Affordable Housing and Talent Retention*

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Abstract

Contrary to conventional wisdom, we find that affordable housing policies have limited effects on misallocation of human capital, instead they help firms retain young talent. After an exogenous loss of rent control, young and college-educated workers at previously distressed firms are more likely to move to the city outskirts, switch employers, and accept lower wages. The same shock does not lead older workers at distressed firms and workers at non-distressed firms to switch employers despite moving residence to the outskirts. A framework equating commuting versus firm-switching costs rationalizes these patterns, and differences between commuting modes offer additional support for the model.

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"High housing costs impact the ability for employers to attract and retain talent"

Amy Gowan, CEO, Anne Arundel Economic Development Corp.

1 INTRODUCTION

The last few decades have seen a rise in superstar cities (Gyourko et al., 2013; Van Nieuwerburgh and Weill, 2010) accompanied by a stark decline in housing affordability (Pew Research, 2022). As a result, many households have moved to urban peripheries, where local wages are lower and commutes to city-center jobs are longer. Policymakers have in turn responded by proposing affordable housing policies — rent control, subsidized housing, etc. — whose goals are to reduce the cost of living and reduce renter displacement. While such goals are well-intended, existing literature suggests that these policies may backfire by distorting labor markets (Favilukis et al., 2022; Glaeser and Luttmer, 2003). We offer an alternative view: affordable housing may help firms in high-cost-of-living areas preserving firm-worker-specific human capital and retain young talent with high expected future productivity.

Exploiting merged employee-employer-housing data, we examine the impact of losing access to low-cost housing on residential migration and human capital allocation across firms. Leveraging a quasi-experimental setting in Portugal, we first show that the loss of rent control, which offers highly discounted housing, leads to increased residential mobility from the city center to the outskirts.[1] This migration, in turn, results in job reallocation across firms, with workers more likely to switch to firms toward the city outskirts and experience a significant wage decline. We then examine whether patterns of job reallocation vary across firm and worker types. The shift to outskirts firms is unique to young, college-educated workers at previously distressed firms, implying that absent affordable housing, talent would have left these firms around the distress event (Baghai et al., 2021). Surprisingly, after losing rent control, older workers in previously distressed

[1] Rent-controlled units charge, on average, 30% of market rents. Further details on the rent distributions for rent-controlled and free-market units are provided in Section 3.2.

firms and workers in non-distressed firms do not switch employers despite moving their residence to the city outskirts. If affordable housing were generating substantial job misallocation, we would expect young, non-college-educated workers at financially healthy firms to exhibit the largest job reallocation, since they are the cheapest to replace. Therefore, in contrast with existing literature, our findings therefore suggest that affordable housing may actually *improve* the allocation of human capital across firms.

To rationalize our results, we first present a two-period, two-location model of urban commuting. We use the model to consider how housing shocks affect workers residential and employment choices. We first show that a shock to housing costs displaces workers to the outskirts of the city. However, when commuting is costly, the desire to switch neighborhoods creates a gravity effect in the labor market that also displaces the worker from their original firm.[2] We then show that stronger distress signals from their original employer—e.g., a recent mass layoff event—exacerbate the labor market response to the housing shock: since the expected value of remaining at a distressed firm is low, their effective commuting costs increase, which increases the likelihood of a job switch.[3] Alternatively, we show that high firm switching costs—e.g., for older workers—dampens the labor market effect: even though they are more likely to live in the outskirts after the shock, the high cost of switching firms lowers their effective commuting cost, leading to a weaker labor market response.

Empirically, it is challenging to identify the reduced-form effect of increasing housing costs on labor outcomes: individuals experiencing increases in housing costs likely differ from those who do not. To address this challenge, we exploit a change in rent control laws in Portugal. In August 2012, in an effort to reform the rental market, the newly elected Portuguese government abolished almost all existing rent control protections. Prior to 2012, rental contracts signed before 1990 were under rent control. The death of the original rent-controlled tenant could lead to the loss

[2] If commuting between jobs is costless, the two decisions are made in isolation, and there is no gravity effect.

[3] It is reasonable that employees assume that past distress might lead to future distress since there is large evidence of recidivism in many countries, including Portugal (Altman, 2013; Bonfim et al., 2012).

of rent protections for surviving co-residents, but several legal exceptions increased the likelihood of continued rent control.[4] However, since 2012, only renters aged 65 or older with contracts dating from before 1990—hereafter, *legacy renters*—are under rent control. Legacy renters can retain their rent-controlled units until they pass away, and the law prevents landlords from resetting rents to market prices.[5] We exploit this law change in two different ways.

First, we examine how the quasi-exogenous death of a legacy renter affects the career paths of co-residents under 65 who share the same dwelling—their age distribution is similar to that of the Portuguese workforce.[6] More concretely, we first match legacy renters by neighborhood, gender, age, and health status. We then exploit deaths of legacy renters and estimate a differences-in-differences model, comparing the career trajectories of workers living with these legacy renters. We assume that after matching legacy by neighborhood, gender, age, and health status, the death is exogenous to labor outcomes of co-residents. A simple balancing test confirms that, on a wide range of observables (e.g., age, educational attainment, pre-shock rent, and pre-shock income), workers living with a legacy renter who dies are identical to those living with a surviving legacy renter. When a legacy renter passes away, co-residents lose access to rent control.[7] They may negotiate with the landlord, but rents can be updated to market prices.

Second, to strengthen the link between a legacy renter's death and the loss of rent control, we split the above difference-in-differences analysis into two groups: legacy renters who died in 2010-2012 and those who died in 2013-2019. Deaths before the law change involved greater protections to surviving co-residents, and occurred during a period when Portugal's housing market was relatively affordable. Therefore, we expect delayed effects in residential mobility and labor market

[4]The pre-2012 law allowed for certain family members, such as spouses or long-term cohabitants, to keep rent control provided they met specific residency and eligibility criteria.

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

[6]In Lisbon and Porto, approximately 20 percent of workers under 65 live with a renter aged 65 or older. Culturally, it is common for children and grandchildren to live with their parents until a later age. Section 5.1.1 reports summary statistics of our sample and compares them with the population of Portugal.

[7]There is an exception if the legacy renter is survived by a spouse aged 65 or older. In our empirical analysis, we account for this by separately examining deaths with a surviving legacy renter and deaths without one.

effects in this group.[8] If the pre-2013 analysis yields limited effects on residential mobility and labor market outcomes, it supports the assumption that the impact of legacy-renter deaths operates primarily through the loss of rent control rather than through alternative channels (e.g., grief).

Our dataset merges housing and employer-employee linked data from Portugal. These data allow us to measure residence and workplace locations, salaries, hours worked, unemployment spells, employer characteristics, rental contract information, and demographic data. We also complement these data with death records. We focus on residents in the two major municipalities in Portugal: Lisbon and Porto. Our panel starts in 2010 and ends in 2019, 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 their residence from the municipalities of Lisbon and Porto to the outskirts of their greater metropolitan areas. The likelihood of moving toward the outskirts increases by over 11.5 percentage points in the four years after losing access to rent control. This is an increase of over 70 percent relative to the unconditional mean. An event-plot analysis offers a test of the parallel-trends assumption and shows that this effect persists over time—workers in our sample do not return to the city center. We also show that the DiD estimation using deaths occurring between 2010 and 2012 yields limited and delayed effects on mobility. In contrast, a death occurring after the 2012 law change leads to a significant and immediate effect on mobility toward the outskirts. Next, we show that workers who lose access to rent-controlled housing are more likely to switch to firms in the outskirts and experience an earnings decline of nearly 2 percent.

We then investigate whether worker mobility in our setting is sensitive to the financial health of the firm (Baghai et al., 2021). We test whether the effects of losing affordable housing on firm reallocation differ between workers at firms with and without a history of financial distress. We classify a firm as financially distressed if it previously experienced a mass layoff event.[9] Among employees at financially healthy firms, the loss of rent control has no effect on job reallocation.

[8] See Figures A.1 and A.2 reporting the affordability index in Lisbon and Porto.
[9] Additional details are provided in Section 5.

In contrast, for workers at financially distressed firms, the probability of switching to a firm in the outskirts increases by nearly 5 percentage points per year, followed by an earnings decline of about 4 percent.[10] Yet residential moves to the outskirts are equally likely for workers at both distressed and non-distressed firms. Employees at non-distressed firms absorb the housing shock by engaging in longer commutes, whereas those at distressed firms are more likely to transition to lower-paying employers in the outskirts.

The evidence on firm type indicates that affordable housing may serve as a form of insurance for firms during downturns. However, because affordable housing policies may also misallocate workers (Favilukis et al., 2023; Glaeser and Luttmer, 2003), we examine more closely whether they, in fact, help firms retain talent. We study how losing access to low-cost housing affects labor outcomes by skill and age. First, the effects vary sharply by age: while younger and older workers are equally likely to move residence to the city outskirts, their labor-market responses are very different. Workers younger than 40 are nearly 6 percentage points more likely to switch to firms on the outskirts and experience an earnings decline of about 6%. By contrast, workers older than 40 are no more likely to switch to firms in outskirts relative to comparable workers who retain affordable housing. Second, turning to skill, among college-educated workers employed at firms with a history of financial distress, losing access to rent-controlled housing increases the likelihood of switching to outskirts jobs by 10.9 percentage points and reduces wages by 7.14%. Thus, affordable housing can help financially distressed firms retain workers with high expected future productivity.

These findings raise a natural question: why do not formerly distressed firms increase wages to retain these young, high-skilled workers? Because rents in rent-controlled units are, on average, 70% lower than market rents, firms would need to raise wages substantially to enable these workers to live in the city center. Such an increase in wages to compensate for the increase in housing costs is

[10] We note that this analysis only compares workers employed at firms with a history of financial distress. Although employees at financially distressed firms may be subject to selection, restricting the comparison to workers within the same firm type holds this selection constant.

likely unattainable for these firms. The results also highlight the trade-off young, high-skill workers face between firm-switching and commuting costs (Amior, 2024; Fox, 2010). Since young, high-skilled workers at non-distressed firms do not switch employers, the evidence highlights the role of firm distress in lowering switching costs and driving workers out, consistent with the literature (Baghai et al., 2021). Overall, the results underscore the insurance role of affordable housing in preserving firm-worker match-specific human capital during economic downturns.

We further explore the trade-off between firm-switching costs and commuting costs by utilizing detailed data on workers' modes of transportation for commuting. Specifically, we divide the sample of workers in distressed firms into two sub-samples: those who commute via public transit and those who use their personal vehicles. In this exercise, we hold constant firm-switching costs while varying commuting costs. Notably, the entire observed effect on firm reallocation in our sample is driven exclusively by commuters using public transit. There is no difference in firm reallocation between treatment and control groups among those who commute by personal vehicle. These results underscore the importance of commuting costs in determining how the loss of affordable housing impacts reallocation of human capital.

We also conduct several robustness tests. We leverage the timing of firm financial distress to strengthen our findings. Specifically, the results are entirely driven by distress events that occur before the housing shock. The findings are unchanged when excluding firms that eventually liquidate, and there is no significant difference in unemployment rates between treated and control workers. To assess the impact of distress during the Great Recession, we focus on firms distressed in 2010-2012.[11] We further distinguish between deaths of legacy renters in 2010-2012 and those after 2013. We find no labor market effects when workers lost access to affordable housing in 2010-2012. By contrast, for workers whose firms experienced distress in 2010-2012 and who subsequently lost affordable housing after 2013, the likelihood of relocating to firms in the outskirts rises by nearly 7 percentage points per year, accompanied by a 4 percent reduction in earnings.

[11] Approximately 45% of distressed firms in our sample experienced distress between 2010 and 2012.

Our work shows that affordable housing helps firms retain high-skill young talent during downturns. It also offers additional evidence on the implications of rent control. This benefit does not imply an endorsement of rent control. This affordable housing policy entails substantial negative externalities: a large literature documents the costs of rent control, including reduced housing supply and quality, negative house-price spillovers, weak wealth transfers, and lower 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).

Related Literature. This paper helps us better understand how financial distress affects labor reallocation. Prior research documents large and persistent income losses for employees after displacement (Couch and Placzek, 2010; Jacobson et al., 1993; Raposo et al., 2021), including when firms file for bankruptcy (Graham et al., 2023). Employees also reduce the quantity of job applications to distressed firms (Brown and Matsa, 2016). Workers often anticipate distress: high-skill employees tend to leave before bankruptcy (Baghai et al., 2021), and even early negative credit events trigger increased networking activity (Gortmaker et al., 2022). Labor flows may predict firm prospects, well before investors learn this information (Agrawal et al., 2021). Some employees even exit the labor force to start businesses in response to corporate distress (Hacamo and Kleiner, 2022). This paper is the first to add housing considerations to this literature. We show that affordable housing can determine whether young employees remain in financially distressed firms. In contrast, for financially healthy firms, employees are willing to engage in longer commutes, reducing the importance of nearby low-cost housing.

Our paper also contributes to the literature on the labor market effects of housing affordability policies.[12] 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), while miscallocating workers across geographies (Favilukis et al., 2023). The introduction of market-rate housing in low-income areas increase in-

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

migration from low-income areas (Asquith et al., 2023). Public housing has mixed effects, often negatively influencing labor outcomes unless it helps individuals move 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). Contrary to conventional wisdom, our work documents that affordable housing has limited effects on job misallocation, instead helps distressed firms retain young talent with high expected future productivity.

Lastly, our paper adds 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, 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 shows the importance of the trade-off between commuting and firm-switching costs. Employees might be willing to engage in large commuting costs when faced with significantly larger firm-switching costs.

2 THEORETICAL FRAMEWORK

We first present a simple theory of worker mobility and commuting in the face of housing shocks and firm distress at their place of employment.

