= \sum_{m=-3}^{3} \beta_m \times \text{Post}_{t,m} \times \text{Legacy death}_{f(i),t-m} \\ &+ \text{Individual FE} + \text{Time FE} + \text{Cohort FE} + \epsilon_{i,t}, \end{aligned} \tag{3}$$

where m is the number of years relative to the death of the legacy renter. $\text{Post}_{t,m=-3}$ and $\text{Post}_{t,m=3}$ are binary dummies that equal one for all years prior and after to $m = -3$ and $m = 3$, respectively. All other $\text{Post}_{t,m}$ equal one for year m and zero otherwise.

The period-by-period breakdown reported in Figure 1 reveals that the effect is statistically different from zero in the four years after the household loses access to rent control. Some households might attempt to negotiate with the landlord or contest the loss of rent control, while others may choose to temporarily live near their previous home before relocating to the outskirts a few years later. As such, it might take some households a few years to transition from the municipality of Lisbon and Porto to the outskirts.

5.2 Does loss rent control affect earnings?

Next, we test the central hypothesis of the paper. We examine the impact of access to rent control on earnings. The trade-off that individuals make between location and employment choices suggests that the effect is unclear. Table 3 reports results that shed light on this trade-off. We use our baseline regression model where the outcome variable is the log of wages per hour, defined as monthly total salary divided by number of hours worked.

Table 3: Does loss of rent control affect wages per hour?

	y-var: Log(Monthly salary/hours)			*y-var*: Job in Outskirts	
	Full sample	High-turnover firm	Other firms	High-turnover firm	Other firms
Treatment × Post	-0.0175**	-.02692**	-.00741	.03153***	-.00227
	(0.007)	(0.011)	(0.009)	(0.009)	(0.005)
Year FE	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
N	106,256	56,578	49,670	52,732	47,297
R-squared	0.938	0.901	0.961	0.137	0.102

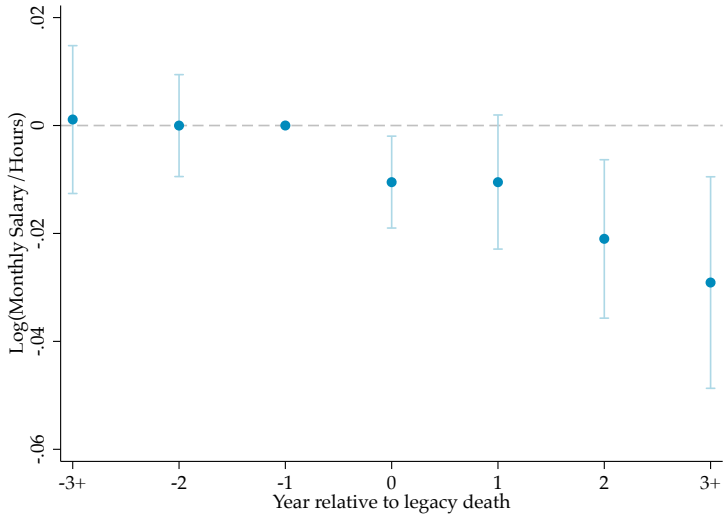
Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable is the log of the of the monthly salary divided by the number of hours worked. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

The results show several patterns consistent with access to rent control helping alleviate commuting-related frictions for an average individual in the sample. First, the loss of access to

rent-controlled homes causes a significant decline in earnings; specifically, it leads to an average decline of 1.8 percent in earnings. Second, the wage decline stems from transitions to new jobs. We focus on likely "forced" moves to avoid examining endogenous job moves. We measure whether an employee is more likely forced to move by assessing if they were working for a high-turnover firm. We define a firm as high-turnover if more than 30 percent of the workforce left the company in one year during our sample period (2010-2020). For those working for a high-turnover firm, earnings decline by 2.7 percent after the loss of rent control. In contrast, for those who were not forced to move jobs, the decline is only 0.7 percent, and this effect is not statistically different from zero.

We also investigate whether those who work in high-turnover companies are more likely to move to jobs in the city outskirts. Among these workers, the likelihood of moving to a job in the outskirts increases by 3.15 percentage points. For those in low-turnover firms, there is no change in the likelihood of taking a job in the outskirts after losing access to rent control. This evidence shows that an average worker loses earnings following the loss of access to rent control, and this loss stems from the increased likelihood of moving to a job in the city outskirts that pays lower wages.

FIGURE 2: EVENT PLOT ANALYSIS: EARNINGS



Note: Our Internet Appendix details the definitions of the variables legacy death, monthly salary, and hours. The event plot reports confidence intervals at the 95 percent level. Standard errors are clustered at the individual level

Table 3 suggests that wage losses following the loss of rent control occur exclusively when employees work for. high-turnover firms. This phenomenon is likely due to low-income workers underestimating wages in other locations (Jäger et al., 2024). In our setting, this issue likely stems from workers relocating to the city outskirts, where they possess limited information about the labor markets. This suggests that the negative effect of loss of rent control on wages could be larger if workers were better informed about labor markets and did not underestimate wages outside their current firms.

Figure 2 presents the event plot analysis for the effect of a legacy renter's death on wages per hour. The plot demonstrates that the parallel trends assumption holds. It also unveils a steady decline in wages per hour following the death of a legacy renter. Four years after losing access to rent control, workers experience an almost 3 percent decline in earnings compared to those who still live in rent control homes. Finally, the plot also indicates that the impact of losing access to rent control on earnings is persistent over time.

Table 4: Age Discontinuity Analysis

	y-var: Log(Monthly salary/hours)		
	Treat: 65-75 Contr: 55-64	Treat: 65-70 Contr: 60-64	Treat: 65-67 Contr: 63-64
Treatment ×Post	-0.0210***	-0.0224***	-0.0218*
	(0.006)	(0.007)	(0.012)
Year FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
N	38,029	21,410	10,084
R-squared	0.943	0.943	0.941

Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable is the log of the of the monthly salary divided by the number of hours worked. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

5.2.1 Age discontinuity

We also exploit the age-based discontinuity in access to rent control introduced by the 2012 law change. All rental contracts established before 1990 retained their rent control status until 2012. However, post-November 2012, only renters older than 65 maintained access to rent-controlled homes. We adopt an identification strategy, detailed in the empirical section, to leverage this discontinuous access to rent control. The results of this analysis are presented in Table 4. The findings show that after 2012, families living with renters with ages between 60-64 or 62-64 experienced more than a 2 percent wage decline compared to families residing with renters in the 65-67 or 65-69 age range. These estimates are nearly identical to our primary specification's results. These additional tests confirm that the outcomes from our primary specification are driven by the loss of rent control due to an legacy renter's death.

Table 5: Effect on earnings by income

	y-var: Log(Monthly salary/hours)		
	Very low-income $[Inc < P10th]$	Low-income $[P10th < Inc < P50th]$	High-income $[Inc > P50th]$
Treatment ×Post	-0.009	-0.022**	-0.007
	(0.023)	(0.009)	(0.012)
Year FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
N	6,838	23,659	35,040
R-squared	0.749	0.744	0.917

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable is the log of the of the monthly salary divided by the number of hours worked. Based on income in 2010, we split the sample into different income groups. Very low-income are those with income below the 10th percentile, low-income have earnings between the 10th percentile and the median, and high-income have earnings above the median. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

5.3 Heterogeneity across income groups

Next, we examine the impact of loss of rent control across different income groups. This analysis is motivated by the potential varying effects that commuting costs may have on these income groups. The financial burden of commuting may be significant for low-income workers, while these costs might be relatively minor for high-income workers living in rent control. On the other hand, the productivity of high-income workers is more likely sensitive to commuting times. These differences could potentially lead to different impacts of rent control on workers with different income levels. To evaluate these conjectures, we divide our sample into three income groups and estimate our baseline regression on the effect of losing access to rent control on earnings. Table 5 reports these results.

Individuals with incomes above the 50th percentile experience no losses in earnings after losing access to rent control. In sharp contrast, households below the median income experience a significant decline in earnings that continues to deepen after losing access to rent controlled homes. This evidence implies that loss of rent control negatively impacts the earnings of low-income workers

by limiting their proximity to higher quality jobs. While high-income workers do not have wage benefits from access to rent-controlled homes since they can afford commuting costs. We also show that very low-income, those with incomes below the 10th percentile, do not experience a loss in earnings. This is because wages of these workers are bounded by minimum wage regulation that is pervasive in Portugal (Oliveira, 2023).

5.4 Does access to rent control affect labor supply and unemployment?

5.4.1 Labor supply

Theoretically, it is unclear whether access to rent control homes leads to an increase or decline of supply of labor. On the one hand, a longer commute could lead to a reduction in number of hours worked. On the other hand, if disposable income declines after the loss of rent control, workers might be more willing to work longer hours. Since we observe number of hours worked in our dataset, we test this trade-off.

Table 6: Does loss of rent control affect to labor supply or unemployment?

	y-var: Log(Hours worked)			*y-var*: Unemployment		
	Full Sample	High-turnover	Other firms	Full Sample	High-turnover	Other firms
Treatment × Post	0.002	0.003	-0.001	0.002	0.002	0.003
	(0.003)	(0.004)	(0.001)	(0.001)	(0.004)	(0.002)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
N	106,256	56,578	49,670	106,148	56,526	49,614
R-squared	0.673	0.564	0.763	0.378	0.338	0.476

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

The first column of Table 6 reports the estimated coefficients of our baseline regression on the number of hours worked. The results show that the effect on hours worked is economically and

statistically zero. This is also true regardless if an individual works in a high- or low-turnover firm (Columns 2 and 3). After the death of a legacy renter, co-resident workers do not increase hours worked, suggesting that the above trade-off might balance out.

5.4.2 Unemployment

The results above show that a worker is more likely to move to the city outskirts following the loss of access to rent control. One natural implication is that these workers might become unemployed. This could help us better understand the loss in earnings. As such, we examine whether workers who lose access to rent control are more likely to become unemployed than counterparts who stay in rent controlled homes.

Columns (4) through (6) of Table 6 report the estimates of this model. The point estimates are neither statistically or economically significant after a legacy death. That is, after a legacy death, family members are not more likely to become unemployed. Additionally, for workers in high-turnover firms, there is no difference in unemployment rates between those who lose rent control and those who continue to live in rent control. This is important as it shows that both groups might face similar search costs. Taken together, these results suggest that the negative wage effect documented above does not stem from differences in unemployment spells.

5.5 Commuting Costs

Our findings indicate that low-income workers face challenges in covering commuting expenses to access better-paying employment opportunities in the city center. In this section, we offer further evidence supporting this hypothesis.

5.5.1 Commuting in Lisbon and Porto

We start by presenting two additional pieces of evidence. First, low-income individuals are more dependent on public transportation for their commute, in contrast to their higher-income counter-

parts, who are more likely to use personal vehicles. Specifically, only 41.24 percent of low-income workers use a vehicle for their commute to the city, whereas 64.60 percent of high-income workers commute by vehicle. Second, our analysis reveals that travel by car is substantially faster, requiring less than half the time compared to public transportation. More concretely, commuting from the outskirts to the city by car takes on average 26 minutes each way, while using public transportation takes 44 minutes.

