

When Real Estate Transaction Taxes Fall: Price Responses, Regional Spillovers, and Policy Outcomes

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Abstract

This paper examines the impact of a major 2022 reduction in registration duty for first-time buyers in the Flanders region of Belgium, using property-level platform data and administrative transaction records within a propensity score matching and spatial Difference-in-Differences framework. We show that, rather than generating broad-based price pressure, the reduction in real estate transaction taxes induced significant negative spillovers in nearby areas due to demand displacement. These effects are robust to alternative specifications that account for pre-treatment dynamics. Price effects are concentrated in houses, reflecting their predominance in Flanders' housing stock and their relative affordability compared to Brussels. Importantly, the reform stimulated market participation among buyers in their 20s and 30s - the intended target group - leading to a noticeable but temporary increase in transaction volumes. Although this effect faded amid tighter monetary conditions, the findings highlight how housing tax policies can facilitate market entry while also triggering cross-regional spillovers and intra-regional tax competition.

Keywords: Apartment, House, Affordability, Housing Prices, Housing Supply, Housing Market, Residential Real Estate, Transaction Tax

JEL Classification: G51, R21, R31, R38

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1 Introduction

Purchasing a home is among the most significant financial commitments individuals make, as it entails not only disbursing the property's price but also substantial ancillary costs - including transaction taxes, legal fees, and financing expenses - that collectively raise the total cost of ownership. As housing affordability becomes an increasing concern across OECD countries, particularly in Europe, governments have adopted a variety of policy instruments to support homeownership. These include mortgage interest tax deductions, subsidised loan schemes, and tax benefits for first-time buyers - most notably, reductions in property transfer taxes.

In response to mounting affordability pressures in the Flemish housing market - particularly for younger and first-time buyers - the regional government introduced a policy reform aimed at stimulating access to homeownership. Announced on 27 September 2021 and implemented on 1 January 2022, the reform halved registration duty on primary residences from 6% to 3%. This paper provides an empirical assessment of the reform's impact on housing market dynamics and policy outcomes.

Transaction taxes on residential properties (referred to as just houses or properties) are ad valorem levies, typically based on the property's market value at the time of sale, corresponding to the purchase price. They are generally applied at a flat rate and are usually paid by the buyer upon completion of the transaction. For newly built residential properties, a Value Added Tax (VAT) is commonly imposed instead (OECD, [2022](#)).

Much like the role attributed to financial transaction taxes in discouraging short-term and speculative trading (Schulmeister, [2008](#), [2009](#), [2010](#); Tobin, [1978](#)), property transfer taxes are often seen as instruments to temper house price growth and reduce market volatility. However, these taxes can also suppress transaction volumes, potentially introducing volatility in public revenues (Norregaard, [2013](#)). Despite their administrative appeal - due to a transparent tax base, typically the purchase price, and alignment with periods of heightened buyer liquidity - transfer taxes are frequently criticised for their economic inefficiency, as they may discourage mobility and impede

the optimal allocation of housing resources (OECD, [2022](#)).

Then how do markets respond to changes in these taxes, whether increases or decreases? Evidence from empirical studies suggests that higher transaction taxes cause declines in prices and market activity. Kopczuk and Munroe ([2015](#)) analysed the New York and New Jersey housing markets and show that real estate transaction tax notches lead to pronounced price discounts at thresholds, placing a burden on sellers and deterring economically beneficial transactions. Using Australian housing data, Davidoff and Leigh ([2013](#)) found that higher stamp duties lead to lower house prices, indicating that the tax burden primarily falls on sellers. They also estimate that even a modest 1% increase in stamp duty can reduce housing turnover by as much as 8% in the short term, underlining the tax's substantial influence on market activity. Similarly, Dolls et al. ([2025](#)) find that a one percentage point rise in real estate transfer taxes reduces German property prices by about 3% after one year, with stronger effects on apartments (−4%) than single-family homes (−2%). In a comparable study in Germany, a broad-based and staggered increase in real estate transfer taxes across 14 of the country's 16 federal states prompted immediate market responses following announcement. Specifically, offer prices declined by approximately 3%, and the volume of property listings fell by around 6% (Christofzik et al., [2020](#)). Across the literature, a consistent pattern emerges: increases in transaction taxes tend to discourage potential buyers and lead to a decline in transaction volumes. This effect is partly offset by sellers, who often respond by lowering asking prices to absorb some or all of the additional tax burden. In doing so, they help preserve a more appealing overall purchase cost - combining price and tax - for prospective buyers.

