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You Pays Your Money and You Takes Your Choice: Real Estate Agent Commission Reform and Austria's Partial Strict Rent Control Regime *

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Abstract

In 2023, Austria introduced the *Bestellerprinzip* ("ordering principle"), requiring the party hiring a real estate agent to pay the commission. Using rental listing and administrative data from Vienna, we study the reform's effects on listings, rents, and agent usage. Using event-study and difference-in-discontinuity designs, we find a sharp decline in listings, driven mainly by reduced agent use, alongside significant rent increases, especially for agent-listed and rent-controlled units. The results suggest that landlords passed brokerage costs on to tenants and that rent controls are weakly enforced. Overall, the reform failed to reduce short-run housing costs, but may enhance residential mobility.

Keywords: Rental Market; Real Estate Agents; Brokerage Commissions; Statutory Incidence; Rent Control; Residential Mobility
JEL codes: D04, H22, L85, R31, R38

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AEA Random Order Confirmation Code: dAB47skT1ilm

1 Introduction

In 2023, a new policy was introduced to the Austrian rental market: the "Bestellerprinzip" (ordering principle), mandating that whoever hires a real estate agent must also pay for their services.[1] Before this reform, it was common practice for landlords to hire the agent, while the tenant signing the rental contract had to pay the brokerage. These fees could be up to two gross monthly rental instalments (plus taxes), potentially imposing a substantial financial burden on new tenants, particularly those with higher residential mobility. Indeed, the stated policy goal was to relieve tenants, particularly young and low-income individuals, of this financial burden.

Notes from the parliamentary debates suggest that policymakers anticipated *no adverse effects for tenants* from the reform. However, the effects of such shifts in statutory incidence remain an open question in the academic literature. A central principle of public economics holds that economic incidence – i.e., which agent ultimately bears a tax or fee burden – is independent of statutory incidence, implying that the party on which a tax or fee is formally levied does not necessarily bear its ultimate economic burden (Kotlikoff and Summers, 1987). Yet, empirical evidence documents systematic deviations from this benchmark (Chetty, Looney and Kroft, 2009; Jiménez, Martinez-Miera and Peydró, 2024). Evidence from real estate brokerage markets likewise suggests that statutory and economic incidence may diverge. Since December 2020, sellers of residential real estate in Germany have been required to pay at least half of the agent's commission. Specifically, the non-hiring party's commission liability was capped at the amount paid by the hiring party, and equal fee splitting became mandatory whenever both sides hired an agent. Claussen, Dubovska and Streich (2023) find that sellers were unable to pass through these additional commission costs to buyers via higher transaction prices, although the reform substantially reduced sellers' demand for agents. In contrast, Sagner (2025), analysing the same German reform, finds evidence that commission costs were passed through to property prices.

To understand the impact of the Austrian reform on the rental market, we focus on the capital Vienna where renting is the dominant tenure form. Our testing strategy is guided by a theoretical framework. Reflecting the defining characteristics of Vienna's rental market, we model a dual market structure consisting of a segment with freely negotiated initial rents and a segment governed by a first-generation rent control regime with capped initial rents. We further incorporate the incentives embedded in the Austrian rent control system, which *does not* impose penalties for advertising rent-capped properties at excessive rates and does not guarantee full information of allowed rents upon advertising. In practice, informed tenants must challenge such excess rents only after signing the contract. In addition, we explicitly model the costs and expected returns associated with the use of real estate agents. While the ordering principle did not alter the overall cost of these services, it did change who nominally bears them. The framework yields clear predictions regarding both the effects on advertised prices and shifts in overall market volume and agent usage.

Accordingly, we assess rent advertisements before and after the policy's enactment. We test for both changes in the trade volume (the number of listings posted) and asking rents. Regarding

[1] The ordering principle had been discussed for years – and in particular when Germany introduced a similar policy in 2020. However, the policy was only announced on 1 March 2023 and enacted on 1 July 2023. See, for instance, https://www.bpv-huegel.com/en/newspost/mietrecht-zum-bestellerprinzip-in-oesterreich-aufgeschoben-aber-nicht-aufgehoben/5/, last accessed in December 2025.

the first, we set up a count model following Hahn, Omerovic and Waltl (2025) that filters out seasonal trends, thereby capturing the net effect of the policy change. Complementarily, we set up a hedonic model (Rosen, 1974) to measure price effects net of the variation in the mix of property characteristics available on the market. For the hedonic model, we make use of geographical splines to ensure precise modelling of spatial dependencies. By that, we achieve a clean, like-for-like comparison.[2]

We estimate both models using a (staggered) event-study framework and a difference-in-discontinuity design, examining effects both in aggregate and across market segments. Our analysis focuses on particularly informative submarkets – such as the agent-mediated market and the rent-controlled sector subject to a rent cap – while also exploring heterogeneity across price tiers, districts, and the socio-demographic composition of the resident population.

We find a sharp decrease in the overall number of listings posted post-enactment. The declines in volume do not differ significantly across regulatory status, nor are there systematic heterogeneities across the price distribution, locations, or the districts' socio-demographic composition. By contrast, the decrease in volumes is much larger in the agent-advertised sector than in the privately advertised sector. The additional effort or costs for landlords may thus have motivated some to exit the rental market – at least in the short run. This suggests that the market for real estate agents became smaller following this reform.

With regards to price effects, the overall level increases by 3.8% per month or 5.2% per quarter following the reform. This shift is robust to accounting for a change in the mix of properties on the market as well as exact econometric specifications. Upon enactment, listings posted by real estate agents show significantly larger price increases than private listings suggesting that landlords passed on extra agency costs endured. However, privately advertised properties catch up within three quarters. A comprehensive heterogeneity assessment further reveals that prices increase relatively most in the rent-controlled segment. In this segment, however, rent increases are permitted only biannually at pre-determined dates that do not coincide with the introduction of the ordering principle. This is consistent with weak enforcement of the Austrian rent control regime at the advertising stage, potentially due to relatively weak enforcement compared to other countries (as, for instance, in Spain Jofre-Monseny, Martínez-Mazza and Segú, 2023). In the regulated segment, immediate increases are most pronounced in the lowest part of the rent price distribution, whereas this heterogeneity is smoothed out within half a year. No such pattern is found for the unregulated segment. Furthermore, increases are largest in absolute terms in central locations (within the ring-road Gürtel), as well as in districts with a high share of non-EU residents or university students.

These findings stand in stark contrast to the predictions presented in the parliamentary regulatory impact assessment aimed to provide a rigorous assessment evaluating potential economic consequences of proposed laws before they are passed to enable evidence-based policymaking. The assessment suggested that landlords would primarily absorb brokerage costs and that no substantial adverse effects on apartment supply were anticipated as it was expected that privately advertised units would compensate a potential reduction of the use of

[2] Hill and Scholz (2018) and Waltl (2016, 2019) directly include such price maps into hedonic equations to control for locational effects.

agents. Consequently, it is doubtful whether the decision-making process was fully grounded in accurate assumptions.

Although this was no explicitly stated policy target, the reform does encourage residential mobility by reducing relocation expenses. Computations based on the average tenancy duration across different demographic groups indicate that, in the long run, more mobile groups (including younger households, families, and first-generation immigrants) will likely benefit most from this reform, while less mobile groups will likely bear higher cumulative long-term costs. Already within few years since policy enactment, tenure durations in Vienna had overall notably decreased. While we are unable to strictly establish causality with this regard, these findings are in-line with increased mobility due to lower transaction costs.

With this study, we contribute to the evolving, yet still very scarce, literature on economic effect associated with real estate intermediation. Stoll (2023) also studies the consequences of the German policy change and finds that the lion's share of the costs is indeed borne by sellers. A potential explanation for this counter-intuitive finding lies in sellers' behavioural traits: although commission rates can be freely negotiated for each sale, a survey of real estate agents confirms that almost all sellers (85%) do not even attempt to negotiate lower commission rates. In the UK, the rent commission system was likewise recently restructured: since June 2019, a strict cap of GBP 50 has been imposed on fees charged to tenants. Bakker and Datta (2025) find some, yet no perfect, pass-through of increased cost into rents and argue with competitive pressure in the market for letting broker services. They do not observe landlords exiting the market, yet can only indirectly test this hypothesis using sales prices and changes in time-on-market (TOM). Berger and Schmidt (2019) show that renters are inattentive to real estate agent commissions, not systematically factoring in these costs in their comparisons of the total cost of renting. More generally related, Eichholtz, Holtermans and Rodrigues (2021) and D'Lima and Smith (2022) assess the benefits and costs of real estate brokers in the commercial real estate sector.

The remainder of this article is structured as follows: section 2 describes the Austrian rental market and introduces the new policy. Thereafter, section 3 introduces a theoretical framework and derives predictions. Next, section 4 presents all data used and section 5 executes the empirical strategy to identify the causal effects of the policy reform on trade volume and asking prices. Section 6 computes the impact for different types of households to identify winners and loser. Finally, section 7 derives policy recommendations and concludes. A comprehensive appendix provides further details and supplemental materials.

2 Institutional Background

2.1 The Structure of the Austrian Rental Market

Austria has one of the highest shares of renters in Europe. In 2023, 44% of households were renters, including 11.2% paying market rents and 32.8% paying reduced rates.[3] Reduced rates encompass various types of controlled or subsidized schemes as well as public and limited-profit housing, some of which entail strong price regulations. In Europe, only Germany

[3] The numbers are obtained from Microcensus data for 2023.

and Switzerland have a higher share of renters (DE: 52.4%, CH: 57.4% in 2023).[4]

Table 1: Rental Market Indicators per Federal State

	Benchmark Rent [EUR per sqm]	Renters [%]		Average Rent [EUR per sqm]		Δ [EUR per sqm]	
		All	Private	All	Private	All	Private
Vienna	**6.67**	**77**	**34**	**7.18**	**9.18**	**0.51**	**2.51**
Burgenland	6.09	22	6	5.51	6.12	-0.58	0.03
Lower Austria	6.85	29	10	6.16	7.28	-0.69	0.43
Carinthia	7.81	36	14	5.31	6.64	-2.50	-1.17
Styria	9.21	36	18	6.70	7.85	-2.51	-1.36
Upper Austria	7.23	36	13	6.34	7.48	-0.89	0.25
Salzburg	9.22	40	22	8.65	10.00	-0.57	0.78
Tyrol	8.14	35	20	8.55	10.15	0.41	2.01
Vorarlberg	10.25	36	22	8.34	9.73	-1.91	-0.52
Weighted Average	–	44	19	6.98	8.67	–	–

Notes: Benchmark Rent refers to rents valid as of 1 April 2023, excluding running costs and taxes. *All* refers to all renters. *Private* refers to renters on the private market (exluding public and limited-profit housing). *Renters* gives the share of renters in 2023. *Average rent* refers to rents excluding running costs and taxes as of 2023. Δ denotes the difference between the benchmark rent and the average rent.
Source: Mikrozensus (Statistik Austria, 2024)

Within Austria, the share of renters is highest in the capital and by far the largest city, Vienna, where renting constitutes the predominant tenancy form (see Table 1). The share of renters remained quite stable over the past 20 years, with no noticeable time trend. According to Statistik Austria (2024), 19% of all households are owner-occupiers, 23% lived in city-owned apartments ("Gemeindewohnungen"), which are owned and managed by the City of Vienna, and 21% lived in limited-profit apartments ("Genossenschaftswohnungen"), which were built using public subsidies from the City yet are owned and run by either a limited-profit firm or co-operative housing association ("Wohnbaugenossenschaft"). For city-owned or limited-profit units, eligibility is subject to means testing at the time of move-in.

2.2 Rent Control in Vienna and Legal Enforcement

Market rents are generally freely negotiated upon initial contract signing. Most multi-annual or open-ended contracts also specify how the monthly rent will be adjusted over time. Such adjustments are regularly linked to the Austrian Consumer Price Index (CPI) or other officially published indices.

In the non-market sector, rents are governed by a large body of complex legislation accumulated over the past century (see Kunnert and Baumgartner, 2012, chapter 4.2 for details on the different categories of regulations in Austria). Austria's rent-controlled segment follows a dual system combining first- and second-generation rent control schemes (see Arnott, 2003; Kholodilin, 2024; Morawetz and Klaiber, 2024). While the second-generation scheme regulates only rent increases over time, the first-generation scheme caps price levels for new contracts. Hence, while units subject to the second-generation scheme only are expected to be advertised

[4] Based on EU-SILC 2023.

in the same way as unregulated units, first-generation schemes should already affect rents at the advertising stage. The latter units are therefore assessed separately and referred to as "rent-capped units."

The first-generation regime generally applies to buildings constructed before 1945 ("Altbau") with a maximum living area of 130 m^2, as well as to publicly subsidized housing ("geförderter Wohnbau").[5] Loft conversions are explicitly exempt. Applicable base rents vary by federal state and by the type of financing for initial construction or refurbishment. Very similar to the short-lived German *Rent Freeze* (Hahn et al., 2024), several unit-specific surcharges or discounts apply corresponding to locations ("Lagezuschlag"), the availability or lack of different types of equipment, or unit-specific disamenities, e.g., noise pollution. Importantly, a free-of-charge online tool provided by the City of Vienna allows both tenants and landlords to determine the permitted unit-specific rent generally ensuring transparency.[6]

Table 1 reports benchmark rents ("Richtwertmieten") per federal state (excluding surcharges and discounts) as of April 2023. Each federal state sets its benchmark independently, and rates exhibit substantial variation. Rents are most stringently capped in nominal terms in the Eastern federal states Burgenland and Vienna. In Vienna, the difference between the benchmark rent and the average rent paid is largest.

Since 2010, also capped contracts are valorized and hence this segment has features of both a first-generation and second-generation-rent control scheme. Valorization is permitted every two years on 1 April and is capped at the annualized change in the *Austrian Consumer Price Index* (*VPI*), i.e., general inflation.[7] By that, rents are ex-post adjusted for realized cumulative inflation since the last increase. The last adjustment before the enactment of the ordering principle took place in March 2023. This implies that also the first-generation rent control regime has by now characteristics of a second-generation rent control regime and price increases are expected yet only at a clearly specified date.

Average rents paid in 2023 are reported in Table 1. Correlating benchmark rents with both mean rents paid by all renters or private renters only yield Pearson coefficients in the region of 60% (see Figure 1). This suggests that these benchmark values may also be indicative for the general market. Still, there is a larger gap between benchmark rents and mean rents paid in Vienna than in most other federal states, suggesting that the relative benefit of securing such a property is largest in the capital.

A key implication of the first-generation regime is that regulated rents are not intended to adjust in response to higher search costs. Consequently, we may expect heterogeneous price effects among units subject to the cap or not.

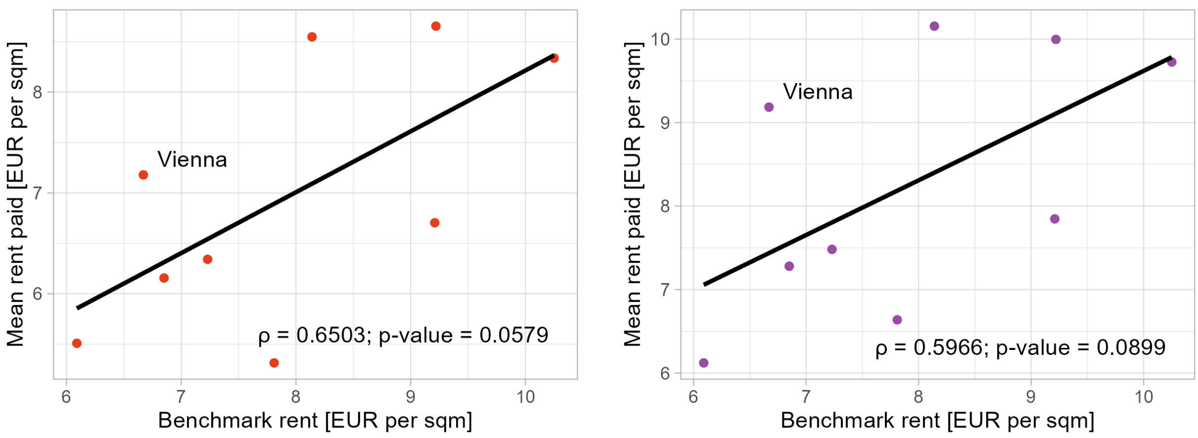
Although regulations prohibit passing search costs on to tenants in this sector, we remain

[5] Rents for units built between 1945 and 1953 are also subject to the cap provided they were built with public funding. As we cannot unambiguously identify these units, we run robustness tests to understand whether effects change when leaving out all properties built in these eight years. See Appendix D.

[6] See https://mein.wien.gv.at/Meine-Amtswege/Richtwert/. Last accessed in January 2026.

[7] See Mietrechtsgesetz §16, https://ris.bka.gv.at/eli/bgbl/1981/520/P16/NOR40268863. The Austrian parliament suspended the valorization during the COVID pandemic. Also, *Wiener Wohnen* – Europe's largest municipal property management, which owns roughly 220,000 units in Vienna – did not execute these regular increases in 2024 and 2025 due to affordability concerns; see https://www.wienerwohnen.at/Neues-aus-dem-Gemeindebau2/2023_keine_Mieterh-hung-im-Gemeindebau.html. Last accessed in August 2025.

Figure 1: Benchmark Rents vs. Paid Rents



(a) All (b) Private

Notes: Benchmark Rents versus Mean Rents Paid as reported in Table 1 together with a fitted linear regression line indicating strong positive correlation. Dashed line shows weighted average rent per sqm for Austria. ***All*** *refers to all rental types.* ***Private*** *refers to private rents (excluding public and limited-profit housing). ρ denotes the Pearson correlation coefficient and p-values relate to pairwise Pearson correlation tests.*

agnostic about whether rent-controlled properties will, in practice, continue to be advertised in the same way after the reform. Strict compliance at the advertising stage may not be the norm (see also Ambrose et al., 2026, documenting substantial non-compliance in New York's rent control scheme), particularly given the absence of penalties for non-compliance or transparency requirements, unlike in Catalonia (Spain), where these measures have been shown to lead to compliance at the advertising stage (Jofre-Monseny, Martínez-Mazza and Segú, 2023).

While ex ante enforcement is not ensured in Austria, tenants *can* demand a rent reduction ex post when the agreed rent exceeds the legal ceiling. Landlords offering indefinite contracts are typically required to adjust only future payments, without reimbursing past overpayments. For fixed-term leases, tenants *can* claim reimbursement for excess payments ex post. In both cases, however, landlords generally face no formal penalties, and understanding of the applicable rules may be limited due to the lack of transparency requirements in advertisements. A service desk by the *City of Vienna* ('Schlichtungsstelle') helps renters to enforce rent reductions if they exceed caps.[8] In 2024, more than 3,300 rent review requests were executed. Although statistics on the share of *successfully* challenged contracts are unavailable, the incentive structure created by this framework effectively *encourages* landlords to disregard the regulations. It is therefore reasonable to assume that profit-maximising landlords will advertise vacant regulated units at rents above the permissible level. These features of the Austrian rent control system are hence explicitly modelled in our theoretical framework presented in section 3.

2.3 The Ordering Principle

On 1 July 2023, the ordering principle was enacted. Its introduction was preceded by a public debate over the fairness of the previously common practice whereby renters were required to

[8]See https://www.wien.gv.at/wohnen/schlichtungsstelle, last accessed in February 2026.

pay agency commissions even when they had not engaged the agent themselves. Until then, commission fees of up to two monthly gross rent payments were the norm (see subsection 6.2). Landlords decided whether to engage an agent or not, yet renters were liable to pay the commission upon successful arrangement.

In the preceding discussion in the Austrian parliament, official documents reveal both policymakers' targets and expectations regarding side effects. Regarding targets, it is stated:

> No financial burden on apartment tenants for the work of real estate agents who have already concluded an agency agreement with the landlord.
>
> In the future, the agent's commission for arranging an apartment rental agreement will be paid by the person who arranged the agent's services and selected them (first client principle). Brokerages arranged by the landlord should therefore also be paid exclusively by the landlord. The agent is only entitled to commission from the apartment seeker if they are the first client.[9]

The parliamentary materials also state the goals and expected effects of this policy ('regulatory impact assessment'). These are:

(1) In the future, the broker's commission will mainly be paid by landlords.[10]

(2) No significant reduction in the housing supply available to tenants is to be expected as a result of the first-to-buy principle. [...] It is difficult to estimate whether the new commission regulation will affect the number of listings in print media or online portals. Even if fewer landlords use an agent in the future than before, they will still be interested in renting out their properties as quickly as possible. As a result, these private listings could offset those previously placed by agents, especially since agents do not always advertise their entire portfolio of available apartments in listings.[11]

The proposal was supported by a large parliamentary majority, including MPs from all parties except the social democrats, and led to the *Maklergesetz-Änderungsgesetz – MaklerG-ÄG* that adds the new ordinance §17a *Brokerage of Apartment Rental Contracts* ('Vermittlung von Wohnungsmietverträgen') determining that tenants cannot be made liable for commission fees of an agent they had not engaged themselves.

[9] Original wording: "Keine finanzielle Belastung von Wohnungsmietern für das Tätigwerden von Immobilienmaklern, die bereits einen Maklervertrag mit dem Vermieter geschlossen haben. In Zukunft soll für die Vermittlung eines Wohnungsmietvertrags die Maklerprovision zahlen, wer die Leistung des Maklers veranlasst und diesen ausgewählt hat (Erstauftraggeberprinzip). Vom Vermieter veranlasste Vermittlungen sollen daher auch ausschließlich von diesem bezahlt werden. Der Makler hat nur dann einen Anspruch auf Provision vom Wohnungssuchenden, wenn dieser erster Auftraggeber ist." Parliamentary notes accompanying the debate, https://www.parlament.gv.at/dokument/XXVII/I/1900/fnameorig_1491470.html, last accessed in February 2026.

[10] Original wording: "Die Maklerprovision wird damit in Zukunft überwiegend von Seiten der Vermieter zu zahlen sein."

[11] Original wording: "Es ist keine wesentliche Verringerung des für Mieter zugänglichen Wohnungsangebots durch das Erstauftraggeberprinzip zu erwarten. [...] Ob die neue Provisionsregelung sich auf die Anzahl an Inseraten in Printmedien oder Online-Portalen auswirken wird, lässt sich schwer abschätzen. Denn selbst wenn in Zukunft weniger Vermieter einen Makler einschalten als bisher, werden sie dennoch an einer möglichst baldigen Vermietung interessiert sein. Im Ergebnis könnten daher diese privaten Inserate die bisher von Maklern geschalteten ausgleichen, zumal Makler nicht immer ihren gesamten Bestand an vermittelbaren Wohnungen in Inseraten darstellen."

We hence explicitly model and assess both the *intended effect* (reduced costs for renters) as well as possible *unintended effects* triggered by the policy. We model the decisions faced by owners of different types of apartments (subject to the rent cap or not), incentives set by (the absence of) penalties, and derive expected behaviour in a theoretical framework introduced in section 3.

3 Theoretical Framework

3.1 Environment

Time is discrete. The rental market consists of a fixed stock of units normalized to one. A fraction φ belongs to a rent-capped segment C, while the remaining units belong to a non-capped segment N.

Landlords discount future payoffs at rate $\beta \in (0,1)$ and differ in vacancy advertising costs c_i. Let C_m denote the per-period advertising cost associated with posting mode m; for self-posting landlords, write $C_{\text{self},i} = c_i$. Landlords owning a vacant unit choose whether to advertise themselves or through an agent,

$$m \in \{\text{self}, \text{agent}\},$$

and choose a posted rent r.

If a landlord in the capped segment posts a rent above the legal cap $\bar{r}$, then with probability p the tenant successfully challenges the above-cap component. In that case, the landlord repays the excess rent and incurs an administrative penalty $\pi \geq 0$. In the pre-reform environment, both penalties and enforcement are weak, so $\pi \approx 0$ and the probability of successful challenge is relatively low.

Expected net rental income in the capped segment is therefore

$$\tilde{r}^C(r) = \begin{cases} r & \text{if } r \leq \bar{r}, \\ (1-p)r + p\bar{r} - p\pi & \text{if } r > \bar{r}. \end{cases}$$

In the non-capped segment,

$$\tilde{r}^N(r) = r.$$

Occupied units become vacant with probability $q \in (0,1)$.

