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

Geraldo Cerqueiro[†]

Católica-Lisbon SBE

Isaac Hacamo[‡]

Indiana University

Pedro Raposo[§]

Católica-Lisbon SBE

March 10, 2024

Employing a quasi-natural experiment, we show that after losing rent control, low-income workers' earnings decline significantly, while high-income workers remain unaffected, even as both are equally likely to relocate to city outskirts, and pay higher rents. The wage decline stems from transition to worse quality jobs in firms located in the city outskirts. New occupants of formerly rent-controlled apartments have higher income and experience wage shocks prior, but not after relocation. Our evidence suggests that, after losing rent control, low-income workers cannot afford commuting costs to higher-paying jobs. More efficient public transportation may accomplish similar labor outcomes than rent control.

1 INTRODUCTION

Most major cities worldwide are facing a housing affordability crisis. In the United States, over 50 percent of residents report that affordability is a significant challenge in their city.[1] Attempting to address this crisis, many politicians have implemented or reinforced 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 altered distance to work. Distance to the workplace may increase commuting costs, reduce productivity, or increase job search costs.[2] 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 (Munch and Svarer, 2002; Svarer et al., 2005), leading to lower wages. Therefore, it is not clear how access to rent control affect the wages of an average worker.

Whether rent control leads to distributional effects depends on the differential impact of commuting frictions on low- and high-income individuals. High-income workers with access to rent control might experience an increase in productivity as their occupational tasks might be more

---

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

[2]For example, long commutes may affect availability to work, health outcomes, sleeping patterns, obesity, and stress levels (Christian, 2012; Evans and Wener, 2006; Krol and Svorny, 2005; Raza et al., 2021).

sensitive to commuting time (Ma and Ye, 2019). Nonetheless, in the absence of rent control, their larger financial resources could allow them to alleviate commuting frictions by optimizing on the best mode of transportation. On the other hand, low-income workers, who might struggle to afford any means of transportation, could experience higher wages with access to rent control (Farré et al., 2023; Moreno-Monroy and Posada, 2018). Understanding these distributional effects is critical, yet our knowledge of this subject remains limited.

Using a natural experiment in Portugal, we show that loss of rent control pushes low- and high-income workers to the city outskirts, increasing their distance to the workplace and rent payments.[3] We then show that individuals who lose access to rent control experience a negative impact on earnings. However, high-income individuals experience no losses, while, low-income workers experience a significant decline in earnings. This decline stems from transitioning to worse quality jobs in firms located in the city outskirts. Those staying in existing jobs do not experience a decline in wages. We offer four additional results showing that low-income workers experience high commuting costs, while high-income workers manage to overcome commuting frictions.

It is non-trivial to identify the effects 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 established 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.[4] Legacy renters can retain their rent-controlled units until they pass away. We exploit this change in rent control laws in two ways.

First, we study the death of a legacy renter to analyze the career paths of residents younger

---

[3]We define low-income (high) as individuals with incomes below (above) the median income in our sample. We note that the sample mean is lower than the population mean since our sample is limited to individuals living in rent-controlled units.

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

than 64 sharing the same dwelling.[5] 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.[6] 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 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.[7] Those living with a renter slightly younger than 65 lost rent control after 2012. In this analysis, we evaluate the core results of our primary specification. However, the limited sample size near the age discontinuity restricts us from conducting extensive 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, which are the only cities in Portugal with rent control laws.[8] Our panel starts in 2010 and ends in 2020, and we only examine the career paths of working individuals aged 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

---

[5]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.

[6]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.

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

[8]We detail the history of rental laws in Portugal in section 3.1.

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. However, these results conceal substantial heterogeneity. Individuals with above-median income in rent-controlled homes experience no losses 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.

To better understand the economic mechanism behind the decline in wages, we investigate whether this effect is due to workers either staying in their current jobs or transitioning to new ones. To avoid the endogeneity associated with job switching, we identify exogenous job transitions. Specifically, we examine distressed employers to determine the effect of rent control on samples of employees working in distressed and non-distressed companies. We define a company as distressed if more than 30 percent of its workforce was laid off in a given year during our sample period (2010-2020). We assume that an employee working for one of these firms is more likely to be pushed out. These results reveal that the negative wage effect we document above stems uniquely from transitions to new jobs. We also show that those who lose rent control and work in distressed firms are more likely to accept new jobs in the city outskirts, with their earnings decreasing by approximately 3 to 4 percent relative to those who work in distressed firms but do not lose access to rent control.

The death of an elder may, however, affect the career outcomes of surviving family members through alternative channels. For example, if the deceased elder provided childcare, households might reduce labor supply. Alternatively, grief associated with a family member's death might adversely affect productivity at work. We offer an additional test to rule out this alternative interpretation of our results. We document similar findings using an alternative specification that

5

exploits the age-based discontinuity in access to rent control. Specifically, 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.[9] These point estimates are almost identical to our primary specification, suggesting that the impact of the death of a legacy renter only affects wages through the loss of rent control.

Our results suggest that low-income workers cannot afford commuting costs to higher-paying jobs in the city center. Supporting this mechanism, we show four 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 find that traveling by car is significantly faster, taking less than half the time required for public transportation. Third, we observe that the majority of wage losses following the elimination of rent control are predominantly among individuals who do not own a vehicle. Lastly, we benchmark the wage losses against the transportation costs in Lisbon and Porto. Our estimates indicate that the wage loss is marginally smaller than the cost of public transportation. This suggests that low-income households are rationally deciding to switch jobs in the outskirts, since the transportation costs from the outskirts to the city center would outweigh the wage benefits in the city center.

Next, we examine the earnings of individuals moving into formerly rent-controlled houses. Removing rent control may increase earnings of high-productivity individuals through reduced spatial misallocation. To analyze this hypothesis, we follow housing units occupied by legacy renters in 2010 and monitor occupancy changes until 2021. Analyzing former rent-controlled units shows that 73 percent are rented at free market prices, 20 percent are purchased by households, and the reminder is re-purposed into other types of properties. Moreover, high-income individuals are more likely to move to former rent-controlled units. We then conduct an event study to examine earnings, labor supply, and wages of new residents of these homes. Our findings show that individuals experience a shock in total earnings and wages in years prior to relocating to previously

---

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

rent-controlled units. Surprisingly, they do not experience any change in earnings after relocating. This evidence further supports our causal evidence that rent control does not impact labor outcomes of high-income workers as they can afford commuting costs.

Lastly, we put into context the wage loss of low-income workers. First, we show that their wage loss is not compensated by lower housing costs in the outskirts since all individuals pushed out of rent controlled homes pay higher rents, even those relocating to the outskirts. Second, we show that rents of formerly controlled units increase by almost 200 percent after being vacated. Measuring this rent increase allows us to put into context the wage losses of low-income workers. These losses only constitute 10-15 percent of the potential gains landlords make under free markets. These insights can inform the design of more effective affordability policies. Policymakers could consider income transfers that promote improved access to public transportation.