TABLE 7: EFFECT ON WAGES AND MODES OF TRANSPORTATION

	y-var: Log(Monthly salary/hours)				*y-var*: Job in Outskirts	
	High-turnover firm		Other firms		No car	
	Owns car	No car	Owns car	No car	High-turnover	Other firms
Treatment × Post	-0.0024	-0.0387***	0.0000	-0.0116	0.0387***	0.0045
	(0.023)	(0.013)	(0.015)	(0.011)	(0.011)	(0.006)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
N	17,506	39,066	19,065	30,586	36,340	29,071
R-squared	0.915	0.884	0.955	0.962	0.141	0.119

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable is the log of the of the monthly salary divided by the number of hours worked. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

5.5.2 Wages effects and modes of transportation

Next, we examine whether, following the removal of rent control, the majority of wage losses are significantly concentrated among those without access to a personal vehicle. Table 7 confirms that the decline in wages is almost 4 percent for those who are likely forced to move jobs and do not own a vehicle. In contrast, for those who are forced to move and own a car, their wages only decline by 0.2 percent, and this effect is not statistically different than zero. We observe a consistent partner for the likelihood of moving to a job in the outskirts. This evidence offers strong support that owning a vehicle helps workers overcome commuting frictions and mitigate earnings losses associated with

relocation to the city outskirts.

5.6 Additional results

We also tabulate our findings by age, gender, and location. Table A1 shows that the effect is concentrated among those with ages between 35 and 50. Table A2 in the Internet Appendix shows the breakdown of our hypothesis by gender. The results show that the effect is almost identical between females and males. This suggests that it is unlikely that there are compensatory mechanisms within a household, instead it suggests that the effects documented above for each worker are likely to be amplified within a household. We also investigate the prevalence of our findings in Lisbon and Porto. Table A3 in the Internet Appendix reports the difference of the effects between Lisbon and Porto. The table shows that the point estimates on the effect of rent control on wages per hour is similar for workers in Lisbon than Porto. The point estimates for Porto are not statistically significant than zero, likely due to the small sample size in Porto.

6 A Quantitative Urban Model with Rent-Control

We present a general equilibrium urban model of residential and labor market choice in the presence of rent-controlled housing. The city is divided into a set of locations, $\mathcal{N}$. Each location simultaneously functions as a residential neighborhood, n, and a labor market, ℓ. There is a unit measure of workers, $i \in [0, 1]$, that live reside in neighborhoods across the city and inelastically supply a unit of labor in their labor market of choice.

6.1 Workers

Each worker i makes decisions in two stages. In the first stage, they jointly choose a residential neighborhood to live and a labor market to work. Conditional on their location choices, they allocate their earnings, $y_{i\ell}$, between a tradable consumption good, $c_{in\ell}$, and housing space, $h_{in\ell}$, to maximize their utility. The consumption good is the numeraire.

Workers are split into two types, $k \in \{L, H\}$ with mass μ_k, that generate wage and preference heterogeneity. We refer to worker i's type as $k(i)$. Worker i's labor income depends on their labor market of choice and their type, $w_{i\ell} = w_{k(i)\ell}$. Their preferences over housing and goods consumption are governed by a type-specific Cobb-Douglas function with weight $\alpha_{k(i)}$ on housing space. Workers have idiosyncratic preferences over neighborhood and labor market locations, $b_{in\ell}$, that are drawn from a type-specific distribution.

Workers may have access to rent controlled housing in a particular neighborhood, $n_i^{\text{rc}} \in \mathcal{N} \cup \{0\}$, where $n_i^{\text{rc}} = 0$ refers to not having a rent control opportunity in any neighborhood. If this is the case, the rent they pay for housing in that neighborhood is $R_{in} = \kappa_n R_n$, where R_n is the market rent in neighborhood n and $\kappa_n \in (0, 1)$ is a rent control discount; otherwise, they pay the market rent, $R_{in} = R_n$. However, if they choose to consume rent controlled housing, they are subject to a housing constraint, $\bar{h}$.[26] We use $\bar{h}_{in}$ to reference a particular household's housing constraint in a given neighborhood, setting $\bar{h}_{in} = \bar{h}$ if household i lives in rent controlled housing and $\bar{h}_{in} = \infty$ if they do not. We discuss the assignment of rent control opportunities to households in Section 6.2.

We allow workers to choose to reside in market rent housing in n_i^{rc} even when rent control is available in the neighborhood.[27] We denote this choice as an indicator $\iota_{i\ell}$, where $\iota_{i\ell} = 1$ if worker i chooses rent controlled housing when they work in ℓ. We write the housing constraint and effective rent as a function of this choice:

$$\bar{h}_{in}(\iota_{i\ell}) = \begin{cases} \bar{h}, & \text{if } \iota_{i\ell} = 1 \text{ and } n = n_i^{\text{rc}} \\ \infty, & \text{if } \iota_{i\ell} = 0 \text{ or } n \neq n_i^{\text{rc}} \end{cases} \qquad R_{in}(\iota_{i\ell}) = \begin{cases} \kappa_n R_n, & \text{if } \iota_{i\ell} = 1 \text{ and } n = n_i^{\text{rc}} \\ R_n, & \text{if } \iota_{i\ell} = 0 \text{ or } n \neq n_i^{\text{rc}} \end{cases}$$

Conditional on their neighborhood and labor market decision, workers choose their goods and

[26] This is not only realistic, as many workers utilizing rent controlled housing in Portugal tend to live in only a section of the house, but also helps to match observed rental space usage of rent controlled versus market rent tenants. Since optimal worker rental space is inversely proportional to rent, without this constraint the model would predict unrealistically high rental space usage from the rent control tenants.

[27] Households may prefer to rent at a higher cost in order to consume more housing space. If so, they should optimally choose market-rate housing over rent-controlled housing.

housing consumption to maximize their utility:

$$\begin{aligned} U_{in\ell} = \quad & \underset{c_{in\ell}, h_{in\ell}, \iota_{i\ell}}{\text{maximize}} \quad b_{in\ell} u_{k(i)}(c_{in\ell}, h_{in\ell}) = b_{in\ell} \left(\frac{c_{in\ell}}{1-\alpha_{k(i)}} \right)^{1-\alpha_{k(i)}} \left(\frac{h_{in\ell}}{\alpha_{k(i)}} \right)^{\alpha_{k(i)}} \\ & \text{subject to} \quad c_{in\ell} + R_{in}(\iota_{i\ell}) h_{in\ell} \leq y_{k(i)\ell} \equiv w_{k(i)\ell} + \pi_i \\ & \qquad\qquad h_{in\ell} \leq \bar{h}_{in}(\iota_{i\ell}) \\ & \qquad\qquad \iota_{i\ell} \in \{0, 1\} \end{aligned} \tag{4}$$

where π_i is non-labor income whose elements we discuss later. The optimal consumption values are

$$h^*_{in\ell} = \min \left\{ \frac{\alpha_{k(i)} y_{k(i)\ell}}{R_{in}(\iota^*_{i\ell})}, \bar{h}_{in}(\iota^*_{i\ell}) \right\}, \quad c^*_{in\ell} = y_{k(i)\ell} - R_{in}(\iota^*_{i\ell}) h^*_{in\ell}. \tag{5}$$

which we derive in Appendix 10.1. Their indirect utility is then $U_{in\ell} = b_{in\ell} u_{k(i)}(h^*_{in\ell}, c^*_{in\ell})$. Since k and n^{rc} embody all sources of non-idiosyncratic heterogeneity across workers, we use the short hand $u_{kn\ell}(n^{\text{rc}}) = u_{k(i)}(h^*_{in\ell}, c^*_{in\ell})$ for workers that are of type k and have rent control access in neighborhood n^{rc}.

We now turn to workers' location choices. We assume that idiosyncratic amenities over neighborhood and labor markets are distributed according to a type-specific and location-pair-specific Fréchet distribution: $b_{in\ell} \sim \text{Fréchet}(B_{k(i)n\ell}, \varepsilon_{k(i)})$. The parameter $B_{kn\ell}$ denotes the average amenity value of living in location n and working in location ℓ for households of type k, and the scale parameter ε_k denotes the dispersion in idiosyncratic preferences. We assume average amenity values can be separated according to $B_{kn\ell} = \beta_{kn} \lambda_{k\ell} / \chi_{kn\ell}$. We refer to the bilateral term, $\chi_{kn\ell}$, as commuting costs, and refer to β_{kn} and $\lambda_{k\ell}$ as residential and labor market amenities, respectively.

Given the Fréchet distribution assumption, standard discrete choice accounting implies that the share of workers of type (k, n^{rc}) that choose to live in neighborhood n and work in location ℓ is

$$\phi_{kn\ell}(n^{\text{rc}}) = \frac{B_{kn\ell} u_{kn\ell}(n^{\text{rc}})^{\varepsilon_k}}{\sum_{n'} \sum_{\ell'} B_{kn'\ell'} u_{kn'\ell'}(n^{\text{rc}})^{\varepsilon_k}} \tag{6}$$

Therefore, location pair choices depend on (i) amenity values, (ii) commuting costs, (iii) labor market wages, and (iv) rents. The relative strength of economic vs. amenity values are governed by the taste dispersion parameters ε_k.

6.2 Rental Markets and Rent Control Opportunities

Each neighborhood has an exogenous amount of housing space owned by absentee landlords and workers, H_n. We assume total housing space in the city sums to 1. Workers each own type-specific shares θ_k of the total housing stock, and landlords own the remaining $1 - \theta_L - \theta_H$ share. This implies that if $\overline{R}$ is the total amount of rental income in the economy, each worker receives $\overline{R}\theta_k/\mu_k$ of rental income.

Each neighborhood has an exogenous share of housing space dedicated to rent controlled housing, $\eta_n \in [0, 1]$. Workers enter a lottery to access rent control in a random neighborhood n_i^{rc}. The probability of drawing neighborhood n_i^{rc} is $\psi(n^{\text{rc}})$. The probability that they do not receive rent control is therefore $\psi(0) = 1 - \sum_{n^{\text{rc}} \in \mathcal{N}} \psi(n)$. Since there is a unit measure of workers, $\lambda(n^{\text{rc}})$ can also be interpreted as the number of households that draw neighborhood n^{rc} in the lottery. Additionally, workers can only enter the lottery once. If they choose to not live in rent controlled housing in their draw n_i^{rc} ($\iota_{i\ell}^* = 0$), they are unable to re-enter the lottery.