There is a mass U of searching renters each period. Renters differ in reservation rents $v \sim F(v)$ with density $f(v)$.

Agent-posted listings receive greater visibility. Self-posted listings have visibility multiplier $\gamma_{\text{self}} = 1$, while agent-posted listings have $\gamma_{\text{agent}} > 1$. Agents charge a match-contingent fee equal to κr. Prior to the reform, renters paid this fee. After the reform, landlords pay it when the match starts.

Before the reform, renters evaluating an agent-posted listing faced the effective rent

$$\alpha r, \qquad \alpha = 1 + \kappa[1 - d(1-q)] > 1,$$

where $d \in (0,1)$ denotes the renter discount factor.

3.2 Search Frictions

Let L_m^s denote the mass of vacant listings in segment s using posting mode m. Aggregate listing visibility is

$$L = \sum_{s,m} \gamma_m L_m^s.$$

A listing posted through mode m receives renter contacts at rate

$$A_m = \gamma_m \frac{U}{L}.$$

The matching hazard for self-posted listings is

$$\lambda_{\text{self}}(r) = A_{\text{self}}[1 - F(r)].$$

Before the reform,

$$\lambda_{\text{agent}}^{\text{pre}}(r) = A_{\text{agent}}[1 - F(\alpha r)],$$

whereas after the reform,

$$\lambda_{\text{agent}}^{\text{post}}(r) = A_{\text{agent}}[1 - F(r)].$$

We assume parameters are such that $\lambda_m(r) \in [0,1]$, so that $\lambda_m(r)$ can be interpreted as a per-period matching probability.

3.3 Value Functions

Let $V_O^s(r)$ denote the value of an occupied unit charging rent r, and let $V_V^s(m,r)$ denote the value of a vacant listing.

The occupied value satisfies

$$V_O^s(r) = \tilde{r}^s(r) + \beta[(1-q)V_O^s(r) + qW^s],$$

where

$$W^s = \max\left\{\max_r V_V^s(\text{self}, r), \max_r V_V^s(\text{agent}, r), V_V^s(\text{not list})\right\}.$$

The vacancy value after the reform is

$$V_V^s(m,r) = -C_m + \beta\left[(1-\lambda_m(r))V_V^s(m,r) + \lambda_m(r)\left(V_O^s(r) - \mathbf{1}_{\{m=\text{agent}\}}\kappa r\right)\right].$$

The corresponding pre-reform vacancy equation is the same except that landlords do not pay the agent fee directly and agent-posted listings use the pre-reform hazard $\lambda_{\text{agent}}^{\text{pre}}(r)$.

3.4 Optimal Rent Setting

For rents away from kinks, especially away from $r = \bar{r}$ in the capped segment, the interior first-order condition is

$$\lambda_m(r)\frac{\partial_r \tilde{r}^s(r)}{1-\beta(1-q)} + \lambda_m'(r)\,[V_O^s(r) - V_V^s(m,r)] = \mathbf{1}_{\{m=\text{agent}\}}\,\kappa[\lambda_m(r) + r\lambda_m'(r)].$$

The reform has two opposing effects on posted rents. First, renters no longer pay the agent fee directly, increasing renter acceptance probabilities at a given posted rent:

$$1 - F(r) > 1 - F(\alpha r).$$

Second, landlords using agents must now pay the agent fee upon matching.

In the non-capped segment, asking rents increase after the reform if the increase in renter acceptance probabilities dominates the direct agent fee burden on landlords.

In the capped segment, the effect depends on enforcement. Under the status quo with weak penalties and limited transparency, some landlords may increase above-cap asking rents because the marginal return to overcharging remains positive:

$$\partial_r \tilde{r}^C(r) = 1 - p > 0.$$

By contrast, under a policy alternative combining large administrative penalties, transparent rent caps, and easy cost-free tenant challenges, expected profits from above-cap rents fall substantially and successful enforcement becomes more likely.

Prediction 1 (Price Effects in the Non-Capped Segment) *In the non-capped segment, asking rents increase after the reform if the increase in renter acceptance probabilities dominates the direct agent fee burden on landlords.*

Prediction 2 (Price Effects in the Capped Segment – Status Quo Enforcement) *Under the current policy environment, enforcement is low ($\pi \approx 0$ and low p). Hence, in the capped segment, some landlords may increase above-cap asking rents after the reform because imperfect enforcement leaves the marginal return to overcharging positive:*

$$\partial_r \tilde{r}^C(r) = 1 - p > 0.$$

Thus, when renter acceptance probabilities increase sufficiently after the reform, some landlords optimally pass the agent fee shock into higher posted rents despite the legal cap.

Prediction 3 (Price Effects in the Capped Segment – Alternative Policy Environment) *Under a policy alternative with large administrative penalties, transparent rent caps, and easy cost-free tenant challenges ($\pi \gg 0$ and high p) above-cap posting becomes substantially less attractive. A high penalty π lowers the level of expected profits from overcharging, while transparency and easy challenges raise the probability p that overcharging is detected and successfully challenged. Together,* *these mechanisms reduce both the extensive and intensive margins of overcharging and weaken or eliminate pass-through into above-cap asking rents.*

3.5 Liquidity Constraints and Equilibrium Spillovers

Suppose landlords differ in liquidity endowments ℓ_i. After the reform, landlords can use agents only if

$$\ell_i \geq \kappa r.$$

Hence, some landlords switch away from agent use after the reform. Since aggregate listing visibility equals

$$L = \sum_{s,m} \gamma_m L_m^s,$$

reduced agent usage lowers aggregate visibility because $\gamma_{\text{agent}} > \gamma_{\text{self}}$.

If aggregate visibility falls sufficiently, then the contact rate for self-posted listings,

$$A_{\text{self}} = \frac{U}{L},$$

increases. Higher contact rates reduce expected vacancy duration and make it optimal for self-posting landlords to hold out for higher rents.

The outside option

$$V_V^s(\text{not list})$$

captures the possibility that landlords temporarily delay or avoid advertisement altogether.

Prediction 4 (Reduced Listing Volume) *The reform reduces agent usage and may reduce overall listing volume if some landlords choose the outside option of delaying or avoiding advertisement.*

Prediction 5 (Free Lunch) *Landlords who self-post listings may increase asking rents if reduced agent usage lowers aggregate listing visibility sufficiently to raise the contact rate for self-posted listings.*

4 Scope and Data

4.1 Scope of the Empirical Analysis

The policy is effective nationwide, yet we strictly focus our analysis on the Austrian capital, Vienna. This selection is reasonable due to several features: First, Vienna is Austria's single largest city and has with 19% the by far lowest share of owner-occupiers across all federal states (Table 1).

Furthermore, benchmark rents, which indicate the legally permissible charges within the capped sector, are lowest in Vienna and Burgenland. While the share of renters is high in Vienna, owner-occupation is the dominant tenure status in Burgenland (see again Table 1). Indeed, Burgenland's owner-occupation rate is even the highest across all Austrian federal states (63.5% in 2024[12]). Above, Vienna is Austria's capital and single large city, while Burgenland is Austria's least populated and largely rural federal state.

[12] Statistik Austria, Mikrozensus Wohnen 2024, https://www.statistik.at/fileadmin/shared/QM/Standardd

Finally, the rent cap is most binding in the capital, where the difference between the benchmark rent per square meter and the paid rent per square meter is by far the largest across all states (Table 1).

4.2 Rent Data

To track changes in landlords' behaviour upon the enactment of the ordering principle, we make use of a collection of rent listings maintained by the company *IREEN GmbH*. The data are drawn from scraped listings across all major advertising platforms used in Austria and have been de-duplicated, yielding a comprehensive and unbiased dataset. We access all listings referring to properties within Vienna and posted between January 2022 and February 2024.

We estimate both price and volume effects (following the taxonomy in Hahn, Omerovic and Waltl, 2025). Thus, we focus on two outcome variables: the number of listings posted per month or quarter to measure volume effects as well as advertised asking rents to measure price effects initiated by the supply-side. Additionally, the data comes with a large set of standard hedonic characteristics including location. Regarding location, roughly a quarter of the listings state the exact address and hence a precise geocode (24.3% in our sample), another third (33.2%) comes with an approximate geocode, while 42.5% report only one of the 23 Viennese districts. Approximate geocodes are produced in case of incomplete addresses, which come together with a confidence radius measuring the level of locational ambiguity, i.e., the larger the radius the less precise the geocode. This procedure is commonly applied for real estate listings data (see also Hahn et al., 2024). Depending on the outcome variable and model specifications, we filter the data in a way that geographic information is included at the necessary level of detail.

Further, we need to classify properties according to their legal status: As described in section 2, properties are subject to the rent cap depending on the year of construction.[13] This information is, however, not consistently reported in rental listings. To reconstruct this information for as many listings as possible and by that minimising a selection bias, we match addresses with the exhaustive, official building registry *Wiener Gebäude- und Wohnungsregister* (*WGWR*).[14] Thus, our final sample (the "address sample") consists of all listings reporting an exact or approximate geocode, and contain information on the year of construction (reported or successfully reconstructed). This leaves us with approximately 28,000 observations. For the "full" sample ($\sim$76,000 observations), we can also impute the year of construction whenever missing yet no geocode (district information is always available).

We run robustness tests to understand the impact of our filtering process, as well as relying on enriched information from register data reported in Appendix D and reconstructed information.

As the "address" sample shows some differences in the mix of hedonic characteristics (Table 6), we add hedonics as controls to all our models and check for differences in rent price trajectories when estimating them from the "full" or "address" samples, respectively. Time-dummy rent

okumentationen/B_2/std_b_mz-arbeitskraefte-wohnungserhebung_ab_2004.pdf, last accessed in February 2026.

[13]Information on whether an apartment is subject to the rent cap is not explicitly stated in the data yet needs to be inferred. We thus assign only apartments in buildings constructed before 1945 with less than 130m² of size that are not located on the roof floor to the regulated segment. This is a conservative identification procedure, since we want to make sure that results for the regulated segment are not distorted by wrongly assigned apartments.

[14]Kadi (2025) also makes use of this data to account for the missing year of construction.

indices show similar behaviour in both samples as confirmed in Figure 7 providing some evidence that market-wide changes in rents are likely not affected by a sample selection bias.

Table 11 in the Appendix reports descriptive statistics for the address sample separately compiled for the pre- and post-reform period. Tests for differences in means and proportions, respectively, non-surprisingly reveal differences in the share of dwellings subject to the rent cap as well as price effects. Both measures indicate the presence of volume and price effects, which we test for in subsection 5.1 and subsection 5.4. The significant changes in additional hedonic characteristics suggest that the reform may not affect all types of dwellings equally. Accordingly, in addition to using hedonic characteristics as controls, we hence test for systematic differences in effect sizes across property types as part of our comprehensive heterogeneity analyses.

4.3 Micro-Census

The Austrian Micro-Census[15] provides detailed and internationally comparable data on housing, employment, unemployment, and education. As a sample survey, it complements the register-based census introduced in 2011 and includes annually rotating thematic modules addressing key socio-economic topics. Conducted quarterly, the survey covers 22,500 randomly selected households drawn from the Central Residence Register (ZMR), with each household participating in up to five consecutive quarters over a ten-year period. Participation is compulsory, with initial interviews carried out in person and subsequent waves conducted by telephone or online.

To match the socio-economic characteristics recorded in the micro-census with the rent data, we make use of data for Vienna only.

4.4 Further Statistics

Data on household characteristics by district stem from administrative sources. First, demographic statistics provided by the *City of Vienna*,[16] labour market statistics and income tax tabulations provided by *Statistik Austria*.[17] and migration statistics again provided by *Statistik Austria*.[18] Figure 9 and Table 12 in the Appendix show the distribution of some socio-economic characteristics across the 23 Viennese districts.

[15]See https://www.statistik.at/en/about-us/surveys/individual-and-household-surveys/microcensus for details. Last accessed in February 2026.

[16]See https://www.wien.gv.at/statistik/bezirksdaten, last accessed in February 2026.

[17]See *Abgestimmte Erwerbstatistik* (AEST) https://www.statistik.at/en/about-us/surveys/register-based-census/register-based-labour-market-statistics and https://www.statistik.at/en/statistics/national-economy-and-public-finance/public-finance/tax-statistics/wage-tax-statistics. Both last accessed in February 2026.

[18]See https://www.statistik.at/fileadmin/shared/QM/Standarddokumentationen/B_2/std_b_wanderungsstatistik.pdf, last accessed in February 2026.

5 Empirical Analysis

5.1 Volume Effects

5.1.1 Outcome Variable

To measure changes in advertising behaviour post-enactment of the policy. Hence, we measure quantity effects using as outcome variable the number of advertised units u_{tml} per month $T \in \{01-2022, \ldots, 02-2024\}$ or quarter $T \in \{Q1-2022, \ldots, Q1-2024\}$, marketing status $m \in \{a, \neg a\}$ where a refers to an agent-inserted and $\neg a$ a privately posted advert, and group G differentiating counts by further property characteristics.

5.1.2 Overall Volume Effects

As long as *some* landlords choose the outside option of delaying or avoiding advertisement their units upon vacancy, we expect a decrease in the overall volume of posted listings following the enactment at $t^* = 07/2023$ (Prediction 4). Figure 2a shows $u_{t\cdot\cdot}$, i.e., quarterly counts over time regardless of marketing channel and regulation status.[19] A sharp drop upon enactment is clearly visible graphically and confirmed by a one-sided Wilcoxon signed-rank test on $H_0 : u_{t\cdot\cdot|t<t^*} = u_{t\cdot\cdot|t\geq t^*}$ vs. $H_1 : u_{t\cdot\cdot|t<t^*} > u_{t\cdot\cdot|t\geq t^*}$ for the number of listings per quarter. The drop is statistically significant at the 10%-level ($W = 12$, p-value < 0.07).

Figure 2: Volume Effects

(a) Overall (b) By Use of an Agent (c) By Regulation Status

Notes: Based on "full" sample without filters. Partial imputation of missing values in 03-22, 11-22 and 12-22. Plot (c) after imputation of missing year of construction.

This is supported by a multivariate regression analysis. For the number of counts per period t_i (quarters or months) and district, we assume $u_{t_i\cdot\cdot} \sim NB(\mu_i, a)$, i.e., a Negative Binomial distribution that allows for overdispersion in count data. The counts have mean $\mu_i = \mu_{t_i}$ and probability mass function

$$\mathbb{P}[u_{t_i\cdot\cdot} = k] = \frac{\Gamma(a+k)}{\Gamma(a)k!} \left(\frac{\mu_i}{\mu_i + a}\right)^k \left(\frac{a}{\mu_i + a}\right)^a, \quad (1)$$

[19] Counts are not available for a handful of shorter periods in 2022 due to random scraping issues. We impute missing data as explained in Appendix C.

where $\Gamma(\cdot)$ denotes the Gamma function. We normalise counts with district population totals from Statistik Austria to ensure a comparison of like with like.

As regressors, we include time dummies (either staggered or as a single *Post*-dummy), district indicators, and a seasonal trend $f(\cdot)$ following Hahn, Omerovic and Waltl (2025). The trend enters the model as an offset, enabling us to disentangle seasonal fluctuations in turnover (Selcuk, 2014; Yilmaz, Talavera and Jia, 2022) from the shock induced by the introduction of the ordering principle.

We estimate the seasonal component – i.e., the parameters $\tilde{\beta}_1$ and $\tilde{\beta}_2$ in (2) – using monthly and quarterly migration register data ('Wanderungsstatistik') from the Austrian Central Residence Register (CRR)[20] covering moves within Vienna as well as in- and out-migration over the period Q1-2010 – Q4-2024. The estimated trend is then included as an offset in the test model.[21] By that, the time dummies t_i measure dynamic volume effects net of seasonalities and can thus be used to identify excess reduction following the reform at $t^* = 07/2023$ as a causal consequence of the ordering principle.

We identify the general volume effect caused by the ordering principle via an event study design by relying on the enactment date for identification. We test the effect using different types of time dummies δ_t:

$$\log\left(\frac{\mu_{it}}{Pop_i^{District}}\right) = \beta_0 + f_{cyclical}(\tilde{\beta}_1, \tilde{\beta}_2) + \beta_3 District_i + \delta' D_{it}. \tag{2}$$

First, we split the time period into pre- and post-periods, yielding δ_{post}. The results from Model (1), reported in Table 2, confirm the descriptive analysis by identifying a general decline in posted listings following enactment. These findings are robust to the choice of dataset, as confirmed by Model (2).

Further, we estimate staggered effects yielding period-wise time-dummies δ_t shown in Figure 3 (full regression results are reported in Table 13 in the Appendix): again, a sharp and statistically significant drop in counts coincides with the policy enactment regardless of temporal aggregation choice, which means a change in advertising practises following the enactment of the policy.

These results thoroughly confirm the predicted overall drop in supply measured via posted listings in-line with Prediction 4.

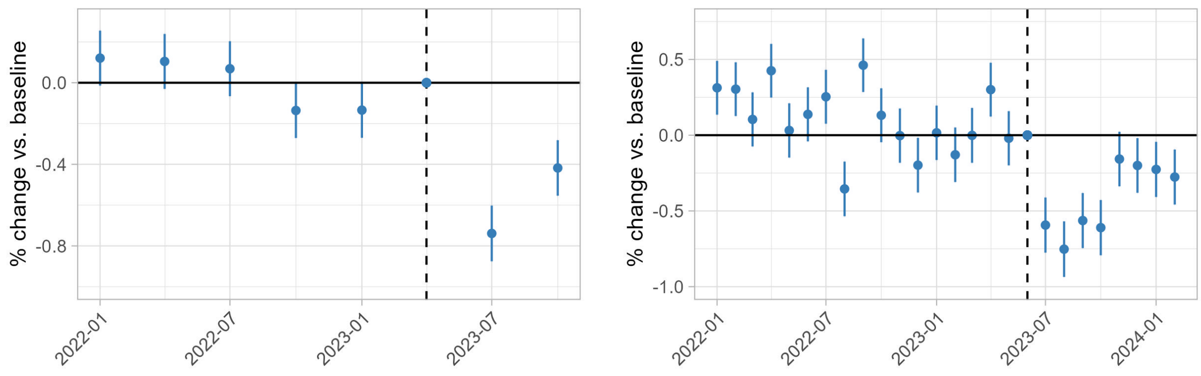
5.2 Transmission Channel

The additional costs associated with hiring an agent may encourage owners to rely less frequently on professional brokerage services. Those who choose not to engage an agent can instead advertise their properties privately or withdraw them from the market. To understand the importance of either channel, Figure 2b plots raw changes in the relative importance of either

[20] See https://www.statistik.at/fileadmin/shared/QM/Standarddokumentationen/B_en/engl_std_b_wanderungsstatistik.pdf, last accessed in August 2025.

[21] Details on the estimation of the seasonal trend are provided in Appendix B. The trend peaks in Q3 and reaches its minimum in Q1 (see Table 5). Similar seasonal patterns in advertising behaviour are documented for the Austrian sales market in Hahn, Omerovic and Waltl (2025), where listings peak in July and are lowest in January. These patterns are robust across periods.

Figure 3: Count Models: Staggered Volume Effects



(a) Quarterly (b) Monthly

Notes: Plots show results for the time-dummies of negative binomial models with cyclical trend and district population as offsets. Based on "full" sample without filters. Partial imputation of missing values in 03-22, 11-22 and 12-22. Detailed results in Table 13.

marketing strategy over time, i.e., counts of listings with $u_{ta\cdot}$ and without $u_{t\neg a\cdot}$ stating an agent as contact person. Visual inspection already suggests that volume effects are mainly driven by a decreased use of agents.

To strengthen the descriptive result, we estimate volume effects in a multivariate setting by separately counting listings posted by an agent or privately $u_{tm\cdot}$ and adding appropriate additional regressors. Interacting the agent identifier with time dummies yields a Difference-in-Discontinuities design (DiDC) for various market segments.[22]

This allows us to split the overall effect documented in Model (1) and (2) by marketing strategy. Model (3) reiterates the univariate results and shows that the general drop is almost exclusively driven by agent-inserted listings as the *Post*-dummy itself is, while still negative, statistically not significantly different from zero. While the number of privately advertised units remains unchanged, we identify a drop by roughly 40% in the agent-segment: exp(-0.545) = 0.580. Assessing again staggered treatment effects in the form of quarterly or monthly time-dummies in Table 14 and Figure 10 confirms that the shift indeed coincides with the introduction of the ordering principle and is driven by fewer agent-inserted listings.

Hence, the data provide little evidence that – at least in the short-run – landlords offset this decline by increasing their own marketing efforts, explaining the sharp drop in overall market turnover. This points towards reduced demand for real estate agents' services following the introduction of the ordering principle and, more broadly, to a contraction in market activity in line with Prediction 4. This finding contrasts the predictions expressed in parliament (see subsection 2.3): the number of private listings did not rise proportionally in the short run and

[22]We test for parallel pre-trends in the spirit of Hahn et al. (2024) by comparing differences in price indices for the respective groups of interest pre-enactment. Pearson product-moment correlation tests for the 12 months before the enactment yield high correlation coefficients (ρ) and reject the null hypothesis of no correlation supporting the parallel pre-trends for agent vs. private ($\rho = 0.813$; p-value=0.001), rent cap vs. exempt ($\rho = 0.952$; p-value=0.000), low third vs. medium third ($\rho = 0.932$; p-value=0.000) and medium third vs. high third ($\rho = 0.939$; p-value=0.000).

Table 2: Diff-in-Disc: Counts

	Counts per Quarter				
	(1)	(2)	(3)	(4)	(5)
Intercept	-5.057***	-4.995***	-7.783***	-5.038***	-5.362***
	(0.094)	(0.075)	(0.083)	(0.092)	(0.079)
Post	-0.508***	-0.575***	-0.048	-0.517***	-0.474***
	(0.045)	(0.037)	(0.058)	(0.060)	(0.062)
Agent			2.398***		
			(0.038)		
Rent Cap				-0.997***	
				(0.043)	
low priced					-0.954***
					(0.044)
high priced					-1.020***
					(0.044)
Post x Agent			-0.545***		
			(0.076)		
Post x Rent Cap				-0.028	
				(0.087)	
Post x low					-0.019
					(0.090)
Post x high					-0.107
					(0.090)
Model	*Total*	*Total*	*Agent*	*Regulation*	*Price (thirds)*
Start	*2022-01*	*2022-01*	*2022-01*	*2022-01*	*2022-04*
End	*2023-12*	*2023-12*	*2023-12*	*2023-12*	*2023-12*
District FE	✓	✓	✓	✓	✓
Cyclical Trend (offset)	✓	✓	✓	✓	✓
Population Weights (offset)	✓	✓	✓	✓	✓
Sample	Full	Full	Full	Full	Full
Imputed	No	Strat. A	Strat. A	Strat. A	Strat. A
Obs.	552	552	1104	1104	1656
AIC	5751.7	5590.0	9279.0	10333.1	14150.2

Notes: Based on Negative Binomial Count Models on monthly basis for different time periods with offsets for seasonal trend based on Table 5 and district population by quarter. Missing values imputed in 03-22, 11-22 and 12-22. Imputation strategy according to Appendix C. Negative Binomial Models of total monthly counts. Alternative time periods in Table 17. Further results in Table 14, Table 15 and Table 16. Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

thus failed to compensate for the decline in professional listings.

5.3 Heterogenous Volume Effects

While the distinction between agent-marketed and privately advertised properties likely accounts already for much of the shift in the transaction portfolio before and after the policy's enactment documented in Table 11, additional heterogeneities may further clarify which segments – beyond marketing status – systematically contribute more to the overall decline. Therefore, we refine the response variable again into separate group-specific counts $u_{t \cdot G}$.

First, we differentiate listings by the unit's rent cap status. We identify rent-capped properties by relying on hedonic characteristics as described in subsection 2.2. Model (4) in Table 2 finds a general lower advertising volume for rent-capped properties indicated by the significant negative coefficient associated with the properties status ("Rent Cap") yet no systematic change in this pattern post-enactment of the rent freeze as evident by the insignificant interaction effect "Post × Rent Cap."

Next, we assess differences by price segment. Therefore, we *a priori* classify properties into three price segments: Low (bottom 20% in terms of rent per square meter), Middle 60% and Top 20%. To account for a general price trend, we compute price segment thresholds in Q2-2023 and deflate them.[23] We identify no heterogenous policy effect across the price distribution.

