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Information Capitalization in the Housing Market: Evidence from Land Leases

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Abstract

This paper provides causal evidence on the reaction of housing market participants to information shocks regarding a complex policy scheme affecting the future user cost of housing. We study the capitalization of large future land rent increases into transaction prices of residential units located on leasehold land in Helsinki, Finland. The policy announcement of land rent renewals creates a quasi-experimental setting, allowing us to investigate the capitalization dynamics over a period of 14 years. The leasehold discount increase is substantially slower than predicted by the present value model, implying only partial adjustment over the short run, but is in line with full capitalization eventually. We further investigate the mechanism behind the sluggish price adjustment.

KEYWORDS
capitalization of information, land rent increases, quasi-experiment

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1 | INTRODUCTION

Housing markets are shaped by a wide range of policy-induced costs that affect the user cost of housing and thereby housing prices. The extant empirical literature suggests that housing market participants fully internalize direct and highly salient costs, such as property taxes and energy expenses (e.g., Busse et al., 2013; Sallee et al., 2016; Myers, 2019; Koster & Pinchbeck, 2022). In practice, however, many policy-related housing costs are complex, nontransparent, and are realized only gradually over time. Understanding how households perceive and incorporate such costs into their housing market decisions is of notable policy importance, with implications for the capitalization of expected cash flows, tax incidence, and allocative efficiency in the housing market.

Housing transactions often involve long horizons, contractual features, and deferred obligations that require substantial effort and financial literacy to evaluate. Households may not exert enough effort to gather information about all the costs involved or may differ in their ability to understand the provided information (DellaVigna, 2009). Prior research shows that households may misunderstand nonlinear pricing schemes (Shaffer, 2020) and that the salience of information can affect market outcomes (Chetty et al., 2009; Taubinsky & Rees-Jones, 2018). In housing markets, evidence points to inattention to deferred tax increases (Bradley, 2017) and incomplete capitalization of less salient operating costs (Almenberg & Karapetyan, 2014), raising concerns about whether prices and choices fully reflect future financial obligations.

This paper provides new causal evidence on how housing market participants learn about and respond to complex, policy-induced changes in future operating costs in a high-stakes setting. We exploit a policy reform in the city of Helsinki, Finland, where the city announced substantial increases in land rents for residential units with land lease contracts expiring in the near future. These renewals induced over a 40% rise in operating costs for affected dwellings. The institutional setting of publicly owned land and predetermined contract expirations creates a unique quasi-experimental setup that allows us to study information processing, anticipation, and capitalization of future housing costs, and to contribute to the literature in several prominent ways.

In Finland (as, e.g., in Sweden), when a multistory-building with several housing units is located on leasehold land, land rents are collected from the condominium that in turn charges maintenance fees from the unit owners. This arrangement makes the land rent system rather complex and nontransparent for the market participants. Although the sales brochures of units typically contain information on condominium's land tenure (freehold or leasehold) and the length of the land lease contract, the quantification of the impact of future land rent payments on the cost of living involves a high cognitive burden and requires a lot of effort to gather all relevant information.

Our empirical strategy is based on a difference-in-differences (DID) design where we compare the transactions of units on leasehold land (leaseholds) to similar units on freehold land (freeholds) in the same neighborhoods before and after the announcement of the land rent renewal policy. This enables us to identify how the future financial flows capitalize into the prices of units on leasehold land. We exploit the fact that before the announcement of the land rent renewal policy, there had been no previous experiences or official decisions regarding the policy that the city of Helsinki would exercise in the upcoming contract renewals. Therefore, the first official announcement in 2008 of the large-scale rent increases acts as a policy change that is used to estimate the causal effect of land rent increases. As there were no changes in the property rights

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or land lease legislation during the study period, the price level difference stemming from the underlying rights on the land is captured by the DID design.

Our empirical analyses are based on data that include detailed information on sales prices and dates, unit characteristics and land lease contracts in Helsinki during 2003–2021. We also track public announcements of the land rent renewal policy and their financial implications. Based on these detailed data, including the announced land rent increases at the unit level, we estimate the capitalization of expected future financial flows into prices. Since we focus on leaseholds that were subject to land lease contract renewal in 2021, we can identify the price effects of accumulating information and experience regarding the contract renewal policy over a time span of 14 years.

The analysis reveals that before the announcement of the land rent renewal policy, homebuyers were inattentive to the expected long-term housing expenses, which resulted in significant overpricing of leaseholds relative to corresponding freeholds. In contrast with the assumption of full optimization, there was also no immediate effect on the leasehold prices after the official announcement in 2008. Instead, the leasehold discount increased sluggishly over the following decade. Eventually, homebuyers seemed to learn the importance of the land ownership feature as more and more information and media coverage about the future rent increases was provided. At the end of the analyzed period, the estimated leasehold discounts imply a long-term discount rate that is aligned with those reported by previous studies (e.g., Giglio et al. 2015; Bracke et al. 2018), suggesting full capitalization.

The most important contributions of our study relate to the unique quasi-experimental setup that allows us to investigate the price adjustment process to an information shock concerning future cash flows, and to shed light on the mechanism behind the observed adjustment pattern. The setting, where the treatment and control groups are similar in their technical quality and are located within the exact same neighborhoods, also increases the reliability of our results. Overall, this paper contributes to several strands of literature.

First, we contribute to the literature on housing market informational efficiency (e.g., Larsen & Weum, 2008; Anundsen & Røed Larsen, 2018; Broxterman et al., 2023). Our article relates closely to studies such as Eerola and Lyytikäinen (2015) and Ben-Shahar and Golan (2019), which investigate the effects of aggregate-level interventions that improve information availability in housing markets. As a major difference compared to these studies, we focus on information shocks that affect only a specific segment of the housing stock. Hence, we can base our identification on within-neighborhood variation, thereby avoiding the potential complication of unobserved differences across neighborhoods (see Hilber, 2017).

Importantly, we can follow the capitalization process over a time span of more than 10 years. Our novel finding is that market participants learn the new policy regime progressively over time, and the market prices (only) gradually reflect the information on future financial flows. The market reaction to new information is neither immediate nor absent but rather sluggish, which is in line with a simple model of information diffusion (see, e.g., Daley & Kendall, 1964). While previous research presents evidence of slow housing price adjustment to, for example, school quality related information shocks (Hussain, 2023), ours appears to be the first study providing causal evidence of sluggish price adjustment to a shock to expected cash flows. In addition, given the quasi-experimental setup where the future cash flow effects were assured before the contract renewal—thanks to which there is no need to impose any assumptions regarding the cash flow impacts—and due to our DID estimation design, we bring robust evidence on the degree of capitalization.

Second, our study connects with the behavioral economics literature, especially related to the housing market, but also more generally. DellaVigna (2009) states three kinds of deviations from the standard model of rational decision making, of which we provide insights into "nonstandard

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decision making" in particular. While the extant literature presents evidence of individuals' underreaction and inattention in the housing market as well as in other markets (Bradley, 2017; Chetty et al., 2009), we also investigate the potential mechanism behind the sluggish housing price reaction to a shock in expected future cash flows. Our results suggest that the observed slow pace of capitalization could plausibly be explained by substantial variation in the extent to which individual transactions reflect the forthcoming rent increases. We observe a significant increase in the leasehold price dispersion after the first official announcement in 2008. The increased dispersion lasts until the last years before the contract renewal and disappears in 2021 when the new contracts come into effect. Moreover, the share of transactions where we observe full or substantial capitalization systematically increases over time as information on the new policy accumulates. These observations suggest that the market is segmented in terms of the optimization behavior of individuals.

Third, the paper contributes to the limited research on long-run discount rates (Bracke et al., 2018; Giglio et al., 2015; Koster & Pinchbeck, 2022) and appears to be the first study utilizing causal evidence to estimate long-run discount rates based on the price effects of future land rent payments. Information on the discount rates is highly policy relevant for many important reasons (Koster & Pinchbeck, 2022).

Finally, since land leases by local authorities yield monetary costs similar to property or other taxes, our study relates to the literature examining the capitalization of taxes and other financial flows also more generally (e.g., Bradley, 2017; Koster & Pinchbeck, 2022). Previous evidence suggests that households might be (in)attentive to (non)transparent costs. However, there is little evidence on whether nontransparent costs remain unconsidered in the longer run, that is, whether accumulating information and experiences affect market outcomes. For example, Chetty et al. (2013) find that EITC (the earned income tax credit) refund utilization increases when an individual moves to a neighborhood with better knowledge of the EITC scheme. Within this literature, our paper provides new causal evidence on the capitalization of information on indirect and nontransparent financial flows.

The remainder of this paper is organized as follows. The next section describes the institutional background of land leases in Helsinki and presents our simple theoretical framework. The empirical strategy is presented in Section 3, and the data are described in Section 4. The results of the main and complementary analyses are reported in Sections 5 and 6. The last section provides the conclusions.

2 | INSTITUTIONAL FRAMEWORK

2.1 | Land rent renewals in Helsinki

The construction of privately owned housing on land leased from local municipalities is common in several countries and in major European cities, such as Amsterdam, Frankfurt, London, Stockholm and Vienna. During the rapid expansion of Helsinki in the 1950s and 1960s more than half of new housing was built on such leasehold sites. A major share of this housing stock is in condominiums.

The land lease contracts in the 1950s and 1960s in Helsinki were typically signed for around 60 years. All these land lease contracts include an annual rent adjustment based on the cost of living index (CLI). Due to the rapid growth of the city, the land prices and, similarly, (implicit) market rents for land in Helsinki have increased substantially faster than the CLI. As a result, rents of these contracts became negligible in relation to the prevailing market rents over time.

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Starting from 2010, many land leases from the 1950s and 1960s reached the end of their contract term. This was the first time large-scale land lease contract renewals took place in Finland. Although the city of Helsinki had a rather clear land rent policy that was applied to new land lease contracts, considerable uncertainty remained regarding the upcoming renewal policy among the market participants. The Finnish legislation on land lease contracts is relatively liberal, stating that the lessee has the right to renew the contract after expiration if the property continues to be used for housing; however, it prescribes neither the maximum rent nor the maximum rent increase at renewal for the contracts.

In January 2008, the city of Helsinki announced its policy for the upcoming large-scale land lease contract renewals. The annual rent was set—aligned with the rent policy applied to new development sites—at approximately 4% of the land value appraised by independent experts. For those leasehold sites subject to renewal in the near future, this translated into a multifold increase in land rents.

Figure A1 shows that the upcoming land rent increases received essentially no media coverage before the 2008 announcement; the announcement therefore constituted a genuine information shock rather than confirmation of already-circulating information. Following the announcement, the policy and the associated increases received high visibility in local and nationwide media, and the reasonableness of the increases was widely debated. The issue was also discussed politically as local decision-makers had to find a balance between the land lease incomes that play a substantial role in the city's finances, potential social aspects, and the experienced unfairness of sudden increases that could affect voter support. The land rent increases were therefore politicized, and the final policy formulation remained uncertain almost until the first contract renewals that took place in 2011. Because of these pressures, and threats of lawsuits, the city decided to implement the land rent increases gradually over a 10-year period: half of the announced increase took place in the first year, after which an additional 5 percent of the total increase was phased in each year, reaching the full level 10 years after the contract renewal. The term of the renewed contracts was set to 50 years, and the annual rent adjustment continued to be based on the CLI.

Our empirical analyses are based on the leaseholds whose land lease contracts were renewed in 2021. This batch of land rent contract renewals included more than 700 lots in total, many of them with condominiums.

This 2021 batch was the third[1] after the announcement of the land rent renewal policy in 2008. As with the two earlier renewal batches the city provided public information on the forthcoming land rent increases in several announcements. The first announcement indicating an estimate for the land rent increases at renewal was made in February 2017, and the second announcement with a more precise rent estimate, including an estimate of the effect of the land rent increase on monthly maintenance fee per square meter was given in December 2017. The city organized information events in the affected neighborhoods in January 2018, and the City Council confirmed the land rent levels (as indicated in the second announcement) in spring 2018.

Figure 1 summarizes the official information flows on the land rent increases throughout the period 2008–2021. As we are only analyzing the 2021 batch, this is not a staggered treatment.

[1] The first two renewal batches contained limited numbers of leasehold sites with condominium structures and were geographically dispersed. The 2011 batch included 87 such sites across eight statistical neighborhoods; the 2016 batch included 148 such sites across 10 statistical neighborhoods. By contrast, the 2021 batch included more than 700 lots. In addition, for some of the 2011/2016 sites, sufficiently comparable freehold units within the same neighborhood and period are not available, further reducing the usable sample. Consequently, the earlier batches do not provide enough observations for a reliable DID analysis.