Conversely, when property transfer taxation is relaxed, we would expect housing market activity to intensify, often resulting in upward pressure on property prices. Evidence from the 2008–09 stamp duty holiday in the United Kingdom provides important empirical insights into the effectiveness of temporary transaction tax reductions as a fiscal stimulus during economic downturns. One study finds that housing market activity increased by approximately 20% in the short term, with less than half of this increase reversed after the tax was reinstated - suggesting that the policy had both

timing and real effects on market activity (Best & Kleven, 2018). Complementary research on the same policy shock shows that around 60% of the tax savings were captured by buyers, confirming that transaction taxes are largely capitalised into house prices. However, this study reports a more modest and short-lived increase in transaction volumes - around 8% - indicating a temporary boost in market activity in response to the policy intervention (Besley et al., 2014).

Nevertheless, some empirical studies cast doubt on the extent to which transaction taxes generate significant market distortions. Slemrod et al. (2016) find that transaction taxes have only modest effects on buying and selling behavior, leading them to conclude that the resulting welfare costs are relatively small. As regards housing transaction volumes, the commonly observed negative relationship with transaction taxes may, in part, be driven by shifts in the timing of transactions (Besley et al., 2014; Fritzsche & Vandrei, 2019), or may be influenced by concurrent non-tax factors, such as economic crisis and tightening of mortgage market regulations (Haider et al., 2016).

While property transaction taxes are administratively efficient, they can reduce housing transactions and hinder both residential and labor mobility (OECD, 2022). Indeed, transaction costs have been also viewed as having adverse effect on household mobility by raising the cost of relocating, thereby limiting adjustment to changing housing needs or employment opportunities. (Van Ommeren & Van Leuvensteijn, 2005) provided empirical evidence from the Netherlands indicating that higher transaction costs are associated with lower residential mobility, particularly among ownership-to-ownership moves, as these costs are primarily borne by buyers. In the United Kingdom, transfer taxes primarily inhibit moves associated with more gradual life-cycle transitions - such as short-distance and housing-related relocations - while having little deterrent effect on relocations prompted by major life events, including long-distance or job-related moves (Hilber & Lyytikäinen, 2013). A more recent study conducted in Finland finds that transfer taxes not only significantly reduce mobility with regard to short-distance moves, but also identifies impacts on long-distance relocations, suggesting potential implications for labour market functioning in

contexts with high homeownership rates (Eerola et al., 2021). Moreover, the effects of the reform appear asymmetric, with stronger impacts on upsizing and minor housing adjustments than on downsizing. This suggests that transaction taxes may distort life-cycle housing decisions by discouraging mobility and impeding efficient reallocation of housing as household needs evolve (Eerola et al., 2021). These findings underscore the heterogeneous nature of residential mobility, shaped significantly by institutional factors within housing and credit markets. Mobility tends to be greater in countries characterised by lower property transfer costs, more responsive housing supply, greater access to housing finance, and less restrictive rental market regulations, particularly among younger households (Caldera Sánchez & Andrews, 2011).

In 2022, Flanders, one of the three Belgian regions, cut registration duty (the equivalent of transaction tax in the country) from 6% to 3%, while Brussels and Wallonia kept them at 12.50%. The policy aimed was to improve housing accessibility and affordability for households with limited incomes, with a particular focus on young people. In the first empirical study on this policy change, Domènech-Arumí and Gobbi (2025) within seven months of implementation, the reduction in registration duty led to an average 3% increase in house prices and a modest decline in housing inequality, driven by compression at the lower end of the property value distribution.

The Belgian housing market offers a distinct and valuable context for empirical research. Although house prices exhibit a consistent upward trajectory, they do so with less volatility than in neighbouring countries. High property transfer taxes may constitute significant entry barriers, particularly for income-constrained groups such as younger households, potentially affecting both homeownership rates and residential mobility. However, in a small, well-connected country with an efficient intermodal transport system and a high prevalence of company cars, the constraints on labour mobility may be less pronounced. These structural characteristics raise important questions about the extent to which registration duties influence market behaviour, mobility patterns, and access to homeownership - especially among younger cohorts - within such a setting.