Setup There are two locations reflecting the outskirts (os) and the city center (cc). Each location can be used as a residential neighborhood (indexed by n) and a labor market (indexed by ℓ). There are two time periods. In period 1, workers are born into a neighborhood and labor market, (n_1, ℓ_1), where they are randomly assigned to a firm. Workers choose a (potentially) new neighborhood-labor market pair, (n_2, ℓ_2), in period 1. If they choose the same labor market, $\ell_2 = \ell_1$, we allow the worker to switch firms. If they change labor markets, they are randomly assigned to another firm in that labor market. In period 2, the worker consumes goods (c) and housing (h) given their location and employment choices from period 1.

Workers are also born in period 1 with two exogenous signals. Their first signal, ρ, provides information about expected (log) rents ($\log r_{n_1}(\rho) \equiv \mathbb{E}_{n_1}[\log R|\rho]$) in period 2, but only within their original residential neighborhood. In their non-origin housing market, we assume that all workers have identical beliefs over rents, so that expected (log) rents are simply $\log r_n \equiv \mathbb{E}_n[\log R]$. We assume that a higher draw of ρ implies a rightward shift in their beliefs about their future rent. For example, ρ could reflect the likelihood that a worker will lose rent control in the future (e.g. due to the likelihood of the death of a legacy renter). We therefore interpret higher draws of ρ as a housing shock.

Their second signal, δ, provides information about the distress of their employer. A high draw of δ reflects a high likelihood of future distress, which affects their beliefs about future wages.[13] If the worker remains at their original employer, their expected period 2 (log) wage is $\log w_{\ell_1}(\delta) \equiv \mathbb{E}_{\ell_1}[\log W|\delta]$, which we assume is decreasing in δ. Workers are allowed to switch jobs, but incur a utility cost described below. If the worker switches labor markets, $\ell_2 \neq \ell_1$, or switches jobs within their original labor market, then expected future (log) wages are identical across workers ($\log w_{\ell_2} \equiv \mathbb{E}_{\ell_2}[\log W]$). The interpretation is that working for a firm provides signals that employees can use to update their beliefs about future earnings, but such signals are unavailable when moving to a new employer.

[13] It is reasonable for employees to expect that past distress may foreshadow future distress, given substantial evidence of recidivism across many countries, including Portugal (Altman, 2013; Bonfim et al., 2012).

Period 2 Problem Consider a worker with realized rental rate R and wage rate W in period 2. The worker's goods-housing consumption decision problem is

$$V(R,W) = \max_{c,h} \quad A + (1-\alpha)\log c + \alpha \log h$$
$$\text{s.t.} \quad c + Rh \leq W$$

where A is a normalizing constant and we treat the freely traded consumption good price as the numeraire. The solution to this optimization problem is standard and implies

$$V(R,W) = \log W - \alpha \log R.$$

Period 1 Problem Given the solution to the consumption decision problem, we can analyze workers' neighborhood and employment location decisions. We make three additional assumptions. First, if the worker moves neighborhoods, they incur a moving cost, $\tau^M > 0$. Second, as mentioned above, if they switch firms they occur an additional switching cost, $\tau^S > 0$. We let $x \in \{0,1\}$ denote their switching decision, where $x = 0$ only if $\ell_2 = \ell_1$ and they do not switch firms within ℓ_1. Third, workers experience an idiosyncratic preference shocks over possible neighborhood-labor-market pairs, $\varepsilon_{n\ell} \sim \text{Gumbel}(0,1)$.[14] Then the problem for a worker with state $S = (n_1, \ell_1, \rho, \delta)$ is

$$\max_{(n,\ell,x)} \underbrace{\mathbb{E}_{n\ell}[V(R,W)|S]}_{\text{consumption value}} + \underbrace{\log B_{n\ell}}_{\text{amenities}} - \underbrace{\log \tau^M \mathbf{1}\{n \neq n_1\} - \log \tau^S \mathbf{1}\{x = 1\}}_{\text{moving and switching costs}} + \underbrace{\varepsilon_{n\ell}}_{\substack{\text{idiosyncratic} \\ \text{preference}}}$$

The worker's expected consumption value takes into account their private signals as well as their original residential neighborhood and labor market. For simplicity, we assume that amenities take a specific form: $B_{n\ell} = \beta_n / \chi_{n\ell}$, where $\chi_{n\ell} \geq 1$ can be interpreted as commuting costs.[15] As such,

[14] It is trivial to add a scale parameter to this distribution and have the results go through. Omitting it saves notation while not adding any additional results.

[15] We could also add a labor market component to the amenities as well. However, since we do not focus on job amenities, this would create additional notational clutter but not additional insights.

we assume $\chi_{nn} = 1$ for all n, and set $\chi_{n\ell} = \chi_{\ell n} = \chi > 1$ when $n \neq \ell$. Conditional on S, the share of workers choosing to live in n and work in ℓ is

$$\phi_{n\ell}(S) = \frac{B_{n\ell}\widetilde{w}_{\ell}(S)\widetilde{r}_{n}^{-\alpha}(S)}{\sum_{n',\ell' \in \mathcal{N}} B_{n'\ell'}\widetilde{w}_{\ell'}(S)\widetilde{r}_{n'}^{-\alpha}(S)} \tag{1}$$

In this equation, we use shorthand expressions for the moving-cost-adjusted expected rent ($\widetilde{r}_n(S)$) and the switching-cost-adjusted expected wages ($\widetilde{w}_\ell(S)$):[16]

$$\widetilde{w}_{\ell}(S) = \begin{cases} w_\ell(\delta) & \text{if } \ell = \ell_1 \text{ and } \delta \leq \delta_\ell^* \\ w_\ell/\tau^S & \text{otherwise} \end{cases}, \qquad \widetilde{r}_{n}(S) = \begin{cases} r_n(\rho) & \text{if } n = n_1 \\ r_n/\tau^M & \text{otherwise} \end{cases} \tag{2}$$

Note that the worker would always choose to switch firms within their original labor market when the expected wage difference is larger than the cost of switching: $\tau_S < \mathbb{E}_\ell[\log W|\delta] - \mathbb{E}_\ell[\log W]$. Therefore, there exists a threshold δ_ℓ^* such that if the worker's distress signal is low enough, they remain at their original firm: $x(\ell, \delta) = 0$ if $\delta \leq \underline{\delta}_\ell^*$. We incorporate this threshold into our notation in (2).

We now turn to our analysis. In the exercises that follow, we will consider workers that are born living and working in the city center: $n_1 = \ell_1 = \text{cc}$. We then analyze how signals of firm distress and housing shocks affect their mobility and labor market outcomes.

2.1 Housing Shocks and Neighborhood Mobility

We begin by verifying that housing shocks in the model predict higher rates of renter mobility. To see this, we can write the likelihood of moving to the outskirts relative to staying in the city center, $\Phi^{\text{live}}_{\text{cc}\to\text{os}}$, as

[16] The unadjusted expected rent and wages are defined in the setup of the model earlier in this section.

$$\Phi^{\text{live}}_{\text{cc}\to\text{os}} = \frac{\sum_\ell \phi_{\text{os},\ell}(S)}{\sum_\ell \phi_{\text{cc},\ell}(S)} = \underbrace{\left(\frac{1}{\tau^M}\right)}_{\text{moving cost}} \times \underbrace{\left(\frac{r_{\text{cc}}(\rho)}{r_{\text{os}}}\right)^{\alpha}}_{\text{relative rent}} \times \underbrace{\left(\frac{\beta_{\text{os}}}{\beta_{\text{cc}}}\right)}_{\substack{\text{relative}\\ \text{amenities}}} \times \underbrace{\left(\frac{w_{\text{os}} + w_{\text{cc}}(\delta)/\chi}{w_{\text{os}}/\chi + w_{\text{cc}}(\delta)}\right)}_{\substack{\text{relative labor market}\\ \text{access value}}}$$

The relative likelihood of a residential move therefore depends on four components. First, it depends on the size of moving costs. If moving costs are high, this dampens residential mobility. Second, it depends on the relative rent between the two locations. Third, it depends on how attractive each of the locations are as a residential neighborhood. Fourth, it depends on the relative accessibility to labor markets. Moving to the outskirts is less attractive if local wages are discounted heavily compared to the city center, but only when commuting costs are large. These insights will play a role in our analysis of labor market decisions in the following sections.

The second component captures the mechanism we are interested in. In particular, when a worker receives a negative signal about housing costs in the city center ($\rho \uparrow$), their anticipated rent in the following period increases ($r_{\text{cc}}(\rho) \uparrow$). This reduces the attractiveness of the city center as a place to live, which incentivizes the worker to move to the outskirts. This gives us our first testable prediction.

Prediction #1: *In response to a housing shock ($\rho \uparrow$), the relative likelihood of moving from the city center to the outskirts increases ($\Phi^{live}_{cc\to os} \uparrow$).*

2.2 Housing Shocks and Labor Mobility

Housing shocks displace renters. Is there a corresponding response across labor markets as well? To analyze this, we can write the likelihood of switching jobs to the outskirts relative to maintaining a job in the city center, $\Phi^{\text{work}}_{\text{cc}\to\text{os}}$, as

$$\Phi^{\text{work}}_{\text{cc}\to\text{os}} \equiv \frac{\sum_n \phi_{n,\text{os}}(S)}{\sum_n \phi_{n,\text{cc}}(S)} = \underbrace{\left(\frac{1}{\tau^S}\right)}_{\text{switching cost}} \times \underbrace{\left(\frac{w_{\text{os}}}{w_{\text{cc}}(\delta)}\right)}_{\text{relative wage}} \times \underbrace{\left(\frac{\beta_{\text{os}} r_{\text{os}}^{-\alpha} + \beta_{\text{cc}} r_{\text{cc}}^{-\alpha}(\rho)/\chi}{\beta_{\text{os}} r_{\text{os}}^{-\alpha}/\chi + \beta_{\text{cc}} r_{\text{cc}}^{-\alpha}(\rho)}\right)}_{\text{relative residential access value}}$$

where we have made the assumption that $\delta \leq \delta^*_{cc}$ so that, in line with our empirical results, the worker does not choose to switch firms within the city center labor market.[17] As with residential mobility, the relative likelihood of job switching to the outskirts has three components. First, it depends on the size of job switching costs. Large switching costs mute the incentive to change firms, effectively dampening any disparties in wages or commuting costs. Second, it depends on the relative expected wage across the two labor markets. If expected wages in the city center decline relative to the outskirts, we should expect more firm and labor market switching.

Finally, the likelihood of firm/labor market switching depends on the relative residential access value across locations. When (amenity-adjusted) rents in the city center are not too large ($r_{cc}(\rho) \approx r_{os}$), workers are more likely to live in the city center and, therefore, assign a penalty to working in the outskirts due to the presence of commuting. In contrast, when city center rents rise, the worker is more likely to be displaced to the outskirts. They therefore put more value on working locally. We can see this formally by studying how $\Phi^{work}_{cc \to os}$ responds to a small housing shock, $d\rho > 0$:

$$\frac{\partial \Phi^{work}_{cc \to os}}{\partial \rho} = \frac{1}{\tau^S} \left(\frac{w_{os}}{w_{cc}(\delta)} \right) \left(\frac{\beta_{os}\beta_{cc}(1-\chi^{-2})}{(\beta_{os} r_{os}^{-\alpha}/\chi + \beta_{cc} r_{cc}^{-\alpha}(\rho))^2} \right) \left(- \frac{\partial r_{cc}^{-\alpha}(\rho)}{\partial \rho} \right) > 0. \tag{3}$$

Note that the expression (3) is only positive when commuting is costly ($\chi > 1$). This implies that housing decisions interact with labor market decisions through a gravity-like relationship. This gives us our next set of testable predictions.

Prediction #2: *In response to a housing shock ($\rho \uparrow$), the relative likelihood of switching to a firm in the outskirts increases ($\Phi^{work}_{cc \to os} \uparrow$), but only when commuting is costly ($\chi > 1$).*

The relationship between housing shocks and firm switching has implications for average wages when there are wage disparities between locations. In particular, note that the average second

[17] In particular, we show in Appendix Table A.1 that housing shocks do not lead to firm switching *within* labor markets. See Section 6.2 for details.

period wage for a type S individual, $\overline{w}(S)$, can be written

$$\begin{aligned}\overline{w}(S) &= \Pr(\ell_2 = \text{os}|S)\overline{w}_{\text{cc}}(S) + \Pr(\ell_2 = \text{cc}|S)\overline{w}_{\text{os}}(S) \\ &= \overline{w}_{\text{cc}}(S) + \Big[\overline{w}_{\text{os}}(S) - \overline{w}_{\text{cc}}(S)\Big]\Pr(\ell_2 = \text{os}|S).\end{aligned}$$

Since housing shocks do not affect expected wages, they should only affect average wages through labor market reallocation. Since $\Pr(\ell_2 = \text{os}|S)$ increases with the size of the housing shock ρ for our representative city center dweller, their average wage will decline as long as wages in the outskirts are smaller than their expected wage in the city center. This gives us our third testable prediction.

Prediction #3: *Suppose (unconditional) average wages are always lower in the outskirts than in the city center ($\overline{w}_{os} < \min_\delta \overline{w}_{cc}(\delta)$). In response to a housing shock ($\rho \uparrow$), average wages of workers that initially live and work in the city center will decline ($\overline{w}(S) \downarrow$).*

2.3 What Drives the Labor Mobility Response to Housing Shocks?

We have now established that housing shocks displace both renters and workers and that the joint effect is connected through costly commuting. As a result, wages of these workers decline on average, and the effect is larger when the displacement is stronger. We now ask how other factors affect this response.