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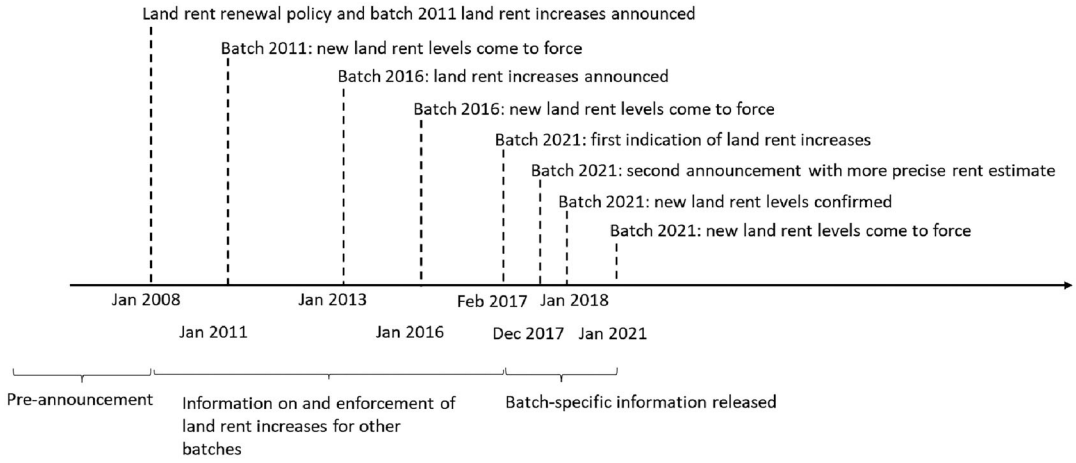


FIGURE 1 Timeline for the release of information regarding land rent renewals in Helsinki.

However, we also include the information flows for the two earlier batches in the figure, as they are relevant for the credibility of the enforcement of the new land rents and provide reference information about the expected land rent increases in 2021.

2.2 | Price differential between freeholds and leaseholds

In this paper, we are interested in the development of the price differential between freehold and leasehold units (i.e., the leasehold discount). Conceptually, the price difference between otherwise similar units reflects differences in the ownership of land and the associated future financial obligations.

A freehold unit includes both the value of the building and the land, whereas a leasehold unit includes the value of the building and the lease contract governing the use of land. The key distinction is that, for leaseholds, households face ongoing land rent payments instead of owning the land outright. Consequently, the price difference between freehold and leasehold units can be understood as reflecting the value of land ownership versus committing to future rent payments under the lease contract.

Intuitively, the leasehold discount should correspond to the present value of expected future land rent payments relative to the prevailing freehold price, as further illustrated by the present value calculations in Section 4.[2] If lease contracts require below-market rents, leaseholds may be

[2] In Finland, real estate taxation and other cash flows create only minor differences between freehold and leasehold units. First, leaseholds do not pay tax on the land, whereas freeholds do. However, as the effective real estate tax rates in Finland are notoriously low (Falkenbach & Krigsholm, 2026)—and have remained so over the sample period, with the tax base growing substantially more slowly than apartment prices—the difference in property tax payments is small. Second, mortgage interest rate payments have been (at least partially) tax-deductible and the maintenance fee, which includes land rent, is not (for owner-occupants). Hence, as leaseholds have lower prices (and hence smaller mortgages), the tax shield has been marginally smaller for leaseholds. These two channels work in opposite directions, and the net effect is only marginal relative to the land rent-increase variation that drives our identification. However, we acknowledge that there is a small trend in the net effect that could potentially bias our estimates. Hence, in the robustness section, we verify that this change in net effect does not affect our estimates by including it as a control interacted with the treatment indicator (see Section 5.3 and Table A9).

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priced relatively higher (i.e., smaller discount), whereas expectations of higher future rents should reduce leasehold prices relative to freeholds. In addition to expected rent levels, differences in perceived risk or uncertainty may also affect pricing, although prior literature suggests that, in well-functioning markets, discount rates applied to freehold and leasehold components should be broadly similar (e.g., Janssen 2003).

If market participants are fully informed and forward-looking, they should incorporate all available information about future land rents into prices. In such a case, the leasehold discount would adjust immediately to reflect new information about future financial obligations. However, in practice, this requires both access to relevant information and the ability to process it.

In the Finnish institutional setting, land rent represents an indirect and relatively nontransparent cost for individual owners of units in condominiums. The condominium is the legal lessee, and land rent payments are bundled into monthly maintenance fees charged to unit owners. While current maintenance fees are observable, evaluating future costs requires substantial effort: buyers must assess future land values, predict rent adjustments at the condominium level, translate these into unit-level costs, and discount them appropriately. This creates a significant cognitive burden.

Moreover, prior to the renewal of land lease contracts, land rents for older contracts were negligible, implying that leasehold and freehold units had very similar observed maintenance fees.[3] Combined with the absence of earlier large-scale renewal experiences, this may have led buyers to underestimate or ignore future increases in land rents.

The first official announcement of substantial land rent increases in 2008 therefore constituted a major information shock. While this announcement received considerable media attention, subsequent information releases, practical experiences from earlier renewal batches, and improved public communication gradually increased awareness and reduced uncertainty regarding future rent levels. This process likely enhanced the salience of land tenure over time, as reflected in the rising public search interest in the topic over the sample period (Figure A2).

We hypothesize that this gradual accumulation of information and experience plays a key role in shaping price dynamics. If market participants initially lacked information or failed to fully process it, leasehold discounts may have been small or absent prior to the policy announcement. As information became more widely available and credible, prices should increasingly reflect the expected future financial burden associated with leaseholds.

Importantly, unlike aggregate information shocks studied in earlier literature (e.g., Eerola & Lyytikäinen, 2015; Ben-Shahar & Golan, 2019), the information regarding land lease renewals is segment-specific, affecting only certain properties. As a result, even if directly affected households update their expectations, broader market participants may remain inattentive if the information is perceived as irrelevant to them. This setting provides a useful framework to study how complex, nontransparent cost components are gradually incorporated into housing prices.

3 | EMPIRICAL STRATEGY

The primary aim of our empirical analysis is to investigate the reaction of leasehold discounts to the announcement of the land rent renewal policy. We analyze prices of leaseholds that were subject to contract renewal in 2021 and denote the year of the announcement of the land rent renewal

[3] We analyze this formally in the appendix. The results presented in Figure A4 indicate that the maintenance fees of leaseholds subject to rent renewals were not systematically different from the maintenance fees of freeholds before the increased rent levels became effective in 2021.

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policy in 2008 as the first treatment year. Our empirical strategy relies on a hedonic regression model and a DID design. The baseline DID specification is:

$$P_{it} = \alpha + \delta * \text{treatment}_i + \sum_{j=2003}^{2006} \sigma_j * \text{treatment}_i * \text{year}_j + \sum_{j=2008}^{2021} \gamma_j * \text{treatment}_i * \text{year}_j + \beta X_i + \omega_n + \theta_t + u_{it}, \quad (1)$$

where P_{it} is the log of sale price of unit i in period t, and treatment_i is either a dummy that indicates that a unit is subject to land rent contract renewal in 2021 or the announced total annual land rent increase (in 1000 s of euros) that is eventually effective for unit i (recall that the rent increases take place gradually over 10 years). For the freehold units, the variable treatment_i equals zero in both cases. The first sum operator is the interaction between the sale-year dummy year_j and the treatment; it applies to transactions that were conducted before the announcement of the land rent renewal policy (the year 2007 is omitted). Thus, coefficients σ_j can be used to analyze the assumption of common pre-treatment trends between the control and treatment groups. Correspondingly, the second sum operator represents yearly estimates for the price effects during the post-announcement era (i.e., since 2008). The estimated post-treatment effects, γ_j, capture the dynamic effect of accumulating information and learning on capitalization for each post-treatment year.

We control for the observable dwelling characteristics on sale price. Vector X_i includes a detailed set of housing attributes, including the (log of) unit size, number of rooms, condition class, (log of) age of building and distance to Helsinki city center, as well as indicator variables for a balcony or a sauna within the unit. We include neighborhood fixed effects, ω_n, to control for spatial characteristics that do not change over time. Shocks that affect both the freehold and leasehold units are controlled with year fixed effects θ_t.[4] Standard errors for the coefficient estimates are clustered at the level of 250 m × 250 m statistical grids by Statistics Finland to account for spatial correlation in the error term within small geographic areas, which guarantees a reasonably large number of clusters (Bertrand et al., 2004).

As we are working with a relatively small number of yearly observations, we also estimate DID specifications where we pool observations in one pre-announcement and two post-announcement periods. In this specification, we distinguish between 2008–2016 ($Post1_t$), when the land rent renewal policy was known and implemented on the earlier batches (2011 and 2016), and 2017–2021 ($Post2_t$), when all relevant information of the land rent increase in 2021 had been released:

$$P_{it} = \alpha + \delta * \text{treatment}_i + \sigma * \text{treatment}_i * \text{Post1}_t + \gamma * \text{treatment}_i * \text{Post2}_t + \beta X_i + \omega_n + \theta_t + u_{it}. \quad (2)$$

[4] We also explored interacting neighborhood fixed effects with year fixed effects to account for potential differences in the price development between neighborhoods. The results remain quantitatively similar with the preferred specification. However, for some neighborhood-year combinations the number of observations is very low, increasing the risk of outliers biasing the results.

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Our identification relies on the assumption that in the absence of lease renewals, the prices in the treatment and the control groups would have evolved correspondingly. Hence, sample selection plays an important role in our research design. The analyzed leaseholds that were subject to land lease contract renewal in 2021 were mainly constructed in the 1950s and the 1960s. During this time period, more than half of the new construction took place on leasehold land, and instead of being spatially clustered in specific neighborhoods, it was common to mix the land tenures within neighborhoods. Most of the supply during this era consists of multi-story condominium buildings that are relatively uniform and conservatively designed. Moreover, during that time, the policy was to encourage mass production of housing and raise technical housing standards. This resulted in restrictive housing construction regulations, which led to a largely homogeneous housing supply (Puustinen & Kangasoja, 2009). The lack of variation in physical characteristics and surrounding amenities is common for both freehold and leasehold units of that time, and it notably increases the homogeneity of housing structures across the two groups.

To ensure close correspondence between leaseholds and freeholds in our sample, we restrict our analysis to condominium units[5] built within the same era (1957–1963) and located within the same neighborhoods. These sample restrictions ensure that we are comparing units with similar implicit prices in similar locations and that the buildings are comparable in their technical quality.

Earlier literature on leasehold discounts (Bracke et al., 2018; Giglio et al., 2015) has generally abstained from estimating leasehold discounts through the comparison of leasehold and freehold units, as the underlying land rights (i.e., right of use and right of ownership) are by definition different, and a simple comparison of pricing between the two types might not only capture the capitalization of future financial obligations but also level differences reflecting the fundamentally different property rights. In our empirical setting, this concern is addressed by the DID design, in which the causal interpretation is focused on the change in the expected future rental cash flows. Any level differences stemming from the differences in property rights would be captured in the pre-treatment price differences. There were no changes in property right or land lease legislation during our study period that would undermine this assumption.

4 | DATA

Our empirical analyses utilize data that include detailed information on both land lease contracts and housing transactions in Helsinki. The data provided by the city of Helsinki contain detailed information on land ownership - including whether land is leasehold or freehold, rents on leasehold land, and the beginning and end dates of land lease contracts. The apartment-level data from the Central Federation of Finnish Real Estate Agencies (Kiinteistönvälitysalan Keskusliitto ry) cover all housing transactions conducted via real estate agents in Helsinki during 2003–2021.[6] These data provide information on unit characteristics (details on structure and exact location) and on sales prices. The two datasets are combined to allocate land lease contract details to each transaction on leasehold land.

Our identification is based on within-neighborhood variation. We construct the neighborhoods by first clustering observations in the treatment group by density-based spatial clustering (DBSCAN clustering in QGIS) with a minimum of 50 observations per cluster and a maximum distance between two treated observations of 800 m. We then create a buffer of 800 m around the

[5] That is, we exclude all other housing types (e.g., single-family homes and terraced houses) from our sample.

[6] The data cover about 70% of all transactions.