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.[10] Glaeser and Luttmer (2003) find that, in New York City, a significant fraction of rent-controlled 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.[11] Diamond et al. (2019) find that landlords reduce housing supply by selling to owner-occupants and redeveloping buildings after the introduction of rent control laws in San Francisco.[12] could earn in free markets. We demonstrate that rent control results in misallocation in the labor market, benefiting low-income workers, but not high-income workers. Furthermore, the wage gains for low-income workers represent only a

---

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

[11]Sims (2007) show that rent control in Massachusetts had little effect on the construction of new housing but did encourage owners to shift units away from rental status.

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

minor portion of the potential profits landlords could earn in free markets.

Second, our paper relates to literature that studies the labor market effects of housing affordability policies. Svarer et al. (2005) show that the probability of finding a local job increases with the rent control intensity. Munch and Svarer (2002) shows that tenancy mobility is severely reduced by rent control. Diamond et al. (2019) show that rent control limits mobility while lowering displacement. Van Dijk (2018) shows that average move into public housing negatively affects labor market outcomes, but moves into high-income neighborhoods generate positive effects, suggesting that targeting public housing in high-income neighborhoods can increase economic self-sufficiency. Öst and Johansson (2023) show that young adults with access to rent-controlled apartments in Sweden experience lower labor income as they increase the likelihood of attending higher education. Our paper offers additional evidence that the introduction of affordable housing near job-rich areas may improve the labor market outcomes of low-income.

Lastly, we add to the literature that studies the impact of spatial frictions on worker's welfare. Bilal and Rossi-Hansberg (2021) conceptualize a "location asset", which has a current cost equal to the location's rent, and a future payoff based on increased employment opportunities. Monte et al. (2018) find that reductions in commuting costs generate welfare gains. Manning and Petrongolo (2017) show that the attractiveness of jobs to applicants sharply decays with distance. Glaeser et al. (2008) suggest that urbanization of poverty comes mainly from better access to public transportation in central cities. Heblich et al. (2020) estimate that reducing the railway network reduces the population and land value in London, and decreases net commuting into the historical center. Couture et al. (2019) explore the impact of rising incomes at the top of the distribution, and show that spatial resorting within large U.S. cities increased the welfare of richer households relative to poorer households. Severen (2019) shows, in a quantitative spatial model, that Metro Rail connections, in Los Angeles, increase commuting by 16 percent but do not have large effects on local productivity or amenities. We contribute to this literature by showing that earnings losses associated with the loss of rent control stem from lower job proximity. Our results suggest that improved

access to public transportation could act as a potential substitute to rent control.

2 HYPOTHESIS DEVELOPMENT

The impact of access to rent controls on wages is unclear, including any potential distributional effects. A potential impact of rent control on labor outcomes is likely to stem from commuting-related frictions. As such, we start by outlining the specific commuting frictions that are potentially alleviated by rent control. We then examine theories that incorporate these frictions and delineate how rent control may influence the wages of an average worker. Lastly, we explore the key ingredients of a framework in which rent control might create distributional differences between low- and high-income workers.

2.1 Commuting Frictions

A canonical frictionless one-employer model, in which workers optimize consumption and leisure, predicts that longer *commuting times* lead to higher wages in equilibrium (French et al., 2020). This is because workers decrease their labor supply in response to extended commutes, pushing employers to offer higher wages to incentivize workers to allocate less time for leisure. When labor costs are sufficiently large, firm relocation might balance-out wage differentials (Erickson and Wasylenko, 1980; Mulalic et al., 2014). In the presence of multiple employers, higher commuting times might lead to job switching. *Financial costs* of commuting may also lead workers to find job options closer to their residence (Molloy and Shan, 2013; Van Ommeren, 1998). Consistent with this hypothesis, higher fuel prices increase the value of real estate that offers shorter commutes and easier access to alternatives to driving, compared to homes that are more dependent on driving (Blake, 2019).

Distance to work could also influence *job search* dynamics. For example, 60 percent of Americans find jobs through personal networks. Residing farther from the workplace can reduce the size and quality of these networks, potentially leading to higher job search costs (Abebe et al.,

2016; Phillips, 2014; Van Ommeren and Fosgerau, 2009; Zenou, 2011). Lastly, a substantial body of research in health economics argues that lengthy commutes can severely impact employee *health and well-being*. Extended commuting times can influence work availability, health outcomes, sleep patterns, obesity levels, and stress, potentially impairing productivity (Christian, 2012; Evans and Wener, 2006; Krol and Svorny, 2005; Raza et al., 2021).

2.2 Average Effects

Rent control may facilitate access to local job opportunities for occupants of controlled units, whereas individuals living in uncontrolled units could experience substantial commuting frictions. Access to rent control housing could then lead to an increase in wages. The presence of rent-controlled housing might also diminish the incentive to relocate (Diamond et al., 2019), resulting in a higher likelihood of rejecting job opportunities located away from controlled units (Svarer et al., 2005). Consistent with this hypothesis, Jiang et al. (2022) provide evidence suggesting that rent stabilization increases tenants' unemployment. Thus, rent control could either raise or lower average wages.

2.3 Distributional Effects

The impact of rent control on the wage distribution hinges on the differential impact of commuting frictions on low- and high-income individuals. High-income workers might experience a larger increase in productivity as their occupational tasks might be more sensitive to commuting-related frictions (Ma and Ye, 2019). For example, if long commuting times affect cognitive skills (Evans and Wener, 2006), it might disproportionately affect those who execute abstract rather than mechanical tasks. Furthermore, a higher marginal rate of substitution between leisure and consumption may lead high-income workers to be willing to accept a wage cut to avoid a long commute. Nonetheless, in the absence of rent control, the larger disposable income of high-income workers could help alleviate commuting frictions by optimizing on the best mode of transportation.

On the other hand, low-income individuals, who may find public transportation unaffordable or lack alternative means of transportation, could disproportionately suffer from wage decreases due to commuting frictions. Longer commuting times might also affect the labor participation rate, especially of low-income women (Farré et al., 2023). Additionally, long commuting times may affect the likelihood of working in the formal versus informal labor market (Moreno-Monroy and Posada, 2018). Thus, it is unclear whether rent control has a differential effect on the wages of low- versus high-income workers.

3 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.

3.1 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.[13] 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.[14] 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.[15]

Additional legislation was introduced to improve tenant protection in the following years. For example, in 1922, the method to determine rent prices was indexed to a building's initial value

---

[13]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*.

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

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

and age; while in 1948, a tax-assessed property value was used to determine rent price updates.[16] 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.[17] 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.[18] 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.[19] 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 political turmoil, this reform did not apply to lease agreements signed prior to this date, effectively

---

[16]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*.

[17]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*.

[18]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.