There are two market clearing conditions for each neighborhood. The market clearing condition for rent-controlled housing in neighborhood n is

$$\eta_n H_n = \psi(n) \sum_{k=L,H} \mu_k \sum_{\ell \in \mathcal{N}} \phi_{kn\ell}(n) h_{kn\ell}^*(n) \iota_{k\ell}^*(n). \tag{7}$$

and the market clearing condition for market rent housing is

$$(1-\eta_n)H_n = \psi(n)\sum_{k=L,H}\mu_k\sum_{\ell\in\mathcal{N}}\phi_{kn\ell}(n)h^*_{kn\ell}(n)(1-\iota^*_{k\ell}(n)) + \sum_{n^{\text{rc}}\neq n}\psi(n^{\text{rc}})\sum_{k=L,H}\mu_k\sum_{\ell\in\mathcal{N}}\phi_{kn\ell}(n^{\text{rc}})h^*_{kn\ell}(n^{\text{rc}}) \quad (8)$$

The first term on the right hand side of (8) is the housing demand of workers that have access to rent control in n but choose to consume market rent housing instead. The second term includes all households that choose to live in market rent housing in n but have access to rent controlled housing in $n^{\text{rc}} \neq n$.

6.3 Labor Markets

Each labor market ℓ has a representative firm that produces the tradable consumption good. The firm's technology is separable across worker types, which we refer to as low-skill (L) and high-skill (H). The firm pays a type-specific wage, $w_{k\ell}$, and has type-specific productivity, Z_k.[28] The firm operates with decreasing returns to scale. The firm chooses low- and high-skill labor, $N_{k\ell}$, to maximize its profits:

$$\Pi_\ell = \underset{N_{L\ell},N_{H\ell}}{\text{maximize}}\sum_{k=L,H}\left\{Z_k N_{k\ell} - \frac{w_{k\ell}}{1+1/\gamma}N_{k\ell}^{1+1/\gamma}\right\}. \quad (9)$$

The firm's optimal choice, and therefore the demand for k-type labor in labor market ℓ, is $N^*_{k\ell} = Z^{\gamma}_{k\ell}w^{-\gamma}_{k\ell}$. Wages for each type-location pair are determined in equilibrium. The labor market clearing condition in each labor market ℓ equates labor demand with labor supply:

$$N^*_{k\ell} = \mu_k\sum_{n^{\text{rc}}\in\mathcal{N}\cup\{0\}}\psi(n^{\text{rc}})\sum_{n\in\mathcal{N}}\phi_{kn\ell}(n^{\text{rc}}) \quad (10)$$

[28] Note that this setup is equivalent to assuming two representative firms in each location, each of which hires only one type of labor.

6.4 Equilibrium

An equilibrium is a rent function $R : \mathcal{N} \to \mathbb{R}_+$ and a rent control lottery $\psi : \mathcal{N} \cup \{0\} \to [0, 1]$ such that (i) workers optimally choose their goods consumption, housing consumption, rent control decisions, and joint neighborhood and labor market locations; (ii) firms maximize profits according to (9); (iii) labor markets clear according to (10); (iv) rental markets clear according to (8) and (7); (v) and $\sum_{n \in \mathcal{N} \cup \{0\}} \psi(n) = 1$. We provide an algorithm that can be used to find the equilibrium in Appendix 10.2.

6.5 Estimating the Model

We estimate the model for the city of Lisbon. We define the set of locations $\mathcal{N}$ to be the 69 4-digit zip codes in the Lisbon metropolitan area.

To classify workers into types, we first estimate the worker-level regression

$$\log w_i = \text{FE}_{\ell(i)} + \epsilon_i,$$

where w_i is worker i's hourly wage, $\text{FE}_{\ell(i)}$ is a labor market fixed effect and $\ell(i)$ is worker i's labor market. We then group workers into two skill types based on the fitted residuals: $k(i) = L$ if $\hat{\epsilon}_i < 0$ and $k(i) = H$ if $\hat{\epsilon}_i \geq 0$.

We use these classifications to compute several model parameters: housing shares $\{\alpha_k\}_k$, rent control parameters $\{\kappa_n, \eta_n\}_n$, amenities $\{\beta_{kn}, \lambda_{k\ell}, \chi_{kn\ell}\}_{k,n,\ell}$, and labor market productivities, $\{Z_{k\ell}\}_{k,\ell}$. We estimate the taste dispersion parameters $\{\varepsilon_k\}_k$, rent control housing constraint $\bar{h}$, and rental income shares $\{\theta_k\}_k$ using simulated method of moments. We assume a labor demand elasticity of $\gamma = 0.6$, consistent with the literature (Addison and Portugal, 1989).

Housing Shares We estimate α_k using the average monthly housing expenditure shares for workers that do not live in rent controlled housing. We recover $\alpha_L = 0.492$ and $\alpha_H = 0.315$.

Rent Control We infer the rent control parameters $\{\kappa_n, \eta_n\}_{n \in \mathcal{N}}$ directly from data on rents and housing space of market and rent-controlled housing. Specifically, we set η_n to be the share of housing space (square meters) allocated to rent controlled units in neighborhood n and set κ_n to be the average rent under rent control divided by the average market rent in neighborhood n.

Amenities and Commuting Costs We compute commuting flows for workers not living in rent-controlled housing, which we will use to estimate amenity values and commuting costs. We omit the dependence on n^{rc} for this section. For each worker type k, we first calculate

$$\hat{\phi}_{knl} = \frac{\#\text{ of workers of type } k \text{ living in } n \text{ and working in } \ell}{\#\text{ of workers of type } k}$$

in the data. Given the observed flows, we first back out commuting costs, $\{\chi_{kn\ell}\}_{n,\ell \in \mathcal{N}}$. Note that if commuting costs are symmetric, e.g. $\chi_{kn\ell} = \chi_{k\ell n}(z)$ for every n, ℓ, and k, the choice probabilities in the model imply

$$\frac{\phi_{kn\ell}/\phi_{knn}}{\phi_{k\ell\ell}/\phi_{k\ell n}} = \frac{\chi_{kn\ell}^{-1}(w_{k\ell}/w_{kn})^{\varepsilon_k}}{\chi_{k\ell n}(w_{k\ell}/w_{kn})^{\varepsilon_k}} = \chi_{kn\ell}^{-2}.$$

We therefore substitute $\phi_{knl} = \hat{\phi}_{knl}$ into this expression and back out commuting costs for every bilateral neighborhood-labor-market pair and worker type.

Next, we estimate residential amenities, $\{\beta_{kn}\}_n$, for each worker type. Consider the relative probability that a type k worker employed in ℓ chooses to live in neighborhood n_2 over n_1. Summing across all possible labor markets, we come to the expression

$$\sum_{\ell \in \mathcal{N}} \frac{\phi_{kn_2\ell}}{\phi_{kn_1\ell}} = \frac{\beta_{kn_2}}{\beta_{kn_1}} \left(\frac{R_{n_1}}{R_{n_2}}\right)^{\varepsilon_k \alpha_k} \sum_{\ell \in \mathcal{N}} \frac{\chi_{kn_1\ell}}{\chi_{kn_2\ell}}.$$

Therefore, given a guess for ε_k, data on average neighborhood-level market rent, estimated commuting costs, and observable commuting flows, we can back out relative amenity values β_{kn}/β_{kn_1} for some n_1 and every n. We normalize $\beta_{kn_1} = 1$ for each worker type, taking n_1 to be zip code

1000.[29]

Labor market amenities are calculated in a nearly identical way. Consider the relative probability that a type k worker that lives in neighborhood n chooses to work in labor market ℓ_2 over ℓ_1. Summing across residential neighborhoods, we come to the expression

$$\sum_{\ell \in \mathcal{N}} \frac{\phi_{kn\ell_2}}{\phi_{kn\ell_1}} = \frac{\lambda_{k\ell_2}}{\lambda_{k\ell_1}} \left(\frac{w_{\ell_2}}{w_{\ell_1}} \right)^{\varepsilon_k} \sum_{n \in \mathcal{N}} \frac{\chi_{kn\ell_1}}{\chi_{kn\ell_2}}.$$

Using data on type-specific labor market wages, as well as the data above used for estimating residential amenities, we can back out the relative labor market amenities $\lambda_{k\ell}/\lambda_{k\ell_1}$ for every labor market ℓ. Again, we normalize $\lambda_{k\ell_1} = 1$, taking ℓ_1 to be zip code 1000.

Labor Market Productivity We internally update each labor market's productivity in the model until equilibrium wages for each type-labor market pair are equal to that in the data.

Simulated Method of Moments We calculate the dispersion in idiosyncratic preferences $\{\varepsilon_k\}_k$, the rent control housing space constraint $\bar{h}$, and the rental income shares $\{\theta_k\}_k$ using simulated method of moments. We match moments that correspond directly to the function of each parameter.

First, a higher ε_k implies that type k workers put more weight on economic versus amenity values. Therefore, type k workers will be more likely to choose similar labor markets or residential neighborhoods, increasing the variance of their choice probabilities. We therefore target the standard deviation in both labor market and residential neighborhood choice probabilities across locations for each type, conditioning on workers not living in rent controlled housing. Since ε_k affects both of these measures, we target the average of the two.

Second, a larger housing constraint $\bar{h}$ increases the optimal housing space choice of workers in rent controlled housing. Therefore, we target the average square meters of housing space of

[29] This normalization is without loss of generality since amenity values enter multiplicatively in choice probabilities. Therefore, they are unique up to scale. Since there is a single degree of freedom, we set $\beta_{kn_1} = 1$.

Table 8: Simulated Method of Moments Results

Parameter	Description	Moment	Data	Model
ε_L	Taste Dispersion	Avg. Choice Prob. SD	0.0090	0.0092
ε_H	Taste Dispersion	Avg. Choice Prob. SD	0.0079	0.0078
θ_L	Rental Income Share	Rent-to-Total Income	0.334%	0.331%
θ_H	Rental Income Share	Rent-to-Total Income	0.621%	0.622%
$\bar{h}$	RC Housing Constraint	Relative Housing Space	0.964	0.963

Note: This table reports our parameter estimates for our targeted moments. The Data column reports the relevant moment in the data, and the Model column reports the relevant moment in the model.

workers living in rent controlled housing relative to market rate housing.

Last, a higher rental income share θ_k increases workers' rental income relative to their total income. We therefore calibrate θ_k by matching the average share of rental income to total income for each household type.

We present the estimation results for the simulated method of moments procedure in Table 8. The table presents the parameter, the description of the parameter, the moment matched, and the moment values in the data and in the model. Each moment is matched precisely, deviating only by a maximum of 2.2% from its respective value in the data.