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TABLE 1 Descriptive statistics, baseline sample.

	Treatment group n= 4545		Control group n=6439	
	Mean	Std. dev	Mean	Std. dev
Sale price (€)	186,982	70,673	200,113	95,411
Price per sq. meter (€)	3643	1011	4335	1281
Exp. inc. in maint. costs (€ /year)	1130	466	0	0
Living area	53.0	17.8	47.6	20.4
Rooms	2.3	0.9	2.0	1.0
Age	52.0	5.8	50.8	6.0
Condition = poor (0/1)	0.06	0.23	0.07	0.25
Condition = decent (0/1)	0.42	0.49	0.45	0.50
Condition = good (0/1)	0.48	0.50	0.43	0.50
Condition = excellent (0/1)	0.00	0.06	0.00	0.05
Condition = unknown (0/1)	0.04	0.20	0.05	0.21
Balcony (0/1)	0.58	0.49	0.57	0.50
Sauna (0/1)	0.01	0.09	0.01	0.09
Floor number	2.7	1.5	2.9	1.8
Floors in the building	4.3	1.8	4.6	2.1
Distance to city center (m)	9726	2186	7404	2654

Note: All prices are deflated to December 2021 using consumer price index.

observations in the treatment group and define these spatial unions as neighborhoods. Finally, we assign units in the control group to these neighborhoods. If a unit in the control group is located within two treatment neighborhoods, it is assigned to the one with the closest treatment observation. For a neighborhood to be included in our sample, we impose a minimum criterion of 50 leasehold and 50 freehold transactions per neighborhood. We drop missing and clearly erroneous data, and outliers where the logarithm of deflated prices or maintenance costs per square meter are further than three standard deviations from the mean.[7]

Our baseline sample contains 10,984 transactions of which 4545 and 6439 are leaseholds and freeholds, respectively. In total, our sample contains 7.3% of all condominium unit transactions in Helsinki during the analyzed period. Figure A3 in the appendix illustrates the neighborhood clusters as well as the observations in the treatment and control groups, in the final estimation sample.[8]

Table 1 presents the descriptive statistics for the data. The average expected increase in unit-level maintenance costs due to land rent increase is 1130 euros per year, varying from 375 to 3693 euros. Leaseholds in our sample are, on average, cheaper, which would suggest that there is a

[7] Estimation results are robust to alternative conventional outlier definitions.

[8] Before the geographic matching procedure described above, our age-restricted sample contains 4606 potential treated units and 15,893 potential control units. Geographic matching at the 800 m buffer reduces this to the baseline estimation sample of 4545 treated and 6439 control units. The reduction at the treated group is small (61 units, 1.3%), reflecting that treated units already form geographic clusters; the larger reduction in the control group (9454 units, 59%) reflects that many freehold units are located far from any treated dwelling and are therefore not useful as controls for our identification strategy.

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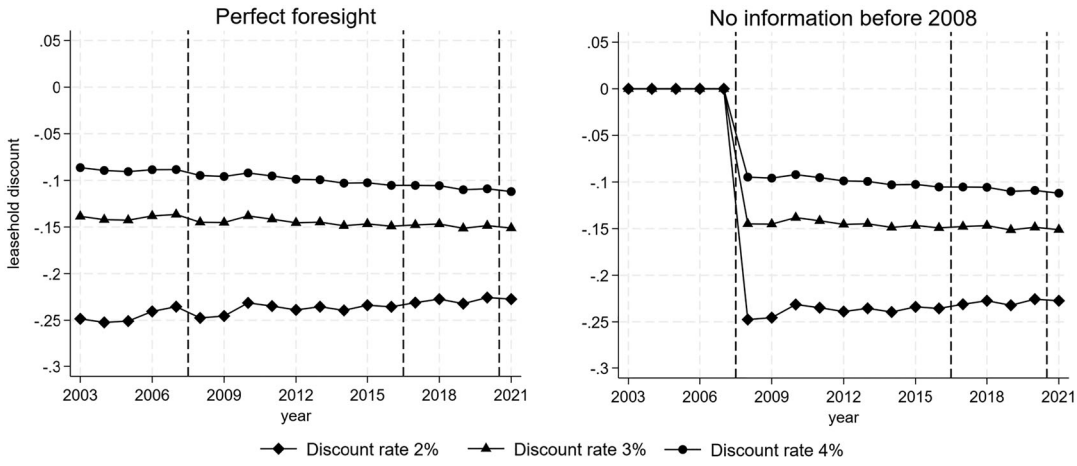


FIGURE 2 Expected leasehold discounts at different discount rates: (i) in the presence of perfect foresight and (ii) assuming no knowledge on rental contract renewal before 2008. Note: The dashed vertical lines represent the first official announcement of land rent renewal policy in January 2008, the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021.

discount associated with leasehold units, as one would expect. In addition, leaseholds locate, on average, further away from the city center. This unconditional gap largely reflects the fact that freehold units are more concentrated in the neighborhoods closest to the center. Within neighborhoods the gap shrinks substantially: conditional on neighborhood fixed effects, the balance test in Table A7 indicates that leaseholds are located on average about 409 m further from the center, down from roughly 2300 m unconditionally. This residual within-neighborhood difference remains statistically significant but is modest in magnitude. It is also stable over time, as the interactions of treatment with the post-announcement periods are statistically insignificant (Table A7), and it is in any case controlled for directly through the distance-to-center covariate in all specifications. Consistent with this, tightening the neighborhood definition so that treated and control units are compared only within progressively smaller areas leaves the estimated discount essentially unchanged, and if anything slightly larger at the 400 m bandwidth (Table 4). This indicates that the residual distance difference does not drive our results. All characteristics regarding structure quality are similar.

Figure 2 illustrates the kind of leasehold discounts we should observe, at different discount rates, in the case that the expected land rent increases fully capitalize into leasehold prices. These expected leasehold discounts are computed based on two different underlying assumptions: (i) full capitalization with perfect foresight; (ii) full capitalization but no knowledge about land rent contract renewal before the 2008 announcement (this reflects an assumption that until 2007, agents expected prevailing leasehold contracts to continue infinitely), and perfect foresight after 2008.

The computations in Figure 2 are based on the assumption that the annual land rent increase taking effect in 2021 equals our sample mean (1130€). The land rent increase is expected to be enforced in stages, as explained in Section 2: 50% in 2021, and thereafter gradual 5% annual increases, with the land rents reaching the contract level in 2031. After 2031, the real land rents for leaseholds are assumed to remain constant, reflecting the actual contract terms. For each year, we calculate the present value (PV) of future land rents using three different discount rates. The expected discounts plotted in Figure 2 are ratios of the PV of future land rents to the value of the unit. The value of the unit (i.e., the denominator) in 2003 is the sample mean price of freehold

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units in our sample in that year. After 2003, the freehold unit price is based on the actual observed real price movements in the market based on a hedonic price index. That is, the denominator reflects the actual price level in each year, and does not necessitate any arbitrary assumptions regarding the price development.[9] During 2003–2021, the observed annual geometric average freehold price increase was 2.5%.

Previous studies provide a wide range of estimates for the real rate of return on housing, ranging from 6.0% (Flavin and Yamashita 2002) to 2.5% (Piazzesi et al. 2007). In the land lease context, Bracke et al. (2018) report discount rates for a 50-year claim in London taking values between approximately 4% (1987–1991) and 2% (2009–2013). Considering the low levels of real risk-free interest rates during our sample period (that are reflected in the real housing loan rates, the average of which was 2.24% in Finland during 2003–2021 and only 0.75% in 2021), the discount rate should be relatively low in the analyzed period. Given this and the findings reported in the previous literature, we graph the expected leasehold discounts using 2%, 3%, and 4% real discount rates.

Under assumption (i) (perfect foresight) the leasehold discount in 2003 should have been 8.6%, 13.9% and 24.8% in the 4%, 3% and 2% real discount rate cases, respectively. Assuming (ii) (no information before 2008) and a real discount rate of 3%, there would have been no price difference between leaseholds and freeholds before 2008, but in 2008 we should observe a treatment effect of −14.5% on the price of leasehold units subject to land rent increases in 2021. In 2021, the average leasehold discount should have been in the range of 11.2%, 15.1%, or 22.7% based on the 4%, 3%, or 2% discount rate assumptions, respectively.

5 | ESTIMATION RESULTS

5.1 | Leasehold discount

In this section, we estimate the evolution of leasehold discounts over the complete sample period. We use DID specifications with a treatment dummy as well as a continuous treatment of eventual yearly lot rent increase in thousands of euros. Figure 3 presents the evolution of the coefficients of the price differential between leasehold and freehold units over time (year 2007 is omitted).[10]

As shown in Figure 3, the coefficients that present the development of the price difference between leaseholds and freeholds are close to zero and statistically insignificant throughout the entire period before the first announcement, suggesting that the market did not anticipate the future lot rent increases.[11] In addition, there is no baseline price difference between freehold and leasehold units in 2007. These findings are at odds with the assumption of full optimization with

[9] We use the actual price level as the denominator to give the rational price discount compared to the freehold price, as the rational discount in each year is given by PV of future rental cash flows divided by the freehold price level in the given year.

[10] Estimation results with full set of coefficients can be found in Tables A1 and A2 in the appendix.

[11] A direct test of the parallel-trends assumption fits common and treatment-specific linear trends over the pre-period and tests the treatment-specific slope. The differential pre-trend slope is statistically insignificant in both specifications (treatment dummy: −0.005 per year, $F(1, 106) = 1.22$, $p = 0.27$; continuous treatment: −0.005, $F(1, 106) = 1.94$, $p = 0.17$), and remains insignificant when the window is restricted to 2005–2007 (−0.011 per year, $p = 0.18$). A joint F-test that the pre-period treatment-year coefficients equal zero likewise fails to reject ($F(4, 111) = 0.46$, $p = 0.77$ and $F(4, 111) = 0.61$, $p = 0.66$).

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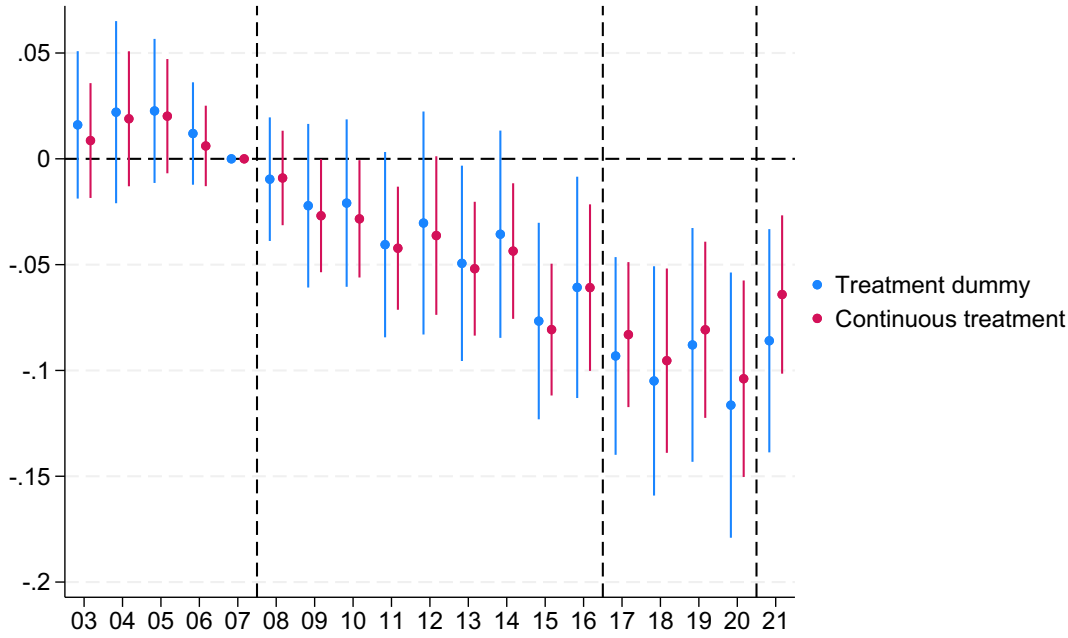


FIGURE 3 Evolution of leasehold discount over time. Note: The figure plots the dynamic treatment effects based on Equation (1). The baseline difference in 2007 with treatment dummy is −0.009 (s.e. 0.018) and with continuous treatment 0.003 (s.e. 0.013). The dashed vertical lines represent the first official announcement of land rent renewal policy in January 2008, the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021.

perfect foresight, and support the hypothesis that the forthcoming rent renewals were not reflected in leasehold prices prior to the information shock of 2008.