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

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.[20] 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.[21] 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. 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.
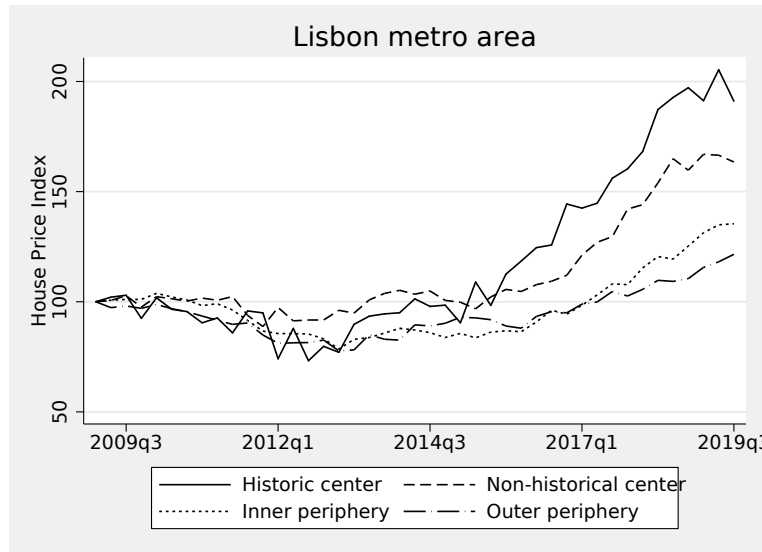
3.2   House Prices

House prices in Portugal remained largely stable from 2010 to 2015 but experienced rapid growth between 2015 and 2019. Figure 1 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 A1 in the Internet Appendix presents the time series of house

---

[20]More details can be found in the *Lei n.º 31/2012, de 14 de Agosto de 1990*.

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

prices in Porto, Portugal's second largest city. Porto exhibits a pattern similar to Lisbon.
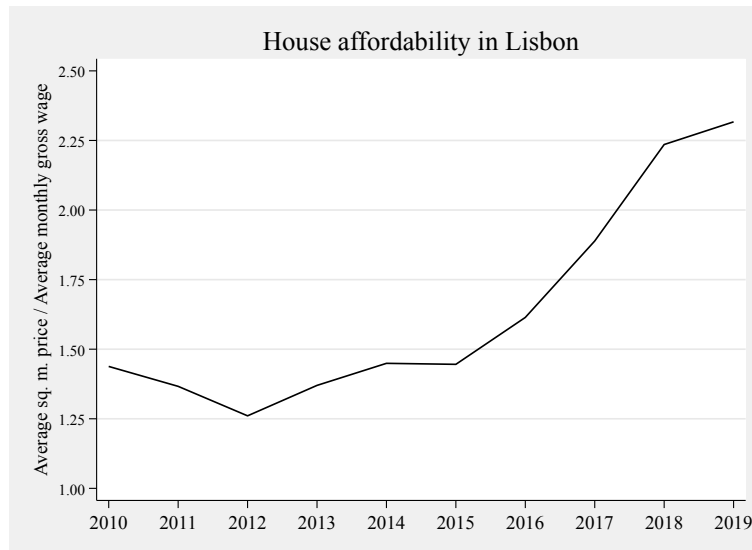
Figure 1: **House prices in Lisbon**

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 A7 shows the neighborhoods in Lisbon that compose the city-center and city-periphery, while Figure A5 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 short-term rentals (e.g., Airbnb) might have contributed to the growth in house prices (Franco and Santos, 2021; Peralta et al., 2020). Consistently, Figure A2 shows that the number of tourists in Portugal remained steady from 2005 to 2013, then more than doubled between 2013 and 2019. Figure A3 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

14

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 2: **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.

3.3 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 2. 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,

15

became even more crucial after 2015.

4 DATA

Our data is provided by the *Statistics of Portugal*. Statistics of 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 processing was conducted. We describe in this section each dataset, the construction of the final dataset, and key summary statistics of these data.

4.1 BPR

Our primary dataset is the Portuguese Administrative Census dataset (BPR).[22] This dataset encompasses all individuals residing in Portugal from 2010 through 2020. This data is gathered annually by the Statistics of 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.

---

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

4.2 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 of Portugal.[23] 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 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.

5 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

---

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

market prices.[24] 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.

5.1   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.[25] 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 A7 and A8 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

---

[24]For more information on rental laws in Portugal, please refer to section 3.1.
[25]See Section 3.1 for more details on this exception.

this procedure, the age difference between a legacy renter that passes and one that survives is only 0.258 years.

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 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.

5.1.1 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*:

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

where and *Legacy death*$_{f(i),t}$ equals one after a *legacy renter* in family f passes away. $Y_{i,f,t}$ 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).

5.2 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 robustness test, we anticipate findings similar to our main specification, despite the smaller sample size around the discontinuity. We then estimate the following differences-in-differences:

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

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.

6 RESULTS

This section examines the impact of rent control on wages and inequality. Section 6.1 documents the effect of rent control on migration. Sections 6.2 through 6.4 introduce the impact on wages of those who lose access to rent-controlled apartments. Section 6.5 analyses the wages of individuals who move into former rent-controlled homes. Section 6.6 discusses the role of commuting costs in explaining our results. Section 6.7 examines rents values paid by individuals who lost access to rent control and also for those who move into former rent-controlled units. Section 6.8 offers additional results.

6.1 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.[26] Figures A5 and A6 depict the municipalities that belong to the outskirts of Lisbon and Porto, respectively. We use our empirical design (1) 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

---

[26]For example, Amadora or Almada municipalities are considered outskirts of Lisbon. See Figures A5 and A6 for further details.
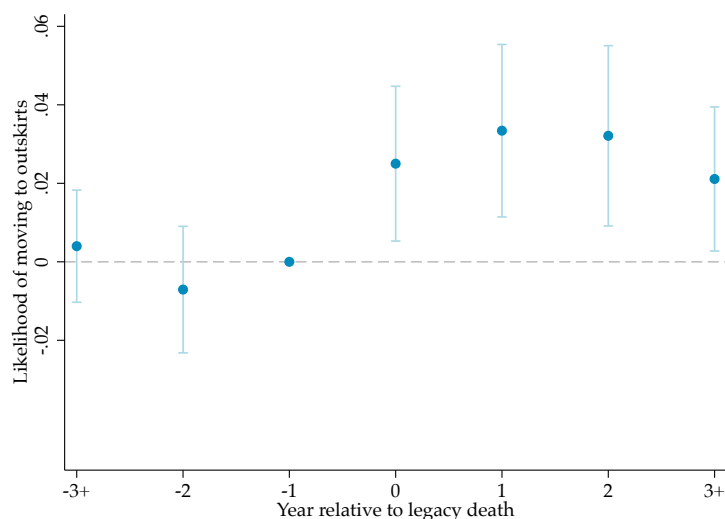
likelihood of moving to the outskirts after the passing of a legacy renter. A family member is 2.5 percentage points more likely to move to the outskirts in a given year. This effect is statistically significant at the one percent level. These coefficients imply that, after losing access to a rent control 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.

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:

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

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

Figure 3: Event plot analysis: Move to outskirts

The period-by-period breakdown reported in Figure 3 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.