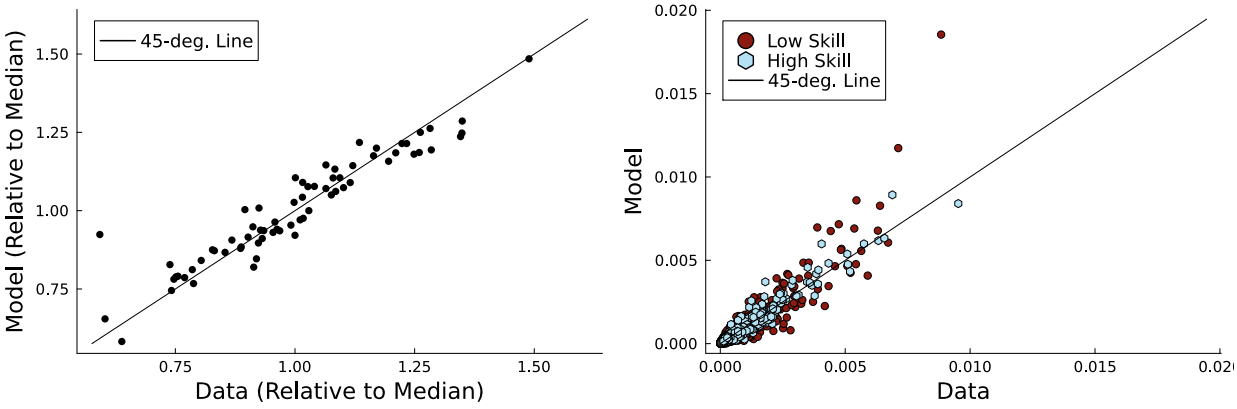
6.6 Model Fit

We characterize the fit of the model along two dimensions. First, we show in panel (a) of Figure 3 that market rents across neighborhoods match the data well ($R^2 = 0.891$). Note that we normalize rents in both the model and the data by the median rent since we do not match the levels of rent.[30]

In panel (b) of Figure 3, we document how well commuting flows in the model fit the data. The fit between the model and the data is also strong ($R^2 = 0.850$). There are a few commuting

[30]This would be possible by reducing the total amount of housing space available in the model, which we have normalized to 1. The level of rents are unnecessary for characterizing the equilibrium since they are multiplicative in the choice probabilities, so to avoid computational issues when workers are constrained in rent controlled housing, we choose to not match the rent levels.

FIGURE 3: ESTIMATED MODEL FIT



(a) Neighborhood Market Rents (b) Commuting Flows

Note: This figure assesses the fit of the model with the data. Panel (a) plots the market rent in the data versus the market rent in the model, adjusting each for the median of their distribution to remove level effects. Panel (b) plots commuting flows for each households type against commuting flows in the data. The 45-degree line is provided in black.

flows that tend to be outliers, nevertheless. These flows typically stem from high wage locations in the outskirts that do not hire many workers. For example, Procter & Gamble and GE International have offices in the zip code 2770 in Lisbon. Labor market amenities for these zip codes are low to account for the small number of workers, but since these amenities are estimated as an average across residential neighborhoods, we do not perfectly match the commuting flows.

We explore the remainder of the estimation results in Appendix 10.3. In particular, we show that high rent locations tend to have better residential amenities, consistent with the existing literature (Albouy and Lue, 2015). Similarly, high productivity labor markets tend to have low labor market amenities. We also document commuting cost heterogeneity across worker types. As suggested in Section 5.5, low-skill workers incur higher commuting costs on average relative to high-skill workers. We also show that our estimates directly relate to travel costs: for both low- and high-skill workers, bilateral commuting costs are increasing in the public transit travel time between neighborhood and labor market.

7 Counterfactual Analysis

We use our estimated model to conduct three counterfactuals. First, we simulate the abolishment of rent control and study the effects on labor markets and worker welfare. Second, we explore the ability of transfer policies (e.g., rent vouchers) to restore welfare and labor market outcomes for those affected by the loss of rent control. We focus on neighborhoods outside of the city center. In the first transfer policy, we target neighborhoods that have high amenity values, focusing on the welfare margin. In the second, we target neighborhoods with the best access (lowest commuting costs) to the city center, where labor markets on average pay higher wages.

In what follows, we analyze worker welfare. Since workers are freely mobile across residential neighborhoods and labor markets, we consider the expected utility of workers within a skill type and rent control access pair, (k, n^{rc}), averaging over their idiosyncratic preferences:

$$W_k(n^{\text{rc}}) = \mathbb{E}_k\Big[U_{kn\ell}(n^{\text{rc}})\Big] = \Gamma\left(\frac{\varepsilon_k - 1}{\varepsilon_k}\right)\left\{\sum_{n\in\mathcal{N}}\sum_{\ell\in\mathcal{N}}\frac{\beta_{kn}\lambda_{k\ell}}{\chi_{kn\ell}}u_{kn\ell}(n^{\text{rc}})^{\varepsilon_k}\right\}^{\frac{1}{\varepsilon_k}}. \tag{11}$$

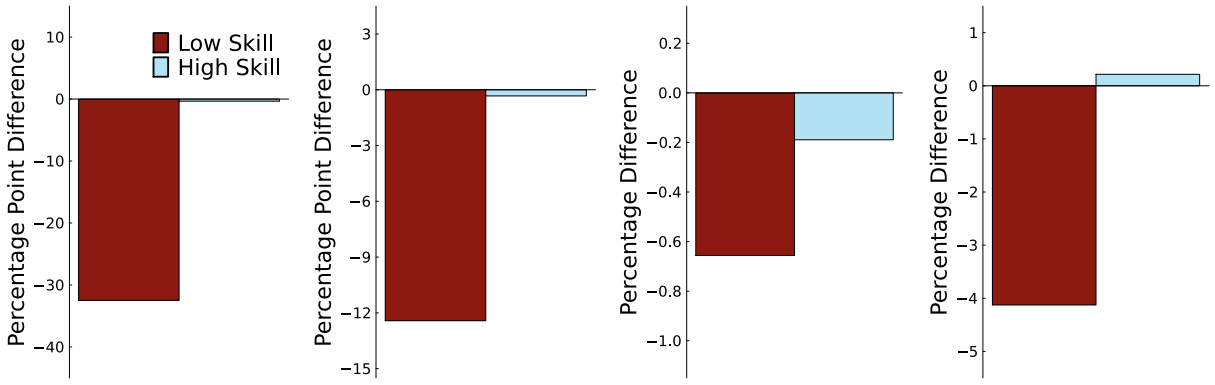
7.1 The Effects of Abolishing Rent Control

In our first experiment, we shut down rent control city-wide by setting $\eta_n = 0$ for all neighborhoods. We explore how the abolition of rent control affects residential and labor market choice probabilities, average wages, and average welfare. Consider a worker of skill type k that had rent access in a city center neighborhood prior to the reform. First, we compare the probability that this worker lives or works in the city center in the pre-reform equilibrium to the same probability for a given worker of the same type in the post-reform equilibrium.[31] We then conduct the same experiment for average wages and welfare.

Figure 4 displays the results for both skill groups. Panels (a) and (b) show the effect on worker

[31] Note that since we assume free mobility, there is no conditioning necessary in the post-reform equilibrium aside from worker type k.

FIGURE 4: THE GE EFFECTS OF LOSING RENT CONTROL ACCESS IN THE CITY CENTER



(a) P(Live in CC) (b) P(Work in CC) (c) Average Wage (d) Average Welfare

Note: This figure plots the effects of losing access to rent control in the city center. Panels (a) and (b) reports the difference in the probability of residing and working in the city center, respectively; panel (c) reports the effect on average wages; and panel (d) reports the effect on average welfare.

location choices. As documented Section 5.1, low-skill workers that previously had access to rent control in the city center are pushed to the outskirts: the probability that they continue to live in the city center falls by 32.5 percentage points. Due to commuting costs, this further leads to a 12.5 percentage point decline in the probability of working in the city center, which, as shown in panel (c), results in a 0.6% decline in their average wage. Panel (d) shows that their welfare declines by 4%, reflecting higher rent payments, lower amenities, higher commuting costs, and lower wages.

The effects for high skill workers are negligible. High skill workers rarely opt in to rent control even if they have access to it, choosing to consume market rent housing. This is due to the housing constraint: even though the rent payments are lower, high skill workers can afford — and usually prefer — to rent apartments with a larger amount of housing space. The majority of the effects therefore stem from equilibrium effects on wages and rents.

There are a few discrepancies between the estimated model and our empirical exercises. First, workers are more likely to be pushed out of the city center in the model relative to the data. This is likely due to the omission of moving costs from the model: if workers find it costly to move, then

many of those that lose rent control will remain in their housing if the rent hike is smaller than the cost to move. Second, wages decline less in the model than they do in the data. This is likely due to omitting realistic labor market dynamics: as documented by (Jacobson et al., 1993), there is usually a wage discount when switching jobs due to matching frictions, which we do not capture in our model. Nevertheless, the model correctly predicts the correct direction of the effects.

7.2 Targeted Transfers

While abolishing rent control reduces low skill worker welfare, landlord rent payments increase by 13.7%. In this section, we explore targeted transfers that redirect surplus back to workers. The first policy focuses on worker welfare, while the second policy focuses on limiting commuting costs. We focus on neighborhoods in the outskirts of the city center. The goal is to incentivize workers to live in the outskirts ($\mathcal{O} \subset \mathcal{N}$), but particularly in high amenity neighborhoods and neighborhoods with good access to productive labor markets.

Transfers to type k workers in neighborhood n take the form $\tau_{kn} = \omega_{kn} T_k$, where ω_{kn} are policy weights that sum to one for each worker type, and T_k is the scale of the policy. The welfare-based transfer weights, ω_{kn}^W, for each neighborhood n depends on the marginal welfare of a small transfer in n relative to all other neighborhoods in the outskirts:

$$\omega_{kn}^W \equiv \frac{dW_{kn}}{\sum_{n' \in \mathcal{O}} dW_{kn'}}, \qquad dW_{kn} = \frac{dW_k(\Delta\pi_{kn} = \tau_n)}{d\tau_n}\bigg|_{\tau_n = 0} \propto \frac{\beta_{kn}}{R_n^{\alpha_k \varepsilon_k}} \sum_{\ell \in \mathcal{N}} \frac{\lambda_{k\ell}(w_{k\ell} + \pi_{kn})^{\varepsilon_k}}{\chi_{kn\ell}} \tag{12}$$

The weights capture both rent-adjusted residential amenities and income and commuting cost adjusted labor market amenities. The second set of weights are constructed in a similar way, but instead only focus on productivity-adjusted commuting costs to the city center:

$$\omega_{kn}^{\chi} = \frac{\widetilde{\chi}_{kn}^{-1}}{\sum_{n' \in \mathcal{O}} \widetilde{\chi}_{kn'}^{-1}}, \qquad \widetilde{\chi}_{kn}^{-1} \equiv \sum_{\ell \in \mathcal{N} \setminus \mathcal{O}} \left(\frac{Z_{k\ell}}{\sum_{\ell' \in \mathcal{N} \setminus \mathcal{O}} Z_{k\ell'}} \right) \frac{1}{\chi_{kn\ell}} \tag{13}$$

We vary the strength of the welfare-based weights (12) and commuting-based weights (13)

through a parameter $\rho_k \in [0, 1]$. The final transfer weights are $\omega_{kn}(\rho_k) = \rho_k \omega_{kn}^W + (1 - \rho_k)\omega_{kn}^\chi$.