Given that the freehold and leasehold prices share a parallel trend before 2008, our analysis plausibly captures the causal effect of the contract renewal policy introduction on leasehold prices. After the treatment in January 2008, the leasehold discounts gradually increase, however, the discounts remain relatively small through the early 2010s and become consistently large and significant from 2015. This clearly differs from the expected capitalization patterns shown in Figure 2 ii. The discounts converge to around 10% after information concerning the financial consequences for our treatment group had been released. We do not observe statistically significant differences in leasehold discounts between 2020 and 2021 when the new lot rents became effective, suggesting that the information was capitalized before the new land rents became effective.

The observed sluggish capitalization plausibly reflects gradually increasing awareness of the financial consequences of land tenure and indicates inefficient utilization of public information. Alternatively, for the first years after the policy announcement, the lack of capitalization could be explained by leasehold unit owners doubting that the municipality will enforce the policy. However, as the municipality enforced the policy for the earlier land rent renewal batches in 2011 and 2016, such explanation is unlikely to explain the observed pattern throughout the period. That is, the market participants should have been able to foresee the forthcoming renewal policy based on the rent renewals for the earlier batches.

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TABLE 2 Estimation results.

Treatment:	Dummy	Continuous
	(1)	(2)
Treatment	0.004	0.013
	(0.014)	(0.010)
Treatment × (2008–2016)	−0.052***	−0.053***
	(0.018)	(0.014)
Treatment × (2017–2021)	−0.111***	−0.095***
	(0.023)	(0.018)
Housing attributes	Yes	Yes
Neighborhood FE	Yes	Yes
Year FE	Yes	Yes
Observations	10,984	10,984
R-squared	0.910	0.911

Note: The table reports the results of estimating Equation (2). Column 1 reports results with a treatment dummy and Column 2 with continuous treatment. The dependent variable is the log of price. Standard errors are clustered at 250 m × 250 m squares and are reported in parentheses $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$.

The DID results with pooled time periods in Table 2 are in line with the yearly results presented in Figure 3.[12] There is no significant price difference between the treatment and control groups before 2008, as the unconditional main effect is close to zero and statistically insignificant. The coefficients for the first post-announcement period (2008–2016) are negative and significant, suggesting that the market reacted to the announcements and increasing information in the market. The leasehold log-discounts remain, however, relatively small during the first post-announcement period, −0.052 and −0.053, raising doubts regarding the efficient capitalization of information in the leasehold prices (see Figure 2 for comparison). For the second post-announcement period (2017–2021), the log-discounts are −0.111 and −0.095. By that period, the 2011 and 2016 renewals had taken place, all lot-specific information had been released, market participants had gained experience on renewals and higher maintenance fees caused by the earlier renewals were observable.

These log-discounts translate to a −10.5% (−28,331€) average price effect in the specification with treatment dummy and a −9.1% (−24,422€) price effect when annual lot rents increase by 1000€ in the specification with continuous treatment. The euro amounts are computed at the average freehold price in our sample during the second post announcement (2017–2021) period, that is, 269,565€.[13]

[12] Estimation results with full set of coefficients can be found in Tables A3 and A4 in the appendix.

[13] Visual inspection of Figure 3 may suggest a slight downward drift in the pre-period point estimates between 2005 and 2007, though this drift is not statistically significant individually or jointly. We have therefore re-estimated the three-period DID with a more conservative pre-period definition (2003–2005), excluding 2006 and 2007 from the pre-period. The post-2017 discount is essentially unchanged at −0.118 (vs. −0.111 in the baseline) in the treatment-dummy specification and −0.100 (vs. −0.095) in the continuous-treatment specification.

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TABLE 3 Implied discount rates.

Panel A: Implied discount rates for 50-years		
	Treatment dummy	**Continuous treatment**
Implied discount rate	2.45	2.48
95% Confidence interval	0.69–5.49	0.81–5.27
Panel B: Implied discount rates to perpetuity		
	Treatment dummy	**Continuous treatment**
Implied discount rate	3.68	3.70
95% Confidence interval	2.67–5.97	2.73–5.79

Note: Implied discount rates are calculated assuming that land rent policy remains constant and real value of land increases by 0.7 percent yearly, as in Giglio et al. (2015).

5.2 | Implied discount rates

A well-known empirical challenge in public finance is that the effect of taxes on asset prices is determined by both the degree of capitalization and the discount rate (e.g., Yinger 1982). Hence, the capitalization rate can be identified only if we know the discount rate, and vice versa. Nevertheless, the investigation of implied discount rates for future expected rental payments provides interesting information. In particular, an unreasonably high implied discount rate could indicate that the future rental payments have not fully capitalized into leasehold prices.

Table 3 reports the implied real discount rates for the estimated leasehold discounts when all relevant information about the land rent renewals for batch 2021 has been released (2017–2021). Hence, the implied discount rates for this period provide insights on how households value the long-run financial flows - assuming that the future land rents have fully capitalized into prices. Panel A shows the implied discount rates for the 50-year rent period and Panel B when land rent increases are discounted in perpetuity. The estimated leasehold discounts translate into average implied real discount rates between 2.5% and 3.7% depending on the length of the discounting period and the model specification.

These discount rates are in line with previous studies, and we argue that they are realistic in the studied time period thereby being consistent with full capitalization in 2017–2021. For example, Giglio et al. (2015) suggest that households apply a 2.6% discount rate more than 100 years in the future for land lease contracts in the UK for the period 2004–2013 and Singapore for the period 1995–2013. Bracke et al. (2018) add that long-term discount rates may change in time and confirm the earlier findings reporting a discount rate between 2% and 3% in the UK for the period 2004–2013. Correspondingly, Gautier and Van Vuuren (2019) report a 4% long-term discount rate for land lease contracts in Amsterdam for the period 1985–2017.

5.3 | Robustness and identifying assumptions

The key identifying assumption in our analyses is that any unobservable time-varying factors are not correlated with the treatment effect. In this section, we investigate the validity of this assumption. We begin by scrutinizing the close correspondence of our treatment and control group units. We then verify that the differential tax treatment of freeholds and leaseholds and the phase-out of mortgage-interest deductibility do not account for the estimated discount. Next, we present

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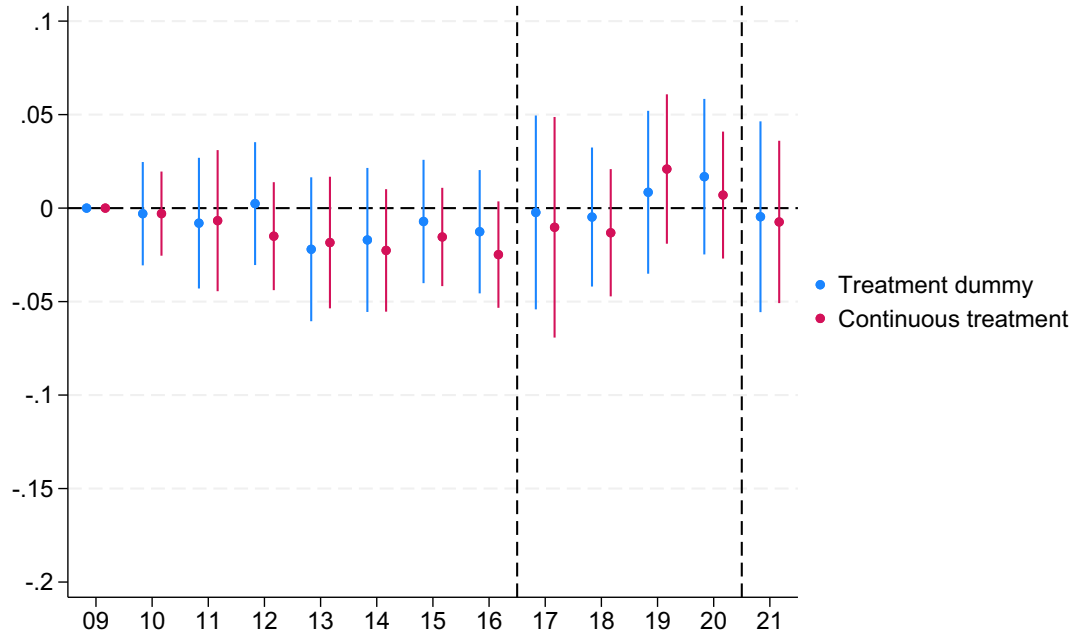


FIGURE 4 Treatment effect on rents. Note: The figure reports the yearly DID estimates for models where the dependent variable is the log of monthly rent. Rent data is only available from 2009 onwards, and the first year (2009) is omitted. The baseline effect in 2009 with treatment dummy is −0.019 (s.e. 0.015) and with continuous treatment −0.008 (s.e. 0.013). Housing attributes include: log(area), log(age), dummies for number of rooms (1,2,3), dummies for condition (poor, decent, good, unknown), distance to Helsinki city center, and dummy for fixed term rental contract. Neighborhood and year fixed effects as well as clustering of standard errors are similar to those in the main estimations. The dashed vertical lines represent the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021.

balance tests to ensure that the treatment and control groups are stable over time. Finally, we evaluate the robustness of our results to alternate neighborhood definitions.

To evaluate the correspondence between the treatment and control units, we turn to their value in use. We take advantage of the fact that in the Finnish condominium system the unit owner has the right to lease their unit in the rental market without any restrictions. We argue that if the groups are otherwise corresponding, it should only affect the investment value, and not the use value, whether a unit is freehold or leasehold. Hence, any unobserved quality differences between free- and leaseholds (e.g., differences in micro-location or structure quality) would be reflected in the rents of these units.

We analyze potential quality differences by conducting DID analysis with rental data in our sample neighborhoods in the period 2009–2021.[14] We regress the log of unit rent with yearly treatment effects (2009 is omitted) and control the dwelling characteristics as well as year and neighborhood fixed effects. These results are presented in Figure 4.[15] According to the results, there is no baseline difference between freehold and leasehold units in 2009. In addition, the yearly effects are close to zero for the entire sample period. We conclude that

[14] We use data from multiple listing site "Etuovi". Unfortunately, we only have data on advertised rents for the period starting in 2009.

[15] Descriptive statistics of rental data and estimation results can be found in Tables A5 and A6 in the Appendix.

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notable systematic unobserved quality differences between the free- and leasehold units are unlikely.

Beyond unobserved quality, two known institutional features could in principle generate a treatment-correlated trend. Condominiums on freehold land pay property tax on the land component while leaseholds do not, and the land-tax rate in Helsinki rose over the sample period; at the same time, the deductibility of mortgage interest was phased out between 2012 and 2022, which bears differentially on freeholds and leaseholds because they carry different mortgage balances. The two channels move the freehold–leasehold cost gap in opposite directions and largely offset. To verify that their net movement does not drive our estimates, we construct a year-specific freehold-leasehold net cost differential that combines both channels, setting the property-tax and interest-shield components toward their upper bounds so that the resulting series overstates the true differential,[16] and we re-estimate Equation (2) with this differential interacted with the treatment indicator. Because the differential is a function of calendar year, its level is absorbed by the year fixed effects and only the treatment interaction is identified. The capitalization estimates are essentially unchanged: in the treatment-dummy specification the 2008–2016 coefficient moves from −0.052 to −0.044 and the 2017–2021 coefficient from −0.111 to −0.116, with the continuous specification behaving analogously, and the net cost interaction is small and statistically insignificant in the dummy specification and only marginally significant (10% level) and economically negligible in the continuous specification (Table A9). Differential taxation and the mortgage-interest deduction phase-out therefore do not account for the leasehold discount we estimate.

Furthermore, the parallel trend assumption requires that differences between the treatment and control groups are stable over time and that the land lease renewals are not associated with changes in the qualities of the units transacted. We investigate this with balance tests, where we examine if the announcements related to land rent renewal policy were associated with changes in the attributes of units transacted. The results are presented in Table A7 in the Appendix and do not indicate any significant changes in the composition of the transacted units in regard to observable housing attributes.[17]

Finally, as the distance used to create our neighborhood clusters in our estimations is chosen ad hoc, we explore the robustness of our results to varying neighborhood definitions. The results of these robustness tests using three time periods are reported in Table 4.[18] Columns

[16] The differential combines the Helsinki statutory land-tax schedule (0.7%, 0.8%, and 0.93% over 2003–2009, 2010–2016, and 2017–2021, applied to an upper-bound taxable land value obtained with a relative fiscal value of 0.2 and a 30% land share) with the 2012–2022 phase-out of mortgage-interest deductibility (fully deductible through 2011, 85% in 2012, then −5 percentage points per year; capital-income tax rate 29%/28%/30%), the interest shield valued at an LTV of 0.80 times the prevailing rate on new mortgages. Both components are set toward their upper bounds, so the series overstates the true differential. The resulting nominal series is deflated to 2021 euros with the cost-of-living index (Elinkustannusindeksi, 1951:10 = 100), consistent with the real price measure used throughout, and is used only as a control.