Finally, we assess changes in the advertising behaviour by location differentiating classifying districts via socio-demographic characteristics. As reported in Table 18 in the Appendix, none of the tested dimensions yields significant heterogeneities.

5.4 Price Effects

5.4.1 Overall Staggered Price Effects

Consistent with Prediction 1, the magnitude of negative volume effects may be attenuated by owners increasing rents to offset higher marketing costs. To test for this channel, we set up a hedonic price model capturing the Viennese rental market.

The general model structure is given as

$$\log(R_{it}) = \alpha + \delta' D_{it} + \beta' X_i + g(location_i) + \varepsilon_{it}, \tag{3}$$

where D denotes again time-dummies with associated parameters δ_t, X contains standard property-specific physical characteristics with associated shadow prices β, $g(\cdot)$ is a function of districts or spatial coordinates specifying the location of property $i \in \{1, \ldots, n\}$, and $\varepsilon \sim^{iid} N(0, \sigma^2 I_n)$.

Locational effects $g(location_i)$ are modelled as regression splines in two ways: First, we use a *Markov Random Field Smooth* (*MRF*) as discussed in Wood (2006) and applied to hedonic models by Naidin, Waltl and Ziegelmeyer (2025). This choice finds a compromise between very restrictive district-specific dummies ignoring the administrative structure of the city and

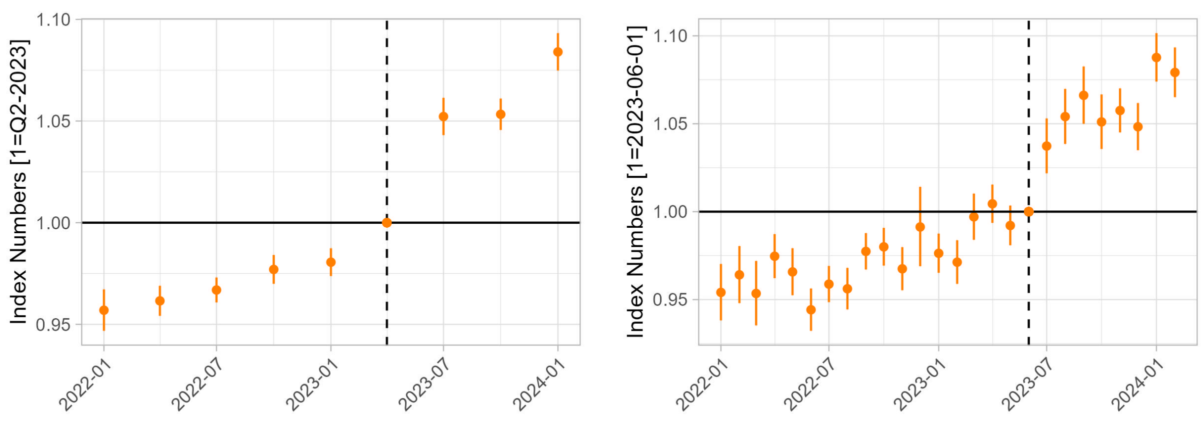
[23] We deflate segment thresholds using a subcomponent of the Austrian consumer price index measuring general changes in new rents across the countries: *041100: Neuvermietungen* .

the sometimes overly wiggly price maps defined on longitudes and latitudes directly. The MRF Smooth models the neighbouring structure of the 23 districts of Vienna via a spatial lag and thus reproduces the administrative structure of the city, and additionally smooths over district edges to avoid artificial jumps along district boundaries. Second, we estimate a standard geographical spline estimated on longitudes and latitudes. To correctly model spatial coordinates, we use a Spline on Sphere (SOS) suggested by Wahba (1981).

Table 3 reports estimation results. Estimated shadow prices for physical characteristics follow standard expectations. In particular for Vienna, the estimated hump-shaped pattern of building age is in-line with a vintage effect acknowledging high demand for apartments in historic buildings characterized by their exceptional architectonic *Gründerzeit*-style developed between 1848 and the First World War (Musil et al., 2022). Furthermore, as expected, we also identify a general price discount for rent-controlled units ('Rent cap') as well as a price premium for newly built or refurbished units ('First-time use'). With regards to the importance of locational attributes, we identify a slightly better fit (according to R^2 and AIC) using a spline estimated on exact or approximate geospatial coordinates than using the always exactly specified district information directly. Hence, we base our main estimation results on models (2) and (4), and report robustness checks with regards to this choice in Appendix D.

To obtain price trends, we perform a back-transformation (necessary as we model logged rents) and a normalisation to a base-period t^* for all time-periods $I_t = \left\{ \frac{\exp(\delta_t)}{\exp(\delta_{t^*})} \right\}_t$ yielding an unbiased and quality-adjusted index tracking changes at the median rent (see Waltl, 2016, for a proof). As base-periods we choose the quarter or month immediately preceding the reform, i.e., 06-2023 and Q2-2023 respectively.

Figure 4: Total Staggered Price Effects



(a) Quarterly (b) Monthly

Notes: These plots show the back-transformed effects of monthly and quarterly time dummies for two different regression specifications using a Spline on Sphere (SOS). Detailed results can be found in Table 3 in columns (2) and (4). Coefficients for time dummies can be found in Table 19.

Distinguishing index numbers I_t before and after the introduction of the policy produces an event-study design fit to measure staggered effects over time. We first perform a visual assessment of the time trend in Figure 4. The time effects indicate a pronounced upward price

Table 3: Hedonic Regression

	log(R)			
	(1)	(2)	(3)	(4)
Intercept	3.002***	3.206***	2.996***	3.205***
	(0.018)	(0.033)	(0.018)	(0.032)
Living Area [log]	0.858***	0.861***	0.858***	0.861***
	(0.003)	(0.003)	(0.003)	(0.003)
Balcony/Terrace	0.089***	0.075***	0.088***	0.073***
	(0.003)	(0.003)	(0.003)	(0.003)
Garden	0.042***	0.043***	0.043***	0.043***
	(0.003)	(0.003)	(0.003)	(0.003)
Cellar	-0.002	0.003	-0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Level: higher than ground floor	-0.069***	-0.017	-0.068***	-0.018
	(0.011)	(0.012)	(0.011)	(0.012)
Level: roof	-0.035*	0.015	-0.033*	0.015
	(0.015)	(0.015)	(0.015)	(0.015)
Elevator	-0.006	0.024+	-0.011	0.023
	(0.013)	(0.014)	(0.013)	(0.014)
First time use	0.096***	0.090***	0.096***	0.091***
	(0.003)	(0.003)	(0.003)	(0.003)
pre-1945	0.049***	0.021***	0.050***	0.022***
	(0.005)	(0.005)	(0.005)	(0.005)
Heating efficiency: A	0.168***	0.161***	0.167***	0.161***
	(0.005)	(0.005)	(0.005)	(0.005)
Heating efficiency: B	0.117***	0.107***	0.117***	0.107***
	(0.004)	(0.004)	(0.004)	(0.004)
Heating efficiency: C	0.019***	0.008*	0.019***	0.008*
	(0.004)	(0.004)	(0.004)	(0.004)
Heating efficiency: unknown	0.073***	0.050***	0.074***	0.049***
	(0.004)	(0.004)	(0.004)	(0.004)
Rentcap	-0.121***	-0.117***	-0.121***	-0.118***
	(0.005)	(0.005)	(0.005)	(0.005)
Agent	-0.023***	-0.046***	-0.022***	-0.046***
	(0.005)	(0.004)	(0.005)	(0.004)
Level: higher than ground floor × Elevator	0.064***	0.031*	0.069***	0.032*
	(0.014)	(0.014)	(0.014)	(0.014)
Level: roof x Elevator	0.081***	0.061***	0.085***	0.062***
	(0.017)	(0.017)	(0.017)	(0.017)
Time-Dummies	Monthly	Monthly	Quarterly	Quarterly
Location	MRF	SOS	MRF	SOS
District FE		✓		✓
Obs.	27904	27904	27904	27904
Adj. R^2	0.818	0.852	0.817	0.852
AIC	350334.1	344982.8	350393.4	345032.0

*Notes: Based on GAM modeling location via Markov Random Field (MRF) Smooth or Spline-on-Sphere (SOS) Smooth. Base groups for categorical variables are "level: groundfloor" and "heating efficiency: DEFG." The category "pre 1945" subsumes apartments subject to the rent cap as well as apartments exempt (larger than 130m^2 or loft conversion). Estimated on the "address" sample. Time dummies are shown in Figure 4 and Table 19. Significance codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

trend, consistent with expectations for a period characterized by elevated inflation. Beyond this general trend, both quarterly and monthly time effects consistently reveal a pronounced, one-off upward shift in the overall price level that coincides precisely with the introduction of the ordering principle, suggesting capitalization of excess costs.

By normalizing the time dummies to the period immediately preceding enactment, the staggered effects post-enactment can be interpreted directly as capitalization effects. In the first month (quarter) after the policy change, asking prices increased by 3.8% (5.2%), as reported in Table 19.

Further, we check whether the policy means a structural break beyond residual price effects. Note that, so far, we have used hedonic characteristics as controls only, which was necessary due to observed variation in the mix of characteristics before and after the policy enactment discussed in section 4. Yet, we have held the parameters associated with hedonic controls, i.e., shadow prices, constant for the entire period. To understand whether the overall price effect is contaminated with changes in the valuation of some physical or locational attributes, we next run a Chow-test (Chow, 1960) and a Wald-test assessing whether the estimated shadow prices changed following the reform, i.e., we test for a potential structural break associated with the ordering principle.

We hence interact all hedonics with the *Post*−dummy and re-estimate the resulting extended model. Results shown in Table 24 indicate that several characteristics are indeed valued differently after enactment. This is suggested by the results from univariate t-tests already and the Chow-test confirms the joint significance indicating indeed a structural break in shadow prices (Chow test: $F(38.5, 27331) = 5.71$, p-value < 0.001; Wald test: $\chi^2(39)$=226.83, p-value $<$ 0.001).

When allowing for this flexibility in the models, an even larger quarterly policy price effect[24] of $\exp(0.133) = 1.14225$ is identified suggesting that the constancy assumption yields only a lower bound of the overall price effect associated with the ordering principle.

This structural break motivates an assessment of the transmission channels at work in subsection 5.5 as well as a broad heterogeneity analysis of the price effect performed in subsection 5.6.

5.5 Transmission Channel

As predicted in Prediction 1, as long as there are renters willing to accept higher rents, an increase in the non-capped segment is expected as landlords likely aim to pass on the extra costs endured when hiring an agent by themselves. To test for this channel, we study price trajectories before and after the enactment separately for agent-posted and privately posted listings.

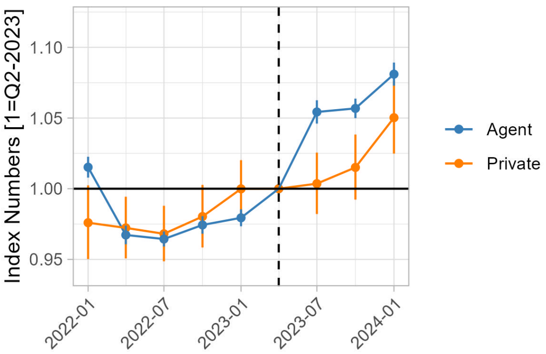
Figure 5a shows the quarterly time effects resulting from a price model interacting time-dummies with an agent dummy.[25] While asking rents jump by roughly 5% between Q2-2023 and Q3-2023 in the agent segment, no significant increase can be found for private insertions. This suggests that the overall effect from Table 19 is driven by increased asking rents among apartments marketed by agents.

However, there appears to be a broader change in the sample composition for apartments

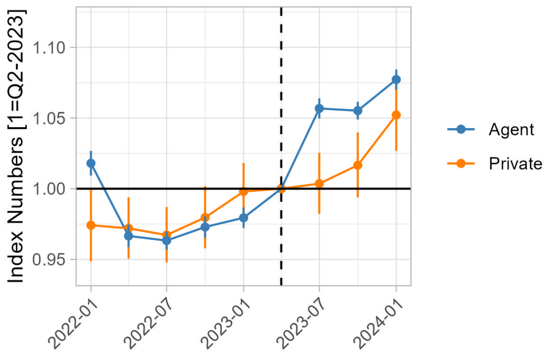
[24]The effect size is robust in significance and size to also adding an interactions of the treatment effect with the locational spline.

[25]We test for parallel pre-trends in the spirit of Hahn et al. (2024) by comparing differences in the price indices between the treatment (agent-inserted) and control groups (privately posted) pre-enactment. A Pearson product-moment correlation test for the five quarters before the enactment yields a high correlation coefficient of $\rho = 0.869$ and rejects the null hypothesis of no correlation with a p-value of 0.02468 supporting parallel pre-trends.

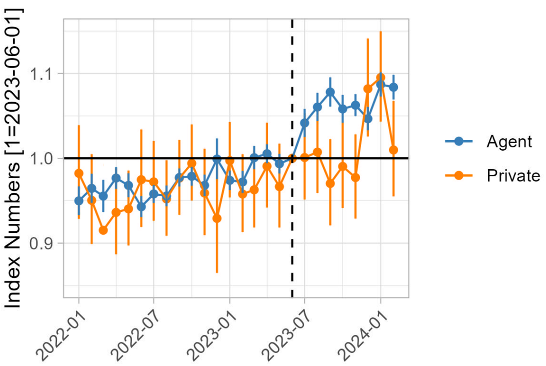
Figure 5: Staggered Price Effects by the Use of Agents



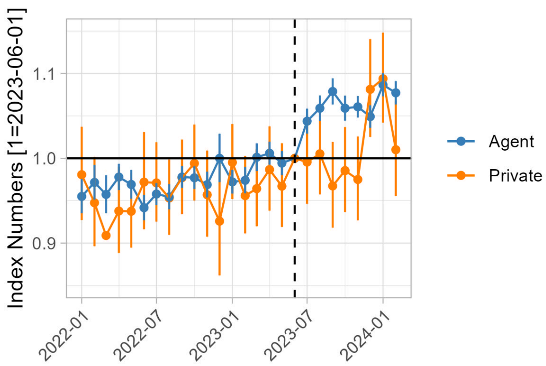
(a) Unweighted Regression - quarterly



(b) Weighted Regression - quarterly



(c) Unweighted Regression - monthly



(d) Weighted Regression - monthly

Notes: These plots show the back-transformed effects of time dummies resulting from Diff-in-Disc models using agent- versus privately advertised units for identification. Panels (a) and (b) report quarterly estimates on the "full" sample, while panels (c) and (d) report monthly estimates on the "address" sample. Unweighted specifications are shown in panels (a) and (c), while weighted specifications enforcing balance are shown in panels (b) and (d). Detailed regression results are reported in Table 9 and Table 10.

marketed via agents. This is represented by significant shifts in the mean apartment size as well as the proportions of apartments offering a balcony or garden, and new apartments.[26] Among private insertions, the sample characteristics remain roughly constant after the reform, while only the share of first-time use shows some deviations.

This different structure in the two groups means that the measured 5% increase in asking rents in Figure 5a could simply be the result of a change in sample composition rather than a price effect associated with the policy enactment. To rule this out, we re-weight the samples and by that enforce a comparison of like with like. Then, we re-estimate the regressions including regression weights (see Appendix E for technical details and robustness checks). Figure 5b and Figure 5d show results. There are almost no changes in estimated effect sizes confirming that the price increase is not a consequence of changes in the mix of properties on the market yet can truly be associated with the ordering principle.

From these analysis, we conclude the price increases being over-proportionally driven by agent-inserted units, and hence landlords' tendency to pass on extra endured costs to tenants. This is particularly the case within the first three quarters following the reform. However, Figure 5 also shows that within a year, prices of privately posted units catch up hinting towards a free-lunch effect stemming from an increased market price level (Prediction 5). To establish a full understanding of which market segments are most affected by this price increase, subsection 5.6 next performs a rigorous heterogeneity assessment.

5.6 Heterogenous Price Effects

Similar to the heterogeneity assessment for volume effects in subsection 5.3 and motivated by the detected structural break in subsubsection 5.4.1, we test for heterogeneity in price changes upon enactment along various dimensions. So far, we have only controlled for strictly rent-capped properties as opposed to the remaining market (Exempt) comprising units with freely negotiated initial rents. As the legally imposed price-setting mechanism differs substantially from the rest of the market, we split the sample and estimate treatment-effects separately for these two market segments. As Austria currently lacks a stringent enforcement mechanism, landlords may still be encouraged to pass on increased search costs to tenants (Prediction 2).

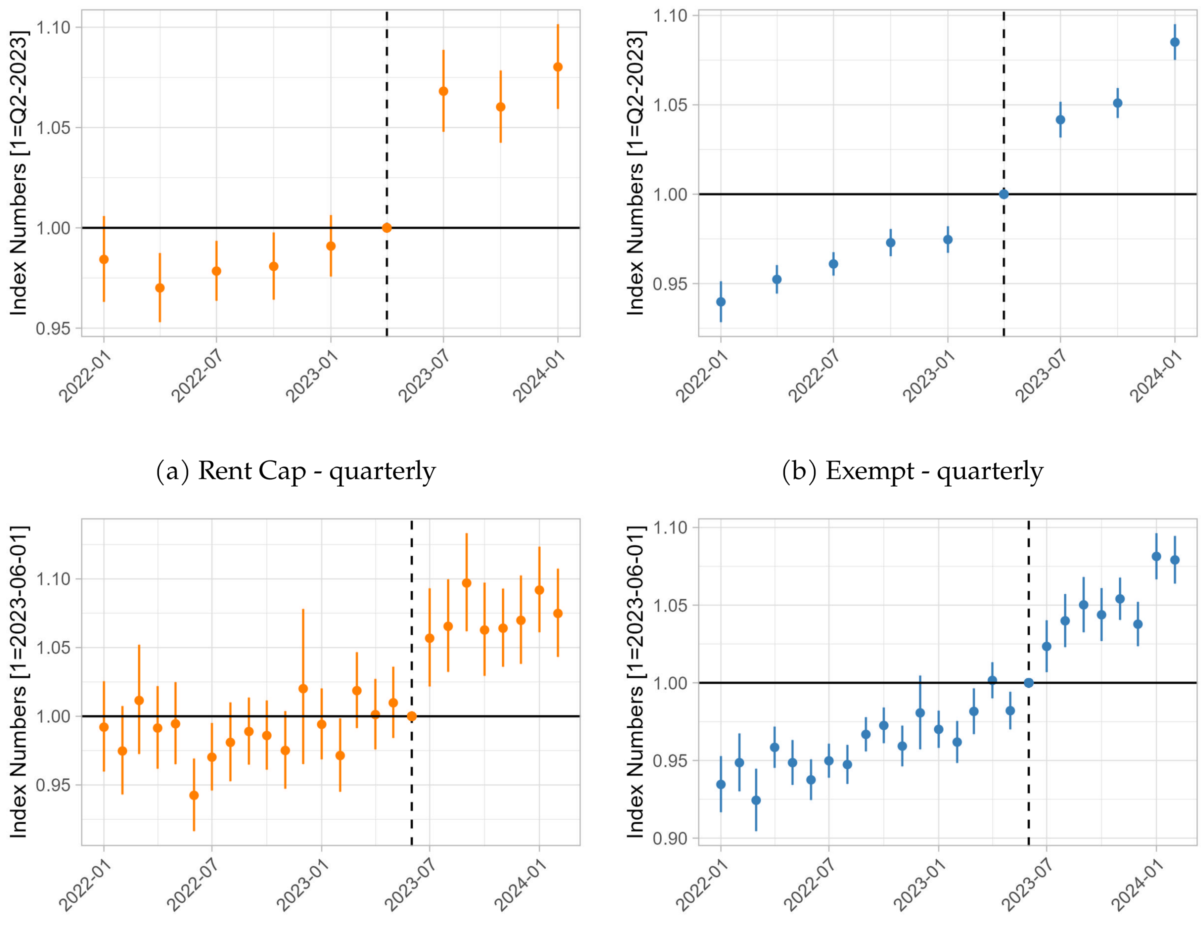
We hence estimate Model (3) separately on rent-capped properties and those not subject to this regulation. We identify the legal regime applicable to each unit as described in subsection 4.2.

Consistent with the general price-setting mechanisms in the two market segments, Figure 6a and Figure 6c show flat prices before and after the reform in the rent-capped segment. In contrast, the unregulated segment exhibits a positive, approximately linear short-run trend, as shown in Figure 6b and Figure 6d.

On top of these general trends, all panels in Figure 6 identify a significant level-shift following the reform regardless of a property's regulation status and temporal aggregation. Using quarters as temporal units, we identify an immediate increase of $\exp(0.066) = 6.8\%$ for

[26] Table 8 shows differences in means and proportions between the pre- and post-period for the "address" sample. A Chow-test confirms a structural break within the agent sample: $F(38.231,25506)=5.7071$, p-value: <0.001. For the control group, only few characteristics are differently evaluated in the post period than in the pre-period at a significant level. This is reflected by an insignificant Chow-test: $F(6.3319,1461.1)=0.1222$, p-value: 0.995.

Figure 6: Staggered Price Effects by Regulation Status



(a) Rent Cap - quarterly

(b) Exempt - quarterly

(c) Rent Cap - monthly

(d) Exempt - monthly

Notes: These plots show the separate back-transformed effects of monthly and quarterly time dummies for the regulated (rent cap) and unregulated (exempt) market segments using a Spline on Sphere (SOS) Smooth. Detailed results can be found in Table 20 and Table 21.

rent-controlled properties and $\exp(0.041) = 4.2\%$ for market rents. Similarly, when assessing the very short-run, effects by relying on monthly time-dummies, we identify an increase of $\exp(0.055) = 5.7\%$ for rent-capped units and of $\exp(0.023) = 2.3\%$ for exempt units between June and July 2023. This clearly indicates capitalisation effects in both market segments and even a larger impact in the officially rent-capped segment in the very short run, although for the latter no such adjustments are foreseen. This indicates that Vienna's strictly regulated first-generation rent-controlled sector seems to face a substantial enforcement issue in-line with Prediction 2.

To understand the association of these increases with the composition of the local tenant population, we create three clusters based on their location relative to the main ring road ("Gürtel") and the Danube: inner-ring districts, outer-ring districts, or edges and Transdanubia. Models (1)-(3) in Table 22 in the Appendix indicate that the wealthiest and most expensive inner-ring districts experience the largest price increases, while the most distant districts experience a significantly smaller price increase post-enactment.

As districts exhibit considerable heterogeneity in population composition across various dimensions (see Table 12), we next link districts directly to the socio-demographic characteristics of their resident population. This information is pulled from register data (see subsection 4.4). Precisely, we step-wise classify districts into two groups. For each district, we compute the shares of high-income or young (≤ 35 years) residents, registered students, Austrians, Non-EU citizens, and families. Again, we assign each district to either the upper ("many" or "upper half") or lower ("few" or "lower half") end of the distribution.

First, Models (4)-(6) rank districts by average net employee income. In-line with the geographical splits, this direct measurement finds again a more significant pass-through in absolute terms for more income-rich districts. This finding is in-line with the notion of on average higher willingness- and ability-to-pay in these districts.

Second, Models (10)-(12) show a weak yet consistent association of more significant price increases upon enactment and the share of registered higher-education students registered in a district. When compared to the assessment of age alone in Models (7)-(9), we conclude that effects appear to be indeed driven by regular university students but not younger residents in general. This finding would be consistent with students representing the most mobile group of residents in a university-city[27] housing market, potentially driving up prices more strongly in a market where matching costs are passed on to tenants in the form of increased rents.

Regarding migration status, models (13)-(15) find no association between the effect size and the share of Austrian citizens. However, when combining all EU-nationals into a single group and comparing them with non-EU citizens, models (16)-(18) identify a slightly lower effect size in districts with a high share of non-EU residents. This heterogeneity could be similarly interpreted as the income effect as non-EU residents are associated with lower incomes.[28]

Models (19)-(21) confirm that districts predominately preferred by families are not differently affected than others.

Overall, we conclude that the largest price increases in *absolute terms* are associated with central locations, a high share of students, and, to a lesser extend, a high share of EU residents.