Our aim is to add to the previous papers by analysing the policy shock through two research ques-

tions: (i) *Did the policy achieve its objective of improving housing affordable for first-time buyers?* and (ii) *How did the policy affect regional housing markets dynamics - including price, transaction volumes, and heterogeneity across regions and property types?* To address these questions, we use two complementary datasets that span a longer period: (i) a unique anonymised real estate sales listing data from Immoweb - the leading and top of mind real estate platform in Belgium - covering more than one million observations from 2019 to 2024; (ii) a completed real estate transactions data, including buyer profile, provided by the Federal Ministry of Finance, and comprising over 231 thousands observations from 2019 to 2024.

As this policy intervention entails a quasi-natural experiment, Flanders is identified as the treatment group, with Brussels and Wallonia serving as control groups that provide a benchmark for isolating the policy's effect. After matching observations based on their propensity cores estimated on properties' characteristics, we implemented the Difference-in-Difference (DiD) method to assess the policy impact, controlling for economic conditions.

Using a spatial difference-in-differences framework, we show that the apparent price increase following the reform largely reflects cross-regional spillovers rather than a genuine expansion of the treated market. Once spatial diffusion is accounted for, prices in Flanders do not rise significantly, while neighbouring region - Brussels and adjacent Walloon districts - experience sizeable price declines, consistent with a displacement of demand. These spillovers are uneven across market segments: price effects are concentrated in houses rather than apartments, reflecting Flanders' housing stock composition and the greater affordability of houses relative to Brussels, particularly for budget-constrained first-time buyers. Market activity and purchases by younger buyers increase temporarily, indicating that transaction tax cuts can facilitate market entry and stimulate demand in the short run, even if their longer-term affordability effects depend on broader market conditions.

This paper contributes to the literature on housing taxation and policy evaluation by adopting a dual-perspective framework, jointly examining market responses and policy effectiveness. While previous study by Domènech-Arurí and Gobbi ([2025](#)) documents average price increases and

distributional effects following the same tax reform, we extend the analysis in three key ways. First, we leverage rich, daily property-level data from Belgium’s largest real estate platform, i.e., the primary marketplace where sellers and prospective buyers interact. Unlike official notary or registry data, which only capture finalised transactions with delay, this source reflects real-time market sentiment, pricing strategies, and buyer interest. It offers a timely and granular lens to assess the immediate impact of policy changes on housing market behaviour. Second, we employ a robust two-step identification strategy - combining propensity score matching with difference-in-differences - to strengthen causal inference. Third, and importantly, we incorporate a spatial DiD design to detect and quantify cross-regional spillover effects, a dimension largely overlooked in existing work. This allows us to disentangle direct policy effects from demand displacement across regions.

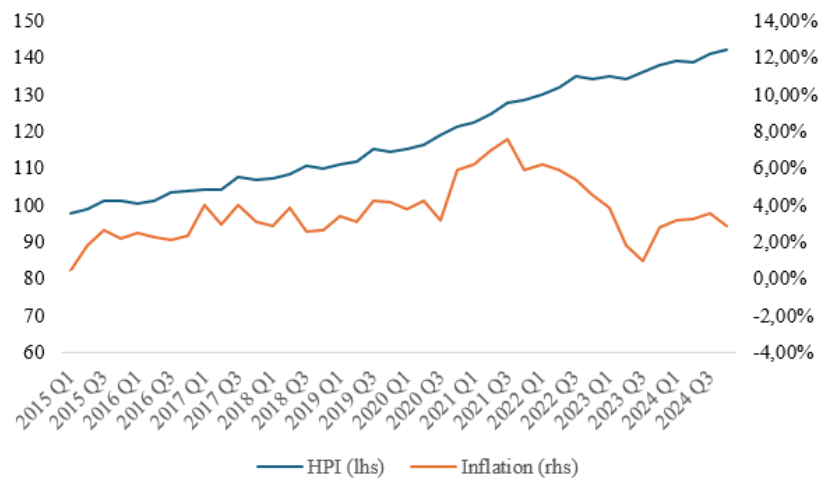
The remainder of this paper is organised as follows. Section 2 discusses the housing market in Belgium and the policy reform context. Section 3 presents the data. Section 4 proposes the methodology. Section 5 presents the baseline results. Section 6 is dedicated to testing the robustness of the results. Section 7 concludes and propose avenues for future research.