6.2 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. Theory discussed in section 2 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	Distressed firm	Other firms	Distressed 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 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 distressed firm. We define a firm as under distress if more than 30 percent of the workforce was laid off in at least one year during our sample period (2010-2020). For those working for a distressed 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 note that the percentage of workers involved in a mass layoff might appear unusually high. This observation can largely be attributed to our technical definition of distressed firms. Specifically, a distressed firm is characterized by experiencing a mass layoff, likely occurring just once within the 10 year sample period. Consequently, only approximately 5% of our sample is expected to lose rent control during a mass layoff. Additionally, it's important to acknowledge that our defi-
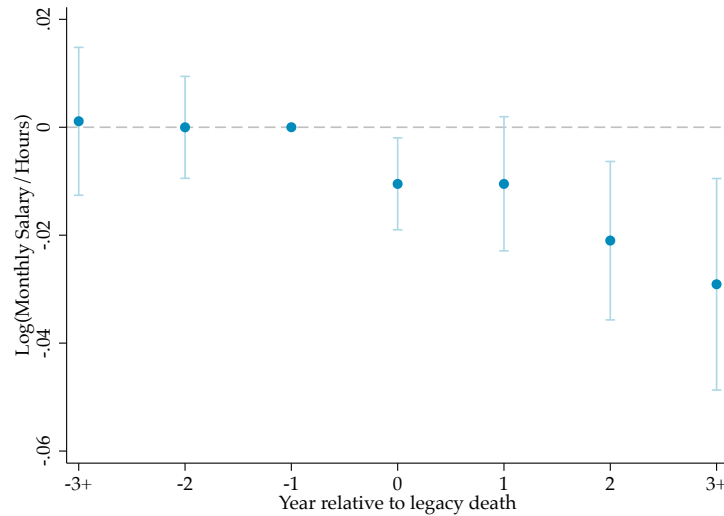
nition of distress could be conflated with high turnover rates. However, in both scenarios, workers face a higher likelihood of being forced to move, aligning with the objective of this test.

We also investigate whether those who were forced to move jobs are more likely to move to jobs in city outskirts. Among those forced to move, the likelihood of moving to job in the outskirts increases by 3.15 percentage points. For those who were not forced to move, there is no change in the likelihood of changing jobs 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.

Table 3 suggests that wage losses following the loss of rent control occur exclusively when employees are working for distressed 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 4 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.

Figure 4: 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.

6.2.1 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, rather than other factors such as grief, inheritances, or assistance with childcare.

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*** (0.006)	-0.0224*** (0.007)	-0.0218* (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.

6.3 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. Additionally, high-income workers are 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.

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 (0.023)	-0.022** (0.009)	-0.007 (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.

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).

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

6.4.1 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.

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 ina distress or non-distress 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 balances out.

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

	y-var: Log(Hours worked)			*y-var*: Unemployment		
	Full Sample	Distressed firm	Other firms	Full Sample	Distressed firm	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.

6.4.2 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 distressed 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.

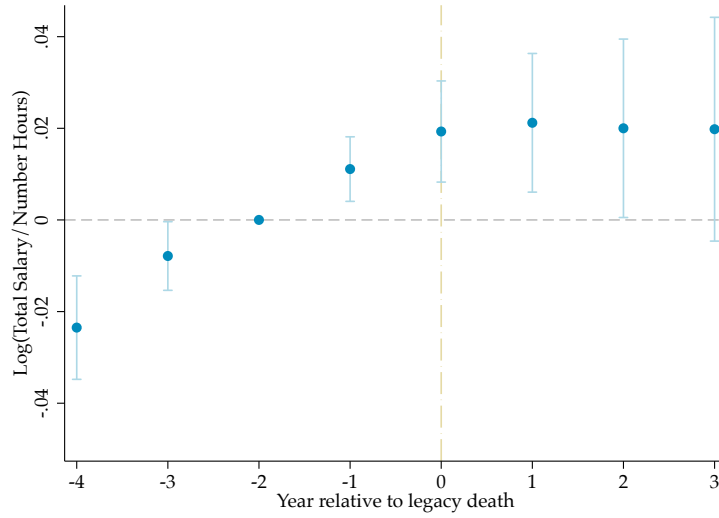## 6.5   New tenants of former rent-controlled units

Next, we examine the earnings of individuals moving into formerly rent-controlled houses. Removing rent control may increase earnings of high-productivity individuals through reduced spatial misallocation (Glaeser and Luttmer, 2003). To analyze this hypothesis, we follow housing units occupied by legacy renters in 2010 and monitored occupancy changes until 2021. We then conduct an event study to examine earnings of new residents of these houses. More concretely, we estimate the following regression model for individual i:

$$
\begin{aligned}
\mathrm{Y}_{i,f} &= \sum_{m=-5}^{4} \beta_m \times \mathrm{Post}_{t,m} \\
&+ \quad \text{Individual FE} + \text{Time FE} + \epsilon_{i,t},
\end{aligned}
$$

Figure 5 reports the event study plot of wages of all occupants. We use $t = -2$ as the reference year.
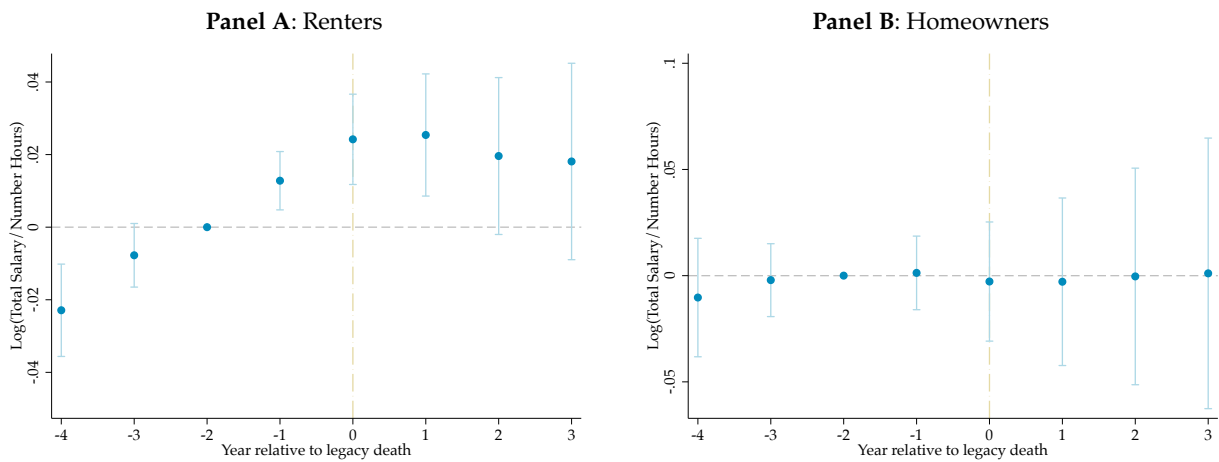
Our findings show that individuals experience a wage shock in years prior to relocating to formerly rent-controlled units. Surprisingly, they do not experience any change in wages up to 3 years after relocating. The breakdown by homeowners and renters, reported in Figure 6, shows the same pattern post moving in. This evidence further supports our causal evidence that rent control does not impact labor outcomes of high-income workers as they can afford commuting costs.

31

Figure 5: Event Study analysis: New occupants

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.

Figure 6: Event Study analysis: Renters vs Homeowners

Panel A: Renters
Panel B: Homeowners

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.