Finally, we estimate effects directly for different price segments and regulatory status. Therefore, we re-estimate the hedonic models using conditional quantile regression evaluating the policy effect at different points of the rent price distribution given hedonic characteristics. We focus on three broad segments informed by the quantile-levels $\tau \in \{0.25, 0.5, 0.75\}$ and differentiating again by regulatory status. Table 23 reiterates the initial overshooting of all segments with systematically larger effects in the rent-capped segment and within this segment particularly among lower-priced units. A certain degree of correction is observed in the following quarters in most segments, and no significant heterogeneities are visible across price segments after three quarters.

[27] According to register data, Vienna and Berlin are the main university-cities in German-speaking Europe measured by the number of registered higher-education students (see City of Vienna, 2024).

[28] See https://www.statistik.at/en/statistics/population-and-society/income-and-living-conditions/household-income, last accessed in April 2026.

6 The Distribution of Gains and Losses: A Simulation

6.1 Identification of Losers and Winners

We elicit the average time horizon owners apply to amortize the additional cost using a set of representative simulations. Concretely, we do so by comparing the total amortised commission payable by contract form and length, and compare it to the estimated permanent price increases in agent-advertised units only. We further elicit the average tenure length per socio-economic group from the micro-census. This allows us to identify which groups on average will pay more ('losers') and which groups will pay less ('winners').

This further allows us to judge whether, on average, rather landlords or renters benefitted from this policy change.

6.2 Commission Amount

Irrespective of the ordering principle, the *commission amount* has been legally capped in Austria since 1st of September 2010.[29] The cap depends on the agreed monthly rent, determining the assessment base R_i, and the multiple κ determined by the ordering party and the type of lease. The payable commission can hence be written as $C_i = C(\kappa_i, R_i)$.

Precisely, the assessment base R_i is set equal to the monthly gross rent (rent plus operating costs excluding heating plus 20% VAT) for each property i. The multiple κ differs between types of lease agreements and ordering party. If charged to the tenant, the maximum commission multiple amounts to $\kappa = 2$ for open-ended or long-term (more than three years) contracts, $\kappa = 1$ for fixed-term contracts with a lease duration of less than three years, open-ended contracts marketed directly by the property management rather than an external agent, and $\kappa = 0.5$ for fixed-term contracts marketed by the property management with a lease duration of less than three years. If charged to the landlord, $\kappa = 3$ for all lease types.

These rules determine a maximum fee chargeable, and undercutting is, though not common practice, possible. Hence, the total payable amount can be expressed via

$$C_i \leq \kappa_i \times R_i \quad \text{for} \quad \kappa_i \in \{0.5, 1, 2, 3\}.$$

6.3 Simulation Set-Up

We express the cost shifted over by landlords to tenants as a share $\tau \in [0;1]$ of monthly counterfactual rent payments $\tilde{R}_i$. The counterfactual captures the notion that without the ordering principle the same landlords would have offered their units at a original lower rate $\tilde{R}_i$, yet do so at an increased rate, i.e., $R_i = (1+\tau)\tilde{R}_i$. Rearranging yields an expression for the cost increase,

$$\tau \tilde{R}_i = R_i - \tilde{R}_i. \tag{4}$$

[29] *Standes- und Ausübungsregeln für Immobilienmakler* (BGBl. Nr. 297/1996), §20 "Vergütung bei der Vermittlung von Mietverträgen über Wohnungen und Einfamilienhäuser"

At the same time, the commission payable is capped at C_i as detailed in subsection 6.2. For simplicity and following incentives prior to the ordering principle and hence common practice in Austria, we assume that agents charge the maximum allowed amount, i.e.,

$$C_i = \kappa R_i. \tag{5}$$

By amortising both costs (4) and (5), we can infer break-even, i.e., the number of months a tenant needs to remain in a unit for the landlord to be indifferent about whether or not to use an agent for commissioning a new rental agreement at the increased rent rate.

As soon as a tenancy duration surpasses the break-even threshold, landlords, on average, realize gains that exceed the level required for cost-recovery and hence benefit financially from the policy change.

Equating amortised costs over $N \in \mathbb{N}$ months assuming a risk-free interest rate $\iota > 0$ yields

$$\tau \tilde{R}_i = \frac{C_i \frac{\iota}{12}}{1 - \left(1 + \frac{\iota}{12}\right)^N}.$$

Plugging in (4) and (5), and solving for N allows us to determine break-even and hence landlords' implicit minimum time horizon,

$$N = \left\lceil \frac{\log\left(1 - \frac{\kappa(1+\tau)}{\tau} \cdot \frac{\iota}{12}\right)}{\log\left(1 + \frac{\iota}{12}\right)} \right\rceil,$$

where $\lceil \cdot \rceil$ denotes the ceiling function.

6.4 Simulation Results

Relying on the average marginal policy effect τ and calibration parameters as specified in Table 26 in the Appendix, we identify break-even after $N = 66$ months (5.5 years). In general, the longer a group's typical tenure duration, the more they lose in comparison to earlier practises of paying a one-time agency fee, yet facing a lower monthly rental burden.

In 2024, 64% of the renting population in Vienna had been living in the same property for more than 66 months. Should residential mobility not change in the future, the majority of tenants would incur a loss over the course of their tenancy due to the policy change. However, if residential mobility further increases, this share will likely drop in the future.

To understand which types of tenants would benefit already today, we compute contemporary tenancy durations for different groups of renters based on socio-economic characteristics (see Table 4). This allows us to infer *ceteris paribus* predictions of winners and losers. As shown in Figure 13 in the Appendix, the average tenancy duration (mechanically) increases with age (Panel a), and is higher for natives and 2nd generation immigrants than for first-generation immigrants (Panel b). Variation across household structure is limited (Panel c).

The low residential mobility of older tenants, natives and first-generations immigrants

Table 4: Tenure length Pre/Post

	Mean		Median			
Group	Pre	Post	Pre	Post	Diff.	SE
Total	137	128	44.6	37.5	-9.29**	3.12
Age group						
Under 25	23.8	24.5	18.7	18.1	0.71	2.8
25 to 34	40.9	34.6	24.9	23.8	-6.38**	2.1
35 to 44	73.9	61.1	35.9	28.9	-12.79***	3.7
45 to 59	144.5	136.2	88.6	63.1	-8.24+	5.0
60 and older	341.4	343.2	355.7	336.9	1.8	8.2
Migration background						
Natives	181.6	177.3	63.6	54.0	-4.29	5.2
1st generation migrants	94.2	82.9	34.3	29.4	-11.2***	3.3
2nd generation migrants	123.6	149.4	42.5	50.3	25.8	16.5
Household type						
HH with kids	108.3	93.1	48.7	33.6	-15.14**	5.8
HH without kids	150.9	147.3	42.5	40.9	-3.6	7.4
Others	86.0	75.7	27.9	26.7	-10.26	8.3
Single	151.0	135.3	46.5	38.6	-15.69**	4.8
Single parents	134.1	168.8	53.3	55.5	34.75**	11.7

Notes: Mean and median tenure length in months for renters in Vienna. "Pre" refers to Q1-2021 to Q2-2023, "Post" refers to Q3-2023 to Q4-2024. Column "Diff." refers to difference in means. SE result from t-tests. Source: Austrian Micro-census. Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

hence suggests that these groups will long-term predominantly bear the costs of this policy intervention as their group-specific break-even points occur later (Panels d-f). Given the extraordinarily long observed tenancy spells (Panels d-f), a divide between tenants having signed their tenancy agreement before the intervention and those signed after will likely characterise the Viennese rental market over the coming decades.

In the past, older native and 2nd generation immigrants have traditionally benefitted the most from strict rent regulation paired with the benefits of open-ended leases – due to their low residential mobility. As sections of society with on average increased residential mobility faced higher life-time costs. As this policy reform to a certain degree balances costs between groups of high and low residential mobility, it also implies a somewhat less unequal rental burden across cohorts and immigration status in the very long-term.

Moreover, a comparison of tenure lengths before and after the reform (see Table 4) suggests a noticeable shift in patterns: for nearly all household types, median tenure declines following the policy's implementation. This indicates an increase in residential mobility, plausibly driven by lower moving costs. However, whether this reflects a short-term adjustment to the reform or a persistent change in mobility behaviour can only be determined through a longer-term analysis.

7 Conclusions and Policy Recommendations

Using data from Vienna, Austria's largest rental market, we assess the effects of the recent introduction of the ordering principle, which requires the ordering party to pay agent fees. Before, it was common practice that landlords would order an agent, yet tenants had to bear the cost.

Our results further show that policy makers' predictions were wrong: A key argument used in the regulatory impact assessment claimed that negative volume effects are not expected from this intervention (prediction (2) in subsection 2.3). This is clearly contradicted by our analysis as all types of properties experienced a significant level-shift upon enactment. This result supports previous studies questioning the effectiveness of currently applied regulatory impact assessment systems (Carroll, 2010).

In addition to the overall decline in listing volume, we find that the reduction was driven primarily by agent-posted listings, while the share of privately listed properties remained largely unchanged. This suggests that a subset of landlords who would otherwise have relied on an agent chose not to re-let their property upon vacancy rather than switch to private advertising. Although the total number of unique listings falls during the six months following the policy's implementation, we cannot fully disentangle whether this reflects properties not being re-advertised at all or a marked slowdown in the re-listing process –potentially indicating reduced market efficiency due to diminished use of intermediaries who typically facilitate faster turnover.

While the reform may improve residential mobility in the long run by lowering upfront moving costs, its short-run incidence falls disproportionately on new and economically less affluent tenants. As those who still advertise their properties do so at on average increased prices. This suggests that agent-costs are rolled over to tenants. This was the case for regulated and unregulated properties. While this is legal for the first, this is a violation of the rent control scheme applicable to those units. Such behaviour is rational for landlords – and in-line with our theory – as they face no costs doing so given the lax implementation and no compliance incentives upon advertisement. As empirically shown by Jofre-Monseny, Martínez-Mazza and Segú (2023), a rent control policy ensuring the features of *transparency* (rental listings and tenancy agreements must state the applicable rent cap) and *enforcement* (non-compliant landlords are fined) may hinder such unintended side-effects as a consequence of a per se unrelated policy change. Also, our theoretical framework modelling the Viennese system predicts that a penalty can – depending on its size and the likelihood of tenants challenging the amount, which can be increased by enforcing transparency – substantially decrease the share of landlords choosing to overcharge (Prediction 3).

While we do not take a normative position on the efficiency implications of the Viennese multi-segmented rental market – particularly the historic remnant of a first-generation rent-controlled segment – we note that lax enforcement practices are generally problematic, as they introduce a wedge between informed and uninformed renters. Inspired by the Spanish example, we hence derive two general recommendations needed if the system as a whole should stay intact and fair: First, transparent communication of applicable rent regimes regulations should be made a requirement throughout the marketing process of vacant unit. Obligatory transparency requirements similar to the present system applied in Catalonia may be an option. Second, the current system encourages landlords to advertise excess rents due to a lack of negative consequences yet potential gains if renters do not challenge rents charged. In-line with these incentives, landlords indeed do not follow regulations upon advertisement questioning the efficiency of this approach for all involved parties. The introduction of fines again inspired by the Catalan example may hence be adopted.

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Appendix

A Theoretical Model: Derivations

A.1 Effective Rent

Before the reform, renters paid an upfront agent fee equal to κr upon signing a lease. The expected discounted tenancy duration is

$$\sum_{t=0}^{\infty} d^t (1-q)^t = \frac{1}{1-d(1-q)}.$$

Spreading the upfront fee over the expected discounted tenancy duration implies an equivalent per-period cost

$$\kappa r[1-d(1-q)].$$

Hence, the effective per-period rent is

$$\alpha r, \qquad \alpha = 1+\kappa[1-d(1-q)].$$

A.2 Matching Hazards

Let L_m^s denote the mass of listings in segment s using posting mode m. Aggregate listing visibility is

$$L = \sum_{s,m} \gamma_m L_m^s.$$

A renter contacts a specific listing with probability

$$\frac{\gamma_m}{L}.$$

Acceptance occurs if reservation rent exceeds effective rent. Therefore, the probability that a renter both contacts and accepts a listing charging rent r equals

$$\frac{\gamma_m}{L}[1-F(r)].$$

Multiplying by the mass of searching renters U yields

$$\lambda_m(r) = U\frac{\gamma_m}{L}[1-F(r)].$$

Defining

$$A_m \equiv \gamma_m \frac{U}{L},$$

gives

$$\lambda_m(r) = A_m[1-F(r)].$$

For agent-posted listings before the reform,

$$\lambda_{\text{agent}}^{\text{pre}}(r) = A_{\text{agent}}[1 - F(\alpha r)].$$

After the reform,

$$\lambda_{\text{agent}}^{\text{post}}(r) = A_{\text{agent}}[1 - F(r)].$$

A.3 Occupied Value

The occupied value satisfies

$$V_O^s(r) = \tilde{r}^s(r) + \beta[(1-q)V_O^s(r) + qW^s].$$

Rearranging yields

$$V_O^s(r) = \frac{\tilde{r}^s(r) + \beta q W^s}{1 - \beta(1-q)}.$$

Differentiating,

$$\partial_r V_O^s(r) = \frac{\partial_r \tilde{r}^s(r)}{1 - \beta(1-q)}.$$

A.4 Vacancy Value and First-Order Condition

The post-reform vacancy value is

$$V_V^s(m,r) = -C_m + \beta \left[(1 - \lambda_m(r))V_V^s(m,r) + \lambda_m(r)\left(V_O^s(r) - \mathbf{1}_{\{m=\text{agent}\}}\kappa r\right)\right].$$

Let

$$V \equiv V_V^s(m,r), \qquad O \equiv V_O^s(r), \qquad \lambda \equiv \lambda_m(r).$$

Then

$$V = -C_m + \beta V + \beta \lambda (O - V) - \beta \mathbf{1}_{\{m=\text{agent}\}} \lambda \kappa r.$$

Differentiating with respect to r,

$$V_r = \beta V_r + \beta \lambda'(O - V) + \beta \lambda (O_r - V_r) - \beta \mathbf{1}_{\{m=\text{agent}\}} \kappa [\lambda + r\lambda'].$$

At an interior optimum, $V_r = 0$, implying

$$0 = \beta \lambda'(O - V) + \beta \lambda O_r - \beta \mathbf{1}_{\{m=\text{agent}\}} \kappa [\lambda + r\lambda'].$$

Using

$$O_r = \frac{\partial_r \tilde{r}^s(r)}{1 - \beta(1-q)},$$

yields

$$\lambda_m(r)\frac{\partial_r \tilde{r}^s(r)}{1 - \beta(1-q)} + \lambda_m'(r)[V_O^s(r) - V_V^s(m,r)] = \mathbf{1}_{\{m=\text{agent}\}} \kappa [\lambda_m(r) + r\lambda_m'(r)].$$

A.5 Capped Segment and Penalties

For rents above the legal cap,

$$\tilde{r}^C(r) = (1-p)r + p\bar{r} - p\pi.$$

Hence,

$$\partial_r \tilde{r}^C(r) = 1-p.$$

Under the status quo with weak enforcement,

$$\pi \approx 0 \qquad \text{and} \qquad p \text{ relatively low},$$

the marginal return to increasing rent above the cap remains positive whenever $p < 1$. However, landlords still trade off higher rents against lower matching probabilities, so overcharging is not necessarily optimal for all landlords.

Under a stricter policy alternative, large administrative penalties increase π, while greater transparency about legal rent caps and easy cost-free tenant challenges increase the probability that overcharging is detected and successfully challenged:

$$p^{\text{policy}} > p^{\text{status quo}}.$$

A higher challenge probability affects landlords through two channels. First, it lowers the marginal return to overcharging:

$$\partial_r \tilde{r}^C(r) = 1-p.$$

Second, it increases the expected penalty term

$$p\pi.$$

Thus, strict enforcement, transparency, and easy tenant challenges jointly reduce both the intensive and extensive margins of overcharging.

A.6 Self-Posted Listings and Equilibrium Spillovers

For self-posting landlords, evaluated at the optimal self-posting continuation value,

$$V_O^s(r) - V_V^s(r) = \frac{\tilde{r}^s(r) + c_i}{1 - \beta + \beta q + \beta\lambda(r)}.$$

This expression applies to landlords whose optimal continuation after separation is to self-post again, so that $W^s = V_V^s(\text{self}, r)$, with $C_{\text{self},i} = c_i$.

Using

$$\lambda(r) = A[1-F(r)], \qquad \lambda'(r) = -Af(r),$$

the self-posting first-order condition becomes

$$\frac{[1-F(r)]\partial_r \tilde{r}^s(r)}{1-\beta(1-q)} = f(r)\frac{\tilde{r}^s(r)+c_i}{1-\beta+\beta q+\beta A[1-F(r)]}.$$

Define

$$H(r,A) = \frac{[1-F(r)]\partial_r \tilde{r}^s(r)}{1-\beta(1-q)} - f(r)\frac{\tilde{r}^s(r)+c_i}{1-\beta+\beta q+\beta A[1-F(r)]}.$$

The optimal rent satisfies

$$H(r^*(A),A) = 0.$$

By the implicit function theorem,

$$\frac{\partial r^*(A)}{\partial A} = -\frac{H_A(r^*(A),A)}{H_r(r^*(A),A)}.$$

Since

$$H_A(r,A) = f(r)(\tilde{r}^s(r)+c_i)\frac{\beta[1-F(r)]}{(1-\beta+\beta q+\beta A[1-F(r)])^2} > 0,$$

and the second-order condition implies

$$H_r(r^*(A),A) < 0,$$

it follows that

$$\frac{\partial r^*(A)}{\partial A} > 0.$$

Reduced agent usage lowers aggregate visibility

$$L = \sum_{s,m} \gamma_m L_m^s,$$

because $\gamma_{\text{agent}} > \gamma_{\text{self}}$. If L falls sufficiently, then

$$A_{\text{self}} = \frac{U}{L}$$

increases, raising the optimal rent for self-posting landlords.

B Seasonal Trend Estimation

To cleanly separate seasonal effects in counts from policy effects, we include a seasonal trend in our count models that mimics typical patterns in moving behaviour. This is particularly important as our study focuses on a rather short time period.

We derive typical within-year moving patterns from migration register data on the district level from the *Wanderungsstatistik*. This allows us to track the number of people deregistering from an address and infer whether they are moving within the city, to another place in Austria or abroad. Our data span from Q1-2010 to Q4-2022, and are available on both a monthly and quarterly basis. We estimate a negative binomial count regression with the number of people deregistering from an address in a district as response, and regress it on an annual (either

Table 5: Seasonal Trend

	log(residents leaving an address)			
	(1)	(2)	(3)	(4)
(Intercept)	-3.113***	-3.724***	-5.037***	-5.502***
	(0.016)	(0.014)	(0.019)	(0.019)
$\hat{\beta}_1$ (Seasonal Trend)	0.040***	0.031***	0.038***	0.030***
	(0.005)	(0.004)	(0.005)	(0.005)
$\hat{\beta}_2$ (Seasonal Trend)	-0.063***	-0.055***	-0.085***	-0.092***
	(0.005)	(0.004)	(0.005)	(0.005)
Covid	-0.060***	0.008	-0.085***	0.011
	(0.012)	(0.010)	(0.013)	(0.013)
Migration Across			-0.166***	
			(0.010)	
Migration Within			1.392***	1.573***
			(0.010)	(0.007)
Immigration			0.395***	
			(0.010)	
District FE	✓	✓	✓	✓
Population Weights (offset)	✓	✓	✓	✓
Num. Obs.	1196	1196	4784	2392
AIC	17317.0	15909.1	61166.9	30045.2

Notes: Negative Binomial count models based on register migration statistics ('Wanderungsstatistik') from Q1-2010 to Q4-2022. No differentiation of migration type (across, within, immigration, emigration) in (1). No foreign immigration and emigration in (2) and (4), only across and within city limits. 'Covid' indicates all four quarters of 2020. Significance codes: $^{+}$ *$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

four quarters or twelve months) cyclical trend which we express as a linear combination of trigonometric terms (see Hahn, Omerovic and Waltl, 2025, for the derivation of this structure). We further include a COVID-dummy for the year 2020, district fixed effects and an offset for the population z in district i at time t yielding

$$\log(\mu_{it}) = \beta_0 + \beta_1 \cos\left(\frac{2\pi t}{4}\right) + \beta_2 \sin\left(\frac{2\pi t}{4}\right) + \beta_3 covid_t + \gamma_i + \log(z_{it}). \tag{6}$$

Table 5 reports results for four different specifications of this model using different migration angles at different levels of aggregation. In column (1), we take into account all migration (within and across city limits, as well as migration to and from abroad). Column (2) excludes migration to and from abroad, while we look at separate counts by migration type in column (3), which is represented by a four-times inflated sample size. In column (4) we only consider migration within and across the city limits, again excluding migration to and from abroad.

Across all these specifications, we find very similar seasonal patterns. When transforming the estimated parameters reported in Table 5 back to quarters, we consistently detect the maximum in the third and the minimum in the first quarter.

Estimating the trend on a monthly basis, only for recent years (after 2015), or when excluding the most recent years (up to 2020) yields no noticeable difference in results. Seasonality in moving behaviour appears to have remained relatively stable over the past decade.

C Missing Data Treatment

C.1 Counts

Due to scraping issues, data gaps are present in in March, November and December 2022 (precisely, two weeks in March, one week in November and four weeks in December are affected). The underlying daily scrapes failed on several days resulting in reduced data for these weeks. As the reason of missingness is known to be a randomly occurring technical failure, we can clearly classify the gaps as missingness completely at random, i.e., missingness does not depend on the variables themselves. Hence, the missingness is not expected to affect price effects in any way as we model means or medians. However, missing observations mechanically lead to an underestimation of counts in the respective months or quarters.

To address this issue, we reconstruct counts for the affected weeks relying on informed interpolation. Concretely, when data is missing for an entire week (March), we average over the remaining non-missing weeks in March. When data is not completely missing (November and December), we impute a weighted average between counts observed in the affected week as well as an average across four preceding and following weeks. We follow an agnostic approach and assign larger weights to weeks closer in time (*Strategy A*).[30] By that we find a compromise between using as much available information as possible and smoothing out the impact of random irregularities. Specifically, let c_t be the counts in week t and missingness occurring in week $t = t^*$ part of month $m = m^*$. The imputed value replacing c_{t^*} is thus

$$\hat{c}_{t^*} = \begin{cases} \frac{\sum_{k \in m^*} c_k}{\sum_{k \in m^*} \mathbb{I}_{\{c_k \neq 0\}}(k)}, & \text{if } c_{t^*} = 0, \\ 0.3\, c_t + \sum_{j=1}^{4} (0.15 - 0.025j)\,(c_{t-j} + c_{t+4+j}), & \text{if } c_{t^*} > 0. \end{cases}$$

This procedure leads to smoother time coefficients in the pre-period and previously significant negative jumps in volumes associated with affected months and quarters reduce substantially or even disappear as confirmed in Table 13. This confirms the success of our applied reconstruction method. However, volume treatment effects at the time of enactment hardly change – neither for a monthly nor quarterly assessment – confirming the robustness of results to these technical flaws in the data.

C.2 Construction Year

Information on a building's year of construction is crucial to identify rent-capped units. The "address" sample consists only of observations that can definitely be assigned to either the rentcap or the exempt segment (via the information given or by matching the address with administrative data). Since many observations are lost by this restriction, we impute missing

[30] Even when dismissing the reduced number of observations from the affected period and relying on adjacent periods only (*Strategy B*) via

$$\hat{c}_{t^*} = \sum_{j=1}^{4} (0.25 - 0.05j)\,(c_{t^*-j} + c_{t^*+j}),$$

treatment effects do not change (see Table 13).

construction years modelling an indicator distinguishing properties built before or after 1945 in a logit regression on hedonic characteristics.[31] Training this model on the "address" sample yields a high predictive power.[32] We thus run it on the remaining sample and assign observations to the rentcap or the exempt segment based on the predicted building age as well as the size and floor level of the apartment. The "full" sample thus partly consists of observations where building age was imputed, while in the "address" sample this information is either given in the data or reconstructed via exact location information.

D Robustness and Sensitivity Analyses

The choice of the sample used for a specific model is dependent on the outcome variable. Volumes are naturally more prone to sample choices. Thus, for our count models we primarily use the "full" sample. Firstly, since our count models are mainly built around district aggregates, missing hedonic characteristics are not as big an issue as they are when it comes to hedonic modeling of price effects. Secondly, we are hesitant to drop observations at large scale since we are interested in the changes in overall counts. In those cases where we run count models on both samples, the resulting volume effects are robust to sample choice. However, staggered effects are larger in magnitude when estimated using the "address" sample than when using the "full" sample (see Table 15 and Table 16). Further, a data scraping issue affects some months in the pre-period. We hence test whether these data gaps impact results by imputing counts for missing periods in two ways as reported in Appendix C. When it comes to measuring price effects, we run all of our models on both samples with results being reported accordingly.