[17] We also estimated Equations (1) and (2) allowing all property-characteristic controls to be interacted with the treatment indicator, so that the implicit prices of unit attributes may differ between treatment and control units. The post-2017 leasehold discount is somewhat smaller in absolute terms in this fully interacted specification (−0.077, s.e. 0.025, in the treatment-dummy and −0.079, s.e. 0.021, in the continuous specification, versus −0.111 and −0.095 in the baseline) but remains statistically significant at the 1% level, and the dynamic pattern is unchanged. The attenuation is consistent with the interacted specification absorbing part of the discount through treatment-specific attribute prices; the implied long-run discount rates remain in the range reported in Section 5.2.

[18] Yearly effects across the alternative neighborhood definitions are presented in Figure A5 in the Appendix. Joint F-tests of the pre-period treatment-year coefficients (Treatment × 2003 through Treatment × 2006) fail to reject the null of no pre-

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TABLE 4 Robustness for neighborhood definition.

Neighborhood	400 m	800 m	1200 m	2000 m	Zip
	(1)	(2)	(3)	(4)	(5)
Treatment	0.012	0.004	0.013	0.011	0.002
	(0.012)	(0.014)	(0.013)	(0.013)	(0.012)
Treatment × post1	−0.047***	−0.052***	−0.058***	−0.062***	−0.038*
	(0.017)	(0.018)	(0.017)	(0.016)	(0.019)
Treatment × post2	−0.120***	−0.111***	−0.124***	−0.125***	−0.080***
	(0.018)	(0.023)	(0.023)	(0.023)	(0.020)
Housing attributes	Yes	Yes	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	7371	10,984	12,808	15,294	6619
R-squared	0.917	0.910	0.907	0.905	0.915

Note: The dependent variable is the log of price. Standard errors are clustered at 250 m × 250 m squares and are reported in parenthesis. $* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$.

1 to 4 report results with varying maximum buffers around the treated observations, Column 2 reporting the definition identical to our baseline analysis. In Column 5, we define neighborhoods simply based on their zip codes. We conclude that the results are qualitatively the same as the baseline results and that our results are not sensitive to the neighborhood definition.

6 | WHAT COULD EXPLAIN THE SLOW CAPITALIZATION?

In this section, we explore potential explanations for the surprisingly sluggish capitalization of information we observed in our baseline results. First, in Section 6.1, we analyze whether the slow adjustment could be a result of sellers adjusting their reservation prices to the new information slowly. We then have a more detailed look at the price dispersion patterns during our observation period in Section 6.2, and finally look for indications of heterogeneity in homebuyers' ability to adjust to new market information in Section 6.3.

6.1 | Slow adjustment of sellers' reservation prices

A potential explanation for the capitalization dynamics observed in Section 5 is that apartment sellers adjust their reservation prices more sluggishly after shocks than the agents looking

trend at all five neighborhood definitions, though the *p*-value falls notably as the neighborhood buffer is widened: $F(4, 75) = 0.23$, $p = 0.92$ at the 400 m buffer; $F(4, 111) = 0.46$, $p = 0.77$ at the 800 m baseline; $F(4, 141) = 1.17$, $p = 0.33$ at the 1200 m buffer; $F(4, 166) = 1.96$, $p = 0.10$ at the 2000 m buffer; and $F(4, 55) = 0.75$, $p = 0.56$ using the zipcode definition. We use the 800 m buffer as our baseline because it balances sample size against the apples-to-apples quality of the comparison: larger buffers include neighborhoods that are increasingly heterogeneous with respect to location, structure and unobserved local characteristics, which is reflected both in the rising *F*-statistic and in the visible increase in individual pre-period point estimates (e.g., the 2003 coefficient is individually significant at the 2000 m buffer).

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to buy do. Such slow seller reservation price reaction is incorporated in several theoretical considerations (e.g., Berkovec and Goodman 1996; Hort 2000; Novy-Marx 2009). The seller reservation price hypothesis would suggest that after a negative shock, such as the announcement of the land rent renewal policy, the transaction volume of treated units decreases and time-on-market increases temporarily, and that the prices adjust slowly to their new equilibrium level.

Although it seems unlikely that such dynamics could distract the market for as long a period as we observe, we explore the possibility formally. First, in Figure A6, we plot the annual share of leaseholds in our sample which depicts no trend in the share of leasehold units sold. We also investigate whether the policy affected the selling times of units. We estimate the treatment effects for (the log of) the selling time by modifying the pooled specification in Equation (2). As results in Table A8 show, there is no statistical or observable trend in the coefficients demonstrating changes in the sale time of leaseholds. With these findings, we conclude that it is unlikely our results could be explained by the slow reservation price adjustment of sellers.

This result can also be interpreted as further evidence of the absence of selection, reinforcing the results of balance tests. In the presence of selection, we would expect the share of leasehold properties to be time-variant.

6.2 | Dispersion

When information is costly or imperfect, market equilibrium may exhibit price dispersion even for homogeneous goods (see, e.g., Stigler 1961). Existing empirical work links shocks that improve market information to decreasing price dispersion (Jensen 2007; Aker 2010; Ben-Shahar and Golan 2019).

We hypothesize that one plausible explanation for the gradual capitalization pattern is the fact that the quantification of new land rents imposes a high cognitive burden and requires significant effort (i.e., information is costly). Alternatively, some buyers might have doubted that the municipality would enforce the land rent increases. These factors would translate into an increase in price dispersion after the first announcement in 2008 and a decreasing price dispersion development afterwards as market participants became more familiar with, or convinced of, the land rent renewal policy.

We estimate the changes in price dispersion following the methodology of Ben-Shahar and Golan (2019). We first obtain price residuals (the difference between the actual and predicted price) from our baseline model (Equation 1). We then regress the absolute values of the price residuals on the treatment indicator and its interaction with time period. The periods are otherwise defined as in Equation (2), but we separate year 2021 (the year when new rent levels became effective) as its own period.

According to Table 5, the level of price dispersion is lower in the leasehold group before the announcement of land rent policy in 2008, which suggests that the leasehold group could have been more homogeneous than the freehold group. During 2008–2016, we observe a statistically significant increase in the leasehold price dispersion. The magnitude of this increase is about 15% (the average price dispersion being 0.091). For the period 2017–2020, when all relevant information has been released, the point estimate remains at the same level, but the standard error increases. In 2021, the price dispersion returns to the pre-treatment level. Overall, the observed pattern for price dispersion suggests heterogeneous capitalization of the new policy across individuals.

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TABLE 5 Price dispersion.

Dependent variable: residuals from the baseline model	
Treatment	−0.0103**
	(0.0044)
Treatment × [2008–2016]	0.0132***
	(0.0050)
Treatment × [2017–2020]	0.0131*
	(0.0074)
Treatment × 2021	0.0013
	(0.0061)
Observations	10,984
R-squared	0.0029

Note: Standard errors are clustered at 250 m × 250 m squares and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6.3 | Heterogeneous capitalization

The sluggish capitalization and changing dispersion patterns presented above could be a product of the market consisting of two types of buyers: those who price the announced policy and those who do not. Such buyer-level heterogeneity is precisely what theories of complex valuation and bounded attention would predict in our setting. The quantification of future land rent payments at the unit level imposes a substantial cognitive burden, and the empirical literature documents that behavioral biases tend to be stronger when valuation tasks are more challenging (Hirshleifer et al., 2013; Kumar, 2009) and when prices are nonlinear or imperfectly salient (Chetty et al. 2009; Shaffer 2020). If some households remain uninformed or unconvinced about the announced policy, or are unable or unwilling to translate the announced rent increases into present-value terms, their participation in the market would mask the discount applied by informed and convinced buyers and translate into greater dispersion in transaction prices.

To investigate potential heterogeneity in capitalization, we look at the yearly distribution of prices. First, we predict a transaction price for each of the leasehold units in our sample, assuming that they were freehold apartments by using our baseline regression (Equation 1). This prediction represents the price that, based on the observable characteristics of the unit, would be the fair price of the unit if it was on freehold land—or if the buyers completely ignored the future land rent increases in their bids. We term this price the *noncapitalized price*. Second, we calculate what would be the fair price for the unit if the future land rent increases were fully capitalized in the price (*capitalized price)*. We do this by deducting from the *noncapitalized price* the leasehold discount that would prevail if calculated as in Figure 2, but using a discount rate of 3.7% (i.e., the implicit discount rate that we observe at the end of our observation period).

Based on these prices, we assign transactions to three groups: noncapitalized, in-between and capitalized transactions, as illustrated in Figure 5. Using the distance between the capitalized price and the noncapitalized price in each transaction as a scale, we define those leasehold transactions for which the price is either lower than the capitalized price or for which the scaled deviation from the capitalized price is less than 0.33 as "capitalized". Similarly, transactions for which the scaled distance from the capitalized price is higher than 0.66 are deemed "noncapitalized."[19] The group for which the transaction price falls into the middle range of the predicted

[19] This approach implicitly assumes that all deviations from predicted price are due to leasehold site.

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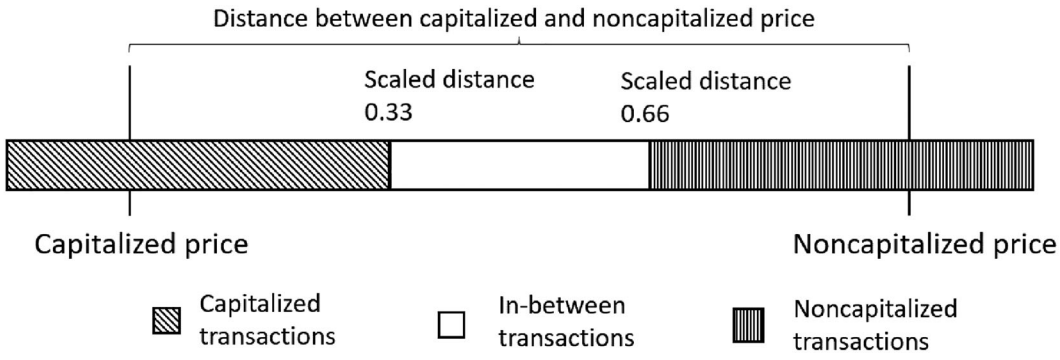


FIGURE 5 Labeling of capitalized, in-between and noncapitalized transactions.

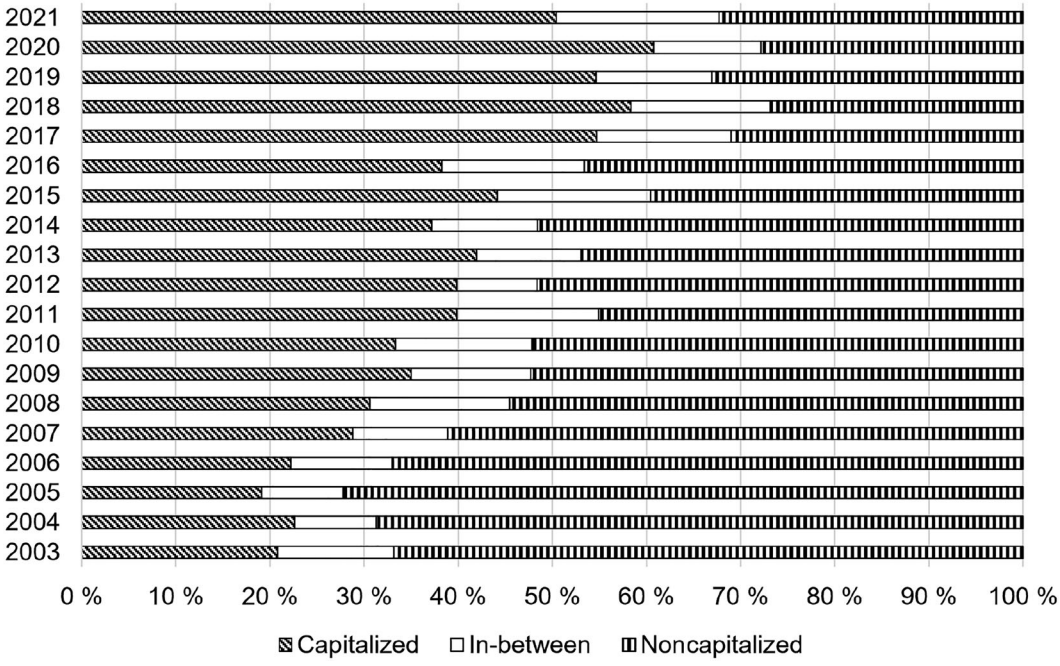


FIGURE 6 Annual distribution of capitalized, in-between and noncapitalized transactions

non-capitalized and capitalized price we term as "in-between." For each year, we then count the number of noncapitalized, in-between and capitalized transactions. This distribution is presented in Figure 6.