First, consider the interaction between housing shocks and the distress of the worker's original firm. When the gap in expected wages becomes small (due to a high distress signal), workers are more likely to switch to a job in the outskirts. In the extreme case that $w_{\text{cc}}(\delta) \to w_{\text{os}}$, both residential and labor market decisions will only depend on neighborhood characteristics. Therefore, the effects of housing shocks are exacerbated by financial distress. We can see this by revisiting the labor market response to housing shocks in (3): when distress increases ($\delta \uparrow$), expected city center wages decline ($w_{\text{cc}}(\delta) \downarrow$), which reinforces the response to the housing shock ($\partial\Phi^{\text{work}}_{\text{cc}\to\text{os}}/\partial\rho \uparrow$).

Similar mechanisms hold when we consider other characteristics such as switching costs (τ^S)

or commuting costs (χ). When switching costs are high, workers are generally less likely to change labor markets. Therefore, this dampens the effect of housing shocks. Conversely, when commuting costs increase, housing shocks are more likely to displace workers to new firms since their incentive to work locally is stronger. This intuition provides us with our final set of empirical predictions.

Prediction #4: *The labor market response to a housing shock* ($\partial \Phi^{work}_{cc \to os} / \partial \rho$) *is* ($i$) *increasing in the the firm distress signal* (δ), (ii) *increasing in commuting costs* (χ), (iii) *negligible when commuting costs are small* ($\chi \approx 0$), *and* (iv) *decreasing in switching costs. Moreover, the added effects of higher commuting costs and switching costs are more pronounced when firm distress is high.*

3 THE HOUSING MARKET IN PORTUGAL

This section offers an overview of the housing market in Portugal. We start with a brief summary of rental laws in Portugal, emphasizing the most recent law changes, especially those affecting renters older than 65.[18]

3.1 Rent Control Laws in Portugal

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.[19] A few years later, during 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.[20] 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 and age, while in 1948, a tax-assessed property value was used to determine rent price updates.[21] Almost two decades later, in 1966, the

[18] 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*.
[19] See for example *Decreto de 12 de Novembro de 1910*.
[20] Landlords were also obligated to rent any empty apartment or building.
[21] 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*.

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.[22] This was a major change in the Portuguese rental market with repercussions for many years.

Additional pro-tenant reforms were signed into law after the establishment of the first democratic government following the 1974 Carnation Revolution.[23] 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 were 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.[24]

With this backdrop, new legislation was introduced in 1990 that aimed to relax some pro-tenant protections and revitalize the rental market. In particular, the legislation removed legal restrictions on lease contracts with a limited term, 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 the date of the legislation, which effectively exempted older tenants and created the existence of legacy contracts.

[22] 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*.

[23] 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.

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

Post 2006, rent control did not end after the death of a renter who singed the original contract if one of the following co-residents survived: spouse (living in the dwelling), de-facto partner (living in the dwelling), parent living with the tenant >1 year, child/step-child who was (i) a minor, or (ii) under 26 and in secondary/higher education, or (iii) an adult child with ≥60% disability (cohabiting >1 year). Priority followed the article's order, and children could also succeed later after a spouse's subsequent death. These were the protected "surviving co-residents" for pre-1990 leases under the 2006 regime.[25]

The next major reform occurred in August 2012, shortly after the election of a pro-business government in June 2011.[26] This reform aimed to fully liberalize rental markets by phasing out most rent control contracts. It allowed landlords to adjust rents to market values within 30 days.[27] Additionally, landlords were allowed to convert indefinite rental contracts into fixed-term contracts with a duration of 2 years. However, this reform did not apply to tenants older than 65 with a contract signed prior to 1990.[28] Consequently, these lease contracts effectively maintained the original protective privileges—i.e., low rents protected by the impossibility of renegotiation or termination of the agreement. This legal status still prevails today.

Landlords are 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, working-age tenant, the landlord can update the rent to market values and convert the indefinite rental contract into a fixed-term contract with a duration of two years.

[25] Additional details are available in the *Lei n.º 6/2006, de 27 de Fevereiro de 2006* .
[26] More details can be found in the *Lei n.º 31/2012, de 14 de Agosto de 2012* .
[27] See Articles 30 and 31 in *Lei n.º 31/2012, de 14 de Agosto de 2012*.
[28] Individuals with at least 60 percent disability with rental contracts signed prior to 1990 were also exempted.

3.2 Rent-control discount

Legacy leases have extremely low rents in current euros. According to the 2011 Census, 71 percent of rent-controlled units have rents under 140 euros, and the average rent in a rent-controlled unit is below 130 euros—well below market levels. For comparison, the average rent in Lisbon in 2011 was 416 euros. Panel A of Figure I shows the distribution of rent levels in metro area of Lisbon. Remarkably, the entire distribution of rents for rent-controlled units lies to the left of the distribution for free-market units. This implies that any renter moving from a rent-controlled unit to a free-market unit—whether in the city center or the outskirts—would face a higher rent. Confirming the steep discount enjoyed by tenants in rent-controlled units, Panel B plots the distribution of the rent discount: the average discount is 31%, with values ranging from about 24% to 47%.

Rent-controlled units in the city center are not smaller than free market units in the outskirts. Panel A of Figure II reports the distribution of floor area (in square meters) per worker living in rent-controlled units. The average floor area per worker in city-center rent-controlled units is slightly larger (69 m^2 per worker) than in free-market units in the city outskirts (67 m^2 per worker). Panels A and B of Figure II further show that the rent-controlled distribution has a longer right tail, indicating that some rent-controlled units are exceptionally large.

3.3 House Prices

House prices in Portugal remained largely stable from 2010 to 2015 but experienced rapid growth between 2015 and 2019. Panel A of Figure A.1 (A.2) depicts the trajectory of house prices in the historical center, non-historical center, inner periphery, and outer periphery of Lisbon (Porto) from 2009-2019. During this period, prices in the historical center nearly doubled; in the non-historical center, house prices increased by approximately 60 percent; and in the periphery, house prices rose by 20 to 30 percent.

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

(Guerreiro et al., 2024). Consistent with this work, Figure A.3 shows that the number of tourists in Portugal remained steady from 2005 to 2013, then more than doubled between 2013 and 2019. Figure A.4 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.

3.4 Affordability

Housing affordability has become a significant concern in Portugal in recent years.[29] 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 Panel B of Figure A.1. The figure clearly illustrates that the ratio of house prices to average wages remained relatively stable between 2010 and 2015, but surged by almost 70 percent between 2015 and 2019, largely driven by relatively slow wage growth ($\approx$ 3.5% per year). Due to 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.

[29] See for example: Beatriz Ramalho da Silva, "Luxury Homes, Short Lets and Shacks: Inside Lisbon's Housing Crisis," The Guardian, December 22, 2021.

4 DATA

Our data are provided by the *Statistics Portugal*. Statistics Portugal combines data from several sources: social security, death records, and 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. Internet Appendix A contains a summary of the main variables with detailed definitions.

4.1 BPR

Our primary dataset is the Portuguese Administrative Census dataset (BPR).[30] This dataset encompasses all individuals residing in Portugal from 2010 through 2020. The data are 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. We proxy for health conditions based on whether an elder receives social benefits for disability.

[30] 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 Portugal.[31] We observe this data for every year between 2010 and 2019. 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. Once we restrict the sample to individuals with wage information in *Quadros de Pessoal*, we obtain our final dataset, which does not include individuals working in the public sector and those who are self-employed.

5 EMPIRICAL DESIGN

Identifying the effect of housing costs on labor outcomes is challenging. For example, households may seek low-cost housing after a negative labor-market shock, whereas those experiencing positive labor-market shocks may opt for more expensive housing. This self-selection makes it difficult to test our hypothesis by simply comparing workers who experience housing-cost shocks with those who do not.

We leverage rent-control laws in Portugal to address these potential endogeneity issues. Renters who are 65 or older with rental contracts signed before 1990—referred to as "legacy renters"—benefit from a rent-control regime that prevents landlords from raising rents to market levels.[32] This special provision for legacy renters remains in effect today. They only lose access to rent-controlled homes upon death. We exploit this fact to test our hypothesis.

[31] For detailed information about *Quadros de Pessoal*, see for example Blanchard and Wolfers (2000).
[32] For more information on rental laws in Portugal, please refer to Section 3.1.

5.1 The death of a *legacy* renter

We investigate how the quasi-exogenous death of a legacy renter influences the career trajectories of co-residents under the age of 65 who share the same dwelling. In Lisbon and Porto, around 20 percent of workers under 65 live with a renter aged 65 or older. It is culturally common for children and grandchildren to remain in their parents' household until later in life. We find that the average age and salary of workers living with a legacy renter are comparable to those of the average Portuguese worker.

We begin by matching legacy renters based on neighborhood, gender, age, and health status. We then exploit the deaths of legacy renters to estimate a differences-in-differences model, comparing the career trajectories of co-resident workers. Rather than assuming that deaths are random after matching, we assume that conditional on neighborhood, gender, age, and health status, the death of a legacy renter is exogenous to the labor outcomes of co-residents. We validate this assumption using balancing tests and parallel trends.

A treated worker meets two criteria: (i) the eldest family member in the household is 65 or older and passes away, and (ii) the rental contract was signed prior to 1990. These criteria lead to the termination of rent control only if the elder is not survived by a household member older than 65 that is listed in the rental contract.[33] Thus, when a legacy renter dies, family members living in the same apartment are likely to lose access to rent control. They have an option to negotiate with the landlord, but rent values may be updated to market prices.

Our control group comprises workers younger than 65 living with a surviving legacy renter. We match legacy renters in the control group to the treatment group based on four conditions: they have similar age, similar health conditions, same gender, and live in the same *freguesia* (neighborhood).[34] We use the receipt of a disability pension by a legacy renter as a proxy for their health

[33] Since we do not have this lease detail, our baseline comprises households with and without surviving family member older than 65—-in additional 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.

[34] Figures A.8 and A.9 depict all freguesias in Lisbon and Porto.

condition and create seven age classes, 65 and above, to group legacy renters of similar ages. Our matching procedure creates *stratums* of matched units. Within a given stratum, there are multiple treatment units that depend on the year in which the legacy renter passes away (e.g., 2012, 2013, etc). We then create a *cohort* group that contains only one treatment unit (e.g., 2013) and all control units within the matched stratum. After applying this procedure, the age difference between a legacy renter who passes away and one who survives is only 0.712 years, while the average age of a legacy renter is 75 years.

Comparing deaths before and after the law change. To further strengthen the link between the death of a legacy renter and the loss of rent control, we estimate the above DiD model for deaths occurring before 2012 and after 2013. Before 2012, rent control laws were more accommodating for surviving co-residents, and the housing market in Portugal was more affordable. Figures A.1 and A.2 show that the unaffordability ratio—measured by the average house price over the average income—only began to rise rapidly after 2014. Prior to that, the ratio remained relatively flat. Thus, a death before 2012 should result in a limited reallocation in either the residential or labor market.

We also examine the dynamics of deaths occurring before 2012. While such events should not have immediate effects, families who lost a legacy renter prior to 2012 were likely to lose rent control after the law change. Therefore, we would expect any impact on residential mobility to be delayed. Since these residential effects are not immediate, any resulting labor market changes may also appear muted and delayed. If the impact of a legacy renter's death on residential and labor outcomes were driven by another factor, such as grief, we would not expect to see a differential effect before and after the law change.

5.1.1 Balancing tests and summary statistics

Table I compares workers in the treatment and control samples across observable variables—including age, income, gender, educational attainment, rent values, and hours worked-measured in 2010. The table reveals no economically or statistically significant differences between the treat-

ment and control workers' demographics or their employers. This is notable because we do not incorporate any worker-level variables in the matching procedure. The table also provides insight into our sample: the average worker is over 41 years old, slightly over 50 percent male, and 22 percent hold a college degree. Among working-aged inhabitants in rent-controlled households, approximately 30 percent are aged 18-35, over 33 percent are 36-50, and 37 percent are 51-64. The average worker in our sample (41 years) is slightly older than the average worker in Portugal (39 years). The age distribution in our sample is similar to that of Portugal: about 58% are aged 18-64, compared with 61% in the total population.[35] Furthermore, the average monthly salary in our sample is approximately 790 euros, compared with 863 euros in the full *Quadros de Pessoal* dataset.[36] The average employer in the sample has approximately 1,100 employees. Thus, we consider our sample representative of the Portuguese labor force.

5.1.2 Regression specification

Our sample is restricted to individuals who live in rent control homes in the municipality of Lisbon and Porto in 2010, and those participating in the labor force with ages between 18 and 64. We estimate the following regression model for worker i in cohort c at year t:

$$\text{Y}_{i,c,t} = \beta_1 \times \text{Post}_{c(i),t} \times \text{Legacy death}_i + \beta_2 \times \text{Post}_{c(i),t} + \alpha_i + \delta_t + \gamma_c + \epsilon_{i,c,t}, \quad (4)$$

where α_i denotes the fixed effect for worker i, δ_t the fixed effect for time t, and γ_c the fixed effect for cohort c. *Legacy death*$_i$ equals 1 if the legacy renter in the household of worker i passes away. $Post_{c(i),t}$ equals one if t is after the death of the legacy renter in worker i's cohort c, and zero otherwise.[37] $\text{Y}_{i,c,t}$ represents various mobility and labor outcomes of individual i at time t. We

[35] See Table 2.16 in *Estatisticas Demograficas 2010* available in the Statistics Portugal website.

[36] We report monthly salaries based on 12-month basis. Some online sources, however, present "monthly" figures on a 14-month basis because Portuguese workers typically receive two additional payments—one in summer and one at Christmas. Reporting averages on a 14-month basis leads to monthly salaries that are 17% larger.