As mentioned above, the "address" sample is obtained by excluding observations with missing data. This may give rise to sample selection bias if information on the year of construction is not missing at random. To assess this concern, we first account for differences in characteristics between the "full" sample and the "address" sample. We identify significant deviations for most characteristics as shown in Table 6. However, these differences are small in magnitude, indicating that the significance likely reflects the large sample size and corresponding statistical power rather than substantively important compositional differences. To rule out that these differences impact our general findings, we compare price trends over time for the two data sets as shown in Figure 7. While there are some deviations at the very beginning of the sample period, from roughly mid-2022 onward estimated time effects are statistically almost the same with only negligible deviations. This holds true for both ways we control for locational effects. Additionally, the coefficients for the hedonic controls are similar in size and significance when running the models on either sample as well as separately by regulation (see Table 7).[33] Furthermore, also our main results for price effects arising from identification through agent use and inverse probability weighting in subsection 5.5 are not affected by sample choice as reported in Table 9 and displayed in Figure 5.

[31] These comprise size, outside areas, cellar, level, elevator, first time use, heating efficiency and district.

[32] The model achieved an area under the receiver operating characteristic curve (AUC) of 0.90 on the out-of-sample data.

[33] When we model location via a Spline-on-Sphere (SOS) Smooth, we only take into account observations containing information about the precision of the given coordinates. When employing the Markov-Random-Field (MRF) Smooth, we solely rely on the district info given. This results in different sample sizes when running models on the "full" sample (see columns (1) and (3) in Table 7).

Table 6: Summary Statistics (Full vs. Address Sample)

		Full (N=75,936)		Address (N=27,904)			
		Mean	SD	Mean	SD	Diff.	SE
Monthly Net Rent [EUR]		831.93	455.09	841.33	445.85	9.40**	3.14
Monthly Net Rent Pre [EUR]		815.08	442.79	817.66	427.68	2.58	3.47
Monthly Net Rent Post [EUR]		890.67	491.10	912.18	489.50	21.51**	6.97
Post [%]		0.22	0.42	0.25	0.43	0.03***	0.00
Year built given [%]		0.54	0.50	0.82	0.38	0.28***	0.00
Built pre 1945 [%, pred.]		0.28	0.45	0.28	0.45	0.00	0.00
Dwelling s.t. rentcap [%, pred.]		0.22	0.42	0.22	0.42	0.00	0.00
Agent [%]		0.92	0.28	0.93	0.25	0.02***	0.00
Living Area [sqm]		64.41	30.24	63.47	29.12	-0.94***	0.21
Balcony/Terrace [%]		0.78	0.41	0.83	0.38	0.04***	0.00
Garden [%]		0.09	0.29	0.11	0.32	0.02***	0.00
Cellar [%]		0.55	0.50	0.60	0.49	0.06***	0.00
Elevator [%]		0.79	0.40	0.82	0.38	0.03***	0.00
First-time use [%]		0.32	0.47	0.33	0.47	0.01*	0.00
		N	Pct.	N	Pct.		
District Type	Within Guertel	23984	31.6	8373	30.0		
	Outside Guertel	38223	50.3	13901	49.8		
	Edges & Transdanubia	13729	18.1	5630	20.2		
Level	Ground floor	1954	2.6	804	2.9		
	Ordinary floors	67166	88.5	24446	87.6		
	Roof floor	6816	9.0	2654	9.5		
Energy efficiency	A	16353	21.5	6733	24.1		
	B	25511	33.6	9969	35.7		
	C	10281	13.5	3626	13.0		
	D-G	12634	16.6	4386	15.7		
	N/A	11157	14.7	3190	11.4		

Notes: 'Full' based on all observations available. 'Address' based on all observations that contain either information on year built and (approximate) geocode or could be matched via exact address. Test results refer to t-tests for differences in means as well as differences in proportions. "Built pre 1945" and "Dwelling s.t. rentcap" partly imputed for full sample.

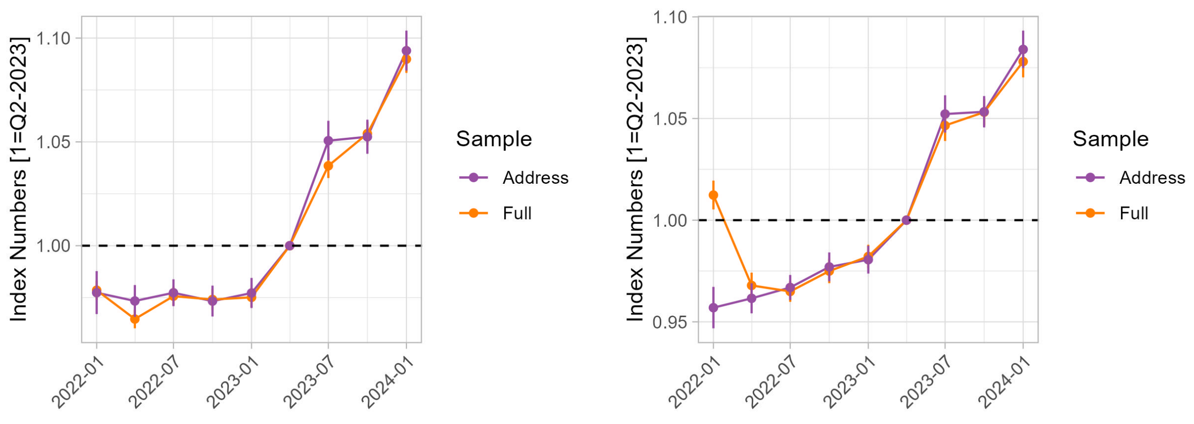
As mentioned in subsection 5, some properties built between 1945 and 1953 are also subject to a rent cap. This applies to buildings that received a public grant when originally constructed. However, this information is not available in any of our merged data sets. To test whether our heterogeneity results by rent cap status are affected by this limitation, we re-estimate results shown in Figure 4 and Figure 6 leaving out *all* units built between 1945 and 1953. As reported in Table 25, policy effects are still highly significant and very similar in magnitude.

As discussed in section 4, a structural break in the mix of hedonic characteristics occurs when the enactment of the policy demands the inclusion of hedonic controls in all price models.

Moreover, since location is a key price-determining factor, we explicitly model locational effects in hedonic pricing models using district fixed effects, a locational regression spline defined on longitudes and latitudes, as well as a locational spline defined on districts. In this spirit, we also prefer including physical hedonic characteristics in all price models, yet refraining from physical and/or locational controls still identifies a jump in rents as reported in Table 11.

Furthermore, we rely on two temporal aggregation units – months and quarters – across all our analyses to assess robustness along this dimension. Similarly, we estimate staggered and total volume and price effects.

Figure 7: Rent Index by Sample and Location Modelling Choice



(a) MRF

(b) District FE and SOS

Notes: These plots show estimated time-dummy price indices extracted from hedonic models based on either the "Full" sample or the "Address" sample (see Table 7). Plot (a) includes a Markov-Random-Field (MRF) Smooth at the district level while plot (b) includes a Spline-on-Sphere (SOS) smooth.

Our interpretations and conclusions are not changed by any of these alternative estimation approaches.

E Reweighting Approach

As alternative identification strategy we test for the difference in the impact of the reform on apartments advertised privately or by hiring a real estate agent (see subsection 5.5). Since the former are not directly affected by the ordering principle, no immediate increase in asking rents is expected for these apartments. Accordingly, this allows us to set up a scenario with private insertions as control group and agent insertions as treatment group.

Precisely, we assign inverse probability weights to all agent-advertised apartments ($D_i = 1$) such that the observations after the enactment mimic the sample structure before the enactment. Therefore, we set up a logistic model regressing an indicator separating the pre ($T_i = 0$) and post ($T_i = 1$) periods on a set of hedonic characteristics X:[34]

$$\log\left(\frac{\Pr(T_i = 1 \mid D_i = 1, X_i)}{1 - \Pr(T_i = 1 \mid D_i = 1, X_i)}\right) = \alpha + X_i'\beta. \tag{7}$$

This model yields predicted probabilities $\widehat{e}(X_i)$ that are used to compute raw inverse probability weights w_i^{raw},

$$w_i^{\text{raw}} = \begin{cases} \dfrac{T_i}{\widehat{e}(X_i)} + \dfrac{1 - T_i}{1 - \widehat{e}(X_i)}, & \text{if } D_i = 1, \\ 1, & \text{if } D_i = 0. \end{cases} \tag{8}$$

We then harmonise the samples by dividing w_i^{raw} by the strata-specific average weight $\bar{w}_i^{raw}$. The

[34] These include size, outside areas, cellar, elevator, level, first-time use and district.

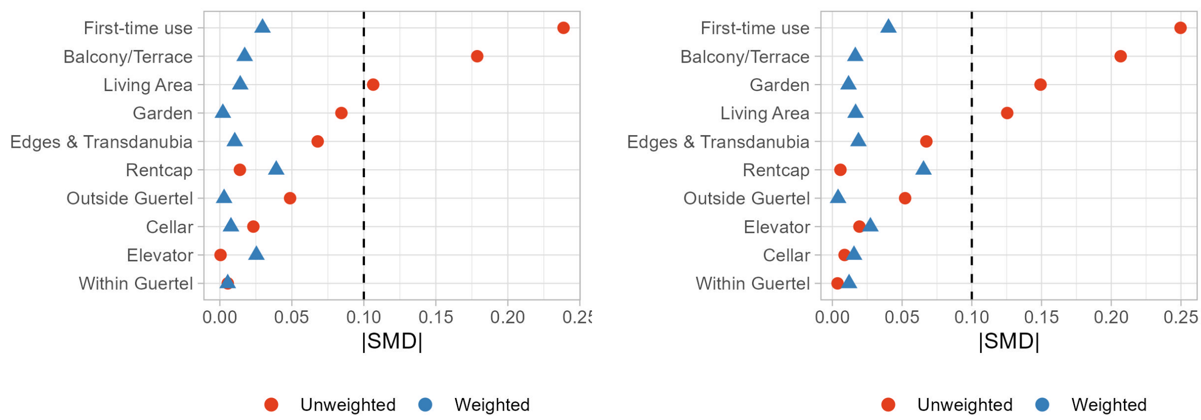
procedure is separately applied to the full and the address sample.

To assess the success of the re-weighting procedure, we compute absolute standardized differences $|SMD|$ for key hedonic characteristics between the pre and the post period with and without the use of the weights for both data sets. For each covariate, we compute

$$|SMD(x)| = \frac{|\bar{x}_{post} - \bar{x}_{pre}|}{\sqrt{s^2_{post} + s^2_{pre}}},$$

where $\bar{x}$ and s^2 are the (weighted) mean and variance in the pre- and post-periods, respectively. Figure 8 reveals that for all characteristics, deviations are post-reweighing clearly below the 0.1-threshold used in previous research[35] indicating that the procedure indeed achieves a high level of balance.

Figure 8: Sample Balance



(a) Full

(b) Address

Notes: The Figures plot the absolute standardized mean differences (see Equation 8) for hedonic and regulation characteristics between the pre- and post-period before and after re-weighting, respectively. The sample consists of listings naming an agent as contact person. Re-weighting is applied for (a) the "full" sample and (b) the "address" sample.

[35]This is proposed, e.g., by Austin (2009). As depicted in Figure 8, none of the hedonic or policy variables is even close to this threshold, neither in the "full" sample (mean = 0.0154; maximum = 0.0392) nor in the "address" sample (mean = 0.0228; maximum = 0.0654).

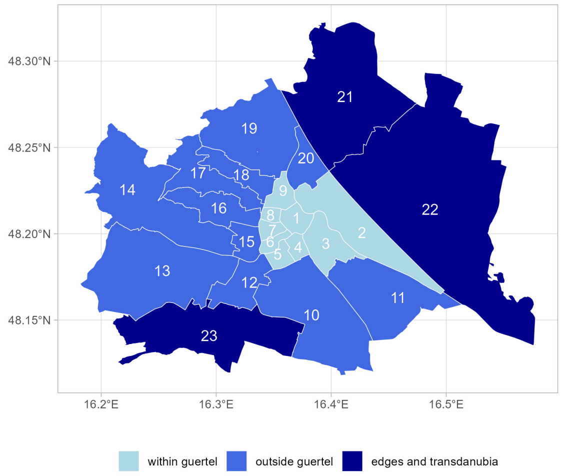
F Additional Tables and Figures

Table 11: Descriptive Statistics

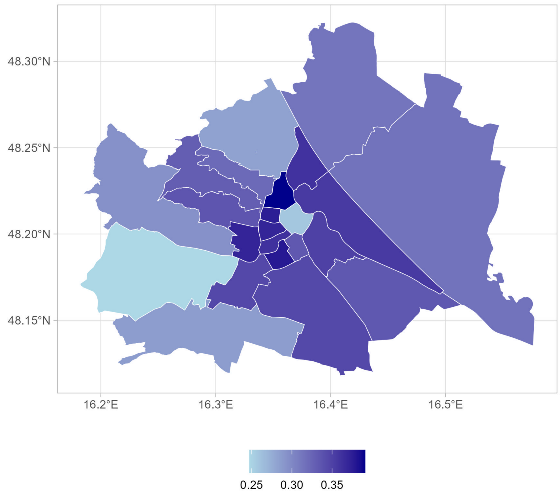
	Pre		Post			
	Mean	SD	Mean	SD	Diff.	SE
Dwellings s.t. rent cap [%]	0.26	0.44	0.25	0.43	-0.02*	0.01
Agent Insertion [%]	0.93	0.25	0.92	0.27	-0.01*	0.00
Monthly Net Rent [EUR]	822.7	446.3	912.5	490.6	89.8***	6.7
Living Area [sqm]	63.8	29.4	66.0	31.1	2.2***	0.4
Balcony/Terrace [%]	0.83	0.38	0.77	0.42	-0.06***	0.01
Garden [%]	0.10	0.31	0.15	0.35	0.04***	0.00
Cellar [%]	0.60	0.49	0.60	0.49	0.00***	0.01
Elevator [%]	0.81	0.40	0.81	0.39	0.01	0.01
First-time use [%]	0.30	0.46	0.24	0.43	-0.06***	0.01
Built pre 1945	0.31	0.46	0.30	0.46	-0.01*	0.01
	N	%	N	%		
Level						
Ground floor	346	1.8	398	5.7		
Ordinary floors	16679	88.2	5943	85.5		
Roof floor	1895	10.0	609	8.8		
Energy efficiency						
A	3765	19.9	1631	23.5		
B	6909	36.5	2420	34.8		
C	2667	14.1	951	13.7		
D-G	3214	17.0	1170	16.8		
N/A	2365	12.5	778	11.2		
Number of Observations	18920	100	6950	100		

Notes: Main estimation sample (address sample) with all observations that contain either information on year built and (approximate) geocode or could be matched via exact address. Test results refer to t-tests for differences in means as well as differences in proportions.

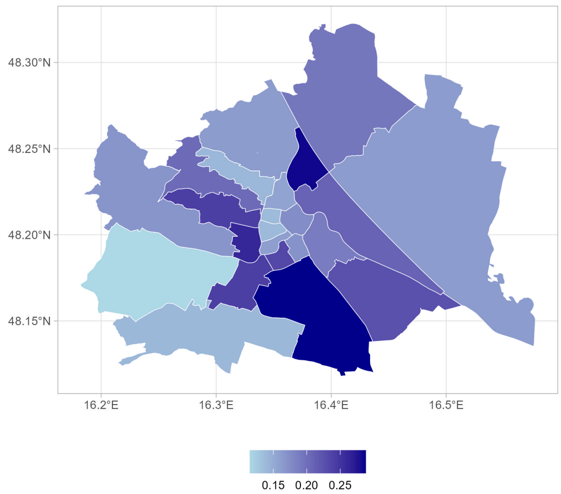
Figure 9: Socioeconomic Characteristics by District



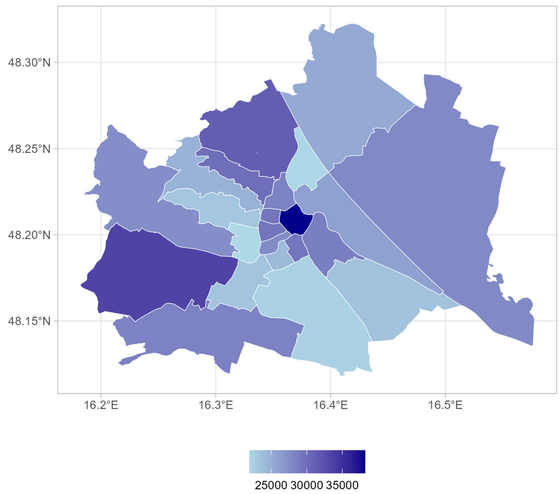
(a) Location



(b) Share under 35 years old



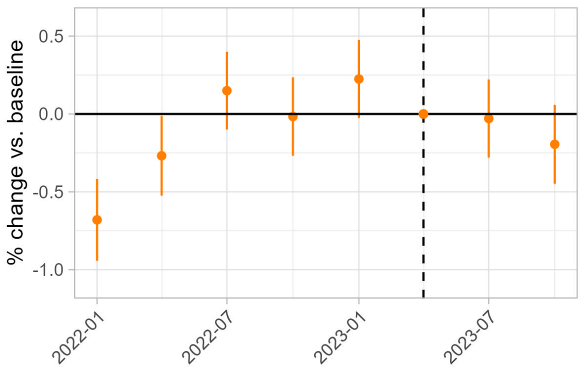
(c) Share Non-EU citizenship



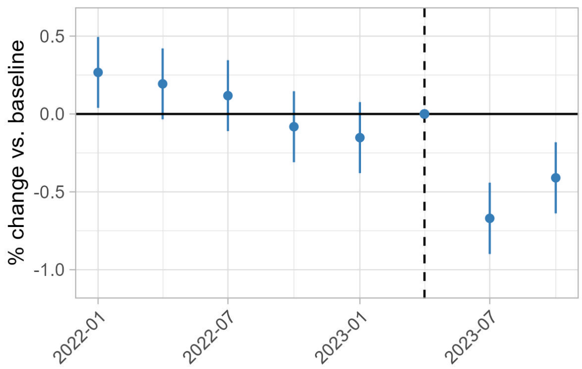
(d) Annual employee income

Notes: These plots show the 23 districts of Vienna classified by location (a), age (b), migrant share (c) and income (d). 'Non-EU' comprises all countries not in the EU, EFTA or the UK. Income refers to mean net annual employee income in 2022. (b) and (c) based on data from 2024. See also Table 12.

Figure 10: Counts by Agent



(a) Private – quarterly



(b) Agent – quarterly

Notes: Plots show results for the time-dummies of negative binomial models with cyclical trend and district population as offsets. Based on "full" sample without filters. Partial imputation of missing values in 03-22, 11-22 and 12-22. Detailed results in Table 14.

Table 7: Hedonic Regression by Sample

	log(R)							
	Total				*By Regulation*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	2.981***	2.996***	3.227***	3.205***	3.273***	3.350***	3.154***	3.108***
	(0.012)	(0.018)	(0.025)	(0.032)	(0.044)	(0.060)	(0.034)	(0.039)
2022-01-01	-0.022***	-0.023***	0.012***	-0.044***	0.015^{+}	-0.016	0.005	-0.062***
	(0.002)	(0.005)	(0.004)	(0.005)	(0.008)	(0.011)	(0.004)	(0.006)
2022-04-01	-0.036***	-0.027***	-0.033***	-0.039***	-0.028***	-0.030***	-0.038***	-0.049***
	(0.002)	(0.004)	(0.003)	(0.004)	(0.007)	(0.009)	(0.004)	(0.004)
2022-07-01	-0.025***	-0.023***	-0.036***	-0.034***	-0.024***	-0.022**	-0.041***	-0.040***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.006)	(0.008)	(0.003)	(0.003)
2022-10-01	-0.026***	-0.027***	-0.025***	-0.023***	-0.017*	-0.019*	-0.029***	-0.027***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.007)	(0.009)	(0.003)	(0.004)
2023-01-01	-0.025***	-0.023***	-0.018***	-0.020***	-0.013*	-0.009	-0.021***	-0.026***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)	(0.008)	(0.003)	(0.004)
2023-07-01	**0.038*** **	**0.049*** **	**0.046*** **	**0.051*** **	**0.050*** **	**0.066*** **	**0.046*** **	**0.041*** **
	(0.003)	**(0.005)**	**(0.004)**	**(0.004)**	**(0.008)**	**(0.010)**	**(0.004)**	**(0.005)**
2023-10-01	0.053***	0.051***	0.052***	0.052***	0.055***	0.059***	0.052***	0.050***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.007)	(0.009)	(0.003)	(0.004)
2024-01-01	0.086***	0.090***	0.075***	0.081***	0.071***	0.077***	0.077***	0.082***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.008)	(0.010)	(0.004)	(0.005)
Living Area [log]	0.858***	0.858***	0.857***	0.861***	0.840***	0.837***	0.866***	0.869***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.006)	(0.007)	(0.003)	(0.003)
Balcony/Terrace	0.092***	0.088***	0.074***	0.073***	0.047***	0.038***	0.085***	0.084***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)
Garden	0.053***	0.043***	0.051***	0.043***	0.049***	0.036**	0.048***	0.041***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.010)	(0.012)	(0.003)	(0.004)
Cellar	0.009***	-0.003	0.012***	0.003	0.021***	0.024***	0.009***	-0.004
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)	(0.002)	(0.002)
Level: higher than ground floor	-0.063***	-0.068***	-0.023*	-0.018	-0.020	-0.021	0.006	0.031^{+}
	(0.008)	(0.011)	(0.009)	(0.012)	(0.013)	(0.017)	(0.017)	(0.018)
Level: roof	-0.036***	-0.033*	0.007	0.015			0.034^{+}	0.060**
	(0.009)	(0.015)	(0.012)	(0.015)			(0.018)	(0.020)
Elevator	0.007	-0.011	0.015	0.023	0.019	0.014	0.042*	0.059**
	(0.009)	(0.013)	(0.012)	(0.014)	(0.020)	(0.023)	(0.019)	(0.020)
Level: higher than ground floor × Elevator	0.050***	0.069***	0.038**	0.032*	0.041*	0.042^{+}	-0.002	-0.019
	(0.009)	(0.014)	(0.012)	(0.014)	(0.020)	(0.024)	(0.019)	(0.020)
Level: roof x Elevator	0.076***	0.085***	0.072***	0.062***			0.036^{+}	0.018
	(0.011)	(0.017)	(0.014)	(0.017)			(0.020)	(0.022)
First time use	0.100***	0.096***	0.092***	0.091***	0.069***	0.061***	0.095***	0.095***
	(0.002)	(0.003)	(0.002)	(0.003)	(0.006)	(0.007)	(0.002)	(0.003)
pre-1945	0.081***	0.050***	0.051***	0.022***			0.042***	0.018***
	(0.003)	(0.005)	(0.004)	(0.005)			(0.004)	(0.005)
Heating efficiency: A	0.175***	0.167***	0.167***	0.161***	0.182***	0.191***	0.144***	0.149***
	(0.003)	(0.005)	(0.004)	(0.005)	(0.019)	(0.019)	(0.005)	(0.006)
Heating efficiency: B	0.122***	0.117***	0.118***	0.107***	0.120***	0.109***	0.097***	0.097***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.008)	(0.008)	(0.004)	(0.005)
Heating efficiency: C	0.016***	0.019***	0.020***	0.008*	0.030***	0.005	-0.001	0.006
	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)	(0.007)	(0.004)	(0.005)
Heating efficiency: unknown	0.063***	0.074***	0.052***	0.049***	0.037***	0.024***	0.040***	0.057***
	(0.002)	(0.004)	(0.003)	(0.004)	(0.005)	(0.007)	(0.005)	(0.005)
Rentcap	-0.139***	-0.121***	-0.120***	-0.118***				
	(0.003)	(0.005)	(0.004)	(0.005)				
Agent	-0.039***	-0.022***	-0.044***	-0.046***	-0.098***	-0.101***	-0.027***	-0.014**
	(0.002)	(0.005)	(0.003)	(0.004)	(0.007)	(0.008)	(0.004)	(0.005)
Sample	Full	Address	Full	Address	Full	Address	Full	Address
Regulation					Rentcap	Rentcap	Exempt	Exempt
Geo-precision info		✓	✓	✓	✓	✓	✓	✓
Location	MRF	MRF	SOS	SOS	SOS	SOS	SOS	SOS
District FE			✓	✓	✓	✓	✓	✓
Obs.	75936	27904	43723	27904	10294	6250	33429	21654
Adj. R^2	0.819	0.817	0.842	0.852	0.819	0.836	0.856	0.865
AIC	954256.1	350393.4	544992.3	345032.0	128567.0	76959.8	414577.4	266632.8

*Notes: Based on GAM modeling location via Markov Random Field (MRF) Smooth or Spline-on-Sphere (SOS) Smooth. Columns (1)-(4) show results for the pooled sample ("full" or "address") while columns (5)-(8) show the separate estimates for the respective regulation subsamples ("rentcap" or "exempt"). Base groups for categorical variables are "level: groundfloor" and "heating efficiency: DEFG." The category "pre 1945" subsumes apartments subject to the rent cap as well as apartments exempt (larger than 130m^2 or loft conversion). Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table 8: Summary Statistics (Private insertion vs. agent insertion)

		Private				Agent			
		Pre	Post	Diff.	SE	Pre	Post	Diff.	SE
Monthly Net Rent [EUR]		754.07	778.33	24.26	19.20	821.85	923.14	101.29***	6.90
Built pre 1945 [%]		0.50	0.52	0.02	0.03	0.26	0.28	0.01*	0.01
Dwelling s.t. rentcap [%]		0.42	0.40	-0.02	0.03	0.21	0.21	0.00	0.01
Living Area [sqm]		62.79	61.16	-1.64	1.35	62.62	66.38	3.76***	0.44
Balcony/Terrace [%]		0.36	0.40	0.04	0.03	0.88	0.80	-0.08***	0.01
Garden [%]		0.05	0.06	0.01	0.01	0.10	0.15	0.05***	0.01
Cellar [%]		0.42	0.43	0.01	0.03	0.62	0.61	0.00	0.01
Elevator [%]		0.67	0.67	0.00	0.02	0.83	0.83	-0.01	0.01
First-time use [%]		0.12	0.07	-0.05***	0.01	0.37	0.26	-0.12***	0.01
		Pre		Post		Pre		Post	
		N	Pct.	N	Pct.	N	Pct.	N	Pct.
District Type	Within Guertel	399	30.8	177	33.5	5874	29.9	1923	29.8
	Outside Guertel	785	60.7	299	56.5	9769	49.8	3048	47.2
	Edges & Transdanubia	110	8.5	53	10.0	3978	20.3	1489	23.0
Floor level	Ground floor	70	5.4	15	2.8	331	1.7	388	6.0
	Ordinary floors	1088	84.1	441	83.4	17386	88.6	5531	85.6
	Roof floor	136	10.5	73	13.8	1904	9.7	541	8.4
Energy efficiency	A	64	4.9	22	4.2	5024	25.6	1623	25.1
	B	201	15.5	85	16.1	7327	37.3	2356	36.5
	C	171	13.2	86	16.3	2503	12.8	866	13.4
	D-G	216	16.7	87	16.4	3000	15.3	1083	16.8
	N/A	642	49.6	249	47.1	1767	9.0	532	8.2
Number of observations		1294	100	529	100	19621	100	6460	100

Notes: Upper blocks show means or shares. Based on "address" sample. Test results refer to t-tests for differences in means as well as differences in proportions.