2 Belgian Housing Market and Policy Context

Belgians are known to be born with a brick in the stomach. Home to over 5 million households, with 57% residing in Flanders, 32% in Wallonia, and 11% in Brussels (Statbel, 2024), Belgium has one of the highest home ownership rates in Europe, reaching nearly 72% in 2024, compared to 69% in the Netherlands, 63% in France, and 47% in Germany (Eurostat, 2024). Despite its uptrend above inflation (Figure 1a), Belgium’s housing prices have remained relatively stable in recent years, contrasting with the greater volatility observed in several neighbouring markets, even throughout the 2008 global financial crisis and the subsequent Eurozone sovereign debt turmoil in 2010. As illustrated in Figure 1b, the Belgian housing market has exhibited greater price stability than its neighbouring countries since 2008. This is reflected in the relatively narrow range

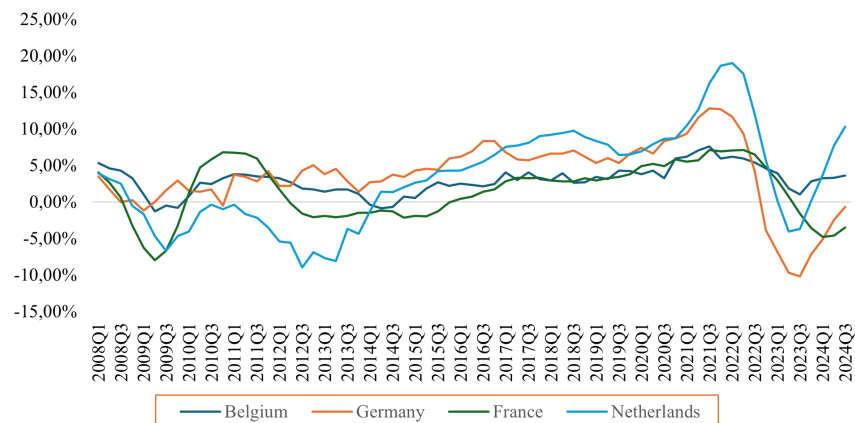
between its maximum and minimum observed housing prices, as well as the lowest standard deviation among the markets analysed.

Fig. 1a: House Price Index and Inflation Belgium



Source: Statbel, the Belgian statistical office; 2025

Fig. 1b: House Price Return Quarterly YoY in Belgium and neighbouring countries

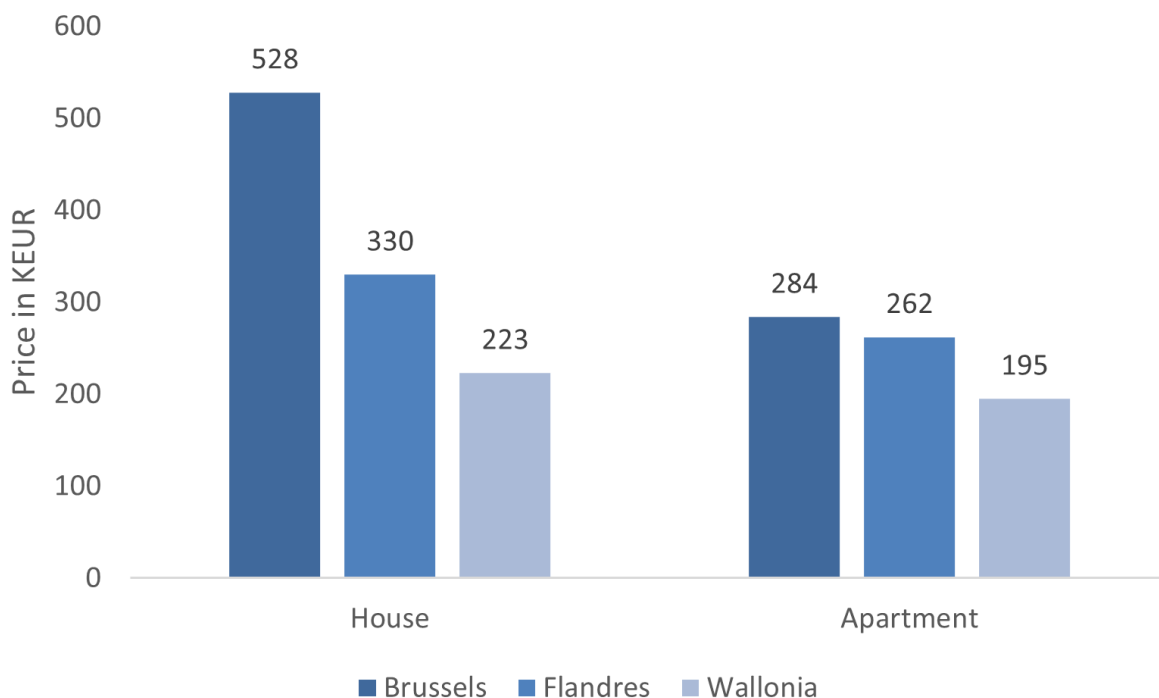


Source: Statbel, the Belgian statistical office, 2025

As of 1 January 2022, the population density in the Flemish Region was 492 inhabitants per km², compared to 217 in the Walloon Region and 7,528 in the Brussels (Statbel, 2022). Brussels, the Capital Region of Belgium, consists of 19 communes and has the highest urban density in the country, shaping a distinctive housing market structure. As the administrative centre of the Euro-