[37] Put differently, $Post_{c(i),t} = \mathbf{1}\{t > D_c\}$, where D_c is the death date for the legacy renter in cohort c. Recall that a cohort group contains only one treatment household and several control households. Please consult Section 5.1 for further details on the construction of cohort groups.

include cohort fixed effects to compare workers within the same cohort, as well as calendar-year and individual fixed effects to account for time trends and any remaining individual time-invariant variation. Errors are robust and clustered at the individual level. Our empirical design only compares a treatment unit with never-treated units due to the cohort fixed-effect. This methodology helps avoid the classic problems of differences-in-differences models (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021).

6 RESULTS

We first examine the impact of losing access to affordable housing on residential mobility and labor market outcomes. We then analyze how this effect differs between firms with and without a history of financial distress. Next, we explore heterogeneity by age and skill. Finally, we investigate whether different modes of transportation mitigate the effects of losing access to low-cost housing on job reallocation.

6.1 Does the loss of affordable housing affect migration to outskirts?

We begin by examining whether the removal of rent control displaces renters toward the outskirts of Lisbon and Porto. To measure residential migration, we classify each location into one of four concentric peripheral rings around the historical city centers of Lisbon and Porto, as illustrated in Figures A.6 and A.7 in the Internet Appendix. Using our empirical framework (4), we define the outcome variable as equal to one if, in a given year, an individual moves away from the city center toward the outskirts, and zero otherwise.

Columns (1) and (2) of Table II show that, following the death of a legacy renter, households are significantly more likely to migrate toward the city outskirts. A household is 2.3 percentage points more likely to move to the outskirts in a given year, following the death of the last legacy renter. This effect is statistically significant at the one percent level. This coefficient implies that a family is 11.5 (=2.3×5) percentage points more likely to move toward the outskirts in the four

years after the passing of the last legacy renter. This effect is unusually large, as it represents over 70 percent of the sample mean.

Column (3) breaks down the treatment effect into two components: the death of the last legacy renter, and the death of a legacy renter who is survived by another household member older than 65. Under the 2012 rent control law, if the household has a member older than 65 that survived, the apartment shall remain under rent control as long as the surviving household member is listed in the rental contract. Consequently, households in this situation are significantly less likely to lose rent control.[38] The estimates confirm that a death of a legacy renter only affects migration to the outskirts when it affects loss of rent control.

We also explore the timing of the death of the legacy renter. Specifically, we compare the DiD point estimates of deaths occurring in 2010-2012 versus those occurring in 2013-2019. Since rent control laws changed in 2012 and the housing markets in Lisbon and Porto were still affordable at that time, as shown in Panel B of Figure A.1 and A.2, we expect limited effects for deaths that occurred prior to 2012. Columns (4) through (7) confirm this conjecture: a death between 2010 and 2012 leads to no significant mobility effect to the outskirts in the first two years following the death. Consistent with a hotter housing market after 2013, households that lost a legacy renter between 2010 and 2012 become more likely to move three years after the death. The delayed effect for deaths between 2010 and 2012 offers strong evidence that the death of a legacy renter affects residential mobility primarily through the loss of rent control. In contrast, a death after 2012 is associated with a 2.8 percentage point increase in residential migration per year over the two years following the death, and a 1.7 percentage point increase per year thereafter.

Lastly, we estimate an event plot diagram, which allow us to detail the breakdown of the effect by each period and, more importantly, test the parallel trends assumption. Figure IV, Panel A,

[38]There is still a positive probability of losing rent control even if the surviving household member is listed on the rent control agreement. Some households were unaware of the details of the new rental laws, leaving them vulnerable to exploitation by abusive landlords. See for example "Assédio a arrendatários punido a partir desta quarta-feira." Expresso, 12 Feb. 2019, or "Rendas em Lisboa. Antes do despejo, vem o 'bullying imobiliário'." Renascenca, 23 Jan. 2018.

shows no parallel trends prior to a legacy death, and 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 gradually move toward the outskirts. Panel B confirms the null effect following the death of a legacy renter survived by a household member older than 65.

6.2 Does the loss of affordable housing affect labor market outcomes?

Firm switching. We then examine the impact of access to affordable housing on labor mobility across firms. Table III, Panel A reports results on firm-to-firm transitions, while Panel B focuses on moves to employers located in the outskirts. Panel A shows that, overall, losing access to rent control does not have a statistically significant effect on the likelihood of changing firms. However, when we break down the results by time period, we observe a sharp increase of nearly 2.4 percentage points in job mobility within two years following the loss of affordable housing. Focusing on those who lost rent control between 2010 and 2012, the effects are consistent with our findings on residential mobility: there is no statistically significant impact on the likelihood of changing jobs. In contrast, individuals who lost rent control after 2013 experienced a 2.7 percentage point increase in job transitions, with this effect occurring almost immediately after the loss.

Panel B shows that the overall effect on labor mobility is largely driven by moves to firms located in the outskirts. Losing access to rent control increases the likelihood of moving to such firms by 1.9 percentage points per year for the average worker in our sample. This contrasts with the insignificant effect observed for overall firm-to-firm changes. The breakdown by treatment year also yields estimates consistent with earlier findings. For deaths occurring between 2010 and 2012, there is no significant short-term effect on the likelihood of moving to a job in the outskirts. Although we observe a delayed effect of 1.46 percentage points, it is not statistically different from zero. In contrast, for those treated between 2013 and 2019, the effect is both large and statistically significant. Similar to residential mobility, these moves to employers in the outskirts occur almost

immediately after the loss of rent control.

Earnings. We next examine the impact of losing access to rent-controlled housing on earnings. Using our baseline regression model, Table IV reports the results for the outcome variable defined as the log of monthly total salary divided by the number of hours worked. The point estimates reveal several patterns consistent with rent control helping alleviate commuting-related frictions for the average individual in the sample. Losing access to a rent-controlled home leads to a significant 1.5 percent decline in earnings. This decline appears to be gradual and typically materializes a few years after the loss. Figure V presents an event study plot showing the effect of a legacy renter's death on earnings. The plot confirms that the parallel trends assumption holds. Panel A reveals a steady decline in earnings following the renter's death; however, this trend may be driven by compositional effects, which we discuss in the next section.

While the primary focus of this paper is the impact of affordable housing on the reallocation of human capital across firms, our findings also underscore the costs of unaffordable housing for workers. The observed decline in earnings is accompanied by rising housing costs that invariably follow moves from rent-controlled units to free-market units, as shown in Figure I. Although we do not conduct a formal welfare analysis, the increased migration to the outskirts—where amenities are likely more limited—suggests that workers' welfare declines following the loss of access to affordable housing in our setting.

Lower wage firms. We disentangle whether the earnings decline reflects transitions to firms that pay lower wages overall. We classify firms into two sub-samples: low-wage (average wage below the sample median) and high-wage (average wage at or above the sample median). Table A.2 in the Internet Appendix shows that, following the loss of rent control, workers are 2.3 percentage points more likely to move to low-wage firms. This suggests that the wage decline that we observe after the housing shock is associated to the types of firms that workers switch to, which is consistent with firms in the outskirts paying lower wages that those in the city center.

6.3 Distress Firms

After demonstrating that residential mobility increases and the likelihood of moving to a firm in the outskirts rises following the loss of rent control, we examine how these effects differ between firms with and without a history of financial distress.

We define distress based on mass layoffs, following the labor literature. A firm is classified as distressed if it laid off more than 30% of its workforce in a given year. Figure III illustrates the distribution of mass layoffs by year. Not surprisingly, approximately 50% of mass layoff events occurred between 2010 and 2012, overlapping with Portugal's deep financial recession. While the majority of our results are estimated using the traditional definition of mass layoff, they remain robust when using alternative layoff thresholds of 40% and 50%.[39]

We first demonstrate that residential mobility to the city outskirts is large for employees at both distressed and non-distressed firms. Table V shows that workers from historically distressed firms experience a 3.3 percentage-point annual increase in the likelihood of moving to the outskirts after losing rent control. Workers in non-distress firms experience a 2.1 percentage-point annual increase in the likelihood of moving to the outskirts after losing access to affordable housing. While the magnitude is larger for employees at distressed firms, both groups exhibit significantly higher mobility following the death of a legacy renter.

Next, we examine reallocation of human capital across firms and the subsequent impact on wages. Workers at historically distressed firms are 4.7 percentage points per year more likely per year to move to firms in the outskirts after losing rent control. In contrast, workers at firms without distress experience only a small, statistically insignificant increase of 0.3 percentage points. Earnings decline by 4 percent for employees at distressed firms after losing rent control. For workers at non-distressed firms, earnings only decline by 0.7 percent and statistically indistinguishable from zero.

Our findings remain robust under multiple alternative definitions of firm distress. Panels A and

[39] Results using alternative layoff thresholds are reported in Internet Appendix Table A.3.

B of Table VI confirm that results hold even when excluding firms that eventually liquidated, alleviating concerns that job reallocation merely anticipates firm closures.[40] Results are also unchanged when focusing only distress events occurring prior to housing shocks. This helps distinguish between employee anticipation of future distress and decisions to remain at firms post-distress. Since our results are driven by instances in which distress precedes housing shocks, they underscore the role of affordable housing in helping distressed firms retain human capital.

Columns (3) and (4) of Panels A and B further clarify that the documented effects primarily occur among households losing rent control after 2013 and working at firms distressed during the Great Recession. For example, losing affordable housing after 2013 increases the likelihood of moving to jobs in the outskirts by 6.6 percentage points per year. In contrast, results for households losing rent control before 2012, though based on a smaller sample, indicate no significant effect, consistent with earlier findings.

Our results show that wage losses associated with losing rent control are concentrated among employees at distressed firms, consistent with our model's predictions. In the absence of distress, firm-switching costs outweigh commuting costs. However, when distress occurs, firm-switching costs decrease, making commuting frictions binding. Consequently, workers losing rent control move to jobs in the outskirts, while those retaining rent-controlled housing remain employed in city-center jobs.

6.4 Does affordable housing help retain young talent?

While the previous section shows that affordable housing leads to increased labor mobility, it is unclear whether it helps retain talent. To shed light on this question, we examine differences between young and older workers, as well as those with and without a college degree. Table VII presents the effects on residential mobility, job reallocation, and wages for workers at distressed firms, split into subsamples of workers younger and older than 40. The first two columns indicate that both

[40] Table A.3 in the Internet Appendix shows that the effects on job reallocation and wage declines are economically larger and statistically significant if we use alternative measures of firm distress.

young and older workers are equally likely to move their residence to the outskirts after losing rent control. However, only young workers significantly reallocate jobs: their likelihood of moving to a job in the outskirts increases by 5.78 percentage points per year, resulting in a wage decline of 5.84 percent. In sharp contrast, older workers do not increase their likelihood of job reallocation to the outskirts and experience no wage losses, showing that they instead opt for longer commutes. Given that older workers typically face higher firm-switching costs, consistent with previous literature (Fox, 2010), these results align with our model's predicted trade-off between commuting and firm-switching costs.

Table VIII reports results by educational attainment. We estimate the effect of losing access to affordable housing on labor outcomes by education and firm type. The effects are concentrated among college-educated workers. After losing rent control, college-educated workers in distressed firms are 10.9 percentage points more likely to move to jobs on the city outskirts than similar workers in non-distressed firms; the corresponding increase for non-college workers is only 2.9 percentage points. Earnings effects mirror this pattern: losing rent control reduces earnings by 7.14% for college-educated workers in distressed firms, compared with 2.99% for non-college workers.

These patterns raise a natural question: why do not previously distressed firms simply raise pay to retain young, high-skill employees? With regulated rents averaging about 70% below market, the wage increase necessary to afford central-city housing would be extremely large for these firms. The evidence also highlights the trade-off these workers face between employer switching and commuting costs. Since young, high-skill employees at non-distressed firms rarely change firms, the departures are driven by distress substantially lowering firm switching costs, consistent with prior work (Baghai et al., 2021). Overall, the findings underscore the role of affordable housing in helping preserve firm-worker-specific human capital, especially during bad times.

6.5 Commuting Costs

We further leverage our commuting data to provide additional evidence for our model highlighting the trade-off between firm-switching and commuting costs.

6.5.1 Commuting in Lisbon and Porto

We begin by describing commuting patterns in Portugal's two major cities, Lisbon and Porto. Over 50% of individuals in these cities commute using personal vehicles, with the remainder relying on public transportation. Commuting by car is significantly faster, requiring approximately half the time compared to public transit—26 minutes versus 44 minutes, respectively, when traveling from the outskirts to the city center.[41]

6.5.2 Labor market effects and modes of transportation

Next, we investigate whether firm reallocation and wage losses following the loss of rent control are primarily concentrated among workers with the highest commuting costs. Table IX presents results for all firms as well as firms experiencing distress. Among all firms, workers commuting via public transit experience a 2.6 percentage-point annual increase in their likelihood of moving to jobs in the outskirts and a wage decline of 2.2 percent after losing rent control. In contrast, workers with personal vehicles neither increase their likelihood of moving jobs nor experience wage declines following the loss of affordable housing.

The pattern is even more pronounced among workers at historically distressed firms. Those commuting by public transit experience a 5-percentage-point annual increase in their likelihood of relocating jobs to the outskirts, coupled with a wage reduction of 4.8 percent. Workers with personal vehicles, however, show no significant increase in job relocation likelihood or wage declines after losing affordable housing.

[41] We estimate these figures using the entire working population in the BPR dataset.

These findings provide additional empirical support for our theoretical framework, underscoring the trade-off between firm-switching and commuting costs. Holding firm-switching costs constant while varying commuting costs, our results show that when commuting via public transit, workers opt to switch jobs to the outskirts despite substantial wage penalties.