Table 9: Private vs. Agent Interaction Model: Time Effects

	log(*R*)					
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	3.238***	3.382***	3.265***	3.206***	3.390***	3.228***
	(0.026)	(0.018)	(0.026)	(0.034)	(0.023)	(0.034)
2022-01-01	-0.024$^+$	-0.024	-0.026$^+$	-0.028	-0.029	-0.030$^+$
	(0.014)	(0.015)	(0.014)	(0.018)	(0.020)	(0.018)
2022-04-01	-0.028*	-0.026*	-0.028*	-0.041**	-0.037*	-0.041**
	(0.011)	(0.012)	(0.011)	(0.015)	(0.017)	(0.015)
2022-07-01	-0.032**	-0.029**	-0.033**	-0.019	-0.011	-0.018
	(0.010)	(0.011)	(0.010)	(0.014)	(0.015)	(0.014)
2022-10-01	-0.020$^+$	-0.016	-0.021$^+$	-0.010	-0.013	-0.010
	(0.012)	(0.012)	(0.011)	(0.015)	(0.017)	(0.015)
2023-01-01	0.000	0.010	-0.002	-0.011	0.007	-0.011
	(0.010)	(0.011)	(0.010)	(0.014)	(0.015)	(0.014)
2023-07-01	**0.004**	**0.011**	**0.004**	**0.008**	**0.020**	**0.006**
	(0.011)	**(0.012)**	**(0.011)**	**(0.015)**	**(0.017)**	**(0.015)**
2023-10-01	0.015	0.018	0.016	0.026$^+$	0.037*	0.024
	(0.012)	(0.012)	(0.012)	(0.015)	(0.017)	(0.015)
2024-01-01	0.049***	0.058***	0.051***	0.074***	0.083***	0.074***
	(0.012)	(0.013)	(0.012)	(0.017)	(0.019)	(0.017)
2022-01-01 x Agent	0.039**	0.047**	0.044**	-0.018	0.011	-0.011
	(0.014)	(0.015)	(0.014)	(0.019)	(0.021)	(0.019)
2022-04-01 x Agent	-0.005	0.000	-0.006	0.002	0.009	0.002
	(0.012)	(0.013)	(0.012)	(0.016)	(0.018)	(0.016)
2022-07-01 x Agent	-0.004	-0.001	-0.004	-0.016	-0.016	-0.017
	(0.011)	(0.012)	(0.011)	(0.014)	(0.016)	(0.014)
2022-10-01 x Agent	-0.006	-0.011	-0.007	-0.014	-0.015	-0.015
	(0.012)	(0.013)	(0.012)	(0.016)	(0.018)	(0.016)
2023-01-01 x Agent	-0.021$^+$	-0.036**	-0.019$^+$	-0.009	-0.034*	-0.010
	(0.011)	(0.012)	(0.011)	(0.014)	(0.016)	(0.015)
2023-07-01 x Agent	**0.049*** **	**0.045*** **	**0.052*** **	**0.049** **	**0.033$^+$**	**0.051** **
	(0.012)	**(0.012)**	**(0.012)**	**(0.016)**	**(0.017)**	**(0.016)**
2023-10-01 x Agent	0.040***	0.035**	0.037**	0.028+	0.012	0.030$^+$
	(0.012)	(0.013)	(0.012)	(0.016)	(0.017)	(0.016)
2024-01-01 x Agent	0.029*	0.026$^+$	0.024$^+$	0.008	0.003	0.004
	(0.013)	(0.014)	(0.013)	(0.018)	(0.019)	(0.018)
Agent	-0.054***	-0.043***	-0.055***	-0.049***	-0.019	-0.051***
	(0.008)	(0.009)	(0.008)	(0.011)	(0.012)	(0.011)
Sample	Full	Full	Full	Address	Address	Address
Reweighted		✓	✓		✓	✓
Hedonics	✓	✓	✓	✓	✓	✓
District FE	✓		✓	✓		✓
Location	SOS	MRF	SOS	SOS	MRF	SOS
Obs.	43723	43723	43723	27904	27904	27904
Adj. R^2	0.842	0.817	0.847	0.852	0.820	0.856
AIC	544935.8	580190.1	570833.8	345017.9	366909.8	359626.3

Notes: Based on GAM modeling location via Markov Random Field (MRF) Smooth or Spline-on-Sphere (SOS) Smooth. Columns (2), (3), (5) and (6) based on weighted GAM using inverse probability weights. Hedonics displayed in Table 10. Time effects displayed in Figure 5. Significance codes: $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 10: Private vs. Agent Interaction Model: Hedonics

	$log(R)$					
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	3.238***	3.382***	3.265***	3.206***	3.390***	3.228***
	(0.026)	(0.018)	(0.026)	(0.034)	(0.023)	(0.034)
Living Area [log]	0.857***	0.859***	0.861***	0.861***	0.859***	0.861***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Balcony/Terrace	0.075***	0.095***	0.081***	0.074***	0.093***	0.079***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Garden	0.051***	0.045***	0.049***	0.043***	0.039***	0.039***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Cellar	0.012***	0.007***	0.011***	0.003	-0.001	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Level: higher than ground floor	-0.023*	-0.061***	-0.029**	-0.017	-0.071***	-0.016
	(0.009)	(0.009)	(0.009)	(0.012)	(0.011)	(0.011)
Level: roof	0.006	-0.028*	0.000	0.016	-0.038*	0.012
	(0.012)	(0.011)	(0.011)	(0.015)	(0.015)	(0.014)
Elevator	0.015	0.003	0.009	0.024^{+}	-0.012	0.017
	(0.012)	(0.011)	(0.012)	(0.014)	(0.013)	(0.013)
Level: higher than ground floor × Elevator	0.037**	0.054***	0.042***	0.031*	0.067***	0.035**
	(0.012)	(0.011)	(0.012)	(0.014)	(0.013)	(0.014)
Level: roof x Elevator	0.073***	0.074***	0.074***	0.062***	0.085***	0.066***
	(0.014)	(0.013)	(0.014)	(0.017)	(0.016)	(0.017)
First time use	0.093***	0.089***	0.092***	0.091***	0.093***	0.089***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
pre-1945	0.051***	0.048***	0.036***	0.022***	0.021***	0.006
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Heating efficiency: A	0.167***	0.174***	0.170***	0.161***	0.170***	0.164***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Heating efficiency: B	0.117***	0.117***	0.118***	0.107***	0.115***	0.111***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Heating efficiency: C	0.020***	0.021***	0.023***	0.008*	0.016***	0.013***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Heating efficiency: unknown	0.052***	0.060***	0.053***	0.049***	0.074***	0.055***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Rentcap	-0.120***	-0.119***	-0.111***	-0.118***	-0.107***	-0.107***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Agent	-0.054***	-0.043***	-0.055***	-0.049***	-0.019	-0.051***
	(0.008)	(0.009)	(0.008)	(0.011)	(0.012)	(0.011)
Sample	Full	Full	Full	Address	Address	Address
Reweighted		✓	✓		✓	✓
Time FE	✓	✓	✓	✓	✓	✓
District FE	✓		✓	✓		✓
Location	SOS	MRF	SOS	SOS	MRF	SOS
Obs.	43723	43723	43723	27904	27904	27904
Adj. R^2	0.842	0.817	0.847	0.852	0.820	0.856
AIC	544935.8	580190.1	570833.8	345017.9	366909.8	359626.3

Notes: Based on GAM modeling location via Markov Random Field (MRF) Smooth or Spline-on-Sphere (SOS) Smooth. Columns (2), (3), (5) and (6) based on weighted GAM using inverse probability weights. Time effects displayed in Table 9 and Figure 5. Significance codes: $^{+}$ *$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table 12: Districts and Characteristics

		Location	*Income*	*Young*	*Students*	*Austrian*	*Non − EU*	*Families*
1010	Innere Stadt	within Gürtel	upper half	few	many	many	many	few
1020	Leopoldstadt	within Gürtel	lower half	many	many	few	many	many
1030	Landstraße	within Gürtel	upper half	many	many	few	many	few
1040	Wieden	within Gürtel	upper half	few	many	few	few	few
1050	Margareten	within Gürtel	lower half	many	many	few	many	few
1060	Mariahilf	within Gürtel	upper half	many	many	many	few	few
1070	Neubau	within Gürtel	upper half	many	many	many	few	few
1080	Josefstadt	within Gürtel	upper half	many	many	many	few	few
1090	Alsergrund	within Gürtel	upper half	many	many	many	few	few
1100	Favoriten	outside Gürtel	lower half	many	few	few	many	many
1110	Simmering	outside Gürtel	lower half	few	few	few	many	many
1120	Meidling	outside Gürtel	lower half	many	few	few	many	many
1130	Hietzing	outside Gürtel	upper half	few	few	many	few	many
1140	Penzing	outside Gürtel	lower half	few	few	many	few	many
1150	Rudolfsheim-Fünfhaus	outside Gürtel	lower half	many	many	few	many	few
1160	Ottakring	outside Gürtel	lower half	many	few	few	many	few
1170	Hernals	outside Gürtel	lower half	few	many	few	many	many
1180	Währing	outside Gürtel	upper half	few	many	many	few	many
1190	Döbling	outside Gürtel	upper half	few	few	many	few	many
1200	Brigittenau	outside Gürtel	lower half	many	few	few	many	few
1210	Floridsdorf	edges and Transdanubia	lower half	few	few	many	many	many
1220	Donaustadt	edges and Transdanubia	upper half	few	few	many	few	many
1230	Liesing	edges and Transdanubia	upper half	few	few	many	few	many

Source: City of Vienna, Statistik Austria. Districts are assigned to thirds according to the share of characteristics among the population in 2024. 'Non - EU' comprises all countries not in the EU, EFTA or the UK. Income refers to net average yearly employee income in 2022.

Table 13: Count Models: Staggered Time Effects

(a) Quarterly

	Counts (1)	(2)	(3)	(4)
Intercept	5.778***	-3.901***	-3.910***	-3.909***
	(0.093)	(0.093)	(0.094)	(0.095)
Q1-2022	-0.073	-0.022	0.121^{+}	0.121^{+}
	(0.068)	(0.068)	(0.069)	(0.069)
Q2-2022	0.088	0.104	0.104	0.105
	(0.068)	(0.068)	(0.069)	(0.069)
Q3-2022	0.144*	0.069	0.069	0.069
	(0.068)	(0.068)	(0.068)	(0.069)
Q4-2022	-0.346***	-0.402***	-0.136*	-0.055
	(0.069)	(0.068)	(0.069)	(0.069)
Q1-2023	-0.160*	-0.133^{+}	-0.134+	-0.134^{+}
	(0.068)	(0.068)	(0.069)	(0.069)
Q3-2023	**-0.653*** **	**-0.739*** **	**-0.739*** **	**-0.739*** **
	(0.069)	**(0.069)**	**(0.070)**	**(0.070)**
Q4-2023	-0.354***	-0.417***	-0.418***	-0.418***
	(0.069)	(0.068)	(0.069)	(0.070)
District FE	✓	✓	✓	✓
Cyclical Trend (offset)		✓	✓	✓
Population (offset)		✓	✓	✓
Sample	Full	Full	Full	Full
Imputed	No	No	Strat. A	Strat. B
Obs.	184	184	184	184
AIC	2134.7	2134.6	2155.4	2161.6

(b) Monthly

	Counts (1)	(2)	(3)	(4)
Intercept	4.572***	-5.148***	-5.138***	-5.136***
	(0.091)	(0.091)	(0.088)	(0.088)
2022-01-01	0.231*	0.313***	0.313***	0.313***
	(0.094)	(0.094)	(0.091)	(0.091)
2022-02-01	0.199*	0.302**	0.304***	0.304***
	(0.094)	(0.094)	(0.091)	(0.091)
2022-03-01	-0.626***	-0.519***	0.104	0.104
	(0.097)	(0.097)	(0.091)	(0.091)
2022-04-01	0.341***	0.424***	0.425***	0.426***
	(0.094)	(0.094)	(0.090)	(0.091)
2022-05-01	-0.021	0.031	0.031	0.031
	(0.095)	(0.095)	(0.091)	(0.091)
2022-06-01	0.121	0.137	0.138	0.138
	(0.094)	(0.094)	(0.091)	(0.091)
2022-07-01	0.278**	0.254**	0.254**	0.253**
	(0.094)	(0.094)	(0.091)	(0.091)
2022-08-01	-0.310**	-0.356***	-0.355***	-0.354***
	(0.095)	(0.095)	(0.092)	(0.092)
2022-09-01	0.513***	0.463***	0.462***	0.462***
	(0.094)	(0.093)	(0.090)	(0.090)
2022-10-01	0.172^{+}	0.132	0.131	0.131
	(0.094)	(0.094)	(0.091)	(0.091)
2022-11-01	-0.165^{+}	-0.174^{+}	-0.003	-0.003
	(0.095)	(0.095)	(0.091)	(0.091)
2022-12-01	-1.424***	-1.396***	-0.198*	0.038
	(0.101)	(0.101)	(0.092)	(0.091)
2023-01-01	-0.039	0.019	0.016	0.015
	(0.095)	(0.095)	(0.091)	(0.092)
2023-02-01	-0.207*	-0.127	-0.129	-0.129
	(0.095)	(0.095)	(0.092)	(0.092)
2023-03-01	-0.083	0.002	-0.001	-0.001
	(0.095)	(0.095)	(0.091)	(0.092)
2023-04-01	0.233*	0.300**	0.301***	0.301***
	(0.094)	(0.094)	(0.091)	(0.091)
2023-05-01	-0.058	-0.022	-0.020	-0.020
	(0.095)	(0.095)	(0.091)	(0.092)
2023-07-01	**-0.559*** **	**-0.593*** **	**-0.593*** **	**-0.593*** **
	(0.096)	**(0.096)**	**(0.093)**	**(0.093)**
2023-08-01	-0.699***	-0.754***	-0.752***	-0.752***
	(0.097)	(0.097)	(0.094)	(0.094)
2023-09-01	-0.503***	-0.562***	-0.563***	-0.564***
	(0.096)	(0.096)	(0.093)	(0.093)
2023-10-01	-0.564***	-0.611***	-0.610***	-0.610***
	(0.096)	(0.096)	(0.093)	(0.093)
2023-11-01	-0.140	-0.157^{+}	-0.157^{+}	-0.157^{+}
	(0.095)	(0.095)	(0.092)	(0.092)
2023-12-01	-0.218*	-0.199*	-0.200*	-0.200*
	(0.095)	(0.095)	(0.092)	(0.092)
2024-01-01	-0.273**	-0.222*	-0.226*	-0.226*
	(0.095)	(0.095)	(0.092)	(0.092)
2024-02-01	-0.344***	-0.272**	-0.276**	-0.277**
	(0.096)	(0.095)	(0.092)	(0.092)
District FE	✓	✓	✓	✓
Cyclical Trend (offset)		✓	✓	✓
Population (offset)		✓	✓	✓
Sample	Full	Full	Full	Full
Imputed	No	No	Strat. A	Strat. B
Obs.	598	598	598	598
AIC	5838.7	5837.3	5883.5	5897.4

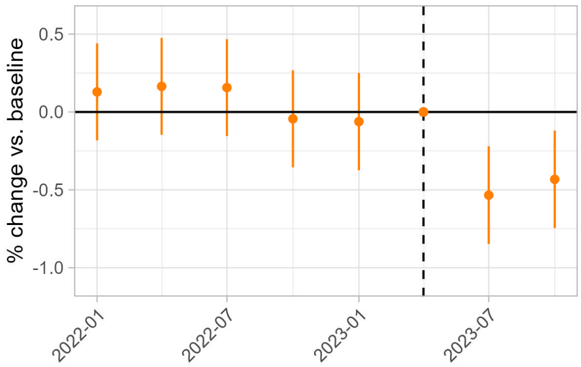
Notes: Negative Binomial Count Models with offsets for seasonal trend based on Table 5 and district population by quarter. Missing values imputed in 03-22, 11-22 and 12-22. Imputation strategies according to Appendix C. Enactment periods are highlighted in bold. Significance codes: $^{+}$ *$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table 14: Counts by Agent

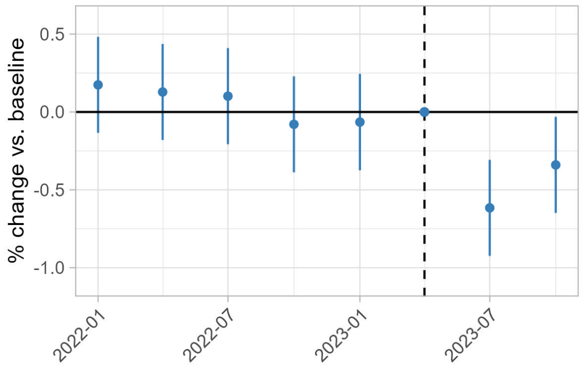
	Counts			
	(1)	(2)	(3)	(4)
Intercept	5.778***	-3.901***	-6.655***	-6.669***
	(0.093)	(0.093)	(0.135)	(0.137)
Agent			2.266***	2.270***
			(0.120)	(0.122)
2022-01-01	-0.073	-0.022	-0.828***	-0.680***
	(0.068)	(0.068)	(0.133)	(0.133)
2022-04-01	0.088	0.104	-0.267*	-0.268*
	(0.068)	(0.068)	(0.127)	(0.129)
2022-07-01	0.144*	0.069	0.152	0.150
	(0.068)	(0.068)	(0.124)	(0.126)
2022-10-01	-0.346***	-0.402***	-0.343**	-0.016
	(0.069)	(0.068)	(0.127)	(0.127)
2023-01-01	-0.160*	-0.133^{+}	0.227^{+}	0.225^{+}
	(0.068)	(0.068)	(0.124)	(0.126)
2023-07-01	**-0.653*** **	**-0.739*** **	**-0.030**	**-0.029**
	(0.069)	**(0.069)**	**(0.125)**	**(0.127)**
2023-10-01	-0.354***	-0.417***	-0.194	-0.195
	(0.069)	(0.068)	(0.126)	(0.128)
2022-01-01 x Agent			0.950***	0.947***
			(0.175)	(0.177)
2022-04-01 x Agent			0.460**	0.462**
			(0.171)	(0.174)
2022-07-01 x Agent			-0.034	-0.032
			(0.168)	(0.171)
2022-10-01 x Agent			-0.005	-0.065
			(0.171)	(0.172)
2023-01-01 x Agent			-0.377*	-0.376*
			(0.169)	(0.172)
2023-07-01 x Agent			**-0.641*** **	**-0.641*** **
			(0.169)	**(0.172)**
2023-10-01 x Agent			-0.214	-0.215
			(0.170)	(0.173)
District FE	✓	✓	✓	✓
Cyclical Trend (offset)		✓	✓	✓
Population Weights (offset)		✓	✓	✓
Sample	Full	Full	Full	Full
Imputed	No	No	No	Strat. A
Obs.	184	184	368	368
AIC	2134.7	2134.6	3723.9	3772.5

Notes: Based on Negative Binomial Count Models with offsets for seasonal trend based on Table 5 and district population by quarter. Missing values imputed in 03-22, 11-22 and 12-22. Imputation strategy according to Appendix C. Enactment periods are highlighted in bold. Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

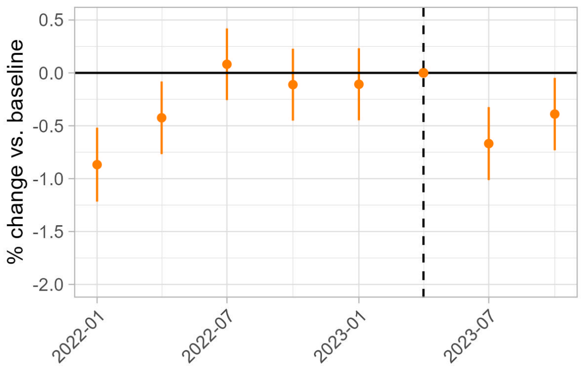
Figure 11: Counts by Regulation Status



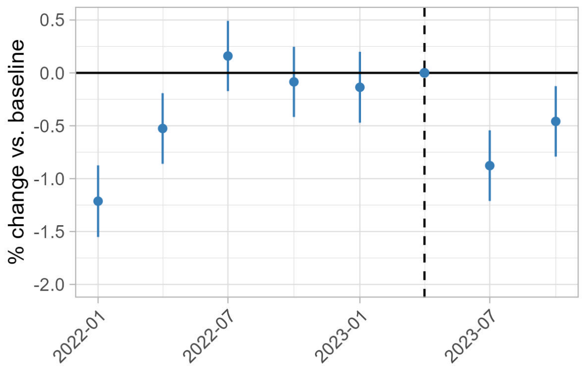
(a) Rent Cap – quarterly (Full)



(b) Exempt – quarterly (Full)



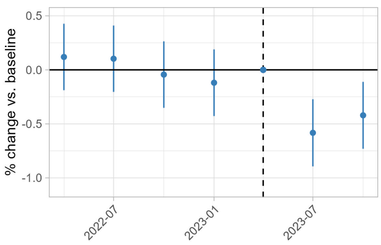
(c) Rent Cap – quarterly (Address)



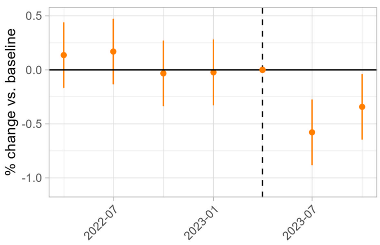
(d) Exempt – quarterly (Address)

Notes: Plots show results for the time-dummies of negative binomial models with cyclical trend and district population as offsets. Based on "full" sample without filters. Partial imputation of missing values in 03-22, 11-22 and 12-22. Detailed results in Table 15.