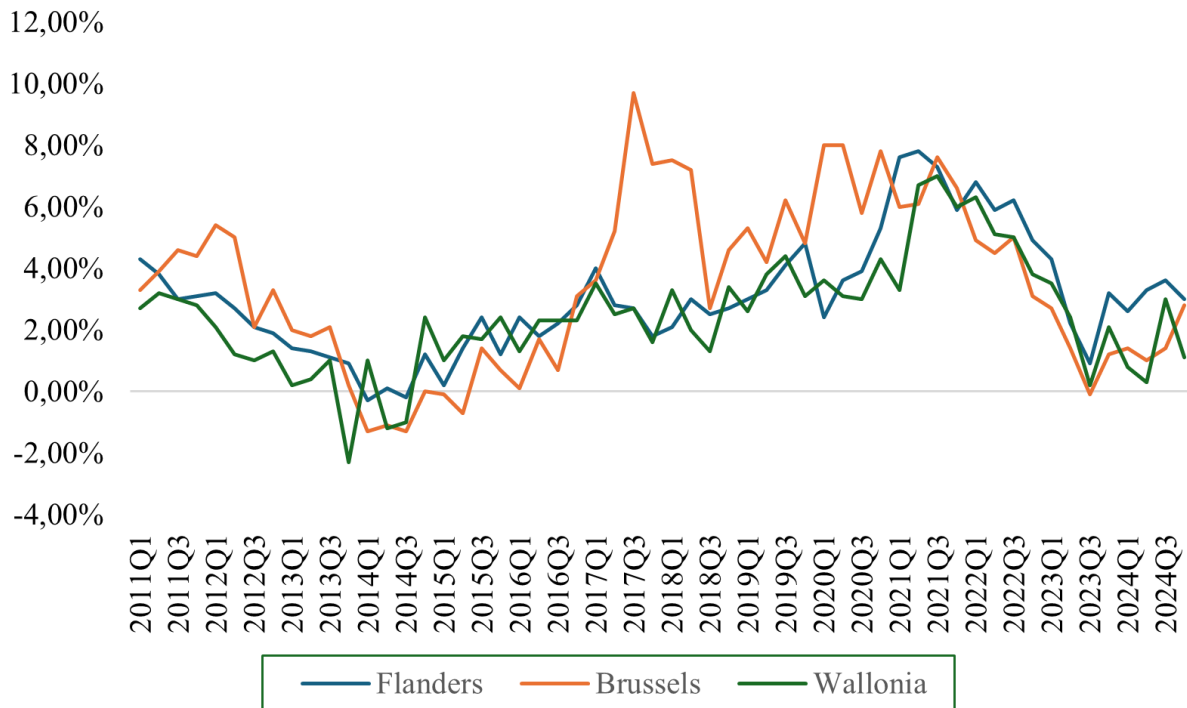
pean Union, NATO, and numerous multinational corporations, Brussels exhibits a predominance of rental dwellings, particularly apartments, which outnumber houses. This contrasts with the Flemish and Walloon Regions, where detached houses are more common and owner-occupancy is significantly higher. According to the 2021 census, 69.8% of conventional housing units in Flanders and 63.8% in Wallonia are owner-occupied, compared to just 38.1% in Brussels (Statbel, 2024). These structural and demographic differences are closely linked to regional disparities in property prices. Brussels records the highest housing prices per square meter in the country, particularly for apartments, reflecting its urban density and rental market orientation (Statbel, 2024). Fig. 2a highlights significant price disparities, especially in the case of houses, which are on average 60% more expensive in Brussels than in Flanders and 137% more expensive than in Wallonia. For apartments, prices in Brussels exceed those in Flanders by 8% and those in Wallonia by 46% (Fednot, 2021). While the three regional housing markets largely move in tandem, price declines since 2022 have been more pronounced in Brussels (Fig. 2b).