6.6 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.

6.6.1 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 counterparts, 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, while using public transportation takes 44 minutes.

6.6.2 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.

Table 7: Effect on wages and modes of transportation

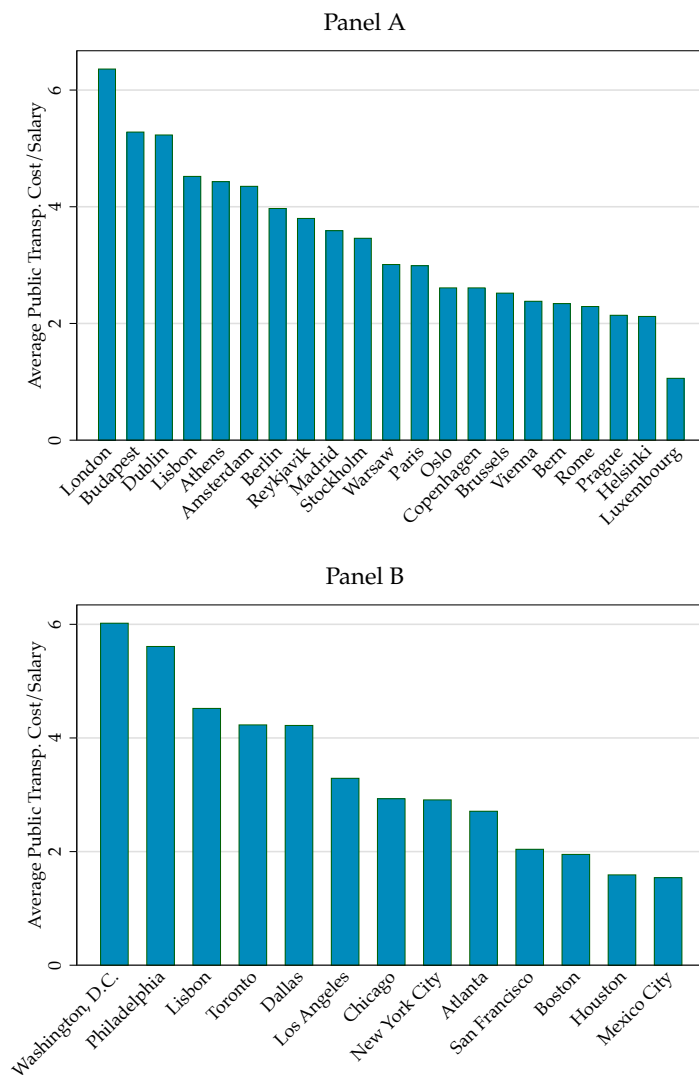| | y-var: Log(Monthly salary/hours) | | | | y-var: Job in Outskirts | |
| | Distressed firm | | Other firms | | No car | |
	Owns car	No car	Owns car	No car	Distressed firm	Other firms
Treatment × Post	-0.0024	-0.0387***	0.0000	-0.0116	0.0387***	0.0045
	(0.023)	(0.013)	(0.015)	(0.011)	(0.011)	(0.006)
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.

6.6.3 Transportation costs in Lisbon

The average public transportation costs in Lisbon are almost 5 percent of the monthly average salary. This value nearly matches the losses in wages for those earning below the median. This suggests that low-income workers may opt to work in the outskirts at lower-wage jobs, as the costs of commuting to the city can outweigh the benefits of higher-paying jobs in the city center.

Figure 7 shows that Portugal has one of the highest public transportation costs relative to the average wage in Europe, and even compared to North American cities. This shows that commuting costs in Portugal are expensive, and lends support that a policy targeting lower commuting costs might be more effective than price controls.

Figure 7: Average public transportation costs per salary

Panel A

Panel B

Notes: We obtained the data for this figure from multiple press releases issued by local government agencies.

6.7   Rent Values

We also examine rents of those leaving price control units as well as those moving into these units.

First, we show that the rents paid by treated and displaced households are significantly higher than

their previous controlled rents. We obtain rent information in 2011 and 2021. Table 8 confirms in a regression framework that low-income workers that lost access to rent control pay nearly 50 euros more in rent relative to those in the control group. We note that this value underestimates the rent increase since our rent data is truncated on the right-hand side. This evidence suggests that, in the absence of rent control policies, treated workers are significantly worse-off.
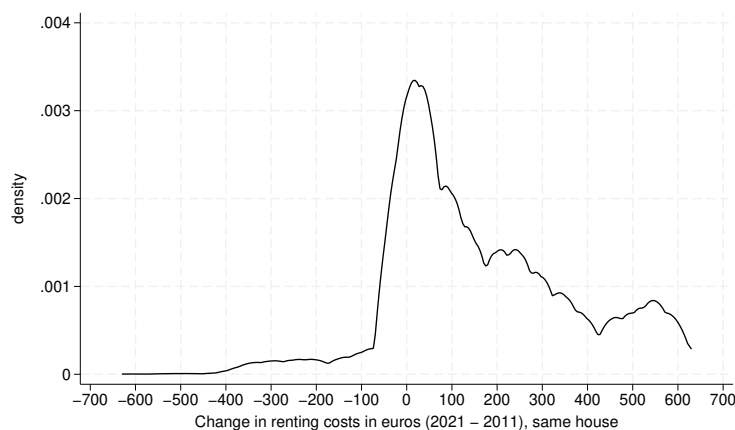
Table 8: Rents paid after loss of rent control

	All	Low-income	High-income
		$[P10th < Inc < P50th]$	$[Inc > P50th]$
Treatment	34.54***	47.90***	55.35***
	(3.01)	(15.12)	(15.28)
N	25,938	8,690	8,506
R-squared	0.006	0.012	0.016

Notes: This table reports the regression coefficients of a model that only compares rents between 2011 and 2021. This because we only observed rents paid in these two years. We estimate the following model: $\Delta\text{Rent paid}_{2011\rightarrow2021,i} = \beta \times \text{Treatment}_i + \varepsilon_i$. The outcome variable is the change in rent between 2011 and 2021. Standard errors are robust and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Next, we examine the increase in rents at the property level, which allows us to measure the potential gains landlords may earn in an open market. Figure 8 shows that rent prices are likely to increase by 192 percent following the end of rent control. Combining these figures with the estimates from Table 5, it suggests that the wage losses for those below the 50th percentile account for only 10 percent to 15 percent of the potential gains landlords could make if their properties were rented in an uncontrolled market. This estimate assumes a household of two working individuals.

These insights can guide the development of more effective affordability policies. Perhaps, to lessen the indirect labor market costs associated with a lack of affordable housing, policymakers could contemplate easing both the financial and time costs related to commuting for low-income workers. Such a policy could potentially carry much less economic inefficiency compared to implementing rent price controls.

Figure 8: Change in rent prices at home-level

Notes: This figure reports the distribution of change in rents for homes that were under rent-control in 2011. The change in rents is computed between 2021 and 2011.

6.8 Additional results

We also tabulate our findings by age, gender, and location. Table 9 shows that the effect is concentrated among those with ages between 35 and 50. Table A1 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 A2 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.