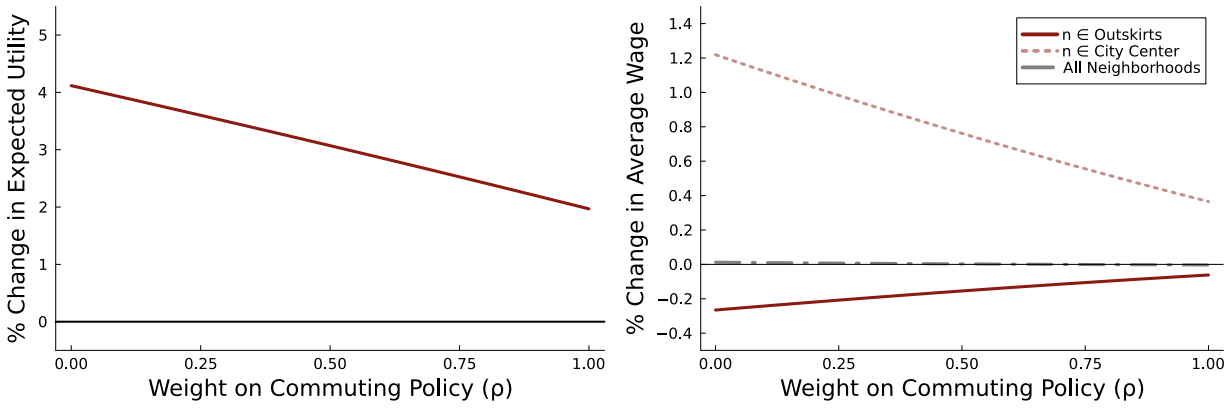
To determine the policy scales $\{T_k\}$, we impose two constraints. First, we require that on average, low-skill workers, high-skill workers, and landlords are all as well off relative to the rent control equilibrium. Second, we require that the budget is balanced: transfers are financed entirely by a lump sum tax on landlord rental income. Given these constraints, we determine the policy scales $\{T_k^*\}$ that maximize average low-skill workers' welfare for the case that $\rho_k = 0$ for all k. We find that the optimal transfer accounts for 16.5% of low-skill worker wages in the outskirts on average and 6.8% of high-skill worker wages in the outskirts on average.

We now turn to the results of the policy as we vary ρ_L, setting $\rho_H = 0$. The results are presented in Figure 5. We focus on low-skill workers, and address the effects on high skill workers in Appendix 10.4. Panel (a) shows how average low-skill worker welfare changes relative to the rent control equilibrium. When $\rho_L = 0$, transfers prioritize high amenity neighborhoods, leading to a 4.12% improvement in average welfare. The increase in welfare declines as the policy shifts toward optimal commuting ($\rho_L \to 1$), but remains at least 1.97% higher than the rent control equilibrium.

However, the policies increase wage inequality across the city as shown in panel (b), despite average wages remaining nearly unchanged. The welfare-based policy ($\rho_L = 0$) increases the gap between average outskirts and city center wages by to a 1.48%. This is due to general equilibrium effects: the policy strongly induces migration into the outskirts, which leads to an increase in the supply of labor. As a result, average wages further decline for workers that live in the outskirts, while they improve for those in the city center. Policies that favor commuting cannot entirely close the gap, but reduce the gap to 0.43%.

The results highlight the trade off that policymakers must make when designing transfer policies. Policies that target the most desirable neighborhoods may lead to undesirable labor market effects. Policy should therefore target a combination of high amenity neighborhoods and neighborhoods that are accessible to desirable labor markets.

FIGURE 5: THE WELFARE AND LABOR MARKET EFFECTS OF TRANSFER POLICIES ON LOW SKILL WORKERS



(a) Change in Expected Utility

(b) Change in Average Wage

Note: This figure plots (a) the percentage change in expected utility and (b) the average wage of low-skill workers relative to the rent control equilibrium given a policy mix ρ. In Panel (a), the star denotes the utility difference under the optimal low-skill policy, which corresponds to $\rho = 0$. In Panel (b), the solid line denotes the change in the average wage conditional on a worker choosing to live in the outskirts, the dotted line denotes the change in the average wage conditional on living in the city center, and the dashed-dotted gray line denotes the change in the unconditional average wage.

8 CONCLUDING REMARKS

Many cities and countries are tackling an affordability crisis by introducing or strengthening rent control policies. Yet an extensive body of research documents the potential negative implications of price policies in the housing market. They might decrease housing supply, lower house prices, and diminish migration. Rent control also causes spacial misallocation of individuals, which may lead to misallocation in the labor market due to commuting frictions. We examine in this paper the causal implications of rent control on labor market outcomes.

Using a quais-natural experiment in Portugal, we show that individuals in rent-controlled units with income above the sample median experience no losses in earnings after losing access to rent control. In contrast, those with below sample median income experience a significant decline in earnings after loss of rent control. We also present evidence suggesting that low-income workers

cannot afford commuting costs and thus transition to lower quality jobs in the outskirts.

Our findings suggest that rent control boosts the wages of low-income workers but has no effect on the wages of high-income workers. While rent control reduces wage disparities by lowering commuting costs to low-income workers, our paper does not offer strong support to rent control policies. Our counterfactual analysis shows that alternative policies with much lower distortions can achieve the same labor market benefits as rent control. Targeted means-tested voucher policies toward neighborhoods with low commuting costs to high-productivity areas may provide a better substitute to rent control.

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INTERNET APPENDIX

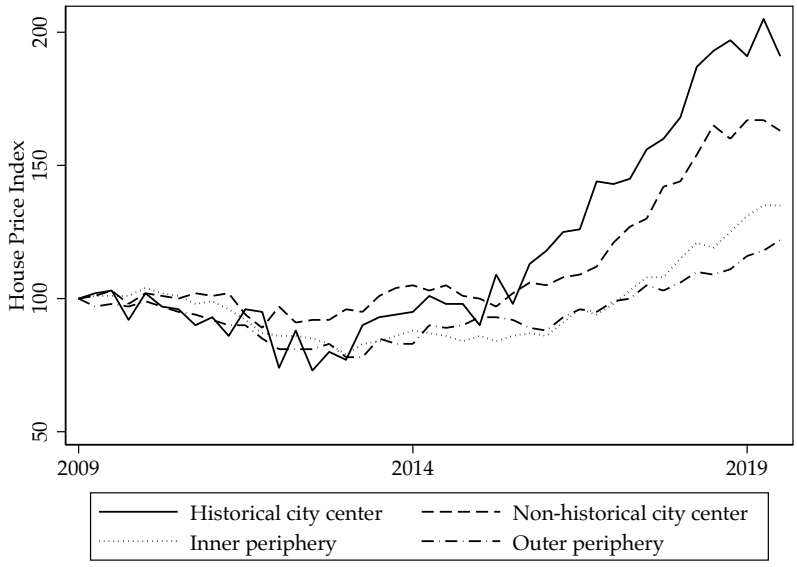
For Online Publication

9 Housing Market in Portugal

9.1 House Prices

House prices in Portugal remained largely stable from 2010 to 2015 but experienced rapid growth between 2015 and 2019. Figure A1 depicts the trajectory of house prices in the historical center, non-historical center, inner periphery, and outer periphery of Lisbon from 2009 through 2019. Prices in the historical center nearly doubled. In the non-historical center, house prices increased by approximately 60 percent during this period. In the periphery, house prices rose by 20 to 30 percent during this later time period. Figure A3 in the Internet Appendix presents the time series of house prices in Porto, Portugal's second largest city. Porto exhibits a pattern similar to Lisbon.

FIGURE A1: **HOUSE PRICES IN LISBON**



Notes: This figure reports the evolution of house prices in Lisbon relative to January 2008. The panel depicts the growth in prices in the historical center, the city-periphery, and for the rest of the district (metropolitan area). Figure A9 shows the neighborhoods in Lisbon that compose the city-center and city-periphery, while Figure A7 shows the municipalities that constitute the outskirts of Lisbon.

Several factors contribute to this rapid rise in house prices. Recent evidence suggests that an upsurge in tourism and foreign residents might have contributed to the growth in house prices (Teles and de Matos,

2024). Consistently, Figure A4 shows that the number of tourists in Portugal remained steady from 2005 to 2013, then more than doubled between 2013 and 2019. Figure A5 in the Internet Appendix demonstrates that the number of Airbnbs in Portugal surged beginning in 2014. This macro evidence supports the conjecture that a shock in tourism may account for a significant portion of the rise in real estate prices in Portugal in recent years.

Another explanation for the rapid growth in house prices is the impact of the introduction of Golden Visas on housing demand (Gaspar and Ampudia de Haro, 2020). This program implemented by the Portuguese government awards residency to any immigrant who invests in residential real estate priced over 500,000 euros. This program was part of a reform introduced following the 2010 recession to boost foreign investment and spending. Collectively, current evidence suggests that tourism and foreign investment as the primary drivers of the increase in home and rent prices in Portugal between 2015 and 2019.

FIGURE A2: **AFFORDABILITY IN LISBON**

Notes: This figure reports an index of housing affordability in Lisbon, defined as the average house price per square meter divided by the average month gross salary. The higher the the index the lower is the affordability.

9.2 Affordability

Housing affordability has become a significant concern in Portugal in recent years. The rapid increase in housing costs after 2015 was not accompanied by a proportionate rise in wages, leading to a substantial disparity. This issue is particularly evident in Lisbon, as depicted in Figure A2. The figure clearly illustrates that the ratio of house prices to average wages remained relatively stable between 2010 and 2015. However, between 2015 and 2019, the ratio surged by almost 70 percent. This dramatic shift is attributed to the fact that average wages only experienced a modest annual increase of 3.5 percent during this period. As a result of the housing boom, the importance of having access to a rent-controlled home, which was already advantageous before the housing boom, became even more crucial after 2015.

Variable definitions

Treatment	
Legacy renter	=1 if renter is 65+ years old and rental contract predates 1990
Legacy death	=1 if a legacy renter dies
Last legacy death	=1 if a legacy renter dies and there are no surviving legacy renters in the household
Treated worker	=1 if the worker lives with a legacy renter who dies

Demographics	
Age	Age in years
Male	=1 if individual is male
College	=1 if individual has a college degree
Children	=1 if household includes children up to 12 years old

Labor market	
Monthly salary	Monthly base salary
Total salary	Monthly total salary
Hours worked	Number of hours worked per month
New job	=1 if worker switches to a different employer (from t-1 to t)
Job change	=1 if worker switches to a different occupation (from t-1 to t)
Promotion	=1 if worker is promoted within the next five years
Unemployment	=1 if worker is unemployed
High-turnover firm	=1 if more than 30 percent of the workforce left the company in one year during our sample period (2010-2020).

Income groups	
Very low income	=1 if income is below the 10th percentile
Low income	=1 if income is between the 10th percentile and the median
High income	=1 if income is above the median

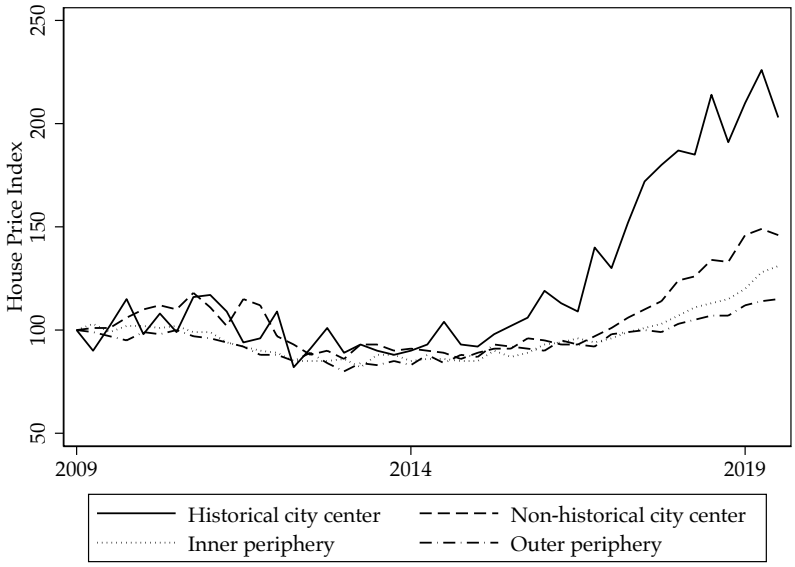
Variable definitions (cont.)