Fig. 2a: Average House Price by region in Belgium in 2021



Source: Fednot, 2022

Fig. 2b: House Price Return Quarterly YoY by region in Belgium



Source: Statbel, the Belgian statistical office, 2025

The functioning of the housing market is strongly influenced by Belgium's multilevel governance structure. Since July 2014, under the framework of the Sixth State Reform, the competence for housing policy, including housing taxation, has been transferred to the Regions. This major institutional shift empowers regional authorities to design and implement policies that are more coherent and better tailored to their specific socio-economic and territorial contexts (Zwart, 2015). In this regard, Flanders, economically more prosperous region with the highest median income and lowest employment rate, has taken policy actions to bolster its housing market. On September 27th, the region announced the reduction of its registration duty from 6% to 3%, while Brussels and Wallonia kept their levy at 12.50%. This tax reduction was designed to support affordability of owner-occupied housing for first-time and sole-home buyers in Flanders by significantly lowering upfront transaction costs amidst increasing property prices.

The registration duty - Belgium's equivalent of the Real Estate Transaction Tax (RETT) - is a key source of revenue for regional governments. It is levied on the purchase price of existing resi-

dential properties and is paid by the buyer at the time of the authentic deed that formalises the property transfer. This tax represents a substantial out-of-pocket expense, as it is not eligible for mortgage financing. As depicted in Tables 1a and 1b, under the new 3% registration duty regime, a buyer purchasing a house in Flanders would benefit from increased free cash flow, which could be allocated to boost available capital for down payment prior to securing mortgage financing or invested in refurbishing or furnishing the newly acquired property.

Table 1a: Registration Duty Comparison by Region (House Price: €300,000)

	Brussels	Flanders	Wallonia
Allowance	€175,000	€0	€20,000
Tax Base	€125,000	€300,000	€280,000
Registration Duty	12.5%	3%	12.5%
Tax Cost	€15,625	€9,000	€35,000
Additional Cost vs. Flanders	€6,625	€0	€26,000

Table 1b: Registration Duty Comparison by Region (House Price: €501,000)

	Brussels	Flanders	Wallonia
Allowance	€0	€0	€20,000
Tax Base	€501,000	€501,000	€481,000
Registration Duty	12.5%	3%	12.5%
Tax Cost	€62,625	€15,030	€60,125
Additional Cost vs. Flanders	€47,595	€0	€45,095

3 Data

For this study, we use two sets of data. The first set is a pooled cross-sectional data consisting of anonymised real estate sales listings covering the whole country from Immoweb, Belgium's largest real estate marketplace, covering over 1 million residential property listings between 2019 and 2024. It includes rich, daily information on asking prices, property characteristics (e.g., build year, size, condition, primary energy consumption, amenities), and geographic location. As a leading venue where buyers and sellers interact, this dataset offers a forward-looking perspective on housing market dynamics. Its granularity enables the analysis of short-term price responses to

the policy, as well as heterogeneity across regions and property types - essential to understanding how the reform shaped housing market behaviour beyond aggregate effects.

We start by restricting our sample to standard residential properties excluding commercial, cottages, bungalows, castles, manoir, mansions. Furthermore, we dropped observations with missing information on price, build year, primary energy consumption and having construction year beyond 2024. Based on energy consumption data, we use regional conversion tables (Table 2 in the Appendix A) to derive the corresponding Energy Performance Certificate (EPC) ratings, which are then included as covariates in our empirical model. Given the strong evidence that energy efficiency significantly influences property values (Cajias & Piazzolo, 2012; Groh et al., 2022), controlling for EPC helps improve the precision of our estimated treatment effects.

The final cleaned dataset comprises 251,802 observations, which serve as the basis for the propensity score matching procedure detailed in the methodology. Descriptive statistics for the matched sample of 59,042 observations are reported in Panel A of Table 3.

The second set of data is the completed real estate transactions - buyer profile data from Federal Ministry of Finance. The dataset comprises quarterly transactions of houses and apartments between 2016 and 2024, enriched with buyer characteristics such as age group, gender, and household composition (individual vs. joint buyers). It also includes median transaction prices and volumes at various geographic levels - commune, district, subregion, and region. We restricted the dataset to transactions from 2019 to 2024, excluding zero-value sales and focusing on houses, apartments, studios, and rooms. Property type “200” was removed due to overlap with types “533,” “534,” and “537.” The data were then reshaped into a panel by age group. By allowing for a clear identification of younger and potentially first-time buyers, as well as regional variation in transaction volumes, the data enables a direct assessment of whether the policy achieved its intended goal of improving housing affordability for first-time buyers. Descriptive statistics for the cleaned sample of 105,335 observations are reported in Panel B of Table 3. An extensive list of variables definitions is provided in Table 4 in appendix A.