Table 9: 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.

7 CONCLUDING REMARKS

More than 50 percent of U.S. residents are concerned with housing affordability in their cities, leading many cities to implement or bolster existing rent control and stabilization policies. However, 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.

Using a 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. In addition to the large costs already documented in the literature, we further uncover that while wage losses are large for low-income workers, they represent a small fraction of the free-market rents that landlords earn after rent-control units are vacated. Our paper instead shows the important of commuting costs to low-income workers. Improved public transportation may provide a better substitute to rent controls.

References

Girum Abebe, Stefano Caria, Marcel Fafchamps, Paolo Falco, Simon Franklin, and Simon Quinn. Curse of anonymity or tyranny of distance? The impacts of job-search support in urban ethiopia. Technical report, National Bureau of Economic Research, 2016.

Kenneth R Ahern and Marco Giacoletti. Robbing Peter to pay Paul? The redistribution of wealth caused by rent control. Technical report, National Bureau of Economic Research, 2022.

David H Autor, Christopher J Palmer, and Parag A Pathak. Housing market spillovers: Evidence from the end of rent control in Cambridge, Massachusetts. *Journal of Political Economy*, 122(3):661–717, 2014.

Adrien Bilal and Esteban Rossi-Hansberg. Location as an asset. *Econometrica*, 89(5):2459–2495, 2021.

Thomas Blake. Commuting costs and geographic sorting in the housing market. *Real Estate Economics*, 47 (4):1089–1118, 2019.

Olivier Blanchard and Justin Wolfers. The role of shocks and institutions in the rise of European unemployment: The aggregate evidence. *The Economic Journal*, 110(462):C1–C33, 2000.

Brantly Callaway and Pedro HC Sant'Anna. Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230, 2021.

Ruoyu Chen, Hanchen Jiang, and Luis Quintero. Measuring the value of rent stabilization and understanding its implications for racial inequality: Evidence from New York City. *Available at SSRN 4077292*, 2022.

Thomas J Christian. Trade-offs between commuting time and health-related activities. *Journal of Urban Health*, 89(5):746–757, 2012.

Victor Couture, Cecile Gaubert, Jessie Handbury, and Erik Hurst. Income growth and the distributional effects of urban spatial sorting. Technical report, National Bureau of Economic Research, 2019.

Rebecca Diamond, Tim McQuade, and Franklin Qian. The effects of rent control expansion on tenants, landlords, and inequality: Evidence from San Francisco. *American Economic Review*, 109(9):3365–94, 2019.

Rodney A Erickson and Michael Wasylenko. Firm relocation and site selection in suburban municipalities. *Journal of Urban Economics*, 8(1):69–85, 1980.

Gary W Evans and Richard E Wener. Rail commuting duration and passenger stress. *Health psychology*, 25 (3):408, 2006.

Lídia Farré, Jordi Jofre-Monseny, and Juan Torrecillas. Commuting time and the gender gap in labor market participation. *Journal of Economic Geography*, 23(4):847–870, 2023.

Jack Favilukis, Pierre Mabille, and Stijn Van Nieuwerburgh. Affordable Housing and City Welfare. *The Review of Economic Studies*, 05 2022. ISSN 0034-6527. rdac024.

Sofia F Franco and Carlos Daniel Santos. The impact of Airbnb on residential property values and rents: Evidence from Portugal. *Regional Science and Urban Economics*, 88:103667, 2021.

Michael T French, Ioana Popovici, and Andrew R Timming. Analysing the effect of commuting time on earnings among young adults. *Applied Economics*, 52(48):5282–5297, 2020.

Sofia Gaspar and Fernando Ampudia de Haro. Buying citizenship? Chinese golden visa migrants in Portugal. *International Migration*, 58(3):58–72, 2020.

Edward L Glaeser and Erzo F P Luttmer. The misallocation of housing under rent control. *American Economic Review*, 93(4):1027–1046, 2003.

Edward L Glaeser, Matthew E Kahn, and Jordan Rappaport. Why do the poor live in cities? The role of public transportation. *Journal of Urban Economics*, 63(1):1–24, 2008.

Joseph Gyourko and Peter Linneman. Rent controls and rental housing quality: A note on the effects of New York City's old controls. *Journal of Urban Economics*, 27(3):398–409, 1990.

Anja M Hahn, Konstantin A Kholodilin, Sofie R Waltl, and Marco Fongoni. Forward to the past: Short-term effects of the rent freeze in Berlin. *Management Science*, 2023.

Stephan Heblich, Stephen J Redding, and Daniel M Sturm. The making of the modern metropolis: Evidence from London. *The Quarterly Journal of Economics*, 135(4):2059–2133, 2020.

Simon Jäger, Christopher Roth, Nina Roussille, and Benjamin Schoefer. Worker Beliefs About Outside Op-

tions. *The Quarterly Journal of Economics*, 2024.

Hanchen Jiang, Luis Quintero, and Xi Yang. Does rent regulation affect tenant unemployment? Evidence from New York City. *Johns Hopkins Carey Business School Research Paper*, (22-04), 2022.

Konstantin A Kholodilin. Rent control effects through the lens of empirical research: An almost complete review of the literature. 2022.

Robert Krol and Shirley Svorny. The effect of rent control on commute times. *Journal of Urban Economics*, 58 (3):421–436, 2005.

Liang Ma and Runing Ye. Does daily commuting behavior matter to employee productivity? *Journal of Transport Geography*, 76:130–141, 2019.

Alan Manning and Barbara Petrongolo. How local are labor markets? Evidence from a spatial job search model. *American Economic Review*, 107(10):2877–2907, 2017.

Andreas Mense, Claus Michelsen, and Konstantin A Kholodilin. Rent control, market segmentation, and misallocation: Causal evidence from a large-scale policy intervention. *Journal of Urban Economics*, 134: 103513, 2023.

Raven Molloy and Hui Shan. The effect of gasoline prices on household location. *Review of Economics and Statistics*, 95(4):1212–1221, 2013.

Ferdinando Monte, Stephen J Redding, and Esteban Rossi-Hansberg. Commuting, migration, and local employment elasticities. *American Economic Review*, 108(12):3855–90, 2018.

Choon-Geol Moon and Janet G Stotsky. The effect of rent control on housing quality change: A longitudinal analysis. *Journal of Political Economy*, 101(6):1114–1148, 1993.

Ana I Moreno-Monroy and Héctor M Posada. The effect of commuting costs and transport subsidies on informality rates. *Journal of Development Economics*, 130:99–112, 2018.

Ismir Mulalic, Jos N Van Ommeren, and Ninette Pilegaard. Wages and commuting: Quasi-natural experiments' evidence from firms that relocate. *The Economic Journal*, 124(579):1086–1105, 2014.

Jakob Roland Munch and Michael Svarer. Rent control and tenancy duration. *Journal of Urban Economics*, 52 (3):542–560, 2002.

Carlos Oliveira. The minimum wage and the wage distribution in portugal. *Labour Economics*, 85:102459, 2023. ISSN 0927-5371.