Mobility	
Move to outskirts	=1 if worker moves from city center to a municipality in the periphery (from t-1 to t). Any municipality outside the municipality of Lisbon (Porto) but still inside the greater metropolitan area of Lisbon (Porto) is considered periphery. Figure A7 and A8 provide more detail.

Figure A3: House Price Growth and Affordability in Porto

Notes: Panel A of this figure reports the evolution of house prices in Porto relative to January 2008. The panel depicts the growth in prices in the historical center, the city-periphery, and for the rest of the district (metropolitan area). Figure A10 shows the neighborhoods in Porto that compose the city-center and city-periphery, while Figure A8 shows the municipalities that constitute the outskirts of Porto. Panel B reports an index of housing affordability in Porto, defined as the average house price per square meter divided by the average month gross salary. The higher the the index the lower is the affordability.

Panel A: **House Prices in Porto**



Panel B: **Affordability in Porto**

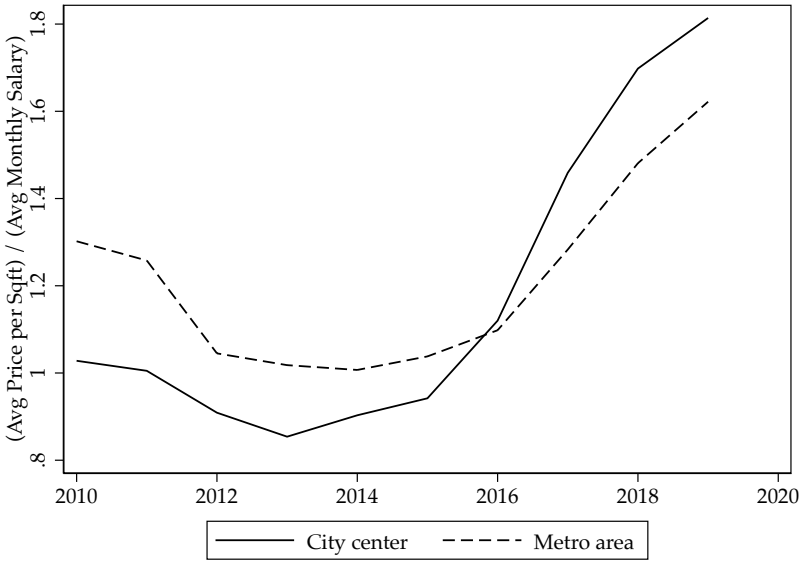


Figure A4: Tourism in Portugal

Notes: This figure reports the number of tourist visitors in Portugal starting in 2006. Numbers are reported in millions.

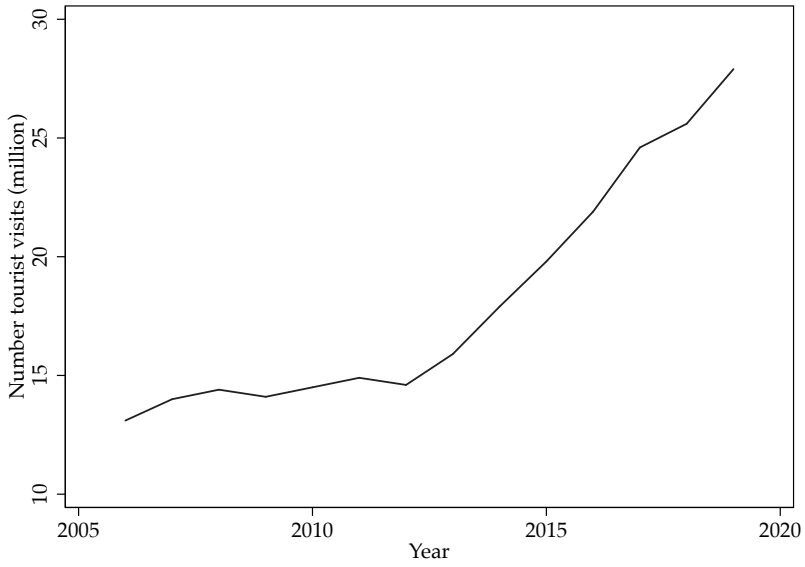


Figure A5: Short-term rentals in Portugal

Notes: This figure depicts the number of houses in Lisbon and Porto that were allocated to short-term rental.

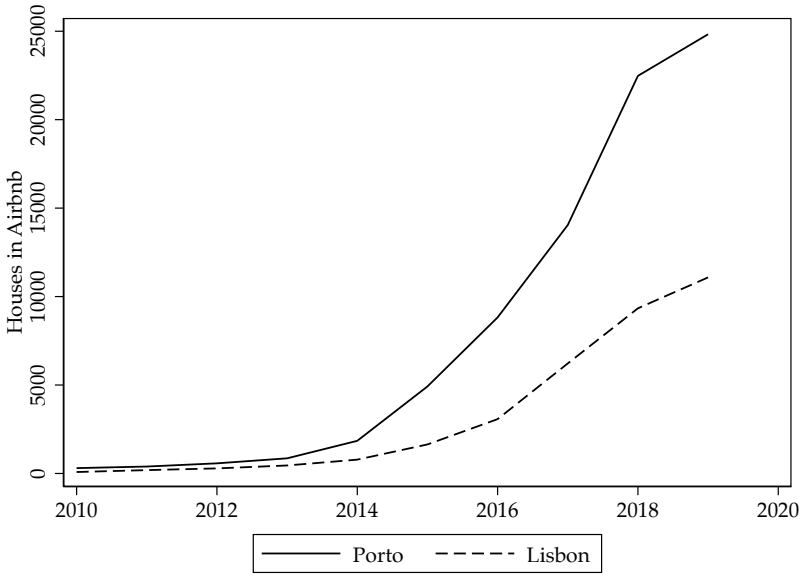


Figure A6: Portugal, Lisbon, and Porto

Notes: The map below depicts Portugal. The dark red region indicates the city of Lisbon, while the light red area represents its metropolitan vicinity. The light blue region highlights the city of Porto, with the dark blue area showing the Porto metropolitan area.

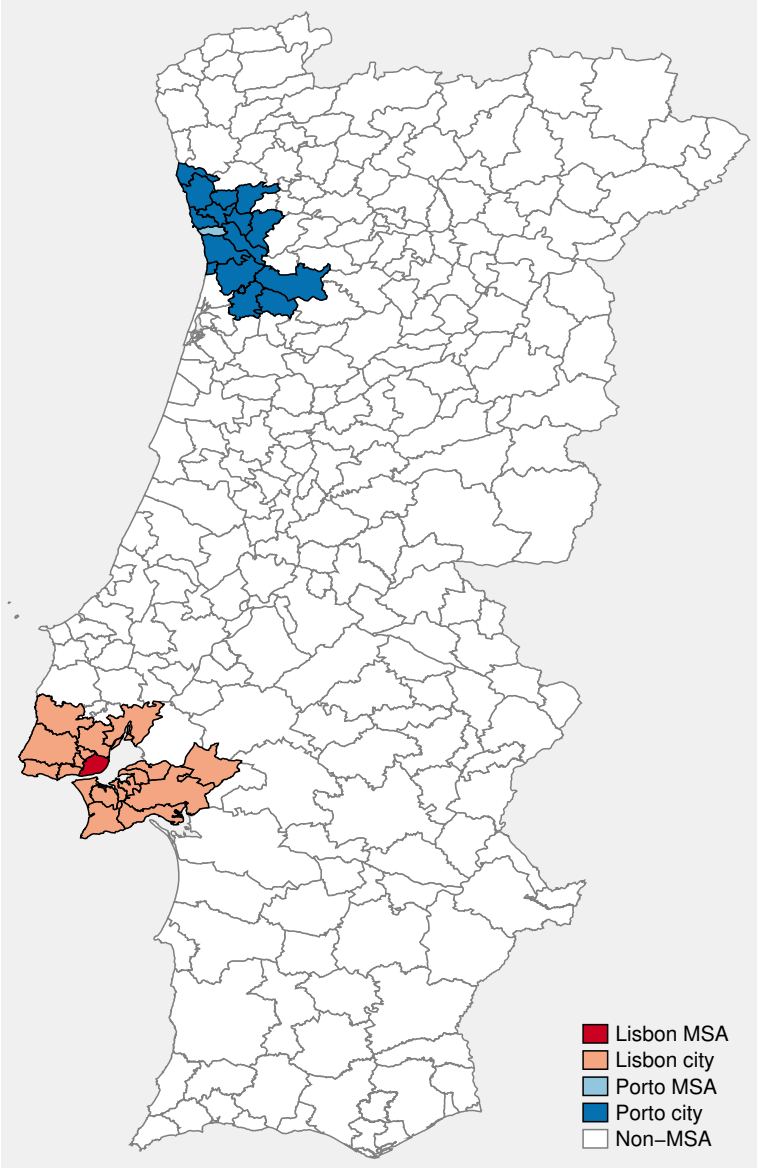


Figure A7: Lisbon metropolitan area

Notes: The picture below shows Lisbon metropolitan area. Any of these regions, except Lisbon municipality, is considered an outskirt/periphery of Lisbon.