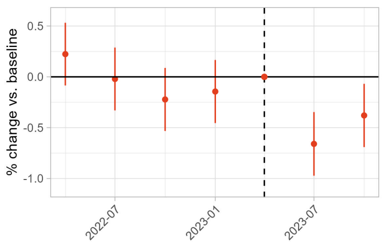
Figure 12: Counts by Price Segment



(a) Low – quarterly



(b) Medium – quarterly



(c) High – quarterly

Notes: Plots show results for the time-dummies of negative binomial models with cyclical trend and district population as offsets. Based on "full" sample without filters. Partial imputation of missing values in 03-22, 11-22 and 12-22. Detailed results in Table 16.

Table 15: Counts by Regulation Status

	Counts					
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	5.778***	-3.901***	-3.974***	-4.965***	-3.982***	-4.975***
	(0.093)	(0.093)	(0.170)	(0.185)	(0.171)	(0.186)
Rentcap			-1.013***	-1.100***	-1.012***	-1.095***
			(0.157)	(0.169)	(0.158)	(0.171)
2022-01-01	-0.073	-0.022	0.026	-1.451***	0.174	-1.213***
	(0.068)	(0.068)	(0.156)	(0.171)	(0.156)	(0.171)
2022-04-01	0.088	0.104	0.127	-0.528**	0.128	-0.526**
	(0.068)	(0.068)	(0.156)	(0.168)	(0.156)	(0.169)
2022-07-01	0.144*	0.069	0.101	0.157	0.102	0.160
	(0.068)	(0.068)	(0.156)	(0.167)	(0.156)	(0.168)
2022-10-01	-0.346***	-0.402***	-0.357*	-0.406*	-0.079	-0.085
	(0.069)	(0.068)	(0.156)	(0.167)	(0.156)	(0.168)
2023-01-01	-0.160*	-0.133+	-0.063	-0.134	-0.065	-0.136
	(0.068)	(0.068)	(0.156)	(0.167)	(0.156)	(0.169)
2023-07-01	**-0.653*** **	**-0.739*** **	**-0.615*** **	**-0.876*** **	**-0.616*** **	**-0.877*** **
	(0.069)	**(0.069)**	**(0.156)**	**(0.168)**	**(0.157)**	**(0.170)**
2023-10-01	-0.354***	-0.417***	-0.339*	-0.460**	-0.340*	-0.459**
	(0.069)	(0.068)	(0.156)	(0.167)	(0.157)	(0.169)
2022-01-01 x Rentcap			-0.030	0.447+	-0.045	0.345
			(0.222)	(0.247)	(0.223)	(0.247)
2022-04-01 x Rentcap			0.038	0.102	0.036	0.101
			(0.222)	(0.241)	(0.223)	(0.243)
2022-07-01 x Rentcap			0.057	-0.074	0.055	-0.078
			(0.222)	(0.239)	(0.222)	(0.241)
2022-10-01 x Rentcap			0.027	-0.042	0.035	-0.026
			(0.223)	(0.241)	(0.223)	(0.242)
2023-01-01 x Rentcap			0.001	0.028	0.003	0.028
			(0.222)	(0.240)	(0.223)	(0.242)
2023-07-01 x Rentcap			**0.081**	**0.210**	**0.082**	**0.209**
			(0.223)	**(0.242)**	**(0.224)**	**(0.244)**
2023-10-01 x Rentcap			-0.093	0.072	-0.093	0.068
			(0.223)	(0.241)	(0.224)	(0.244)
District FE	✓	✓	✓	✓	✓	✓
Cyclical Trend (offset)		✓	✓	✓	✓	✓
Population Weights (offset)		✓	✓	✓	✓	✓
Sample	Full	Full	Full	Address	Full	Address
Imputed	No	No	No	No	Strat. A	Strat. A
Obs.	184	184	368	368	368	368
AIC	2134.7	2134.6	4178.7	3479.2	4219.4	3530.2

Notes: Based on Negative Binomial Count Models with offsets for seasonal trend based on Table 5 and district population by quarter. Missing values imputed in 03-22, 11-22 and 12-22. Imputation strategy according to Appendix C. Enactment periods are highlighted in bold. Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 16: Counts by Price Segment

	Counts					
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	5.778***	-3.901***	-4.301***	-5.270***	-4.306***	-5.279***
	(0.093)	(0.093)	(0.157)	(0.170)	(0.156)	(0.169)
low			-0.922***	-0.871***	-0.920***	-0.871***
			(0.155)	(0.167)	(0.155)	(0.167)
high			-1.067***	-0.997***	-1.063***	-0.996***
			(0.156)	(0.167)	(0.156)	(0.167)
2022-01-01	-0.073	-0.022				
	(0.068)	(0.068)				
2022-04-01	0.088	0.104	0.136	-0.410*	0.137	-0.410*
	(0.068)	(0.068)	(0.154)	(0.166)	(0.154)	(0.165)
2022-07-01	0.144*	0.069	0.169	0.187	0.170	0.187
	(0.068)	(0.068)	(0.154)	(0.164)	(0.154)	(0.164)
2022-10-01	-0.346***	-0.402***	-0.317*	-0.346*	-0.032	-0.030
	(0.069)	(0.068)	(0.154)	(0.165)	(0.154)	(0.164)
2023-01-01	-0.160*	-0.133^{+}	-0.022	-0.034	-0.023	-0.036
	(0.068)	(0.068)	(0.154)	(0.165)	(0.154)	(0.165)
2023-07-01	**-0.653*** **	**-0.739*** **	**-0.578*** **	**-0.801*** **	**-0.578*** **	**-0.802*** **
	(0.069)	**(0.069)**	**(0.155)**	**(0.166)**	**(0.155)**	**(0.166)**
2023-10-01	-0.354***	-0.417***	-0.342*	-0.391*	-0.342*	-0.394*
	(0.069)	(0.068)	(0.154)	(0.165)	(0.154)	(0.165)
2022-04-01 x low			-0.017	-0.231	-0.017	-0.233
			(0.220)	(0.239)	(0.220)	(0.239)
2022-07-01 x low			-0.066	-0.141	-0.066	-0.142
			(0.219)	(0.235)	(0.219)	(0.235)
2022-10-01 x low			-0.016	-0.169	-0.011	-0.155
			(0.221)	(0.238)	(0.220)	(0.236)
2023-01-01 x low			-0.097	-0.221	-0.096	-0.219
			(0.220)	(0.237)	(0.220)	(0.237)
2023-07-01 x low			**-0.005**	**-0.042**	**-0.004**	**-0.041**
			(0.221)	**(0.240)**	**(0.221)**	**(0.240)**
2023-10-01 x low			-0.079	-0.102	-0.079	-0.099
			(0.221)	(0.238)	(0.221)	(0.238)
2022-04-01 x high			0.087	-0.161	0.087	-0.161
			(0.220)	(0.240)	(0.220)	(0.240)
2022-07-01 x high			-0.191	-0.154	-0.191	-0.153
			(0.220)	(0.236)	(0.220)	(0.236)
2022-10-01 x high			-0.165	-0.218	-0.190	-0.253
			(0.222)	(0.239)	(0.221)	(0.237)
2023-01-01 x high			-0.120	-0.302	-0.121	-0.301
			(0.221)	(0.238)	(0.221)	(0.238)
2023-07-01 x high			**-0.080**	**-0.007**	**-0.081**	**-0.005**
			(0.222)	**(0.241)**	**(0.222)**	**(0.241)**
2023-10-01 x high			-0.034	-0.105	-0.037	-0.104
			(0.221)	(0.239)	(0.221)	(0.239)
District FE	✓	✓	✓	✓	✓	✓
Cyclical Trend (offset)		✓	✓	✓	✓	✓
Population Weights (offset)		✓	✓	✓	✓	✓
Sample	Full	Full	Full	Address	Full	Address
Imputed	No	No	No	No	Strat. A	Strat. A
Obs.	184	184	483	483	483	483
AIC	2134.7	2134.6	5052.0	4277.2	5090.3	4318.4

Notes: Based on Negative Binomial Count Models with offsets for seasonal trend based on Table 5 and district population by quarter. Missing values imputed in 03-22, 11-22 and 12-22. Imputation strategy according to Appendix C. Enactment periods are highlighted in bold. Price segments computed based on rent per m² as described in subsection 5.3. Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 17: Volume Effects for Different Periods

	Counts per Month											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intercept	-5.076***	-7.872***	-5.014***	-7.798***	-5.057***	-7.857***	-4.995***	-7.783***	-5.221***	-7.800***	-5.156***	-7.719***
	(0.091)	(0.088)	(0.074)	(0.080)	(0.094)	(0.093)	(0.075)	(0.083)	(0.108)	(0.103)	(0.085)	(0.091)
Post	-0.452***	0.034	-0.519***	-0.029	-0.508***	0.015	-0.575***	-0.048	-0.463***	-0.134*	-0.523***	-0.198***
	(0.041)	(0.056)	(0.034)	(0.052)	(0.045)	(0.063)	(0.037)	(0.058)	(0.048)	(0.064)	(0.037)	(0.057)
Agent		2.399***		2.403***		2.393***		2.398***		2.166***		2.164***
		(0.042)		(0.038)		(0.042)		(0.038)		(0.049)		(0.043)
Post x Agent		-0.524***		-0.525***		-0.544***		-0.545***		-0.313***		-0.307***
		(0.075)		(0.068)		(0.084)		(0.076)		(0.085)		(0.075)
Start	*2022-01*	*2022-01*	*2022-01*	*2022-01*	*2022-01*	*2022-01*	*2022-01*	*2022-01*	*2022-07*	*2022-07*	*2022-07*	*2022-07*
End	*2024-02*	*2024-02*	*2024-02*	*2024-02*	*2023-12*	*2023-12*	*2023-12*	*2023-12*	*2023-12*	*2023-12*	*2023-12*	*2023-12*
District FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cyclical Trend (offset)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Population Weights (offset)	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Sample	Full	Full	Full	Full	Full	Full	Full	Full	Full	Full	Full	Full
Imputed	No	No	Strat. A	Strat. A	No	No	Strat. A	Strat. A	No	No	Strat. A	Strat. A
Obs.	598	1196	598	1196	552	1104	552	1104	414	828	414	828
AIC	6215.6	10065.4	6059.9	9996.9	5751.7	9341.4	5590.0	9279.0	4272.3	6966.2	4125.4	6878.9

Notes: Based on Negative Binomial Count Models on monthly basis with offsets for seasonal trend based on Table 5 and district population by quarter. Missing values imputed in 03-22, 11-22 and 12-22. Imputation strategy according to Appendix C. Further results in Table 13 and Table 14. Significance codes: $^{+}$ *$p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table 18: Heterogeneity: Counts by District Characteristics

	Location				*Income*				*Young*				*Students*				*Non − EU*				*Families*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Intercept	-3.931***	-4.729***	-5.050***	-5.745***	-3.946***	-5.330***	-5.054***	-6.230***	-3.960***	-5.094***	-5.046***	-6.059***	-3.946***	-5.444***	-5.055***	-6.318***	-3.947***	-5.169***	-5.037***	-6.118***	-3.944***	-4.802***	-5.057***	-5.798***
	(0.100)	(0.087)	(0.136)	(0.084)	(0.101)	(0.085)	(0.135)	(0.082)	(0.101)	(0.089)	(0.135)	(0.085)	(0.101)	(0.082)	(0.135)	(0.079)	(0.101)	(0.088)	(0.135)	(0.085)	(0.101)	(0.081)	(0.135)	(0.077)
Post	-0.619***	-0.646***	-0.478***	-0.484**	-0.481***	-0.549**	-0.506***	-0.550***	-0.471***	-0.614***	-0.495***	-0.559***	-0.482***	-0.552***	-0.511***	-0.534***	-0.486***	-0.511**	-0.428***	-0.399*	-0.553***	-0.597***	-0.452***	-0.460**
	(0.076)	(0.175)	(0.099)	(0.158)	(0.070)	(0.171)	(0.088)	(0.154)	(0.071)	(0.178)	(0.090)	(0.159)	(0.071)	(0.164)	(0.089)	(0.148)	(0.071)	(0.177)	(0.090)	(0.159)	(0.070)	(0.163)	(0.089)	(0.144)
Outside Guertel		-0.570***		-0.491***																				
		(0.118)		(0.113)																				
Edges and Transdanubia		-1.126***		-0.863***																				
		(0.175)		(0.167)																				
Post x Outside Guertel	0.119	0.095	-0.018	-0.046																				
	(0.103)	(0.236)	(0.133)	(0.212)																				
Post x Edges and Transdanubia	0.345*	0.372	0.028	0.074																				
	(0.154)	(0.350)	(0.196)	(0.313)																				
Upper half						0.434***		0.324**																
						(0.118)		(0.113)																
Post x Upper half					-0.066	-0.054	0.044	0.087																
					(0.097)	(0.237)	(0.123)	(0.213)																
Many										0.025		0.017		0.610***		0.467***		0.163		0.126		-0.628***		-0.550***
										(0.123)		(0.117)		(0.113)		(0.110)		(0.122)		(0.117)		(0.113)		(0.106)
Post x Many									-0.083	0.058	0.022	0.112	-0.063	-0.046	0.053	0.058	-0.056	-0.132	-0.104	-0.187	0.074	0.040	-0.059	-0.096
									(0.097)	(0.246)	(0.123)	(0.220)	(0.097)	(0.227)	(0.123)	(0.206)	(0.097)	(0.245)	(0.123)	(0.220)	(0.097)	(0.226)	(0.123)	(0.199)
District FE	✓		✓		✓		✓		✓		✓		✓		✓		✓		✓		✓		✓	
Sample	Full	Full	Address	Address	Full	Full	Address	Address	Full	Full	Address	Address	Full	Full	Address	Address	Full	Full	Address	Address	Full	Full	Address	Address
Obs.	184	184	161	161	184	184	161	161	184	184	161	161	184	184	161	161	184	184	161	161	184	184	161	161
AIC	2193.7	2476.9	1710.3	1833.7	2196.4	2505.2	1708.2	1857.8	2196.1	2521.1	1708.3	1870.0	2196.4	2487.8	1708.1	1845.5	2196.5	2519.5	1707.6	1869.3	2196.3	2484.8	1708.1	1833.3

Notes: All specifications include offsets for cyclical trend and population by district and quarter. Districts are assigned to three categories based on administrative data. All headings except 'Location' refer to coefficients 'few' and 'many'. Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 19: Staggered Price Effects – Time Dummies

(a) Quarterly

	$log(R)$ (1)	$log(R)$ (2)
2022-01-01	-0.023***	-0.044***
	(0.005)	(0.005)
2022-04-01	-0.027***	-0.039***
	(0.004)	(0.004)
2022-07-01	-0.023***	-0.034***
	(0.003)	(0.003)
2022-10-01	-0.027***	-0.023***
	(0.004)	(0.004)
2023-01-01	-0.023***	-0.020***
	(0.004)	(0.004)
2023-07-01	**0.049*** **	**0.051*** **
	(0.005)	**(0.004)**
2023-10-01	0.051***	0.052***
	(0.004)	(0.004)
2024-01-01	0.090***	0.081***
	(0.004)	(0.004)
Hedonics	✓	✓
District FE		✓
Location	MRF	SOS
Obs.	27904	27904
Adj. R^2	0.817	0.852
AIC	350393.4	345032.0

(b) Monthly

	$log(R)$ (1)	$log(R)$ (2)
2022-01-01	-0.040***	-0.047***
	(0.009)	(0.009)
2022-02-01	-0.018*	-0.037***
	(0.009)	(0.009)
2022-03-01	-0.018^{+}	-0.048***
	(0.010)	(0.010)
2022-04-01	-0.010	-0.026***
	(0.007)	(0.007)
2022-05-01	-0.035***	-0.035***
	(0.008)	(0.007)
2022-06-01	-0.047***	-0.057***
	(0.007)	(0.006)
2022-07-01	-0.028***	-0.042***
	(0.006)	(0.005)
2022-08-01	-0.043***	-0.045***
	(0.007)	(0.006)
2022-09-01	-0.015**	-0.023***
	(0.006)	(0.005)
2022-10-01	-0.023***	-0.020***
	(0.006)	(0.006)
2022-11-01	-0.042***	-0.033***
	(0.007)	(0.006)
2022-12-01	-0.030*	-0.009
	(0.012)	(0.012)
2023-01-01	-0.025***	-0.024***
	(0.006)	(0.006)
2023-02-01	-0.039***	-0.029***
	(0.007)	(0.007)
2023-03-01	-0.014^{+}	-0.003
	(0.007)	(0.007)
2023-04-01	-0.003	0.004
	(0.006)	(0.006)
2023-05-01	-0.006	-0.008
	(0.006)	(0.006)
2023-07-01	**0.031*** **	**0.037*** **
	(0.008)	**(0.008)**
2023-08-01	0.048***	0.053***
	(0.008)	(0.008)
2023-09-01	0.065***	0.064***
	(0.008)	(0.008)
2023-10-01	0.043***	0.050***
	(0.008)	(0.008)
2023-11-01	0.058***	0.056***
	(0.007)	(0.006)
2023-12-01	0.039***	0.047***
	(0.007)	(0.007)
2024-01-01	0.088***	0.084***
	(0.007)	(0.006)
2024-02-01	0.086***	0.076***
	(0.007)	(0.007)
Hedonics	✓	✓
District FE		✓
Location	MRF	SOS
Obs.	27904	27904
Adj. R^2	0.818	0.852
AIC	350334.1	344982.8

Notes: Based on GAM modeling location via Markov Random Field (MRF) Smooth or Spline-on-Sphere (SOS) Smooth. Based on "address" sample. Hedonic characteristics in Table 3. Enactment periods are highlighted in bold. Significance codes: $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 20: Staggered Price Effects by Regulation Status – Time Dummies

(a) Quarterly

	$log(R)$	
	(Rent Cap)	(Exempt)
2022-01-01	-0.016	-0.062***
	(0.011)	(0.006)
2022-04-01	-0.030***	-0.049***
	(0.009)	(0.004)
2022-07-01	-0.022**	-0.040***
	(0.008)	(0.003)
2022-10-01	-0.019*	-0.027***
	(0.009)	(0.004)
2023-01-01	-0.009	-0.026***
	(0.008)	(0.004)
2023-07-01	**0.066*** **	**0.041*** **
	(0.010)	**(0.005)**
2023-10-01	0.059***	0.050***
	(0.009)	(0.004)
2024-01-01	0.077***	0.082***
	(0.010)	(0.005)
Hedonics	✓	✓
District FE	✓	✓
Location	SOS	SOS
Obs.	6250	21654
Adj. R^2	0.836	0.865
AIC	76959.8	266632.8

(b) Monthly

	$log(R)$	
	(Rent Cap)	(Exempt)
2022-01-01	-0.008	-0.068***
	(0.017)	(0.010)
2022-02-01	-0.026	-0.053***
	(0.017)	(0.010)
2022-03-01	0.011	-0.079***
	(0.020)	(0.011)
2022-04-01	-0.009	-0.042***
	(0.015)	(0.007)
2022-05-01	-0.006	-0.053***
	(0.015)	(0.008)
2022-06-01	-0.059***	-0.064***
	(0.014)	(0.007)
2022-07-01	-0.030*	-0.052***
	(0.013)	(0.006)
2022-08-01	-0.019	-0.054***
	(0.015)	(0.007)
2022-09-01	-0.011	-0.034***
	(0.013)	(0.006)
2022-10-01	-0.014	-0.028***
	(0.013)	(0.006)
2022-11-01	-0.025$^+$	-0.042***
	(0.015)	(0.007)
2022-12-01	0.020	-0.020
	(0.028)	(0.012)
2023-01-01	-0.006	-0.030***
	(0.013)	(0.006)
2023-02-01	-0.029*	-0.039***
	(0.014)	(0.007)
2023-03-01	0.018	-0.019*
	(0.014)	(0.008)
2023-04-01	0.001	0.002
	(0.013)	(0.006)
2023-05-01	0.010	-0.018**
	(0.013)	(0.006)
2023-07-01	**0.055** **	**0.023** **
	(0.017)	**(0.008)**
2023-08-01	0.063***	0.039***
	(0.016)	(0.008)
2023-09-01	0.093***	0.049***
	(0.017)	(0.009)
2023-10-01	0.061***	0.043***
	(0.016)	(0.008)
2023-11-01	0.062***	0.053***
	(0.014)	(0.007)
2023-12-01	0.068***	0.037***
	(0.015)	(0.007)
2024-01-01	0.088***	0.078***
	(0.015)	(0.007)
2024-02-01	0.072***	0.076***
	(0.015)	(0.007)
Hedonics	✓	✓
District FE	✓	✓
Location	SOS	SOS
Obs.	6250	21654
Adj. R^2	0.837	0.866
AIC	76946.9	266601.6

Notes: Based on GAM modelling location via spline on sphere. Based on "address" sample. Hedonic characteristics in Table 21. Graphical results in Figure 6. Enactment periods are highlighted in bold. Significance codes: $^+$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 21: Hedonics by Regulation Status

	$log(R)$			
	Quarterly		*Monthly*	
	(Rent Cap)	(Exempt)	(Rent Cap)	(Exempt)
Intercept	3.350***	3.108***	3.352***	3.113***
	(0.060)	(0.039)	(0.061)	(0.039)
Living Area [log]	0.837***	0.869***	0.837***	0.869***
	(0.007)	(0.003)	(0.007)	(0.003)
Balcony/Terrace	0.038***	0.084***	0.042***	0.085***
	(0.005)	(0.004)	(0.005)	(0.004)
Garden	0.036**	0.041***	0.035**	0.041***
	(0.012)	(0.004)	(0.012)	(0.004)
Cellar	0.024***	-0.004	0.024***	-0.004
	(0.005)	(0.002)	(0.005)	(0.002)
Level: higher than ground floor	-0.021	0.031^{+}	-0.024	0.033^{+}
	(0.017)	(0.018)	(0.017)	(0.018)
Level: roof		0.060**		0.060**
		(0.020)		(0.020)
Elevator	0.014	0.059**	0.014	0.061**
	(0.023)	(0.020)	(0.023)	(0.020)
Level: higher than ground floor × Elevator	0.042^{+}	-0.019	0.042^{+}	-0.022
	(0.024)	(0.020)	(0.024)	(0.020)
Level: roof x Elevator		0.018		0.016
		(0.022)		(0.022)
First time use	0.061***	0.095***	0.062***	0.094***
	(0.007)	(0.003)	(0.007)	(0.003)
pre-1945		0.018***		0.017***
		(0.005)		(0.005)
Heating efficiency: A	0.191***	0.149***	0.191***	0.148***
	(0.019)	(0.006)	(0.019)	(0.006)
Heating efficiency: B	0.109***	0.097***	0.110***	0.097***
	(0.008)	(0.005)	(0.008)	(0.005)
Heating efficiency: C	0.005	0.006	0.006	0.006
	(0.007)	(0.005)	(0.007)	(0.005)
Heating efficiency: unknown	0.024***	0.057***	0.024***	0.057***
	(0.007)	(0.005)	(0.007)	(0.005)
Agent	-0.101***	-0.014**	-0.100***	-0.014**
	(0.008)	(0.005)	(0.008)	(0.005)
District FE	✓	✓	✓	✓
Location	SOS	SOS	SOS	SOS
Time	Dummies	Dummies	Dummies	Dummies
Obs.	6250	21654	6250	21654
Adj. R^2	0.836	0.865	0.837	0.866
AIC	76959.8	266632.8	76946.9	266601.6