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

6.6.1 Labor supply

The effect of the loss of rent control on labor supply remains unclear. A longer commute could result in a reduction in the number of hours worked, while a drop in disposable income might lead to an increase in labor supply. We evaluate this trade off in Table X. The first column 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 distressed firm or not (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.

6.6.2 Unemployment

The results above show that workers in distressed firms are more likely to experience a decline in wages after losing rent control. One natural implication is that they might become unemployed after losing rent control. This could help us better understand the loss in earnings. Therefore, we estimate our baseline regression model replacing the outcome variable with binary variable equal to one if an individual experiences an unemployment spell in the next period.

Columns (4) through (6) in Table X report the estimates of this model. The point estimates are neither statistically or economically significant after a legacy death. 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 consistent with workers who lost rent control

voluntary finding jobs in the city outskirts.

6.7 Additional results

We also tabulate our findings by income, gender, and location. We first 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 by 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. To evaluate these hypotheses, we divide our sample into two income groups based on earnings observed in 2010. We then estimate our baseline regression on the effect of losing access to rent control on mobility and labor outcomes. Table A.4 reports these results.

Interestingly, both high- and low-income workers at distressed firms exhibit an equal likelihood of moving their residence to the outskirts following the loss of affordable housing. However, the effects on labor outcomes differ substantially. Low-income workers experience nearly a 5 percentage-point annual increase in their likelihood of moving to a job in the outskirts, whereas high-income workers show no statistically significant increase. A similar pattern emerges for earnings: low-income employees at distressed firms experience a wage decline exceeding 5 percent, while the earnings impact on high-income workers is economically and statistically insignificant. These findings are consistent with the prior that high-income workers are likely to have lower commuting costs; not only they have larger disposable incomes and low relative commuting costs, but are also more likely to own a personal vehicle. Additionally, we reconcile these results with our prior evidence on skills, since the young workers in our sample are low income despite holding college degrees.

Internet Appendix Table A.5 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, but rather that the effects documented above for each worker are likely to be amplified within a household.

We also compare the prevalence of our findings between Lisbon and Porto in Internet Appendix Table A.6. The table shows that the point estimates for the effect of rent control on firm switching and earnings is similar for workers in both cities. Importantly, in both cities, the decline in wages is concentrated among those working for distressed firms and without a personal vehicle. While the magnitudes are large, some point estimates are not statistically significant due to small sample issues.

7 CONCLUDING REMARKS

In recent decades, rising housing costs in cities across the world have made it harder for firms to retain talent, particularly as employees face increasing commuting costs. In response, some companies have begun investing in affordable housing near their workplace. Our findings suggest that such efforts may be especially valuable during times of distress, when employees are more likely to leave. In this context, affordable housing can act as a form of insurance, helping firms retain human capital.

Using matched employee-employer-housing data and a quasi-exogenous empirical setting in Portugal, we find that, after losing rent control, workers are more likely to relocate to city outskirts and switch to firms also located in the outskirts. These effects are concentrated among young and high-skill employees at firms with a history of financial distress, highlighting how affordable housing can shape the reallocation of human capital during times of firm distress. Surprisingly, workers in healthy firms do not change jobs after losing access to affordable housing, despite engaging in much longer commutes.

Our paper provides policymakers with valuable insights into how affordable housing can positively affect both employees and firms, highlighting the instances when these benefits are more pronounced. Although our identification strategy uses rent control to analyze access to affordable housing, our results should not be viewed as an endorsement of rent control policies, as such policies are linked to substantial negative externalities.

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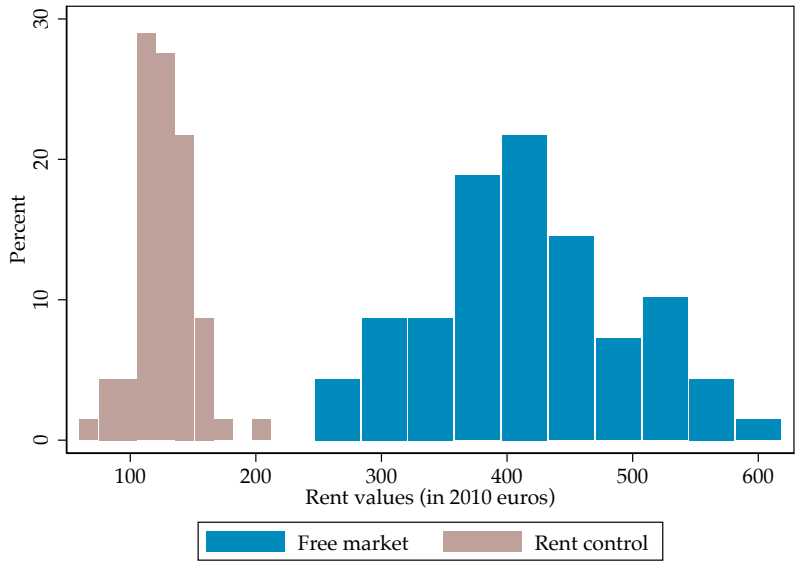
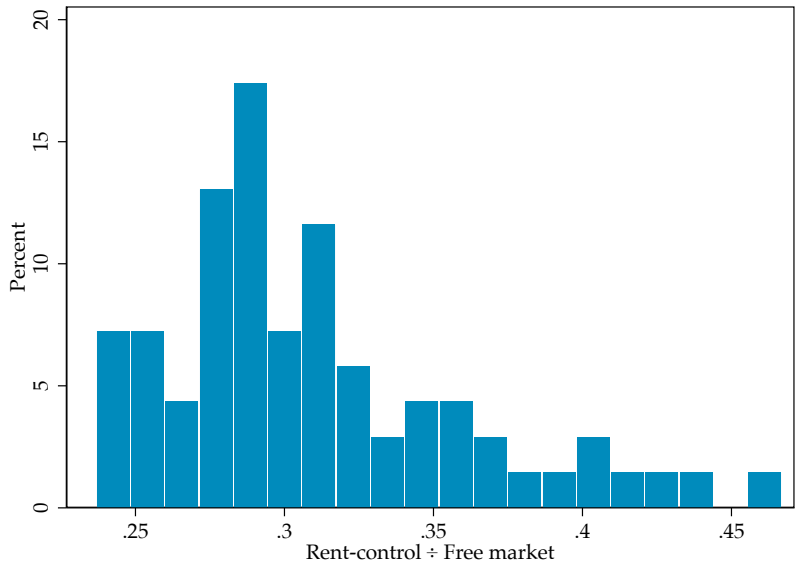
TABLES AND FIGURES

FIGURE I: DISTRIBUTION OF RENT VALUES

Panel A: Distribution of rent values



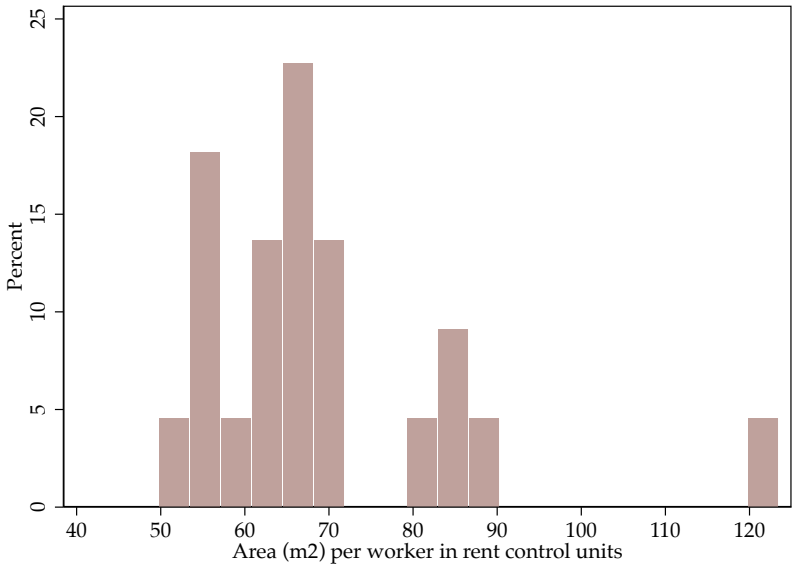
Panel B: Distribution of rent discounts



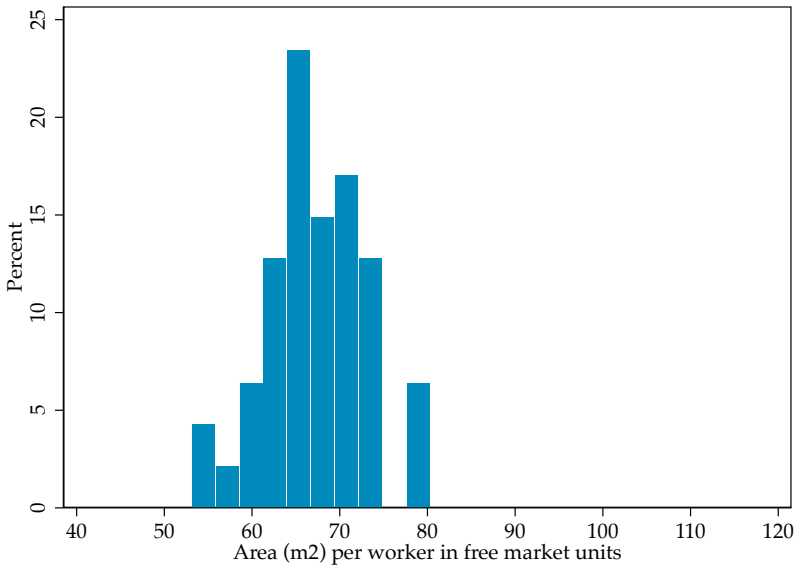
Note: Panel A shows the rent distributions for rent-controlled and free-market units in the Lisbon metropolitan area. Panel B shows the ratio of rents (rent-controlled to free-market) for units in the Lisbon metropolitan area. In both panels, values were first aggregated at the 4 digit zip code level. In Panel B, the discount is computed as the ratio of rent-controlled to free-market rents within the same 4-digit ZIP code.

FIGURE II: DISTRIBUTION OF FLOOR AREAS

Panel A: Distribution of areas (m^2) per worker in rent control

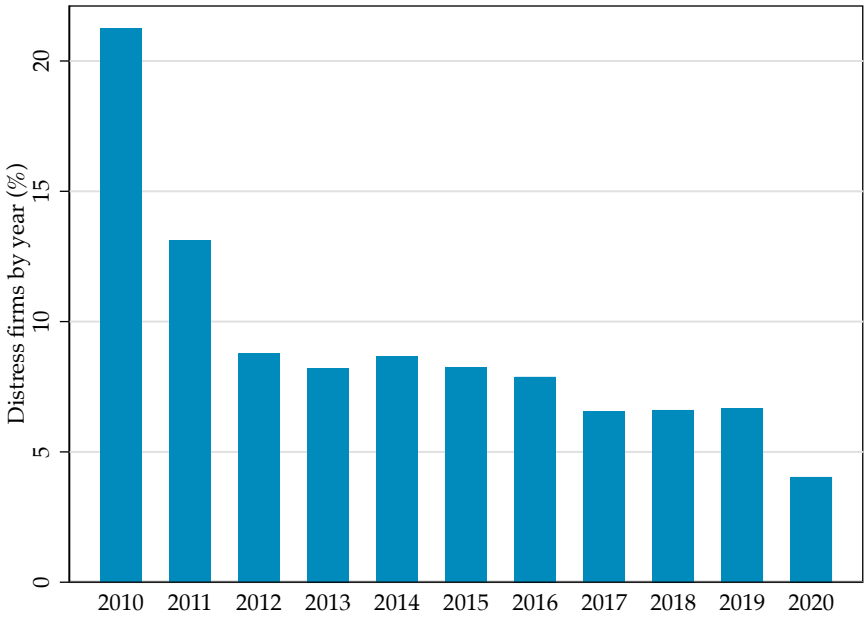


Panel B: Distribution of areas (m^2) per worker in free market



Note: Panel A shows the distribution of floor area (in square meters) per worker for free-market units in the city outskirts. Panel B shows the corresponding distribution for rent-controlled units in the city center. In both panels, values were first aggregated at the 4 digit zip code level.

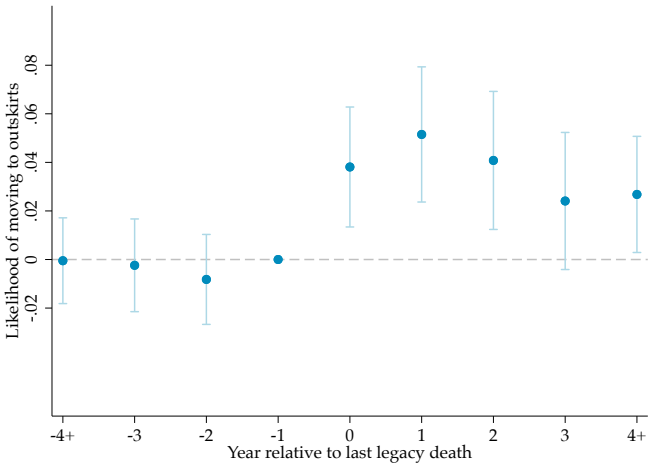
FIGURE III: DISTRESS FIRMS BY YEAR



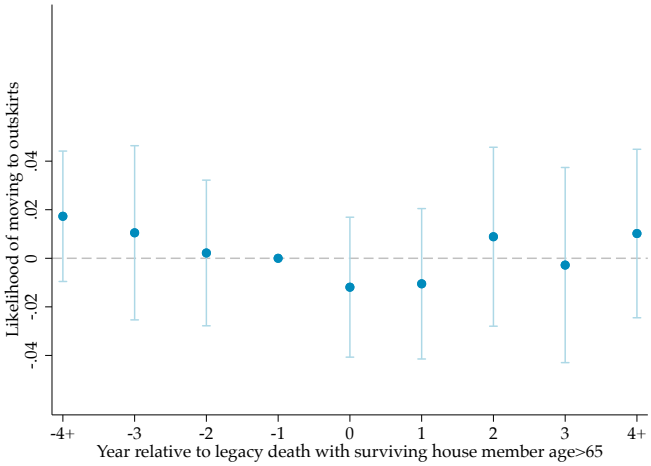
Note: This figure plots the fraction of firms experiencing financial distress in a given year. Following the labor literature, we assume that a firm experiences distress if it lays off more than 30% of their workforce in a given year.