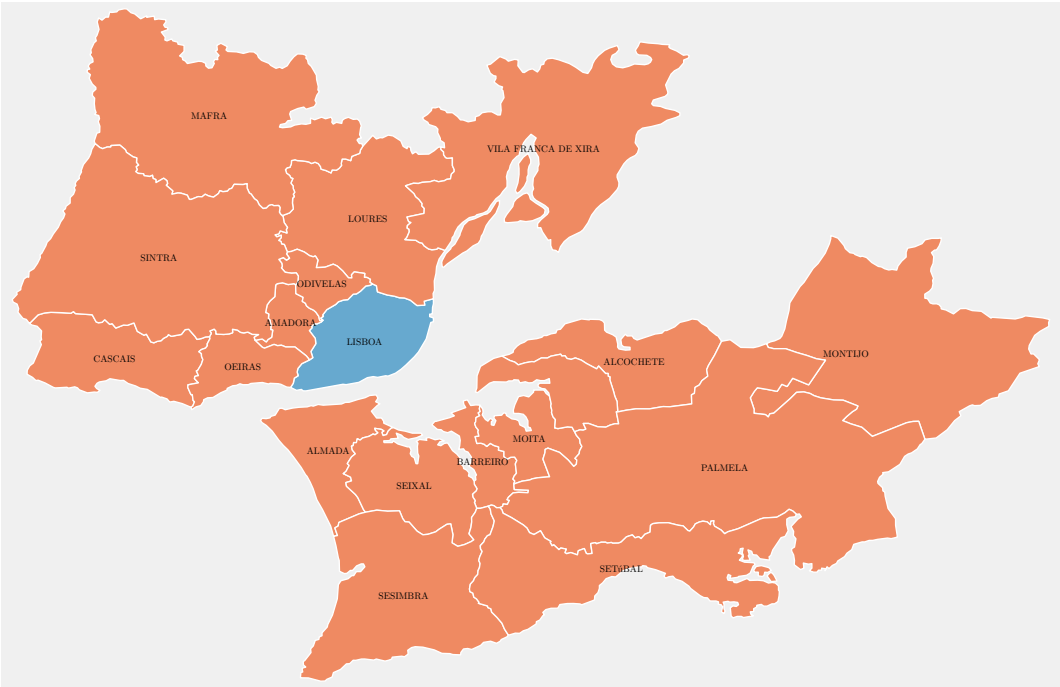


Figure A8: Porto metropolitan area

Notes: The picture below shows Porto metropolitan area. Any of these regions, except Porto municipality, is considered an outskirt/periphery of Porto.

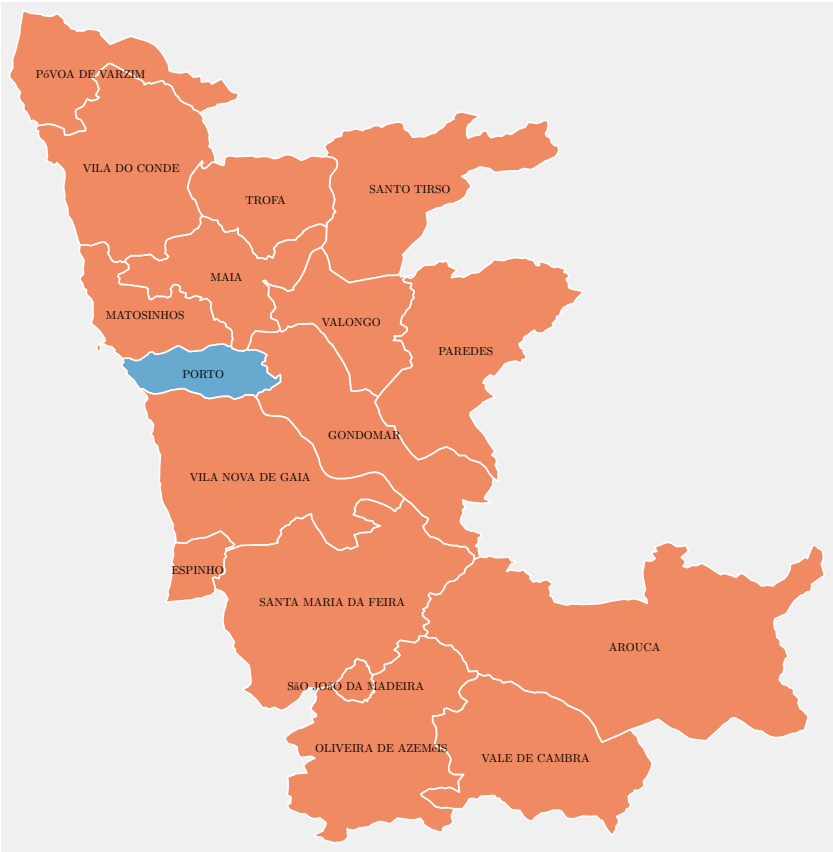


Figure A9: Lisbon Freguesias (neighborhoods)

Notes: The picture below depicts all *freguesias* (neighborhoods) inside the municipality of Lisbon.

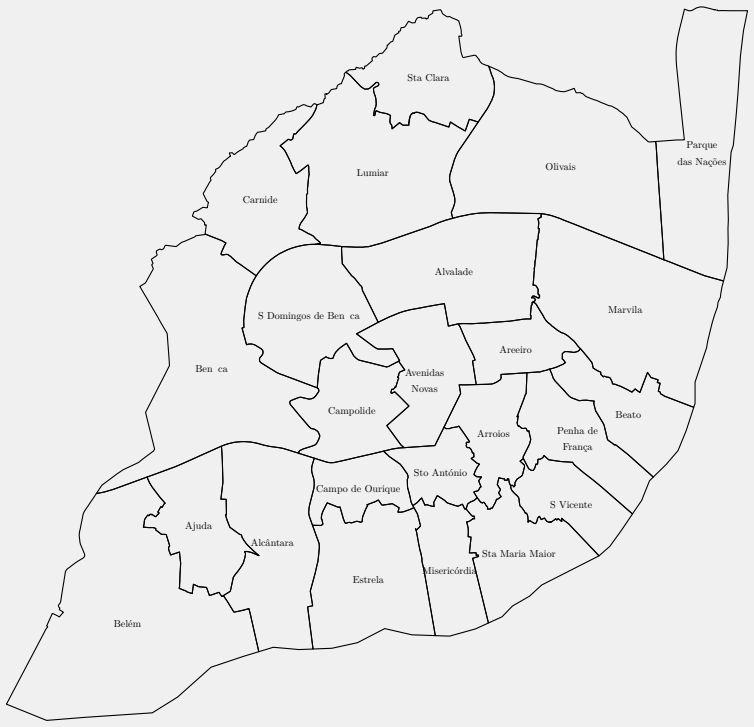


Figure A10: Porto Freguesias (neighborhoods)

Notes: The picture below depicts all *freguesias* (neighborhoods) inside the municipality of Porto.

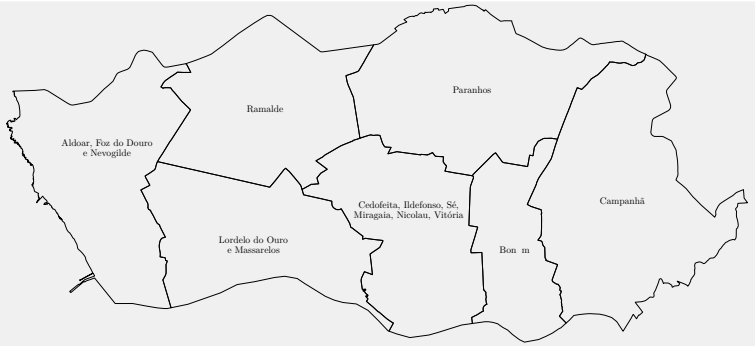


Table A1: Effect on earnings by age

	y-var: Log(Monthly salary/hours)		
	age<35	35<age<50	age>50
Treatment ×Post	-0.005	-0.024**	0.003
	(0.013)	(0.009)	(0.013)
Year FE	Yes	Yes	Yes
Individual FE	Yes	Yes	No
Cohort FE	Yes	Yes	Yes
N	43,635	43,469	19,098
R-squared	0.898	0.959	0.968

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable is the log of the total monthly salary divided by the total number of hours worked. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table A2: Effects on earnings by gender

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable is the log of the of the monthly salary divided by the number of hours worked. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

	y-var: Log(Monthly salary/hours)	
	Full sample	New Job
Treatment ×Post	-0.0186*	-0.0341*
	(0.010)	(0.017)
Treatment × Post × Female	0.002	0.003
	(0.012)	(0.024)
Post × Female	-0.007	-0.0137
	(0.006)	(0.012)
Year FE	Yes	Yes
Individual FE	Yes	Yes
Cohort FE	Yes	Yes
N	106,256	26,720
R-squared	0.938	0.882

Table A3: Effects on hourly in Lisbon versus Porto

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Our Internet Appendix details the definitions of our main variables. The outcome variable is the log of the of the monthly salary divided by the number of hours worked. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

	y-var: Log(Monthly salary/hours)	
	Lisbon	Porto
Treatment ×Post	-0.0175**	-0.0173
	(0.008)	(0.0125)
Year FE	Yes	Yes
Individual FE	Yes	Yes
Cohort FE	Yes	Yes
N	73,830	32,426
R-squared	0.939	0.936

10 Additional Model Details

10.1 Derivation of Workers' Indirect Utility

This section derives the optimal policies for a worker living in residential neighborhood n and working in labor market ℓ. Recall that their problem is

$$\begin{aligned} U_{in\ell} = \quad & \underset{c_{in\ell}, h_{in\ell}, \iota_{i\ell}}{\text{maximize}} \quad b_{in\ell} u_{k(i)}(c_{in\ell}, h_{in\ell}) = b_{in\ell} \left(\frac{c_{in\ell}}{1-\alpha_{k(i)}} \right)^{1-\alpha_{k(i)}} \left(\frac{h_{in\ell}}{\alpha_{k(i)}} \right)^{\alpha_{k(i)}} \\ & \text{subject to} \quad c_{in\ell} + R_{in}(\iota_{i\ell}) h_{in\ell} \leq y_{k(i)\ell} \equiv w_{k(i)\ell} + \pi_i \\ & \qquad\qquad h_{in\ell} \leq \bar{h}_{in}(\iota_{i\ell}) \\ & \qquad\qquad \iota_{i\ell} \in \{0, 1\} \end{aligned} \tag{14}$$

When the worker does not have access to rent control in neighborhood n, (14) becomes a simple Cobb-Douglas optimization problem. Standard techniques imply that the optimal choices are constant fractions of total income:

$$c^*_{in\ell} = (1 - \alpha_k(i)) \Big(w_{k(i)\ell} + \pi_i \Big), \qquad h^*_{in\ell} = \alpha_{k(i)} \frac{w_{k(i)\ell} + \pi_i}{R_n}. \tag{15}$$

Suppose now that the worker has access to rent control. In this case, their housing space consumption is potentially constrained by the housing constraint, $\bar{h}$, if they choose to exercise their rent control option. Consumption is then determined from the budget constraint. Therefore, when $\iota^*_{i\ell} = 1$ and $n^{\text{rc}}_i = n$, we have

$$c^*_{in\ell} = w_{k(i)\ell} + \pi_i - \kappa_n R_n h^*_{in\ell}, \qquad h^*_{in\ell} = \min \left\{ \alpha_{k(i)} \frac{w_{k(i)\ell} + \pi_i}{\kappa_n R_n}, \ \bar{h} \right\}. \tag{16}$$

Note that when workers are unconstrained even with rent control, (16) reduces to (15). The last choice to derive is the choice of rent control over market rent housing within n^{rc}_i. Clearly, $\iota^*_{i\ell} = 1$ when workers are unconstrained since $\kappa_n < 1$. Otherwise, the choice becomes

$$\iota^*_{i\ell} = \begin{cases} 1, & \text{if} \quad u_{k(i)}(w_{k(i)\ell} + \pi_i - \kappa_n R_n \bar{h}, \bar{h}) \geq (w_{k(i)\ell} + \pi_i) / R_n^{\alpha_{k(i)}} \\ 0, & \text{if} \quad u_{k(i)}(w_{k(i)\ell} + \pi_i - \kappa_n R_n \bar{h}, \bar{h}) < (w_{k(i)\ell} + \pi_i) / R_n^{\alpha_{k(i)}} \end{cases} \tag{17}$$

The solution in the main text therefore combines (15)-(17).