Table 3: Summary Statistics

Panel A: Listings	Obs.	Mean	Std. Dev.	Min.	Max.
Log of Price per m ²	59,042	7.760	0.505	2.23	11.04
Construction Year	59,042	1970	34.84	1752	2024
Passenger Lift	59,042	0.29	0.45	0	1
Floor	59,042	1.13	3.51	-1	100
Number of Floors	59,042	2.28	2.88	0	80
Number of Bedrooms	59,042	3.06	1.50	0	73
Number of Bathrooms	59,042	1.84	1.60	0	50
Number of Toilets	59,042	1.64	1.17	0	27
Cellar	59,042	0.63	0.48	0	1
Number of Terraces	59,042	0.60	0.49	0	1
Number of Non-shared Walls	59,042	2.81	0.88	1	10
Number of Indoor Parking Spaces	59,042	0.79	1.34	0	30
Number of Outside Parking Spaces	59,042	1.17	2.23	0	30
Garden	59,042	0.00	0.05	0	1
Swimming Pool	59,042	0.05	0.21	0	1
EPC Label A	59,042	0.06	0.24	0	1
EPC Label B	59,042	0.14	0.35	0	1
EPC Label C	59,042	0.17	0.37	0	1
EPC Label D	59,042	0.20	0.40	0	1
EPC Label E	59,042	0.16	0.37	0	1
EPC Label F	59,042	0.11	0.31	0	1

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Variable	Obs.	Mean	Std. Dev.	Min.	Max.
EPC Label G	59,042	0.16	0.37	0	1
Condition: Mint/As New	59,042	0.28	0.45	0	1
Condition: Renovated	59,042	0.04	0.19	0	1
Condition: To Refurbish	59,042	0.11	0.31	0	1
Condition: To Renovate	59,042	0.09	0.29	0	1
Condition: To Restore	59,042	0.00	0.04	0	1
Condition: Well-kept	59,042	0.48	0.50	0	1
Economic Sentiment Indicator	59,042	-0.23	0.27	-1.39	0.30
Economic Policy Uncertainty	59,042	159.38	71.58	73.39	505.75
Interest Rate (bps)	58,976	74.89	116.76	-43.6	362.8
Panel B: Completed Transactions - Buyer Profile					
Year	105,038	2021.276	1.702	2019	2024
Quarter	105,038	246.729	6.811	236	259
Region ID	105,144	1.730	0.937	1	3
Panel ID	105,038	18216.37	11165.64	2	39647
Age Bracket ID	105,038	9.186	5.698	1	22
Age Group	52,450	1.534	0.499	1	2
Parcel Nature	105,038	225.476	89.022	200	537
Parcel Number	105,038	2.333	2.356	1	69

Note: This table reports summary statistics for the variables used in the empirical analysis. The variables are defined in Table 4 in appendix A. Panel A refers to the dataset on housing listings, while Panel B refers to completed real estate transactions - buyer profile. Statistics include mean, standard deviation, and minimum and maximum values.

4 Methodology

Since our aim is to perform an ex-post evaluation of a policy intervention, we will employ Difference-in-Difference (DiD) method to reliably estimate the impact of 3% reduction in registration duty in Flanders, on the outcomes - housing market dynamics and access to younger buyers, the main policy target.

To improve causal inference and address selection bias in this observational study, we will adopt a two-step strategy combining Propensity Score Matching (PSM) and (DiD). PSM first constructs a control group statistically similar to the treated group based on observed pre-treatment characteristics (Rosenbaum & Rubin, 1983), thereby reducing bias from observables. Since matching alone cannot address unobserved time-invariant factors, we will then apply DiD on the matched sample to exploit the temporal variation in treatment and control groups. This allows us to difference out time-invariant unobservable heterogeneity, consistent with the framework proposed by Heckman et al. (1997), Heckman et al. (1998). This combined method has been widely recommended for policy evaluation (A. Smith & E. Todd, 2005; Blundell & Dias, 2009) and is particularly relevant in the context of housing markets, where treatment may vary across space and time. The approach yields more credible causal estimates and enhances external validity - particularly valuable when randomized experiments are not feasible.