FIGURE IV: EVENT PLOT ANALYSIS: RESIDENTIAL MIGRATION TO THE OUTSKIRTS

Panel A: No surviving house member age>65



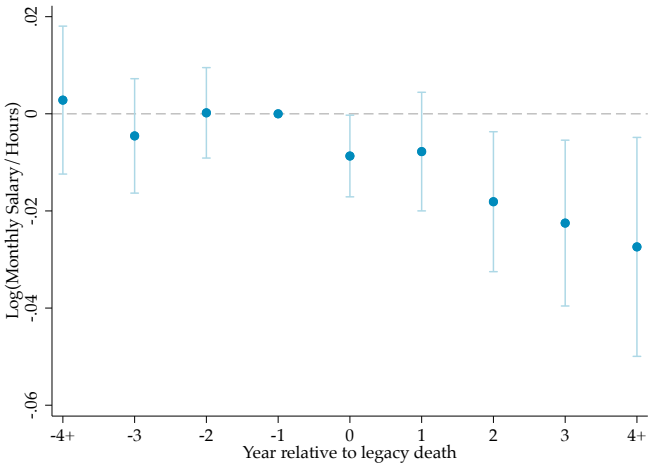
Panel B: With surviving house member age>65



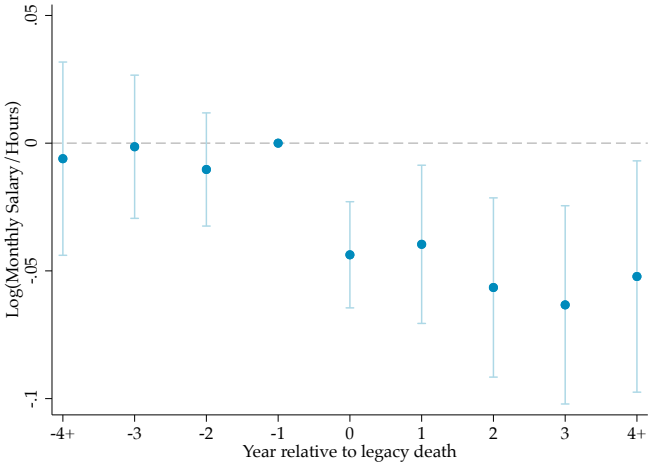
Note: Our Internet Appendix details the definitions of the variables legacy death and moving to outskirts. The event plot reports confidence intervals at the 95 percent level. Standard errors are clustered at the individual level

FIGURE V: EVENT PLOT ANALYSIS: IMPACT ON EARNINGS

Panel A: Average Effect



Panel B: Workers in distress firms



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 I: BALANCING TESTS IN 2010

	Treatment	Control	Treatment vs Control	
	Mean	Mean	Difference	t-stat
Age (in years)	41.75	41.59	0.169	0.364
Male	0.566	0.535	0.031	1.431
College	0.218	0.234	-0.016	-0.892
Percentage of 18-35 (%)	16.52	17.10	-0.577	-0.701
Percentage of 36-50 (%)	19.01	19.39	-0.389	-0.440
Percentage of 51-64 (%)	21.52	21.33	0.186	0.191
Log(Total Salary/Hours worked)	1.550	1.573	-0.023	-1.069
Hours worked (monthly)	166.0	165.9	0.033	0.049
Rent category	5.083	5.103	-0.0203	-0.211
Firm size	1090	1118	-27.16	-0.215
Firm distress (20%)	0.264	0.291	-0.0271	-1.390
Firm distress (30%)	0.156	0.183	-0.0276	-1.714
Firm distress (40%)	0.108	0.121	-0.0128	-0.992
Firm closure	0.0298	0.0319	-0.0021	-0.317

Notes: This table reports sample means for all variables by treatment and control group. The last two columns show the difference in means and the associated t-statistics. The Internet Appendix A defines all main variables.

TABLE II: DOES THE LOSS OF RENT CONTROL PUSH WORKERS TO THE *OUTSKIRTS*?

	y-var: Move to outskirts						
	Average effect		Last legacy	Treat_{10-12}		Treat_{13-19}	
Treat × Post	0.0228***			0.0189**		0.0231***	
	(0.005)			(0.009)		(0.006)	
Treat ×$\text{Post}_{t=\{0,1\}}$		0.0247***			0.0023		0.0280***
		(0.007)			(0.006)		(0.007)
Treat ×$\text{Post}_{t=\{2,+3\}}$		0.0207***			0.0265**		0.0174***
		(0.006)			(0.011)		(0.007)
Treat (Last legacy) × Post			0.0349***				
			(0.007)				
Treat (Non-last Leg) × Post			-0.0062				
			(0.007)				
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	101,023	101,023	101,023	16,452	16,452	84,365	84,365
R-squared	0.183	0.183	0.183	0.178	0.178	0.183	0.183

Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. The outcome variable only equals one in the year when the worker moves toward the outskirts. Our Internet Appendix A details the definition of *Move to outskirts*, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

TABLE III: DOES THE LOSS OF RENT CONTROL LEADS WORKERS TO CHANGE FIRMS?

Panel A

	y-var: Moving firms					
	Average effect		Treat_{10-12}		Treat_{13-19}	
Treat × Post	0.0110		-0.0339		0.0134	
	(0.0108)		(0.0389)		(0.0110)	
Treat × $\text{Post}_{t=\{0,1\}}$		0.0243**		-0.0211		0.0270**
		(0.0115)		(0.0420)		(0.0120)
Treat × $\text{Post}_{t=\{2,+3\}}$		-0.00365		-0.0400		-0.00222
		(0.0131)		(0.0401)		(0.0136)
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	101,259	101,259	16,503	16,503	84,552	84,552
R-squared	0.393	0.393	0.372	0.372	0.393	0.393

Panel B

	y-var: Jobs in firms in the outskirts					
	Average effect		Treat_{10-12}		Treat_{13-19}	
Treat × Post	0.0191**		0.0134		0.0195**	
	(0.009)		(0.029)		(0.009)	
Treat ×$\text{Post}_{t=\{0,1\}}$		0.0293***		0.0101		0.0323***
		(0.010)		(0.032)		(0.0104)
Treat ×$\text{Post}_{t=\{2,+3\}}$		0.0079		0.0146		0.0047
		(0.011)		(0.031)		(0.012)
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	101,259	101,259	16,503	16,503	84,552	84,552
R-squared	0.371	0.371	0.340	0.341	0.370	0.371

Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. In Panel A, the outcome variable only equals one in the year when the worker moves to a different firm. In Panel B, the outcome variable only equals one in the year when the worker moves to a different firm in the city outskirts. Our Internet Appendix A details the definition of these outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table IV: Does the loss of rent control affect earnings?

	y-var: Log(Monthly salary/hours)					
	Average effect		Treat_{10-12}		Treat_{13-19}	
Treat × Post	-0.0142**		-0.0115		-0.0145**	
	(0.007)		(0.017)		(0.007)	
Treat ×$\text{Post}_{t=\{0,1\}}$		-0.0080		-0.0030		-0.0084
		(0.006)		(0.012)		(0.007)
Treat ×$\text{Post}_{t=\{2,+3\}}$		-0.0203**		-0.0152		-0.0210**
		(0.009)		(0.020)		(0.010)
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	111,923	111,923	18,268	18,268	93,447	93,447
R-squared	0.935	0.935	0.932	0.932	0.936	0.936

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

Table V: Does firm distress explain job reallocation?

	y-var: Move outskirts		*y-var*: Job Outskirts		*y-var*: Log(Salary/hours)	
	No Distress	Distress	No Distress	Distress	No Distress	Distress
Treat × Post	0.0210***	0.0335***	0.0028	0.0473**	-0.0069	-0.0397***
	(0.006)	(0.012)	(0.010)	(0.021)	(0.008)	(0.014)
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	60,759	39,887	60,848	40,033	69,509	41,939
R-squared	0.203	0.230	0.443	0.411	0.961	0.899

Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. We define distress based on mass layoffs. A firm is classified as distressed if it laid off more than 30% of its workforce in a given year. Our Internet Appendix A details the definition of all outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table VI: Timing of distress and firm closures

Panel A

	y-var: Jobs in firms in the outskirts			
	Firm distress		Firm distress between '10 and '12	
	No firm closure	Pre-house shock	Treat_{10-12}	Treat_{13-19}
Treat × Post	0.0464**	0.0472**	-0.111	0.0660***
	(0.022)	(0.021)	(0.007)	(0.025)
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
N	37,752	39,296	3,654	19,838
R-squared	0.413	0.411	0.328	0.325

Panel B

	y-var: Log(Salary/hours)			
	Firm distress		Firm distress between '10 and '12	
	No firm closure	Pre-house shock	Treat_{10-12}	Treat_{13-19}
Treat × Post	-0.0396***	-0.0396***	-0.0022	-0.0357*
	(0.015)	(0.015)	(0.032)	(0.019)
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
N	39,325	41,202	4,176	22,565
R-squared	0.901	0.898	0.867	0.869

Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. A firm is classified as distressed if it laid off more than 30% of its workforce in a given year. Our Internet Appendix A details the definition of all outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table VII: Effect on reallocation by age

	Distress firm					
	y-var: Move outskirts		*y-var*: Job outskirts		*y-var*: Log(Salary/hours)	
	age<40	age≥ 40	age<40	age≥ 40	age<40	age≥ 40
Treatment $\times$ Post	0.0318**	0.0340**	0.0578**	0.0227	-0.0584***	0.00413
	(0.0152)	(0.0184)	(0.0265)	(0.0332)	(0.0182)	(0.0231)
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	26,442	13,387	26,569	13,406	27,630	14,259
R-squared	0.229	0.238	0.399	0.432	0.890	0.925

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. A firm is classified as distressed if it laid off more than 30% of its workforce in a given year. Our Internet Appendix A details the definition of all outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table VIII: Effect on job reallocation by education

	y-var: Job in Outskirts				*y-var*: Log(Monthly salary/hours)			
	All firms		Distress firm		All firms		Distress firm	
	No college	College	No college	College	No college	College	No college	College
Treatment $\times$ Post	0.0171	0.0207	0.0287	0.109**	-0.0110	-0.0189	-0.0299**	-0.0714*
	(0.0105)	(0.0192)	(0.0238)	(0.0435)	(0.0072)	(0.0191)	(0.0149)	(0.0396)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	74,943	26,302	30,570	9,364	83,128	28,789	32,073	9,776
R-squared	0.391	0.312	0.424	0.353	0.927	0.915	0.871	0.901

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. A firm is classified as distressed if it laid off more than 30% of its workforce in a given year. *No college* equals one if an individual does not hold a college degree. *College* equals one if the individual holds a college degree. Our Internet Appendix A details the definition of all outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

TABLE IX: EFFECT ON WAGES AND MODES OF TRANSPORTATION

	y-var: Job in Outskirts				*y-var*: Log(Monthly salary/hours)			
	All firms		Distress firm		All firms		Distress firm	
	Owns car	No car	Owns car	No car	Owns car	No car	Owns car	No car
Treatment × Post	0.0094	0.0258**	0.0433	0.0496**	0.0006	-0.0224**	-0.0234	-0.0477***
	(0.015)	(0.012)	(0.037)	(0.025)	(0.013)	(0.009)	(0.029)	(0.017)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	34,596	66,659	11,982	28,008	38,467	73,452	12,542	29,356
R-squared	0.336	0.384	0.362	0.430	0.941	0.925	0.917	0.881

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. A firm is classified as distressed if it laid off more than 30% of its workforce in a given year. *Owns car* equals one if an individual uses a personal vehicle to commute to work. *No car* equals one if the individual uses public transit to commute to work. Our Internet Appendix A details the definition of all outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

TABLE X: DOES LOSS OF RENT CONTROL AFFECT LABOR SUPPLY OR UNEMPLOYMENT?

	y-var: Log(Hours worked)			*y-var*: Unemployment		
	Full Sample	Other firms	Distress firm	Full Sample	Other firms	Distress firm
Treatment × Post	0.00415	0.00541	0.00612	0.00219	0.00196	0.00406
	(0.00282)	(0.00354)	(0.00575)	(0.00241)	(0.00204)	(0.00660)
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	111,923	69,509	41,939	111,841	69,472	41,896
R-squared	0.649	0.720	0.585	0.354	0.461	0.450