Notes: Based on GAM modeling location via spline on sphere. Based on "address" sample. District FE and hedonic characteristics included. Time effects are reported in Table 20 and graphically depicted in Figure 6. Significance codes: $^{+}$ *p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.*

Table 22: Heterogeneous Price Effects by District Characteristics

	Location			*Income*			*Young*			*Students*			*Austrian*			*Non-EU*			*Families*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)
Intercept	2.944***	2.914***	3.203***	2.757***	2.914***	3.203***	2.734***	2.914***	3.204***	2.763***	2.914***	3.204***	2.724***	2.913***	3.203***	2.764***	2.914***	3.205***	2.784***	2.914***	3.205***
	(0.019)	(0.018)	(0.032)	(0.019)	(0.018)	(0.032)	(0.020)	(0.018)	(0.032)	(0.019)	(0.018)	(0.032)	(0.019)	(0.018)	(0.032)	(0.020)	(0.018)	(0.032)	(0.019)	(0.018)	(0.032)
Time	0.006***	0.010***		0.005***	0.010***		0.006***	0.010***		0.006***	0.010***		0.005***	0.010***		0.006***	0.010***		0.006***	0.010***	
	(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)	
Post	0.075***	0.056***	0.055***	0.052***	0.037***	0.042***	0.042***	0.045***	0.046***	0.046***	0.038***	0.042***	0.053***	0.037***	0.044***	0.060***	0.048***	0.048***	0.055***	0.040***	0.044***
	(0.005)	(0.005)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)	(0.005)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)	(0.003)
Outside Guertel	-0.140***																				
	(0.003)																				
Edges and Transdanubia	-0.124***																				
	(0.004)																				
Post x Outside Guertel	-0.013*	-0.017***	-0.015**																		
	(0.006)	(0.005)	(0.005)																		
Post x Edges and Transdanubia	-0.062***	-0.028***	-0.026***																		
	(0.007)	(0.007)	(0.007)																		
Upper half				0.101***																	
				(0.003)																	
Post x Upper half				0.013*	0.014**	0.011*															
				(0.005)	(0.005)	(0.005)															
Many							-0.014***			0.105***			0.056***			-0.051***			-0.063***		
							(0.003)			(0.003)			(0.003)			(0.003)			(0.003)		
Post x Many							0.016**	-0.003	-0.001	0.025***	0.012**	0.011*	-0.001	0.013**	0.005	-0.013*	-0.009+	-0.005	-0.008	0.004	0.005
							(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Hedonics	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
District FE			✓			✓			✓			✓			✓			✓			✓
Location		SOS	SOS		SOS	SOS		SOS	SOS		SOS	SOS		SOS	SOS		SOS	SOS		SOS	SOS
Obs.	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904	27904
Adj. R^2	0.796	0.850	0.852	0.790	0.850	0.852	0.774	0.850	0.852	0.788	0.850	0.852	0.779	0.850	0.852	0.778	0.850	0.852	0.779	0.850	0.852
AIC	353414.7	345312.4	345019.5	354268.0	345321.6	345027.9	356284.1	345330.2	345034.0	354471.2	345323.7	345028.6	355733.2	345324.5	345033.3	355795.6	345327.0	345033.1	355632.3	345329.8	345033.2

*Notes: Based on GAM modeling location via Spline-on-Sphere (SOS) Smooth. All columns include hedonic controls. Details on district characteristics in Table 12. Significance codes: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.*

Table 23: Conditional Quantile Regressions (by Regulation Status)

	$log(R)$					
	Rent Cap			Exempt		
	$\tau = 0.25$	$\tau = 0.5$	$\tau = 0.75$	$\tau = 0.25$	$\tau = 0.5$	$\tau = 0.75$
Intercept	3.198***	3.360***	3.470***	3.130***	3.096***	3.077***
	(0.052)	(0.048)	(0.052)	(0.047)	(0.035)	(0.040)
2022-01-01	-0.011	-0.004	0.013	-0.074***	-0.070***	-0.062***
	(0.011)	(0.011)	(0.012)	(0.007)	(0.006)	(0.007)
2022-04-01	-0.014	-0.020*	-0.021*	-0.043***	-0.047***	-0.049***
	(0.009)	(0.009)	(0.009)	(0.005)	(0.004)	(0.005)
2022-07-01	-0.002	-0.017*	-0.022**	-0.026***	-0.036***	-0.043***
	(0.008)	(0.008)	(0.008)	(0.004)	(0.003)	(0.004)
2022-10-01	0.006	-0.005	-0.011	-0.024***	-0.032***	-0.037***
	(0.009)	(0.008)	(0.009)	(0.004)	(0.004)	(0.004)
2023-01-01	0.002	-0.004	-0.003	-0.025***	-0.026***	-0.026***
	(0.008)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
2023-07-01	**0.082*** **	**0.071*** **	**0.068*** **	**0.039*** **	**0.040*** **	**0.037*** **
	(0.010)	**(0.009)**	**(0.010)**	**(0.006)**	**(0.005)**	**(0.005)**
2023-10-01	0.069***	0.068***	0.063***	0.052***	0.052***	0.049***
	(0.009)	(0.008)	(0.009)	(0.005)	(0.004)	(0.004)
2024-01-01	0.074***	0.082***	0.089***	0.086***	0.091***	0.083***
	(0.011)	(0.010)	(0.011)	(0.005)	(0.005)	(0.005)
Living Area [log]	0.829***	0.833***	0.840***	0.824***	0.855***	0.880***
	(0.008)	(0.007)	(0.007)	(0.005)	(0.004)	(0.004)
Balcony/Terrace	0.029***	0.042***	0.053***	0.083***	0.080***	0.080***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Garden	0.014	0.020^{+}	0.024*	0.044***	0.045***	0.045***
	(0.012)	(0.011)	(0.011)	(0.004)	(0.003)	(0.004)
Cellar	0.031***	0.027***	0.016**	-0.013***	-0.009***	-0.007**
	(0.005)	(0.005)	(0.005)	(0.003)	(0.002)	(0.003)
Level: higher than ground floor	-0.020	-0.020	0.005	0.014	0.012	0.033
	(0.017)	(0.016)	(0.018)	(0.030)	(0.018)	(0.020)
Level: roof				0.020	0.013	0.052*
				(0.031)	(0.020)	(0.023)
Elevator	0.007	0.011	0.010	0.042	0.038^{+}	0.074***
	(0.019)	(0.021)	(0.023)	(0.032)	(0.020)	(0.022)
Level: higher than ground floor × Elevator	0.047*	0.045*	0.047^{+}	0.005	-0.002	-0.043^{+}
	(0.020)	(0.021)	(0.024)	(0.032)	(0.020)	(0.022)
Level: roof x Elevator				0.053	0.063**	0.006
				(0.034)	(0.022)	(0.025)
First time use	0.071***	0.070***	0.066***	0.097***	0.094***	0.083***
	(0.007)	(0.007)	(0.008)	(0.003)	(0.003)	(0.003)
pre-1945				0.029***	0.021***	0.023***
				(0.007)	(0.006)	(0.007)
Heating efficiency: A	0.206***	0.205***	0.189***	0.162***	0.147***	0.133***
	(0.019)	(0.018)	(0.019)	(0.007)	(0.006)	(0.007)
Heating efficiency: B	0.127***	0.113***	0.098***	0.107***	0.103***	0.098***
	(0.009)	(0.008)	(0.008)	(0.006)	(0.005)	(0.006)
Heating efficiency: C	0.005	0.003	0.000	0.006	0.008	0.011^{+}
	(0.007)	(0.006)	(0.007)	(0.006)	(0.005)	(0.006)
Heating efficiency: unknown	-0.001	0.013^{+}	0.027***	0.053***	0.064***	0.083***
	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.007)
Agent	-0.047***	-0.100***	-0.147***	0.018*	-0.010^{+}	-0.033***
	(0.009)	(0.008)	(0.009)	(0.007)	(0.006)	(0.007)
District FE	✓	✓	✓	✓	✓	✓
Location	SOS	SOS	SOS	SOS	SOS	SOS
Obs.	6250	6250	6250	21654	21654	21654
AIC	-4355.6	-5007.3	-3964.2	-17225.0	-22430.0	-17635.6

Notes: Conditional quantile regressions WITH spatial smooth. Significance codes: $^{+}$ *p* < 0.1, * *p* < 0.05, ** *p* < 0.01, *** *p* < 0.001.

Table 24: Structural Break

	(1)	(2)		(3)			
	Main effect	Main effect	Post × X	Main effect	2023 x X	2024 x X	Post × X
Intercept	3.205***	3.191***		3.186***			
	(0.032)	(0.034)		(0.045)			
2022-01-01	-0.044***	-0.046***		-0.032			
	(0.005)	(0.005)		(0.046)			
2022-04-01	-0.039***	-0.04***		-0.024			
	(0.004)	(0.004)		(0.045)			
2022-07-01	-0.034***	-0.033***		-0.017			
	(0.003)	(0.003)		(0.045)			
2022-10-01	-0.023***	-0.025***		-0.009			
	(0.004)	(0.004)		(0.045)			
2023-01-01	-0.02***	-0.021***		-0.017***			
	(0.004)	(0.004)		(0.004)			
2023-07-01	**0.051*** **	**0.133** **		**0.118* **			
	(0.004)	**(0.044)**		**(0.059)**			
2023-10-01	0.052***	0.131**		0.115^{+}			
	(0.004)	(0.044)		(0.059)			
2024-01-01	0.081***	0.158***		0.181*			
	(0.004)	(0.044)		(0.075)			
Living Area [log]	0.861***	0.858***	0.01	0.849***	0.021**	0.068***	-0.017^{+}
	(0.003)	(0.004)	(0.007)	(0.005)	(0.007)	(0.014)	(0.009)
Balcony/Terrace	0.073***	0.063***	0.041***	0.051***	0.027***	-0.024^{+}	0.041***
	(0.003)	(0.003)	(0.006)	(0.004)	(0.007)	(0.014)	(0.008)
Garden	0.043***	0.048***	-0.019**	0.051***	-0.013^{+}	-0.019	-0.006
	(0.003)	(0.004)	(0.007)	(0.005)	(0.008)	(0.015)	(0.009)
Cellar	0.003	0.001	0.008	0.004	-0.006	-0.016	0.018**
	(0.002)	(0.003)	(0.005)	(0.003)	(0.005)	(0.01)	(0.006)
Level: higher than ground floor	-0.018	0.003	-0.093**	0.026	-0.057*	-0.23***	0.007
	(0.012)	(0.013)	(0.028)	(0.016)	(0.028)	(0.059)	(0.039)
Level: roof	0.015	0.033*	-0.105**	0.048*	-0.046	-0.242**	-0.004
	(0.015)	(0.017)	(0.036)	(0.021)	(0.036)	(0.076)	(0.048)
Elevator	0.023	0.046**	-0.108**	0.054**	-0.02	-0.12^{+}	-0.05
	(0.014)	(0.016)	(0.034)	(0.019)	(0.034)	(0.071)	(0.046)
Level: higher than ground floor x Elevator	0.032*	0.012	0.095**	-0.001	0.029	0.132^{+}	0.029
	(0.014)	(0.016)	(0.035)	(0.02)	(0.035)	(0.072)	(0.047)
Level: roof x Elevator	0.062***	0.049*	0.089*	0.043^{+}	0.024	0.105	0.033
	(0.017)	(0.019)	(0.041)	(0.024)	(0.041)	(0.087)	(0.056)
Agent	-0.046***	-0.053***	0.023*	-0.048***	-0.011	-0.06**	0.044***
	(0.004)	(0.005)	(0.009)	(0.007)	(0.01)	(0.021)	(0.012)
First time use	0.091***	0.092***	-0.009	0.105***	-0.031***	-0.006	0.002
	(0.003)	(0.003)	(0.006)	(0.004)	(0.006)	(0.013)	(0.007)
pre-1945	0.022***	0.043***	-0.074***	0.054***	-0.027*	-0.028	-0.06***
	(0.005)	(0.006)	(0.011)	(0.008)	(0.012)	(0.023)	(0.015)
Heating efficiency: A	0.161***	0.164***	-0.014	0.146***	0.039***	0.148***	-0.07***
	(0.005)	(0.005)	(0.01)	(0.007)	(0.01)	(0.021)	(0.013)
Heating efficiency: B	0.107***	0.108***	-0.007	0.095***	0.034***	0.105***	-0.05***
	(0.004)	(0.004)	(0.008)	(0.005)	(0.009)	(0.018)	(0.011)
Heating efficiency: C	0.008*	0.006	0.005	0.004	0.003	0.038*	-0.009
	(0.004)	(0.004)	(0.009)	(0.006)	(0.009)	(0.018)	(0.011)
Heating efficiency: unknown	0.049***	0.052***	-0.008	0.046***	0.014	0.016	-0.017
	(0.004)	(0.005)	(0.009)	(0.006)	(0.009)	(0.02)	(0.012)
Rent cap	-0.118***	-0.136***	0.06***	-0.141***	0.017	-0.004	0.057***
	(0.005)	(0.006)	(0.011)	(0.008)	(0.012)	(0.024)	(0.015)
District FE	✓	✓		✓			
Location	SOS	SOS		SOS			
Obs.	27904	27904		27904			
Adj. R^2	0.852	0.853		0.854			
AIC	345032.0	344884.4		344669.7			

Notes: Enactment periods are highlighted in bold. (1) is the basic model while (2) and (3) include interactions with all hedonic characteristics (including districts) except the locational smooth. Significance codes: $^{+}$ *p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001.*

Table 25: Robustness Check: Staggered Price Effects Total & Exempt (excluding 1945-1953)

(a) Quarterly

	$log(R)$			
	(Total)	(Total)	(Exempt)	(Exempt)
Intercept	3.205***	3.254***	3.108***	3.152***
	(0.032)	(0.035)	(0.039)	(0.046)
2022-01-01	-0.044***	-0.048***	-0.062***	-0.070***
	(0.005)	(0.006)	(0.006)	(0.007)
2022-04-01	-0.039***	-0.042***	-0.049***	-0.050***
	(0.004)	(0.004)	(0.004)	(0.004)
2022-07-01	-0.034***	-0.034***	-0.040***	-0.040***
	(0.003)	(0.003)	(0.003)	(0.003)
2022-10-01	-0.023***	-0.023***	-0.027***	-0.027***
	(0.004)	(0.004)	(0.004)	(0.004)
2023-01-01	-0.020***	-0.016***	-0.026***	-0.022***
	(0.004)	(0.004)	(0.004)	(0.004)
2023-07-01	**0.051***	**0.052***	**0.041***	**0.042***
	(0.004)	**(0.005)**	**(0.005)**	**(0.005)**
2023-10-01	0.052***	0.055***	0.050***	0.055***
	(0.004)	(0.004)	(0.004)	(0.004)
2024-01-01	0.081***	0.082***	0.082***	0.085***
	(0.004)	(0.004)	(0.005)	(0.005)
45-53 excluded		✓		✓
Hedonics	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Location	SOS	SOS	SOS	SOS
Obs.	27904	26021	21654	19771
Adj. R^2	0.852	0.859	0.865	0.877
AIC	345032.0	321106.3	266632.8	242290.1

(b) Monthly

	$log(R)$			
	(Total)	(Total)	(Exempt)	(Exempt)
Intercept	3.206***	3.256***	3.113***	3.154***
	(0.033)	(0.035)	(0.039)	(0.046)
2022-01-01	-0.047***	-0.050***	-0.068***	-0.076***
	(0.009)	(0.009)	(0.010)	(0.011)
2022-02-01	-0.037***	-0.048***	-0.053***	-0.070***
	(0.009)	(0.009)	(0.010)	(0.011)
2022-03-01	-0.048***	-0.043***	-0.079***	-0.072***
	(0.010)	(0.010)	(0.011)	(0.012)
2022-04-01	-0.026***	-0.028***	-0.042***	-0.044***
	(0.007)	(0.007)	(0.007)	(0.007)
2022-05-01	-0.035***	-0.035***	-0.053***	-0.049***
	(0.007)	(0.007)	(0.008)	(0.008)
2022-06-01	-0.057***	-0.064***	-0.064***	-0.068***
	(0.006)	(0.007)	(0.007)	(0.007)
2022-07-01	-0.042***	-0.044***	-0.052***	-0.054***
	(0.005)	(0.005)	(0.006)	(0.006)
2022-08-01	-0.045***	-0.044***	-0.054***	-0.053***
	(0.006)	(0.006)	(0.007)	(0.007)
2022-09-01	-0.023***	-0.025***	-0.034***	-0.034***
	(0.005)	(0.005)	(0.006)	(0.006)
2022-10-01	-0.020***	-0.021***	-0.028***	-0.027***
	(0.006)	(0.006)	(0.006)	(0.006)
2022-11-01	-0.033***	-0.035***	-0.042***	-0.041***
	(0.006)	(0.006)	(0.007)	(0.007)
2022-12-01	-0.009	-0.003	-0.020	-0.020
	(0.012)	(0.012)	(0.012)	(0.012)
2023-01-01	-0.024***	-0.024***	-0.030***	-0.032***
	(0.006)	(0.006)	(0.006)	(0.006)
2023-02-01	-0.029***	-0.024***	-0.039***	-0.031***
	(0.007)	(0.007)	(0.007)	(0.007)
2023-03-01	-0.003	0.003	-0.019*	-0.011
	(0.007)	(0.007)	(0.008)	(0.008)
2023-04-01	0.004	0.003	0.002	0.001
	(0.006)	(0.006)	(0.006)	(0.006)
2023-05-01	-0.008	-0.009	-0.018**	-0.019**
	(0.006)	(0.006)	(0.006)	(0.006)
2023-07-01	**0.037***	**0.038***	**0.023**	**0.026**
	(0.008)	**(0.008)**	**(0.008)**	**(0.008)**
2023-08-01	0.053***	0.051***	0.039***	0.042***
	(0.008)	(0.008)	(0.008)	(0.008)
2023-09-01	0.064***	0.064***	0.049***	0.048***
	(0.008)	(0.008)	(0.009)	(0.009)
2023-10-01	0.050***	0.053***	0.043***	0.050***
	(0.008)	(0.008)	(0.008)	(0.008)
2023-11-01	0.056***	0.058***	0.053***	0.058***
	(0.006)	(0.006)	(0.007)	(0.007)
2023-12-01	0.047***	0.050***	0.037***	0.041***
	(0.007)	(0.007)	(0.007)	(0.007)
2024-01-01	0.084***	0.083***	0.078***	0.082***
	(0.006)	(0.007)	(0.007)	(0.007)
2024-02-01	0.076***	0.078***	0.076***	0.079***
	(0.007)	(0.007)	(0.007)	(0.007)
45-53 excluded		✓		✓
Hedonics	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Location	SOS	SOS	SOS	SOS
Obs.	27904	26021	21654	19771
Adj. R^2	0.852	0.859	0.866	0.877
AIC	344982.8	321051.5	266601.6	242257.9

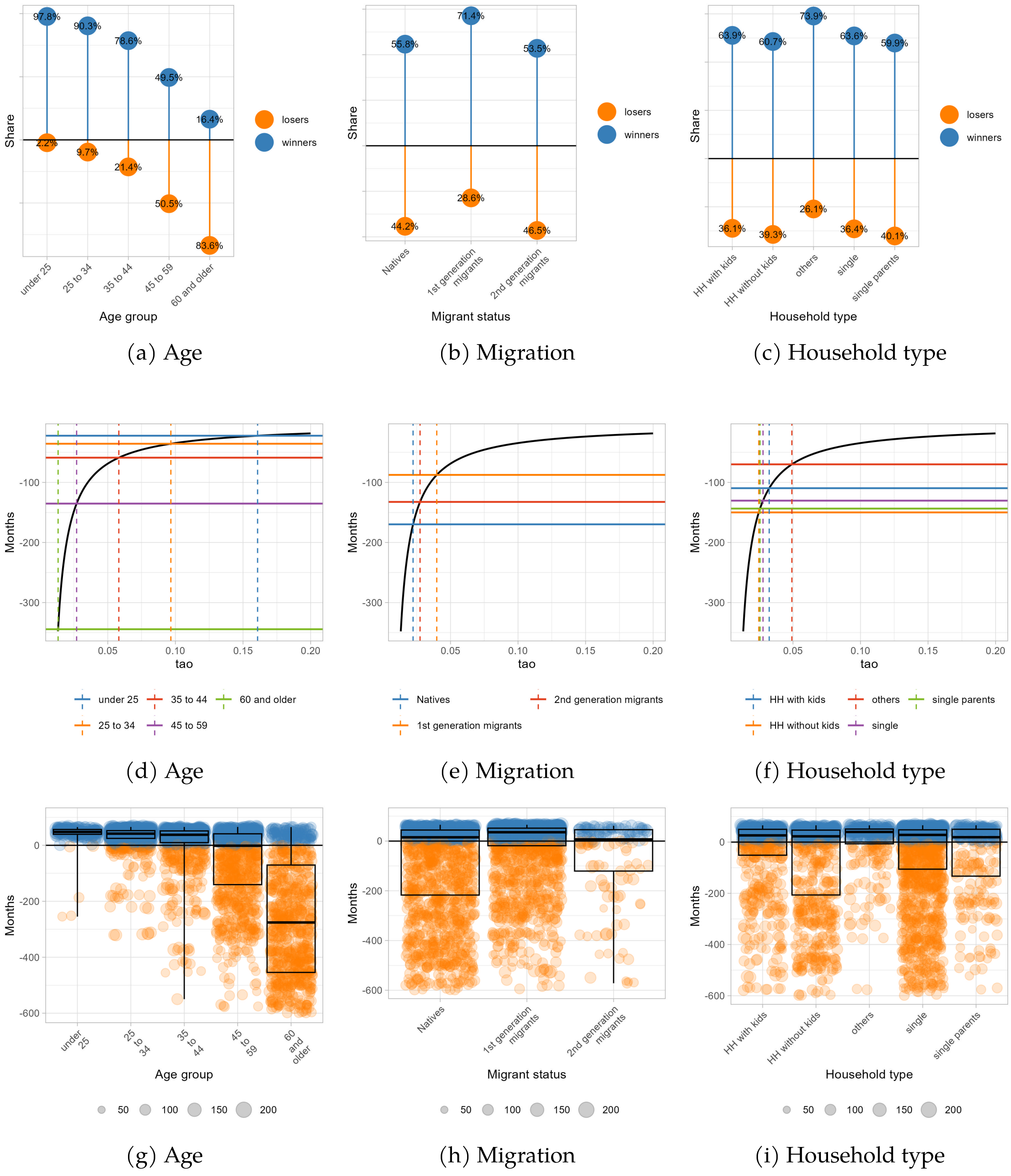
Notes: Based on GAM modelling location via spline on sphere. Based on "address" sample. Columns (2) and (4) in both tables exclude observations with year built between 1945 and 1953. Hedonic controls included. Enactment periods are highlighted in bold. Significance codes: $^{+}$ *p* < 0.1, * *p* < 0.05, ** *p* < 0.01, *** *p* < 0.001.

Table 26: Calibration Parameters

Variable	**Value**	**Definition**	**Source**
κ	3	Maximum number of monthly gross instalments for commission chargeable to landlords	RIS (Legal Information System of the Republic of Austria)
ι	3.066%	Risk-free rate (average government bond yields weighted by outstanding amounts, period average), value for July 2023	OeNB, UDRB ("Umlaufgewichtete Durchschnittsrendite für Bundesanleihen")
τ	$\exp(0.051) - 1$	Marginal policy effect (quarterly) ignoring effects of a shift in the mix of properties available for rent	Table 19a

Notes: The Table summarises calibration parameters for simulating break-even.

Figure 13: Winners and Losers



(a) Age (b) Migration (c) Household type

(d) Age (e) Migration (f) Household type

(g) Age (h) Migration (i) Household type

Notes: These plots relate (average) tenure length by household characteristics (Micro-census 2023) to results based on the model in section 6. Simulation parameters are summarised in Table 26. Plots (a), (b) and (c) show relative shares of winners and losers. Plot (d), (e) and (f) show break-even points N for different average tenure lengths. Plots (g), (h) and (i) show the difference between general break-even (44.5 months for $\tau = 0.051$) and average tenure lengths for different groups. Point sizes represent survey weights. Y-axis cut off at -600 months.