Cecilia Enström Öst and Per Johansson. The consequences of the Swedish rent control system on labor

income: Evidence from a randomized apartment lottery. *Journal of Public Economics*, 221:104864, 2023.

Susana Peralta, João Pereira dos Santos, Duarte Gonçalves, et al. Do short-term rentals increase housing prices? Quasi-experimental evidence from Lisbon. Technical report, Gabinete de Estratégia e Estudos, Ministério da Economia, 2020.

David C Phillips. Getting to work: Experimental evidence on job search and transportation costs. *Labour Economics*, 29:72–82, 2014.

Auriba Raza, Anna Pulakka, Linda L Magnusson Hanson, Hugo Westerlund, and Jaana I Halonen. Commuting distance and behavior-related health: A longitudinal study. *Preventive Medicine*, 150:106665, 2021.

Christopher Severen. Commuting, labor, and housing market effects of mass transportation: Welfare and identification. *The Review of Economics and Statistics*, pages 1–99, 2019.

David P Sims. Out of control: What can we learn from the end of Massachusetts rent control? *Journal of Urban Economics*, 61(1):129–151, 2007.

Liyang Sun and Sarah Abraham. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199, 2021.

Michael Svarer, Michael Rosholm, and Jakob Roland Munch. Rent control and unemployment duration. *Journal of Public Economics*, 89(11-12):2165–2181, 2005.

Winnie Van Dijk. The socio-economic consequences of housing assistance. 2018.

Jos Van Ommeren. On-the-job search behavior: The importance of commuting time. *Land Economics*, pages 526–540, 1998.

Jos Van Ommeren and Mogens Fosgerau. Workers' marginal costs of commuting. *Journal of Urban Economics*, 65(1):38–47, 2009.

Yves Zenou. Search, migration, and urban land use: The case of transportation policies. *Journal of Development Economics*, 96(2):174–187, 2011.

INTERNET APPENDIX

For Online Publication

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

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

(continues next page)

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 A5 and A6 provide more detail.

Figure A1: 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 A8 shows the neighborhoods in Porto that compose the city-center and city-periphery, while Figure A6 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

Panel B

46

Figure A2: Tourism in Portugal

Notes: This figure reports the number of tourist visitors in Portugal starting in 2006. Numbers are reported in millions.
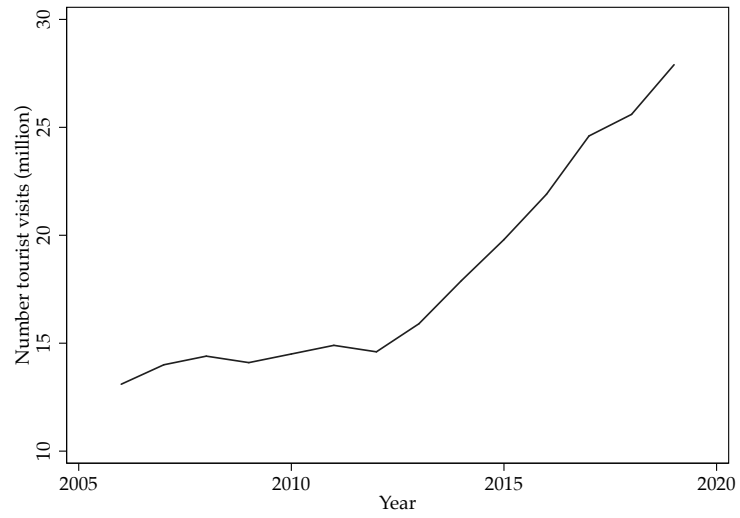
Figure A3: Short-term rentals in Portugal

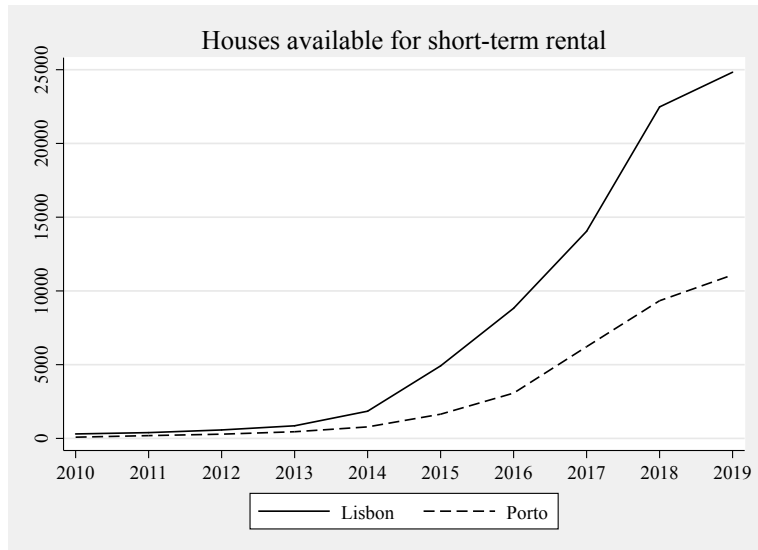*Notes:* This figure depicts the number of houses in Lisbon and Porto that were allocated to short-term rental.

Figure A4: 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.
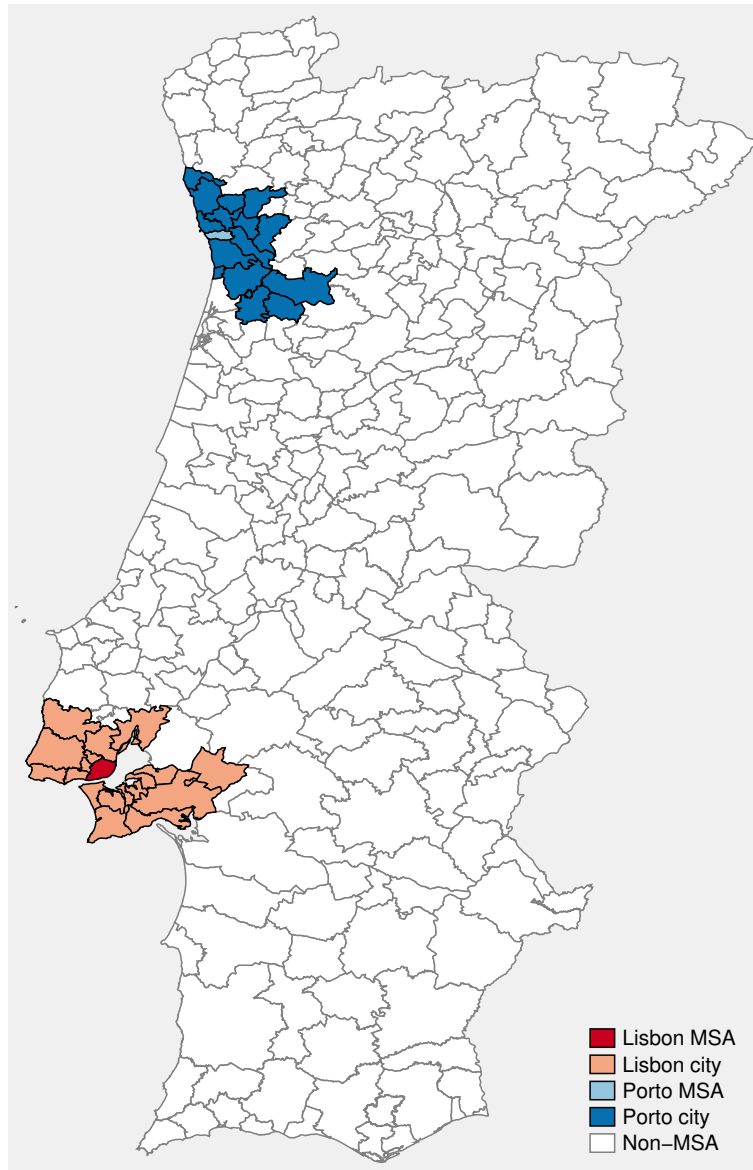
Figure A5: Lisbon metropolitan area