According to Figure 6, the distribution of transactions in the capitalized and noncapitalized groups systematically changes over our observation period as information on the new policy and its execution accumulates. In addition, the size of the group of in-between buyers remains relatively stable throughout the period. On average, 13% of transactions fall into this group, the number being at its smallest (9%) in 2004, 2005, and 2012, and at its highest (17%) in 2021. Moreover, the size of the noncapitalized group decreases rather consistently from the level of 67%–72% in 2003–2006 to about 30% in 2018–2021, while the size of the capitalized group increases correspondingly.

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These observations suggest that the market is segmented and that the average leasehold discounts we observe in Section 5 could represent an average of the pricing applied by these groups. Identifying the precise sources of this segmentation—financial literacy, attention, prior experience with leasehold units, or the credibility buyers attach to the city's enforcement of the policy—would require buyer-level data beyond what we observe. What we can establish is that substantial heterogeneity in pricing behavior coexists within the same neighborhoods and time periods, and the share of buyers pricing the announced policy grows systematically as information accumulates.

7 | CONCLUSIONS

This study provides causal evidence on the reaction of housing market participants to information shocks regarding a complex policy scheme affecting the user cost of housing. Our unique quasi-experimental setup enables the investigation of housing price adjustment process after an information shock concerning future cash flows. This allows us to contribute to several strands of literature, including those studying the capitalization of taxes and other financial flows, the long-run discount rates, the informational efficiency of asset markets, in addition to the literature on housing price dynamics and the pricing of housing located on leasehold land. The study also relates more generally to the behavioral economics literature.

We focus on a policy announcement regarding significant land rent increases in Helsinki, Finland. By comparing transactions of leasehold units that were subject to land lease contract renewal in 2021 with transactions of corresponding freehold units, we are able to study how the announcement of future housing cost increases capitalized in the leasehold housing prices. We find that before the first announcement of the land lease contract renewal policy in 2008, households did not properly take into account the financial consequences of the units' land tenure in their home-buying decisions. After the first policy announcement, the prices of leasehold and freehold units gradually diverged during 2008–2021. The observed leasehold discount increase is substantially slower than that predicted based on present value of future land rent payments.

Our results suggest that the capitalization of future land rents improved as the information and experience on land rent increases accumulated in the housing market over a decade. During this adjustment period the price dispersion of leasehold units was elevated, providing suggestive evidence of the market having been segmented in the way the buyers utilized information. The share of transactions where we observe full or substantial capitalization systematically increases over time, and the estimated leasehold discounts are in line with perfect capitalization with a reasonable discount rate assumption at the end of the analyzed period.

In addition to the increased price dispersion, our novel finding is that in the analyzed case, capitalization is neither perfect nor absent but rather a gradual and slow process. To the stream of literature on long-term discount rates, we contribute by estimating the discount rate based on a causal framework utilizing changes in future land rent payments. Despite the differences in institutional framework, analyzed period and empirical strategy, our implied discount rate for 50-year housing claims falls into the range suggested by Giglio et al. (2015) and Bracke et al. (2018).

In terms of the housing market, our analysis suggests that changes in public policies, such as large land rent increases or changes in tax code, may create significant inefficiencies and mispricing, at least in the short term. Our analysis in Section 6.3 documents that the housing market in this setting is segmented: substantial heterogeneity in capitalization coexists across buyers within the same neighborhoods and time periods, and the share of transactions consistent with full capitalization increases systematically as information accumulates. Identifying which buyer

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characteristics—financial literacy, attention, prior leasehold experience, or others—drive this segmentation is beyond the reach of our data, but is an important question for future research. If, for instance, buyers with weaker financial literacy systematically fail to capitalize future rent obligations, the financial burden of mispricing falls disproportionately on these groups, with potential distributional consequences across socioeconomic lines. We see this as an important direction for follow-up research using transaction-level micro-data linking buyers to attributes.

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We thank Nikodem Szumilo, Tracy Turner, and participants and discussants at several conferences for helpful comments and suggestions. We gratefully acknowledge the financial support from the Strategic Research Council at the Research Council of Finland (grant numbers 352450 and 352451), Research Council of Finland (grant number 324393), OP Group Research Foundation, and West-Finland Housing Association of Public Utility. All errors that remain are ours.

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APPENDIX
ADDITIONAL FIGURES AND TABLES

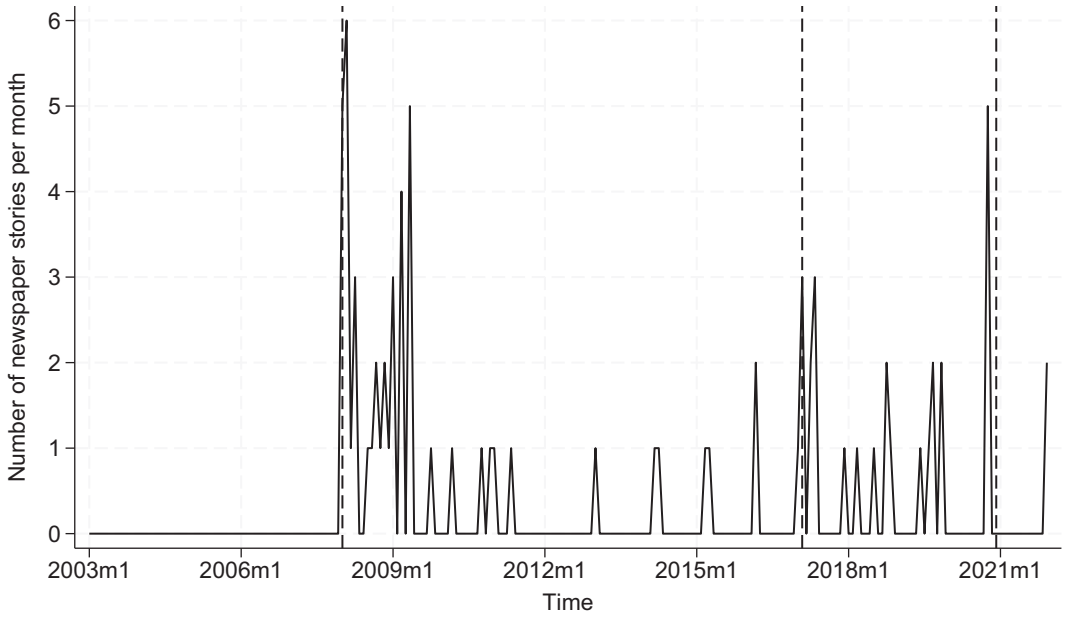


FIGURE A1 Number of newspaper stories regarding the land rent increases in Helsinki. Note: We have manually collected all news items from the Sanoma Archive (a database covering all articles published in the three most popular local and nationwide mainstream newspapers—Helsingin Sanomat, Taloussanomat, and Ilta-Sanomat) that address ground rent increases in Helsinki during the period 2003–2021. The dashed vertical lines represent the first official announcement of land rent renewal policy in January 2008, the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021.

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TABLE A1 Yearly DID estimates, full set of coefficients (Part A).

	(1) Treatment dummy		(2) Continuous treatment	
	Coef.	**Std. err.**	**Coef.**	**Std. err.**
Treatment	−0.009	(0.018)	0.003	(0.013)
Treatment × year (2007 omitted)				
Treatment × 2003	0.016	(0.018)	0.008	(0.014)
Treatment × 2004	0.022	(0.022)	0.018	(0.016)
Treatment × 2005	0.022	(0.017)	0.019	(0.013)
Treatment × 2006	0.012	(0.012)	0.005	(0.010)
Treatment × 2008	−0.009	(0.015)	−0.009	(0.011)
Treatment × 2009	−0.023	(0.020)	−0.028**	(0.014)
Treatment × 2010	−0.022	(0.020)	−0.030**	(0.014)
Treatment × 2011	−0.041*	(0.022)	−0.043***	(0.015)
Treatment × 2012	−0.031	(0.027)	−0.037*	(0.019)
Treatment × 2013	−0.050**	(0.023)	−0.053***	(0.016)
Treatment × 2014	−0.036	(0.025)	−0.044***	(0.016)
Treatment × 2015	−0.078***	(0.024)	−0.082***	(0.016)
Treatment × 2016	−0.061**	(0.026)	−0.062***	(0.020)
Treatment × 2017	−0.094***	(0.024)	−0.084***	(0.017)
Treatment × 2018	−0.105***	(0.027)	−0.096***	(0.022)
Treatment × 2019	−0.088***	(0.028)	−0.082***	(0.021)
Treatment × 2020	−0.117***	(0.032)	−0.105***	(0.023)
Treatment × 2021	−0.086***	(0.027)	−0.065***	(0.019)
log(area)	0.653***	(0.020)	0.661***	(0.020)
log(age)	0.176*	(0.095)	0.153	(0.094)
Distance to CBD (m)	−0.000	(0.000)	−0.000	(0.000)
Balcony	0.004	(0.004)	0.005	(0.004)
Sauna	0.009	(0.026)	0.009	(0.027)
Number of rooms (4+ rooms omitted)				
1 room	−0.177***	(0.022)	−0.186***	(0.023)
2 rooms	−0.177***	(0.014)	−0.187***	(0.015)
3 rooms	−0.127***	(0.012)	−0.133***	(0.012)
Condition (decent omitted)				
Poor	−0.067***	(0.005)	−0.068***	(0.005)
Good	0.084***	(0.003)	0.084***	(0.003)
Excellent	0.114***	(0.018)	0.113***	(0.018)
Unknown	0.019**	(0.009)	0.018**	(0.009)

Note: The dependent variable is the log of price. Continued in Table A2. Both columns report estimates from a single regression each; the table is split for readability. Standard errors clustered at 250m × 250m squares in parentheses. *$p<0.10$, **$p<0.05$, ***$p<0.01$.

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TABLE A2 Yearly DID estimates, full set of coefficients (Part B).