10.2 Algorithm to Solve the Model Equilibrium

Quantitatively, the model can be solved according to the following algorithm.

1. Guess $\boldsymbol{R}^{(0)}$, $\boldsymbol{w}_k^{(0)}$, and $\boldsymbol{\psi}^{(0)}$.

2. Given $(\boldsymbol{R}^{(0)}, \boldsymbol{\psi}^{(0)})$, solve for equilibrium wages using the following algorithm.

   2.1. Solve for the optimal worker choices, $c_{kn\ell}(n^{\text{rc}})$ and $h_{kn\ell}(n^{\text{rc}})$, and rent control choices, $\iota_{kn\ell}(n^{\text{rc}})$

   2.2. Update choice probabilities, $\phi_{kn\ell}(n^{\text{rc}})$

   2.3. Update wages using labor market clearing condition:

$$w_{k\ell} = Z_{k\ell}\left(\mu_k \sum_{n^{\text{rc}} \in \mathcal{N} \cup \{0\}} \psi(n^{\text{rc}}) \sum_{n \in \mathcal{N}} \phi_{kn\ell}(n^{\text{rc}})\right)^{-\frac{1}{\gamma}}$$

   2.4. Repeat 2.1-2.3 until convergence

3. Given $\boldsymbol{\psi}^{(0)}$, update rental rates using market housing clearing condition:

$$R_n = \frac{1}{(1-\eta_n)H_n}\Bigg[\psi(n) \sum_{k=L,H} \mu_k \sum_{\ell \in \mathcal{N}} \phi_{kn\ell}(n)\alpha_k(w_{k\ell} + \pi_{kn})(1 - \iota^*_{k\ell}(n)) + \sum_{n^{\text{rc}} \neq n} \psi(n^{\text{rc}}) \sum_{k=L,H} \mu_k \sum_{\ell \in \mathcal{N}} \phi_{kn\ell}(n^{\text{rc}})\alpha_k(w_{k\ell} + \pi_{kn})\Bigg]$$

4. Repeat Steps 2 and 3 until $\boldsymbol{R}$ converges.

5. Update rent control lotteries for $n \in \mathcal{N}$ using the rent-control market clearing condition:

$$\psi(n) = \eta_n H_n \left[\sum_{k=L,H} \mu_k \sum_{\ell \in \mathcal{N}} \phi_{kn\ell}(n) h^*_{kn\ell}(n) \iota^*_{k\ell}(n)\right]^{-1}$$

   Set $\psi(0) = 1 - \sum_n \psi(n)$. **Break if $\boldsymbol{\psi(0) < 0}$ — no solution given parameters.**

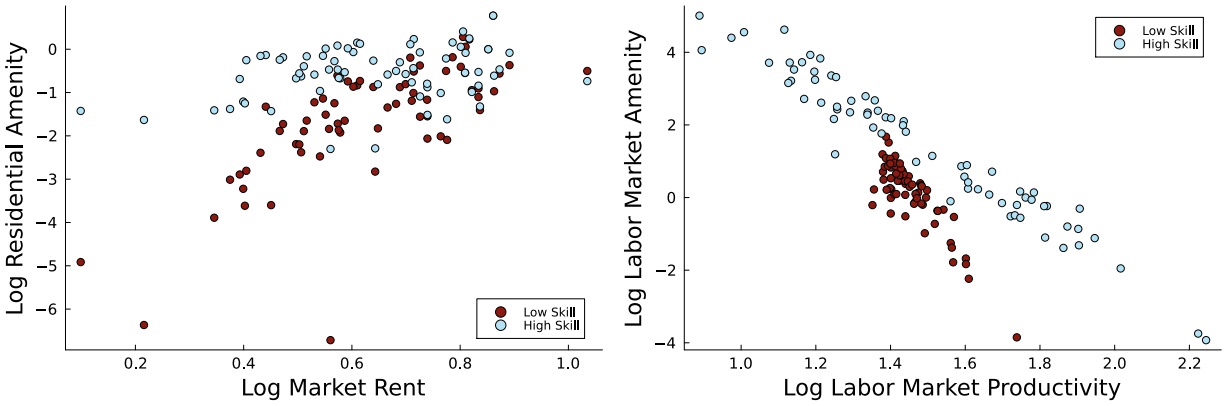
6. If $\psi(0) > 0$, repeat Steps 2-4 until convergence.

10.3 Additional Estimation Results

This section discusses the estimation results for neighborhood and labor market amenities and commuting costs.

Figure A11 documents how estimated amenity values relate to the underlying characteristics of their locations. Panel (a) shows that high rent neighborhoods tend to have better amenities. This relationship is stronger for low skill workers, reflecting their desire to live in better neighborhoods, but being priced out. Panel (b) shows that high wage labor markets tend to have lower labor market amenities. The estimation procedure requires this relationship if there are labor markets in the data that do not have a large number of workers.

FIGURE A11: AMENITY ESTIMATES BY MARKET CHARACTERISTICS

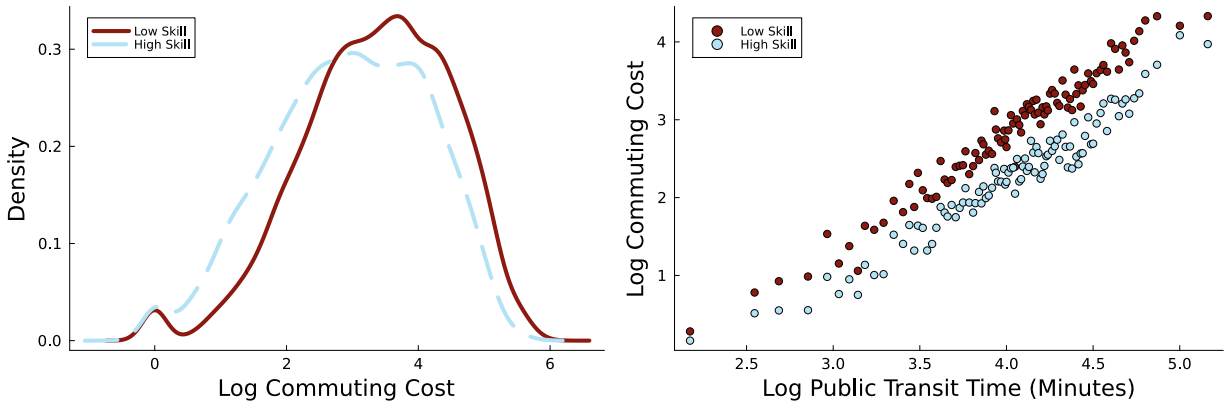


(a) Residential Amenities (b) Labor Market Amenities

Note: This figure plots log amenities against their relevant market characteristic. Panel (a) reports log residential amenities, β_{kn}, against log market rent, R_n, for each skill group. Panel (b) reports log labor market amenities, $\lambda_{k\ell}$, against log wages, $w_{k\ell}$, for each skill group.

Figure A12 documents the estimated commuting costs for each worker type. Panel (a) shows that the distribution of commuting costs shifts to the right for low skill workers, implying generally larger costs. Since commuting costs to worker's residential neighborhood are normalized to 1, this implies a larger cost of travel. Panel (b) confirms this by plotting estimated log commuting costs against the log travel time via public transit. Commuting costs are increasing in travel time, and the relationship is steeper for low skill workers, implying larger commuting frictions to distant labor markets.

FIGURE A12: ESTIMATED COMMUTING COSTS BY SKILL



(a) Distribution of Commuting Costs

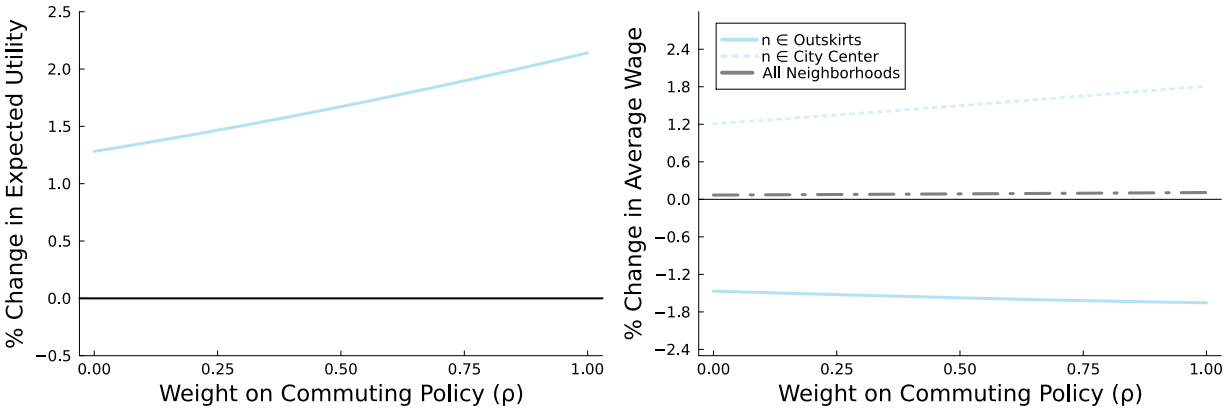
(b) Commuting Costs and Travel Time

Note: This figure plots information about the estimated commuting costs for each skill group. Panel (a) displays the distribution of commuting costs for low skill (red) and high skill (blue) workers. Panel (b) displays a bin scatter plot of the log commuting cost between two zip codes against the log public transit travel time between the two zip codes.

10.4 Additional Counterfactual Results

Figure A13 displays the effects of the transfer policies on high-skill workers. Unlike low-skill workers, high-skill worker welfare increases when low-skill workers are reallocated to optimal transit locations (Panel (a)). This is due to general equilibrium effects in the rental markets: low-skill households leave the highest amenity neighborhoods, which reduces rent and improves average welfare for the high-skill. Panel (b) shows that wage inequality across the city also increases for high-skill workers, but that this effect is exacerbated as the weight on shifts to the commuting policy. This is again due to housing market effects: since housing is now cheaper in the desirable neighborhoods in the outskirts, high-skill workers move and work there rather than the city center, leading to a negative labor market supply effect on wages in the outskirts and a positive labor market supply effect in the city center.

FIGURE A13: THE WELFARE AND LABOR MARKET EFFECTS OF TRANSFER POLICIES ON HIGH SKILL WORKERS



(a) Change in Expected Utility

(b) Change in Average Wage

Note: This figure plots (a) the percentage change in expected utility and (b) the average wage of low-skill workers relative to the rent control equilibrium given a policy mix ρ. In Panel (a), the star denotes the utility difference under the optimal low-skill policy, which corresponds to $\rho = 0$. In Panel (b), the solid line denotes the change in the average wage conditional on a worker choosing to live in the outskirts, the dotted line denotes the change in the average wage conditional on living in the city center, and the dashed-dotted gray line denotes the change in the unconditional average wage.