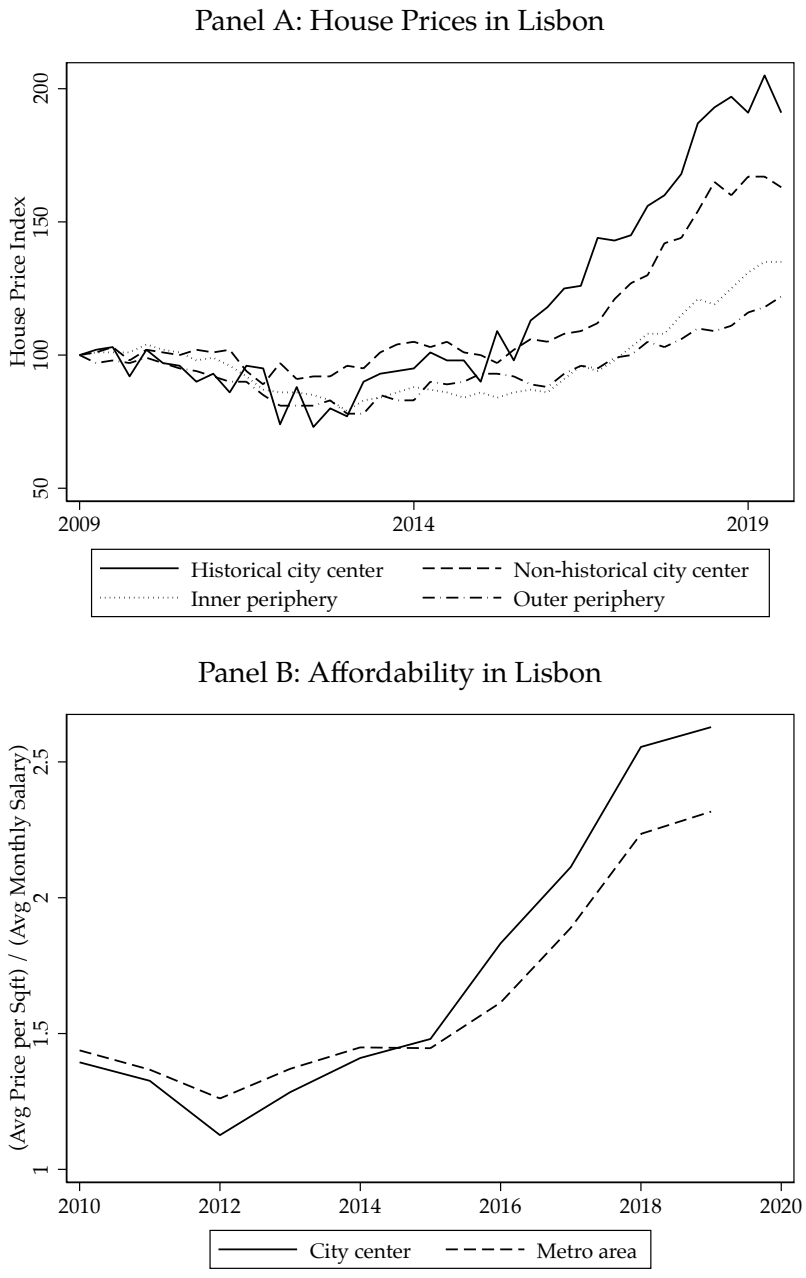
Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. A firm is classified as distressed if it laid off more than 30% of its workforce in a given year. Our Internet Appendix A details the definition of all outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

INTERNET APPENDIX

A Variable definitions

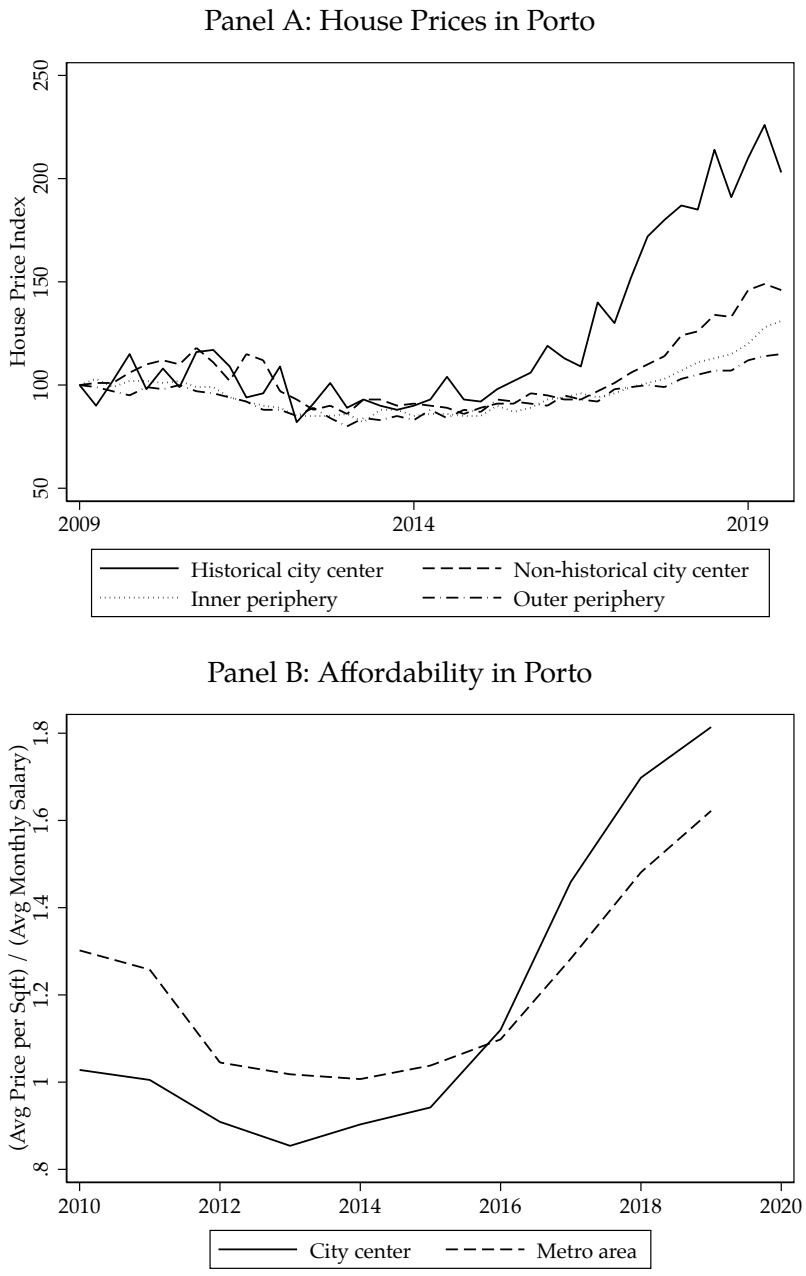
Treatment	
Legacy renter	=1 if renter is 65+ years old and rental contract predates 1990
Legacy death	=1 if a legacy renter dies
Treated worker	=1 if the worker lives with a legacy renter who dies
Demographics	
Age	Age in years
Male	=1 if individual is male
College	=1 if individual has a college degree
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
Unemployment	=1 if worker is unemployed
Distress firm	=1 if more than 30 percent of the workforce left the company in a given year or the preceding year.
Income groups	
Low income	=1 if income is between below the median
High income	=1 if income is above the median
Mobility	
Move to outskirts	$= 1$ if the worker moves from an inner ring to a peripheral ring between $t-1$ and t, or moves outside the greater metropolitan area (peripheral rings for the municipalities of Lisbon and Porto are shown in Figures A.6 and A.7); $= 0$ otherwise.
Jobs in firms in the outskirts	$= 1$ if the worker moves from a firm in an inner ring to a firm in a peripheral ring between $t-1$ and t, or to a firm outside the greater metropolitan area (peripheral rings for the municipalities of Lisbon and Porto are shown in Figures A.6 and A.7); $= 0$ otherwise.

FIGURE A.1: HOUSE PRICE GROWTH AND AFFORDABILITY IN LISBON



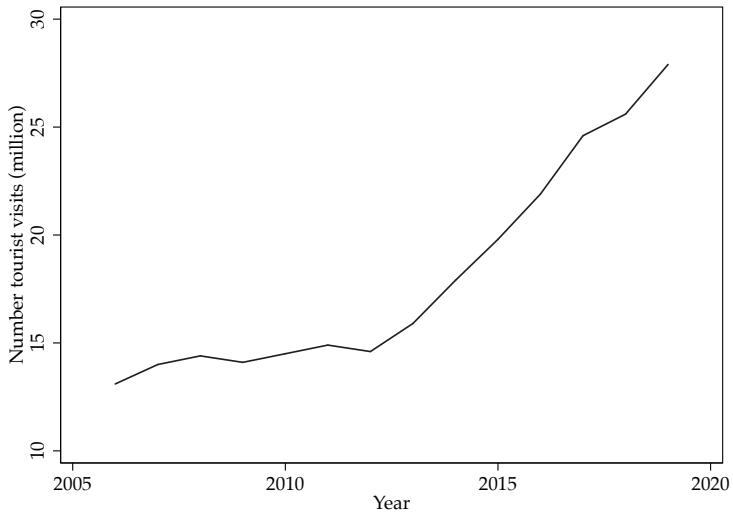
Notes: Panel A of this figure reports the evolution of house prices in Lisbon relative to January 2008. The panel depicts the growth in prices in the historical center, the city-periphery, and for the rest of the district (metropolitan area). Figure A.8 shows the neighborhoods in Lisbon that compose the city-center and city-periphery, while Figure A.6 shows the municipalities that constitute the outskirts of Lisbon. Panel B reports an index of housing affordability in Lisbon, defined as the average house price per square meter divided by the average month gross salary. A larger index implies lower affordability.

FIGURE A.2: HOUSE PRICE GROWTH AND AFFORDABILITY IN PORTO



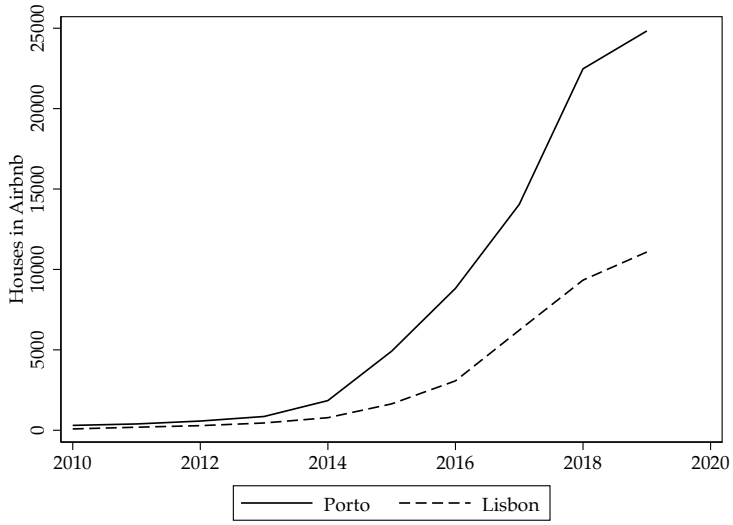
Notes: Panel A of this figure reports the evolution of house prices in Porto relative to January 2008. The panel depicts the growth in prices in the historical center, the city-periphery, and for the rest of the district (metropolitan area). Figure A.9 shows the neighborhoods in Porto that compose the city-center and city-periphery, while Figure A.7 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. A larger index implies lower affordability.

Figure A.3: Tourism in Portugal



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

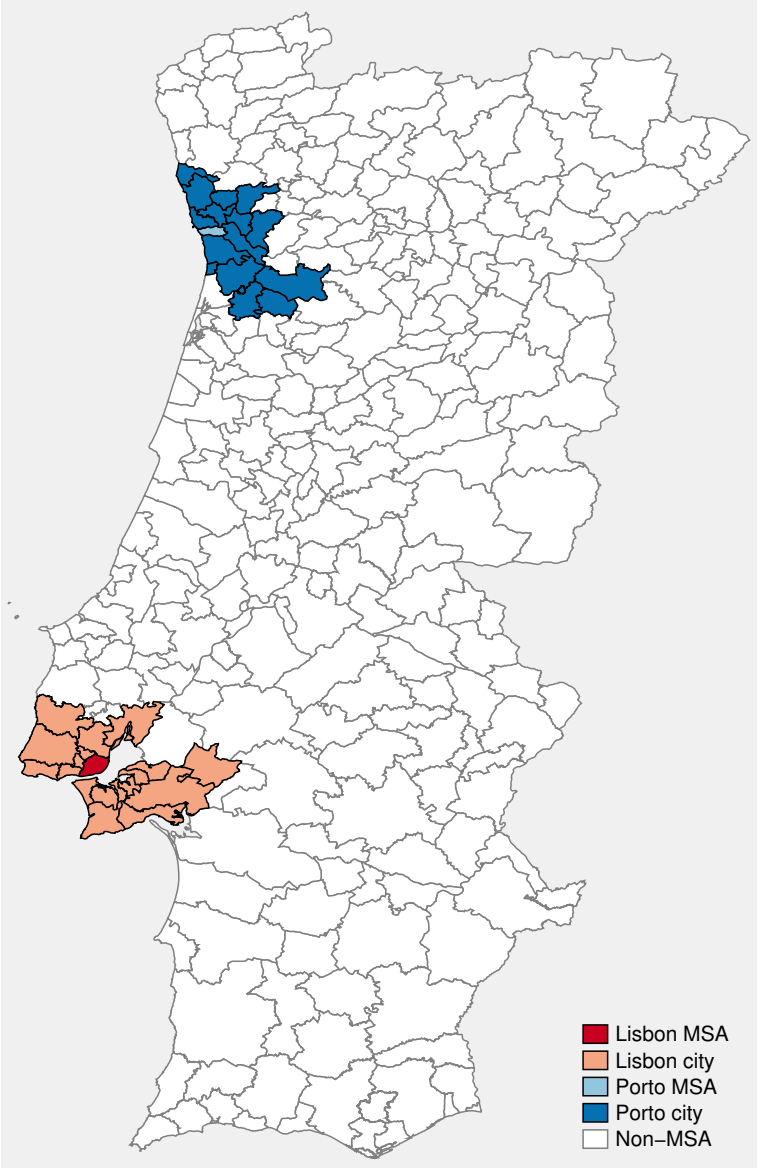
Figure A.4: Short-term rentals in Portugal