	(1) Treatment dummy		(2) Continuous treatment	
	Coef.	**Std. err.**	**Coef.**	**Std. err.**
Floor of unit (floor 1 omitted)				
Floor 2	0.009***	(0.003)	0.010***	(0.003)
Floor 3	0.015***	(0.003)	0.014***	(0.003)
Floor 4	0.020***	(0.005)	0.020***	(0.005)
Floor 5	0.029***	(0.008)	0.029***	(0.008)
Floor 6	0.041***	(0.009)	0.042***	(0.009)
Floor 7	0.030***	(0.008)	0.030***	(0.008)
Floor 8	0.052***	(0.008)	0.055***	(0.008)
Floor 9	0.067***	(0.012)	0.064***	(0.013)
Floor 10	0.174**	(0.075)	0.176**	(0.071)
Floors in building (1 floor omitted)				
2 floors	0.098	(0.060)	0.097	(0.060)
3 floors	−0.010	(0.014)	−0.010	(0.014)
4 floors	0.008	(0.015)	0.005	(0.014)
5 floors	0.033*	(0.019)	0.034*	(0.019)
6 floors	0.041	(0.034)	0.039	(0.033)
7 floors	−0.003	(0.017)	−0.005	(0.017)
8 floors	−0.010	(0.019)	−0.011	(0.018)
9 floors	0.023	(0.022)	0.021	(0.022)
10 floors	0.015	(0.016)	0.020	(0.016)
Neighborhood (cluster 1 omitted)				
Cluster 2	0.240***	(0.035)	0.252***	(0.035)
Cluster 3	−0.011	(0.020)	−0.013	(0.020)
Cluster 4	0.235***	(0.068)	0.251***	(0.068)
Cluster 5	−0.003	(0.020)	−0.006	(0.019)
Cluster 6	0.089***	(0.027)	0.091***	(0.026)
Cluster 7	0.047**	(0.022)	0.050**	(0.022)
Year (2003 omitted)				
2004	0.004	(0.011)	0.004	(0.010)
2005	0.029**	(0.012)	0.030***	(0.011)
2006	0.098***	(0.012)	0.101***	(0.011)
2007	0.142***	(0.016)	0.143***	(0.015)
2008	0.110***	(0.019)	0.112***	(0.017)
2009	0.140***	(0.022)	0.145***	(0.021)
2010	0.234***	(0.023)	0.241***	(0.021)
2011	0.242***	(0.022)	0.247***	(0.021)
2012	0.244***	(0.025)	0.251***	(0.024)
2013	0.289***	(0.028)	0.297***	(0.026)
2014	0.291***	(0.032)	0.301***	(0.030)

(Continues)

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TABLE A2 (Continued)

	(1) Treatment dummy		(2) Continuous treatment	
	Coef.	**Std. err.**	**Coef.**	**Std. err.**
2015	0.350***	(0.032)	0.362***	(0.030)
2016	0.365***	(0.035)	0.373***	(0.033)
2017	0.421***	(0.034)	0.428***	(0.033)
2018	0.474***	(0.035)	0.481***	(0.034)
2019	0.474***	(0.035)	0.481***	(0.034)
2020	0.547***	(0.037)	0.554***	(0.035)
2021	0.568***	(0.039)	0.568***	(0.037)
Observations	10,984		10,984	
R-squared	0.910		0.911	

Note: The dependent variable is the log of price. Continued from Table A1. Both columns report estimates from a single regression each; the table is split for readability. Standard errors clustered at 250 m × 250 m squares in parentheses. *p<0.10, **p<0.05, ***p<0.01.

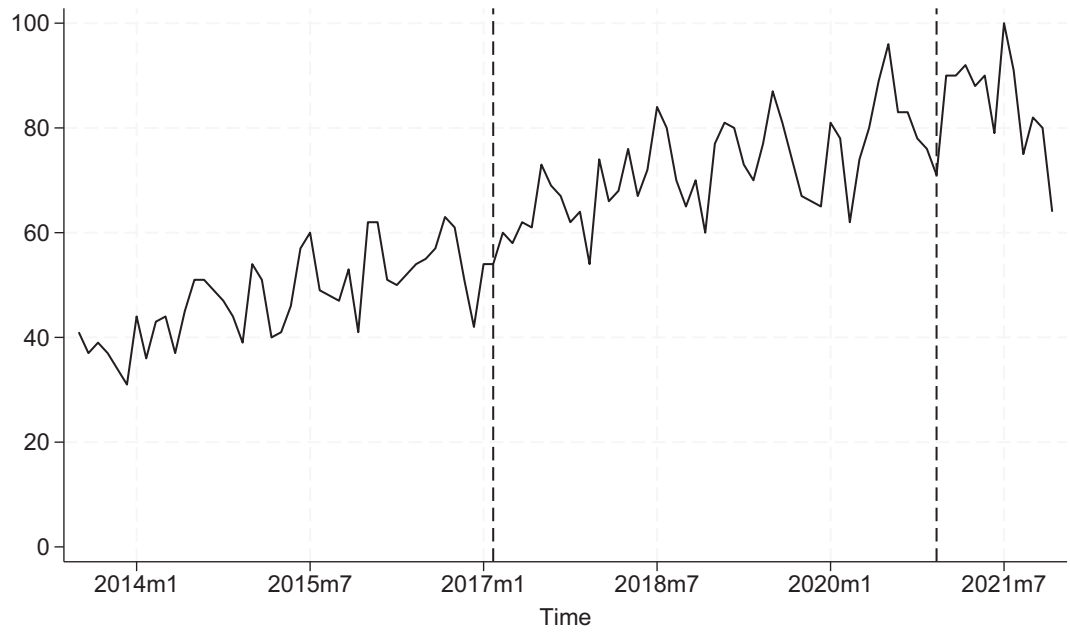


FIGURE A2 Google search interest index for land rent increases in Uusimaa, the province where Helsinki is located. The index is normalized, with 100 representing peak search interest during the period shown. Note: The dashed vertical lines represent the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021.

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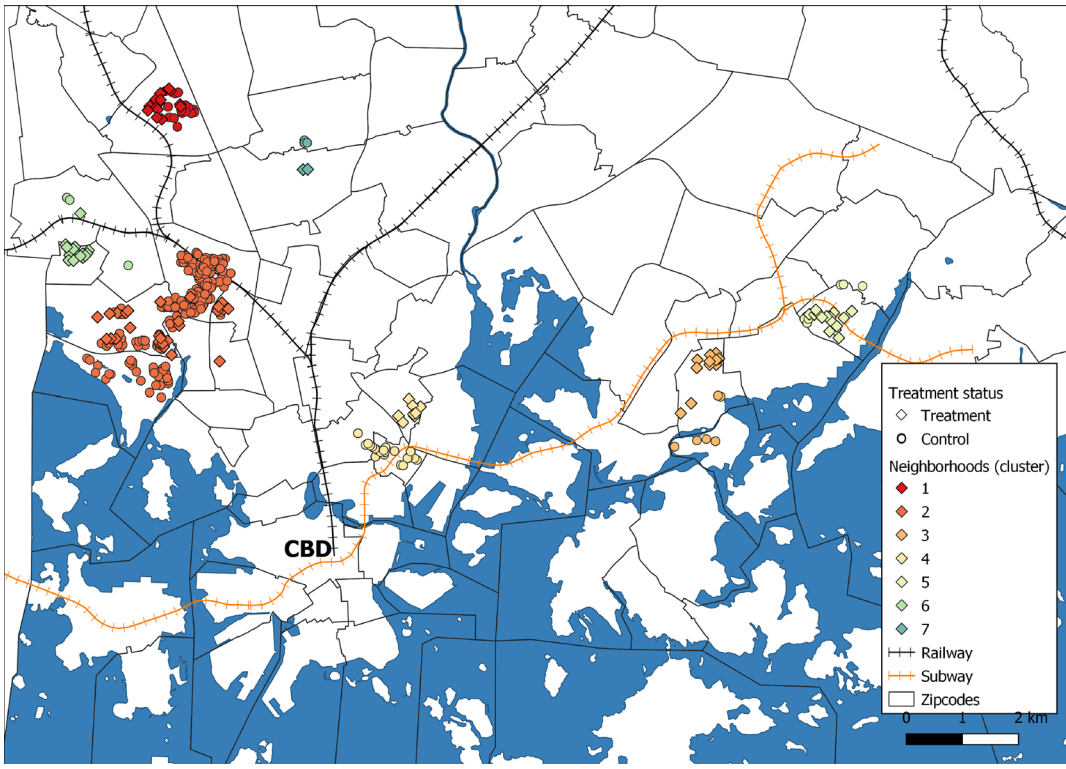


FIGURE A3 Map of neighborhoods and observations.

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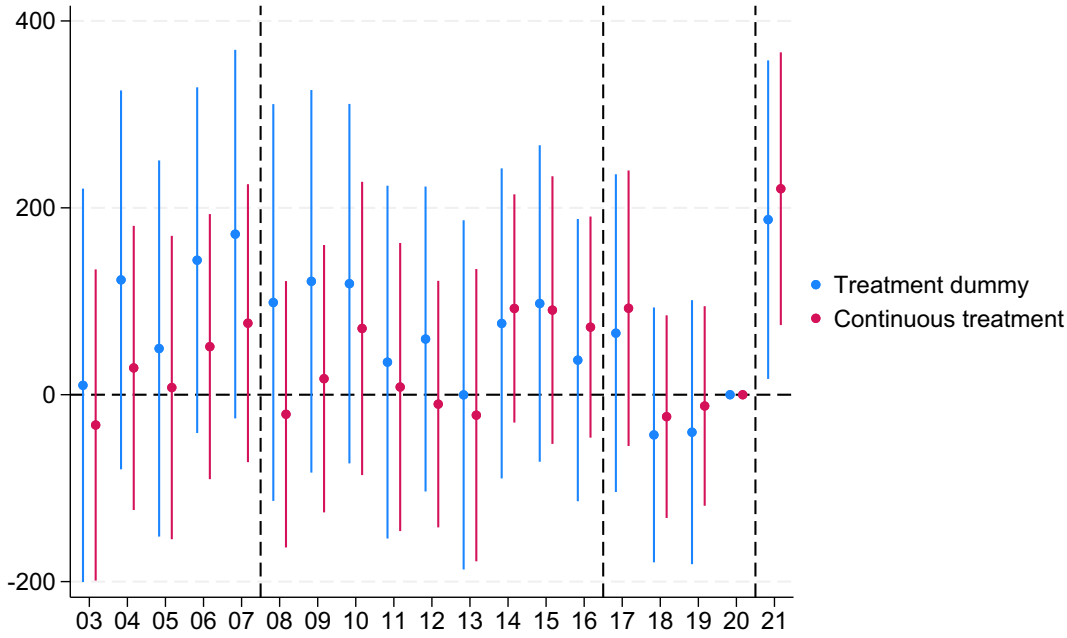


FIGURE A4 Comparison of maintenance costs of freeholds and leaseholds. Note: The figure reports the yearly DID estimates for models where dependent variable is yearly maintenance costs and last year before the rent increases (2020) is omitted. The dashed vertical lines represent the first official announcement of land rent renewal policy in January 2008, the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021. Housing attributes include: log(area), log(age), number of floors, floor number, and the distance to Helsinki city center. Neighborhood and year fixed effects as well as clustering of standard errors is similar to those in the main estimations. The baseline effect in 2020 with treatment dummy is −70.5 (s.e. 101.2) and with continuous treatment −24.1 (s.e. 84.8).

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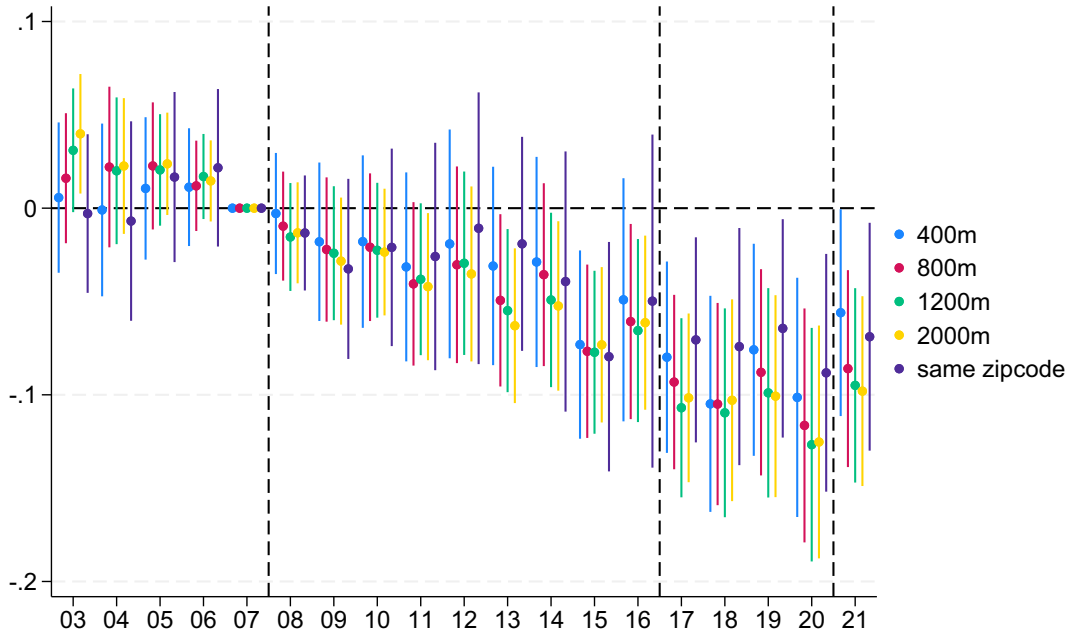


FIGURE A5 Robustness of DID results with respect to different neighborhood definitions. Note: The figure reports the yearly DID estimates (using treatment dummy) where the distance used to create our neighborhood clusters is modified. Other model specifications are similar to those in the main estimations. The dashed vertical lines represent the first official announcement of land rent renewal policy in January 2008, the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021.