Notes: The picture below shows Lisbon metropolitan area. Any of these regions, except Lisbon municipality, is considered an outskirt/periphery of Lisbon.
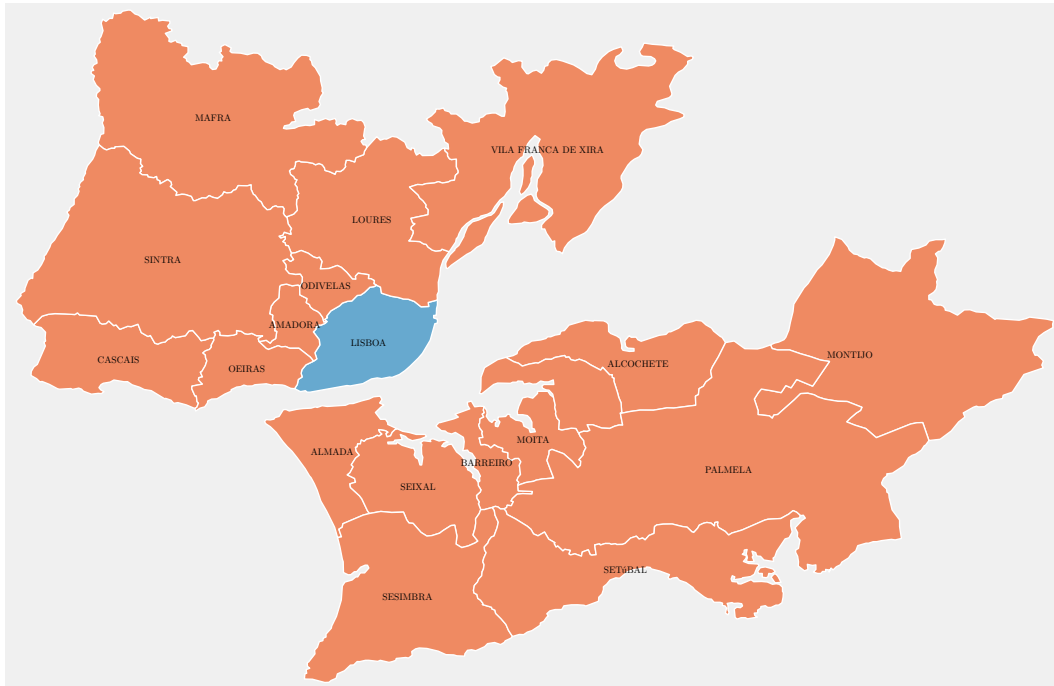
Figure A6: Porto metropolitan area

Notes: The picture below shows Porto metropolitan area. Any of these regions, except Porto municipality, is considered an out-skirt/periphery of Porto.
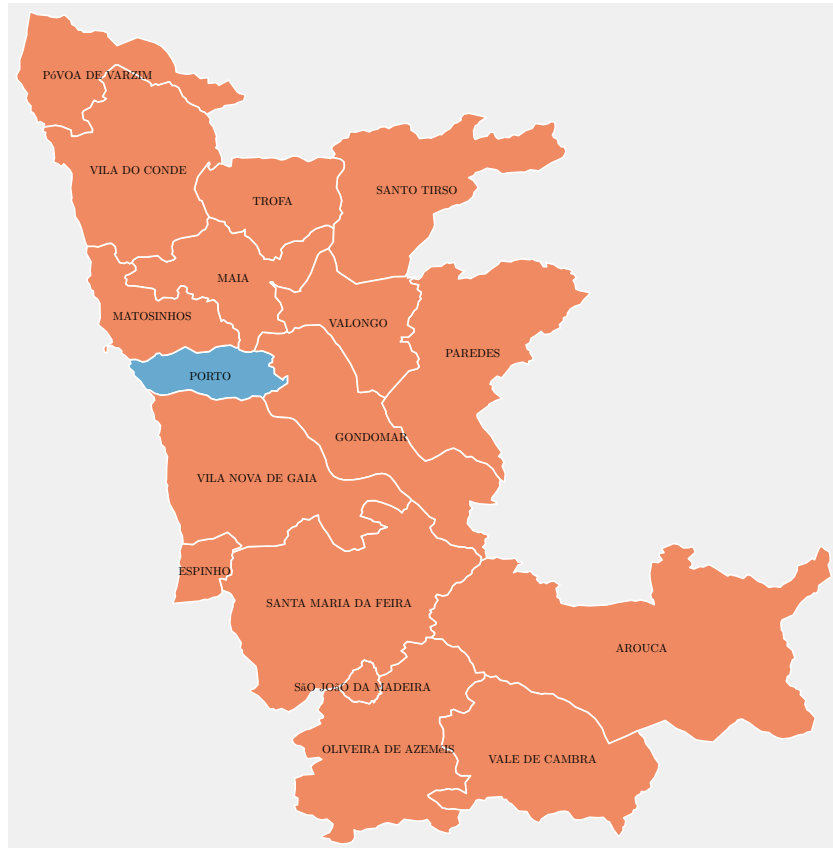
Figure A7: Lisbon Freguesias (neighborhoods)

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

Figure A8: Porto Freguesias (neighborhoods)

Notes: The picture below depicts all *freguesias* (neighborhoods) inside the municipality of Porto.
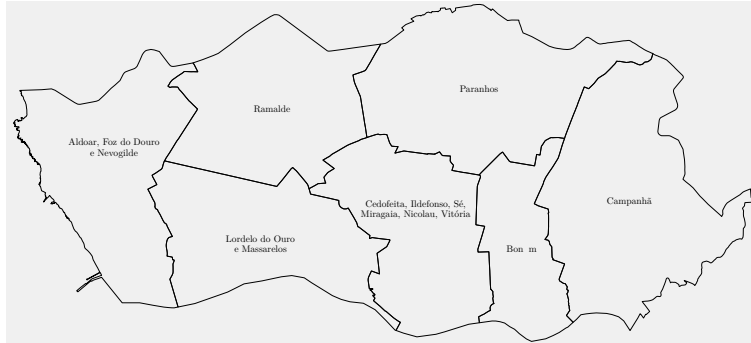
Table A1: Effects on earnings by gender

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 A2: 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