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

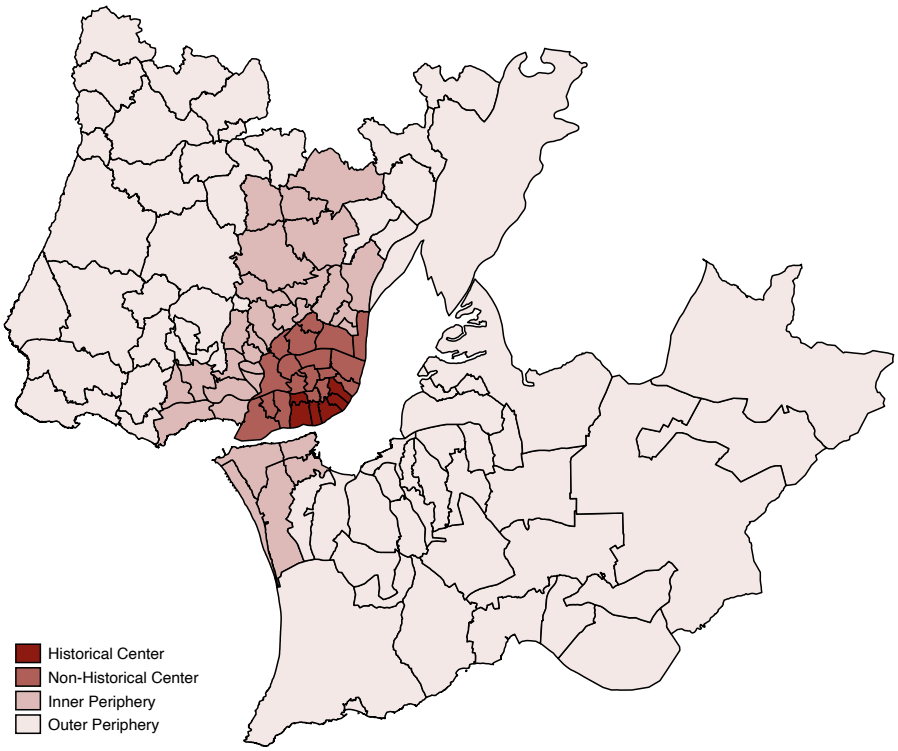
B The Geography of Portugal

Figure A.5: Portugal, Lisbon, and Porto



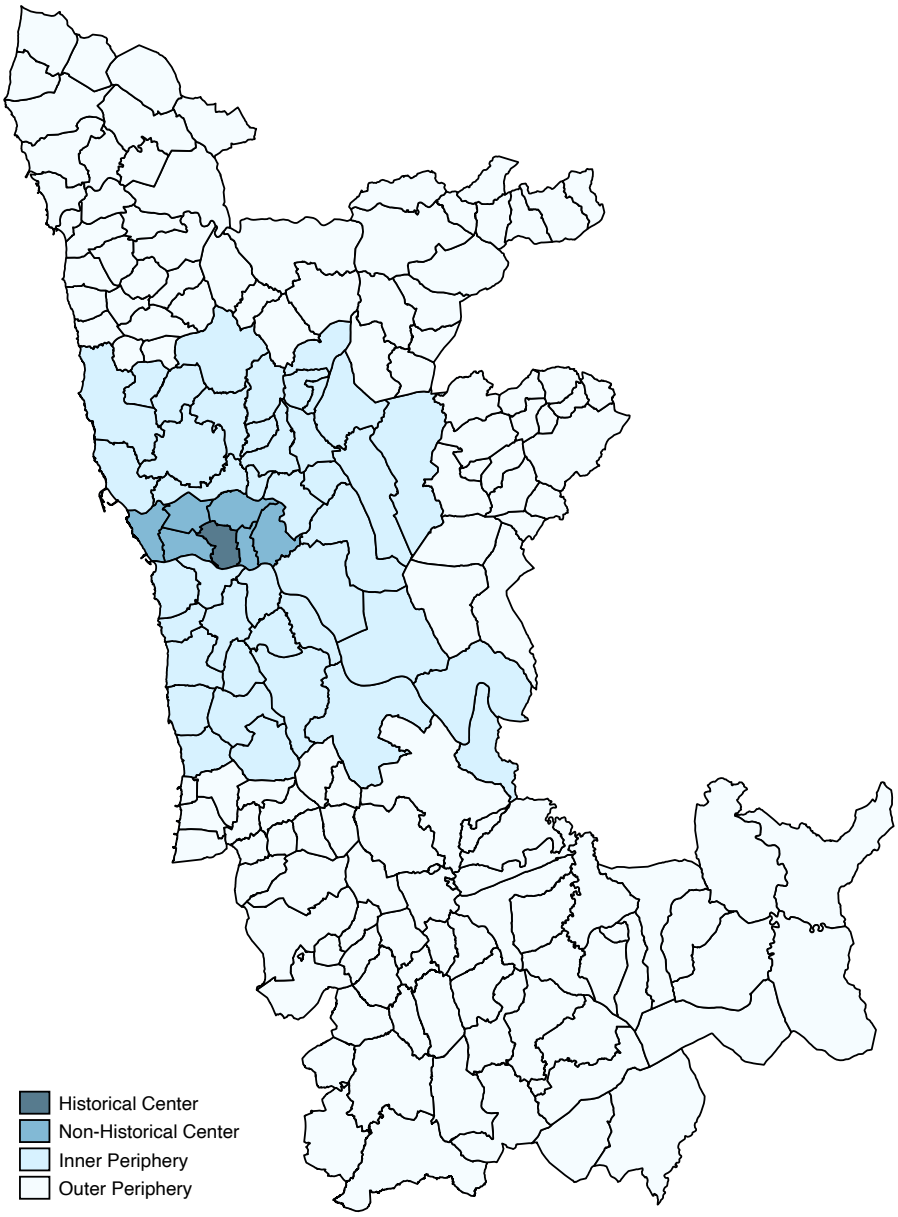
Notes: The map below depicts Portugal. The dark red region indicates the city of Lisbon, while the light red area represents its metropolitan vicinity. The light blue region highlights the city of Porto, with the dark blue area showing the Porto metropolitan area.

FIGURE A.6: LISBON METROPOLITAN AREA



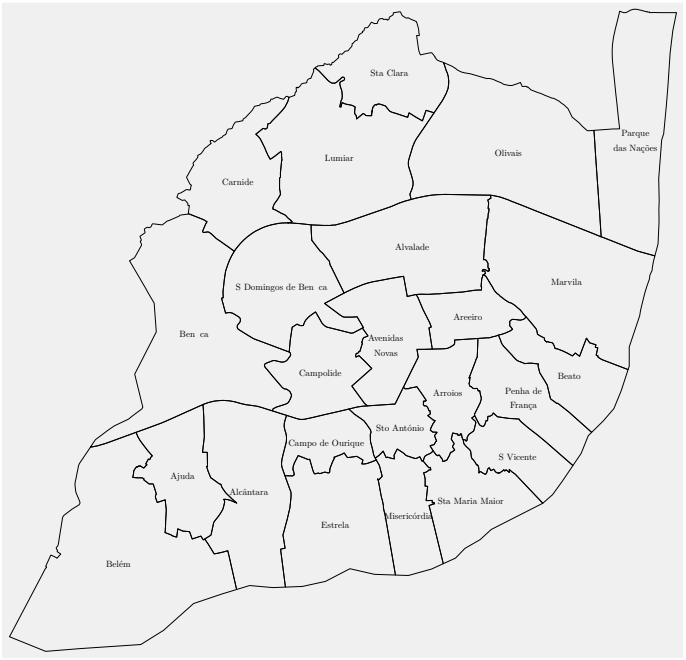
Notes: The picture below shows Lisbon metropolitan area, including the peripheral rings used to define outskirts.

Figure A.7: Porto metropolitan area



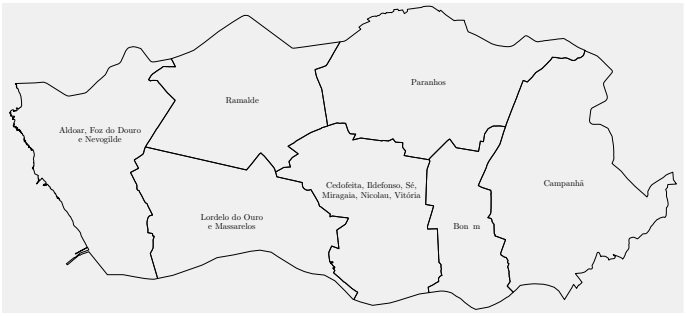
Notes: The picture below shows Porto metropolitan area, including the peripheral rings used to define outskirts.

Figure A.8: Lisbon Freguesias (neighborhoods)



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

Figure A.9: Porto Freguesias (neighborhoods)



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

C Additional Empirical Results

TABLE A.1: DOES THE LOSS OF RENT CONTROL LEADS WORKERS TO CHANGE FIRMS WITHIN THE CITY?

Panel A

	y-var: Moving firms within city	
	Average effect	
Treat × Post	0.000179	
	(0.000861)	
Treat × $\text{Post}_{t=\{0,1\}}$		-0.000119
		(0.000652)
Treat × $\text{Post}_{t=\{2,+3\}}$		0.000512
		(0.00133)
Year FE	Yes	Yes
Individual FE	Yes	Yes
Cohort FE	Yes	Yes
N	101,259	101,259
R-squared	0.208	0.208

Note: All regressions include *Post* dummies, although they are not reported for clarity in exposition. The outcome variable only equals one in the year when the worker moves to a different firm within the city center, which is defined as the historical center, as depicted in Figure A.6 and A.7. Our Internet Appendix A details the definition of any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table A.2: Moving to lower-wage firms

	y-var: Move to low-wage firm		
	All firms	Non-distressed	Distressed
Treatment × Post	0.0228**	-0.00558	0.0593***
	(0.0091)	(0.0057)	(0.0216)
Year FE	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
N	109,179	51,314	39,325
R-squared	0.890	0.984	0.824

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. The outcome variable is the likelihood that a worker moves to a low-wage firm, which is defined as firm with average wages below the median in the sample. Our Internet Appendix A details the definition of any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table A.3: Distress robustness

	Firm distress robustness			
	y-var: Job outskirts		*y-var*: Log(Salary/hours)	
	Distress 40%	Distress 50%	Distress 40%	Distress 50%
Treatment × Post	0.0434*	0.0559*	-0.0549***	-0.0486***
	(0.0260)	(0.0289)	(0.0170)	(0.0185)
Year FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
N	29,620	24,467	30,882	25,498
R-squared	0.422	0.426	0.896	0.893

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. We define distress based on mass layoffs. In the first and third columns a firm is classified as distressed if it laid off more than 40% of its workforce in a given year, while the second and fourth column we use a 50% threshold. Our Internet Appendix A details the definition of the outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table A.4: The effect on earnings by income

	Firm distress					
	y-var: Move outskirts		*y-var*: Job outskirts		*y-var*: Log(Salary/hours)	
	Low-income	High-income	Low-income	High-income	Low-income	High-income
Treatment × Post	0.0398**	0.0414*	0.0493*	0.0395	-0.0518***	0.0095
	(0.018)	(0.025)	(0.028)	(0.035)	(0.016)	(0.034)
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	13,207	9,331	13,306	9,339	14,673	9,897
R-squared	0.210	0.196	0.368	0.325	0.786	0.888

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. Based on income in 2010, we split the sample into different income groups. Low-income have earnings below the median, and high-income have earnings above the median. Since we do not observe earnings for all individuals in 2010, the sample size for this test is smaller than the size in our baseline regressions. Our Internet Appendix A details the definition of the outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table A.5: Effects on earnings by gender

	Firm distress & No car					
	y-var: Move outskirts		*y-var*: Job outskirts		*y-var*: Log(Salary/hours)	
	Male	Female	Male	Female	Male	Female
Treatment × Post	0.0327*	0.0339	0.0417	0.0588	-0.0453*	-0.0498**
	(0.0199)	(0.0218)	(0.0356)	(0.0368)	(0.0249)	(0.0221)
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	14,153	13,677	14,191	13,751	14,860	14,440
R-squared	0.224	0.255	0.424	0.435	0.890	0.919

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. *No car* means that a worker uses public transit to commute to work. Our Internet Appendix A details the definition of the outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.

Table A.6: Effects on Earnings in Lisbon versus Porto

Panel A: No Car

	No car					
	y-var: Move outskirts		*y-var*: Job outskirts		*y-var*: Log(Salary/hours)	
	Lisbon	Porto	Lisbon	Porto	Lisbon	Porto
Treatment × Post	0.0249***	0.0196*	0.0282**	0.0188	-0.0240**	-0.0184
	(0.0080)	(0.0107)	(0.0136)	(0.0225)	(0.0104)	(0.0160)
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	47,252	19,234	47,379	19,280	52,201	21,251
R-squared	0.186	0.205	0.371	0.419	0.923	0.928

Panel B: Firm Distress and No Car

	Firm Distress & No car					
	y-var: Move outskirts		*y-var*: Job outskirts		*y-var*: Log(Salary/hours)	
	Lisbon	Porto	Lisbon	Porto	Lisbon	Porto
Treatment × Post	0.0402**	0.0249	0.0343	0.0724	-0.0397**	-0.0641**
	(0.0189)	(0.0212)	(0.0304)	(0.0449)	(0.0202)	(0.0287)
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	20,358	7,538	20,426	7,582	21,369	7,987
R-squared	0.226	0.278	0.414	0.479	0.876	0.891

Notes: All regressions include *Post* dummies, although they are not reported for clarity in exposition. *No car* means that a worker uses public transit to commute to work. Our Internet Appendix A details the definition of the outcome variables, as well as any other variable included in the regression. Standard errors are clustered at the individual level, and reported in parentheses. Statistic significance: ***=1 percent; **=5 percent; and *=10 percent.