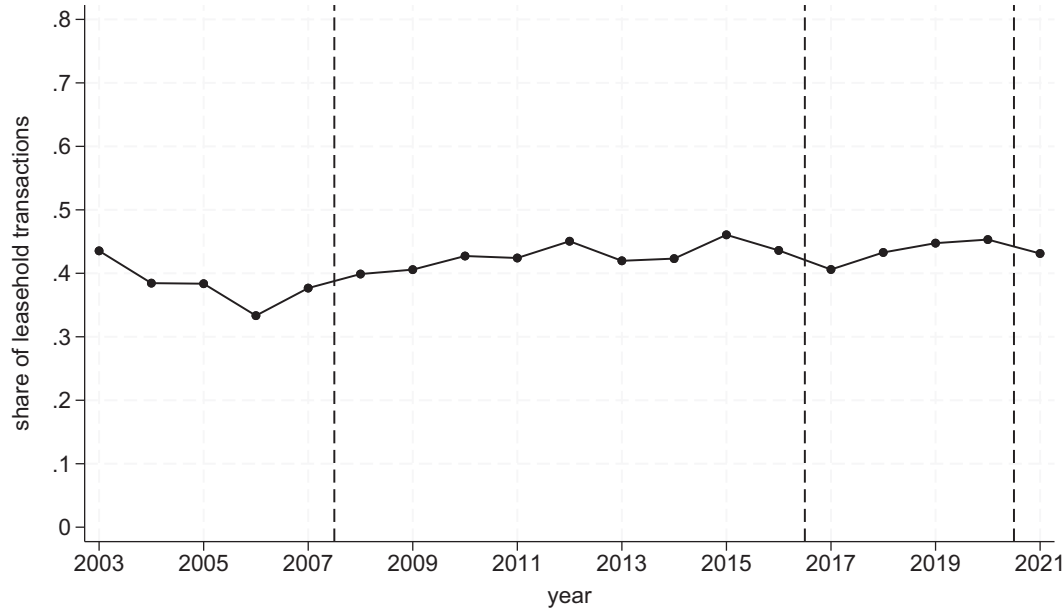


FIGURE A6 Share of leasehold transactions. Note: The dashed vertical lines represent the first official announcement of land rent renewal policy in January 2008, the first announcement of the estimated land rent increase in February 2017 and the new land rents coming into effect in 2021.

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TABLE A3 Three-period DID estimates, full set of coefficients (Part A).

	(1) Treatment dummy		(2) Continuous treatment	
	Coef.	**Std. err.**	**Coef.**	**Std. err.**
Treatment	0.004	(0.014)	0.013	(0.010)
Treatment × Post 1 (2008–2016)	−0.052***	(0.018)	−0.053***	(0.014)
Treatment × Post 2 (2017–2021)	−0.111***	(0.023)	−0.095***	(0.018)
log(area)	0.654***	(0.020)	0.662***	(0.020)
log(age)	0.181*	(0.095)	0.161*	(0.095)
Distance to CBD (m)	−0.000	(0.000)	−0.000	(0.000)
Balcony	0.004	(0.004)	0.005	(0.004)
Sauna	0.009	(0.026)	0.009	(0.027)
Number of rooms (4+ rooms omitted)				
1 room	−0.176***	(0.022)	−0.184***	(0.023)
2 rooms	−0.177***	(0.014)	−0.186***	(0.015)
3 rooms	−0.127***	(0.012)	−0.132***	(0.012)
Condition (decent omitted)				
Poor	−0.068***	(0.005)	−0.068***	(0.005)
Good	0.084***	(0.003)	0.084***	(0.003)
Excellent	0.111***	(0.017)	0.111***	(0.018)
Unknown	0.019**	(0.009)	0.018*	(0.009)
Floor of unit (floor 1 omitted)				
Floor 2	0.009***	(0.003)	0.009***	(0.003)
Floor 3	0.014***	(0.003)	0.014***	(0.003)
Floor 4	0.020***	(0.005)	0.020***	(0.005)
Floor 5	0.029***	(0.008)	0.029***	(0.008)
Floor 6	0.041***	(0.009)	0.042***	(0.009)
Floor 7	0.031***	(0.009)	0.030***	(0.008)
Floor 8	0.052***	(0.008)	0.054***	(0.008)
Floor 9	0.065***	(0.012)	0.062***	(0.012)
Floor 10	0.174**	(0.073)	0.177**	(0.069)
Floors in building (1 floor omitted)				
2 floors	0.098	(0.060)	0.096	(0.060)
3 floors	−0.010	(0.014)	−0.012	(0.014)
4 floors	0.007	(0.015)	0.004	(0.014)
5 floors	0.033*	(0.019)	0.033*	(0.020)
6 floors	0.041	(0.034)	0.039	(0.033)
7 floors	−0.005	(0.017)	−0.007	(0.017)
8 floors	−0.010	(0.019)	−0.012	(0.018)
9 floors	0.024	(0.022)	0.020	(0.022)
10 floors	0.014	(0.016)	0.018	(0.017)

Note: The dependent variable is the log of price. Continued in Table A4. Both columns report estimates from a single regression each; the table is split for readability. Standard errors clustered at 250m × 250m squares in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

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TABLE A4 Three-period DID estimates, full set of coefficients (Part B).

	(1) Treatment dummy		(2) Continuous treatment	
	Coef.	**Std. err.**	**Coef.**	**Std. err.**
Neighborhood (cluster 1 omitted)				
Cluster 2	0.240***	(0.035)	0.252***	(0.035)
Cluster 3	−0.011	(0.020)	−0.013	(0.020)
Cluster 4	0.234***	(0.068)	0.251***	(0.068)
Cluster 5	−0.003	(0.020)	−0.006	(0.019)
Cluster 6	0.089***	(0.027)	0.090***	(0.026)
Cluster 7	0.048**	(0.022)	0.052**	(0.022)
Year (2003 omitted)				
2004	0.006	(0.008)	0.008	(0.008)
2005	0.031***	(0.011)	0.034***	(0.010)
2006	0.096***	(0.010)	0.100***	(0.010)
2007	0.135***	(0.014)	0.139***	(0.014)
2008	0.120***	(0.018)	0.127***	(0.017)
2009	0.145***	(0.020)	0.152***	(0.020)
2010	0.239***	(0.022)	0.247***	(0.021)
2011	0.239***	(0.022)	0.246***	(0.021)
2012	0.245***	(0.024)	0.253***	(0.023)
2013	0.282***	(0.027)	0.291***	(0.025)
2014	0.290***	(0.030)	0.300***	(0.029)
2015	0.330***	(0.031)	0.340***	(0.030)
2016	0.353***	(0.032)	0.363***	(0.031)
2017	0.420***	(0.034)	0.427***	(0.033)
2018	0.468***	(0.034)	0.474***	(0.033)
2019	0.475***	(0.035)	0.481***	(0.034)
2020	0.536***	(0.036)	0.542***	(0.035)
2021	0.570***	(0.038)	0.576***	(0.037)
Observations	10,984		10,984	
R-squared	0.910		0.911	

Note: The dependent variable is the log of price. Continued from Table A3. Both columns report estimates from a single regression each; the table is split for readability. Standard errors clustered at 250 m × 250 m squares in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

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TABLE A5 Descriptive statistics, rent data.

	Treatment group $n = 2122$		Control group $n = 3622$	
	Mean	**Std. dev.**	**Mean**	**Std. dev.**
Rent (€ /month)	904.7	261.7	877.8	224.1
Rent per sq. meter (€ /month)	21.6	5.1	24.1	5.4
Living area	44.9	18.0	38.9	15.1
Age	56.1	4.2	55.0	4.2
Rooms	1.9	0.9	1.6	0.7
Condition = poor (0/1)	0.00	0.00	0.01	0.07
Condition = decent (0/1)	0.13	0.00	0.15	0.35
Condition = good (0/1)	0.78	0.42	0.80	0.40
Condition = unknown (0/1)	0.09	0.28	0.05	0.20
Distance to city center (m)	9504	1931	7321	2709
Fixed term contract	0.12	0.32	0.10	0.30

Note: All prices are deflated to December 2021 using consumer price index.

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TABLE A6 Yearly treatment effects on rents (2009 omitted).

Treatment	Dummy	Continuous
	(1)	(2)
Treatment	−0.019	−0.008
	(0.015)	(0.013)
Treatment × 2010	−0.003	−0.003
	(0.014)	(0.011)
Treatment × 2011	−0.008	−0.007
	(0.018)	(0.019)
Treatment × 2012	0.002	−0.015
	(0.017)	(0.015)
Treatment × 2013	−0.022	−0.018
	(0.019)	(0.018)
Treatment × 2014	−0.017	−0.023
	(0.019)	(0.017)
Treatment × 2015	−0.007	−0.015
	(0.017)	(0.013)
Treatment × 2016	−0.013	−0.025
	(0.017)	(0.014)
Treatment × 2017	−0.002	−0.010
	(0.026)	(0.030)
Treatment × 2018	−0.004	−0.013
	(0.019)	(0.017)
Treatment × 2019	0.008	0.021
	(0.022)	(0.020)
Treatment × 2020	0.017	0.007
	(0.021)	(0.017)
Treatment × 2021	−0.005	−0.007
	(0.026)	(0.022)
Housing attributes	Yes	Yes
Neighborhood FE	Yes	Yes
Year FE	Yes	Yes
Observations	5744	5744
R-squared	0.776	0.776

Note: The table reports the yearly DID estimates for models where the dependent variable is the log of monthly rent. Rent data is only available from 2009 onwards, and the first year (2009) is omitted. Housing attributes include: log(area), log(age), dummies for number of rooms (1, 2, 3), dummies for condition (poor, decent, good, unknown), distance to Helsinki city center, and dummy for fixed term rental contract. Neighborhood and year fixed effects as well as clustering of standard errors is similar to those in the main estimations. Standard errors are clustered at 250 m × 250 m squares and are reported in parenthesis. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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TABLE A7 Balance tests.

Dependent variable	Area	Rooms	Cond. poor	Sauna	Balcony	CBD dist.
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	3.630*	0.192**	−0.012	0.002	−0.026	408.893***
	(−1.878)	(−0.088)	(−0.011)	(−0.004)	(−0.037)	(−113.411)
Treatment × post1	2.173*	0.100*	0.005	−0.004	0.072**	−23.223
	(−1.173)	(−0.055)	(−0.011)	(−0.004)	(−0.031)	(−28.718)
Treatment × post2	0.441	0.042	0.012	0.003	0.062*	8.737
	(−1.375)	(−0.063)	(−0.011)	(−0.005)	(−0.036)	(−33.285)
Neighborhood FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,984	10,984	10,984	10,984	10,984	10,984
R-squared	0.050	0.053	0.010	0.004	0.059	0.971

Note: Standard errors are clustered at 250 m × 250 m squares and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE A8 Selling time DID.

Dependent variable: log (selling time)	
Treatment	0.066
	(0.041)
Treatment × post1	0.006
	(0.045)
Treatment × post2	0.032
	(0.053)
Housing attributes	Yes
Neighborhood FE	Yes
Year FE	Yes
Observations	10,922
R-squared	0.064

Note: Standard errors are clustered at 250 m × 250 m squares and are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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TABLE A9 Robustness to differential taxation and the mortgage-interest deduction.

	Treatment: Dummy		Continuous	
	(1)	(2)	(3)	(4)
	Baseline	+ net cost	Baseline	+ net cost
Treatment	0.004	−0.016	0.013	−0.009
	(0.014)	(0.025)	(0.010)	(0.016)
Treatment × Post 1 (2008–2016)	−0.052***	−0.044**	−0.053***	−0.045***
	(0.018)	(0.019)	(0.014)	(0.015)
Treatment × Post 2 (2017–2021)	−0.111***	−0.116***	−0.095***	−0.100***
	(0.023)	(0.023)	(0.018)	(0.018)
Treatment × net cost differential		0.00025		0.00027*
		(0.00023)		(0.00014)
Housing attributes	Yes	Yes	Yes	Yes
Neighborhood FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	10,984	10,984	10,984	10,984
R-squared	0.910	0.910	0.911	0.911

Note: The dependent variable is the log of price. Columns (1) and (3) reproduce the baseline three-period DID estimates; columns (2) and (4) add the year-specific freehold–leasehold net cost differential, expressed in real terms (2021 euros, deflated with the cost-of-living index), interacted with the treatment indicator. Because the differential is a function of calendar year, its level is absorbed by the year fixed effects and only the treatment interaction is identified. Standard errors clustered at 250m × 250m squares in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

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