From the policy intervention, we can easily identify Flanders as the treated region and Brussels and Wallonia as the control ones. In the first stage, we estimate propensity scores using a logistic regression model, where the dependent variable indicates treatment status D of the housing policy shock (reduction of registration duty), conditional on pre-treatment characteristics, the property's characteristics such as build year, passenger lift, energy efficiency performance rating, number of rooms, and presence of amenities like garden, terrace, or parking.

$$p(x) = \Pr(D = 1 \mid x) = \mathbb{E}[D \mid x] \quad (1)$$

These scores capture the likelihood of a property being located in the treated region prior to the policy change. We then implement multiple matching algorithms - kernel, nearest neighbour, and radius - to construct a control group comparable to the treated group. Propensity score matching greatly improves covariate balance (Pseudo-R² from 0.267 to 0.016; mean bias from 21.4% to 3.4%), and R-ratio of 0.98 remains within the required range of 0.5 to 2). Yet the B-statistic remains above the conventional 25% threshold (falling from 138.2% to 30.4%), indicating that although matching substantially reduces imbalance, structural differences in housing characteristics persist - an expected feature of real-estate data with heterogeneous property stock. Moreover, the consistency of matching outcomes across methods supports the reliability of the constructed sample, setting foundations for the subsequent causal estimates.

In the second stage, we estimate DiD models using the matched sample, comparing the evolution of housing prices between treated and matched control properties before and after the policy intervention. Standard errors are clustered at the commune (city) level to account for intra-cluster correlation, as clustering at the region level is not feasible given the limited number of treatment clusters (three regions).

We estimate treatment effect with the following econometric model:

$$\ln(\text{Price_Sqm}_{irt}) = \alpha + \beta_1 \text{Treat}_r + \beta_2 \text{Post}_t + \tau(\text{Treat}_r \times \text{Post}_t) + \mathbf{X}'_{irt} \beta_3 + \mathbf{Z}'_t \theta + \gamma_r + \delta_t + \varepsilon_{irt} \quad (2)$$

Where: $\ln(\text{Price_Sqm}_{irt})$ is the log price per square meter of property i in region r at time t . Treat_r equals 1 for treated region and 0 otherwise. Post_t indicates the post-policy period. τ is the difference-in-differences estimate. $\mathbf{X}_{irt}$ includes property characteristics and energy performance.

$\mathbf{Z}_t$ contains macroeconomic controls. γ_r and δ_t denote region and time fixed effects. ε_{irt} is the error term.

To evaluate aggregate market transaction volumes, we estimate the following model:

$$\mathbb{E}[\text{Transactions}_{rt} \mid \mathbf{Z}_t] = \exp(\tau (\text{Treat}_r \times \text{Post}_t) + \mathbf{Z}_t' \boldsymbol{\theta} + \gamma_r + \delta_t) \quad (3)$$

Where: Transactions_{rt} denotes the number of housing transactions in region r at time t . The conditional mean is estimated using a Poisson pseudo–maximum likelihood (PPML) estimator. Treat_r equals 1 for treated region and 0 otherwise. Post_t indicates the post-policy period. τ captures the difference-in-differences effect. $\mathbf{Z}_t$ is a vector of macroeconomic controls. γ_r and δ_t denote region and time fixed effects.

To examine the policy’s effectiveness in stimulating purchases among the targeted age groups 20s (20 - 29) and 30s (30 - 39), we further estimate the following age-specific econometric specification:

$$\mathbb{E}[\text{Transactions}_{it}^{(g)} \mid \mathbf{X}_{it}] = \exp(\beta_g (\text{Treat}_i \times \text{Post}_t) + \mathbf{Z}_t' \boldsymbol{\theta} + \gamma_r + \delta_t), \quad g \in \{20s, 30s\} \quad (4)$$

Where: $\text{Transactions}_{it}^{(g)}$ denotes the number of housing transactions for age group g in region i at time t . The model is estimated using Poisson pseudo–maximum likelihood (PPML). β_g captures the age-group–specific difference-in-differences effect of the policy. Remaining specifications are similar to equation (3).

5 Results

To assess the broader effects of the policy on the housing market, we examined prices responses (including heterogeneity across regions), market activity, and policy effectiveness in opening market access to first-time buyers.

5.1 Price responses

Regarding price behaviour, our estimates in Table 5 indicate that the reduction in registration duty led to an average house price increase of approximately 4.75% in Flanders relative to other regions.

Table 5: Policy impact on real estate prices

	(1)	(2)	(3)	(4)	(5)
Treat $\times$ Post	0.0464** (0.0218)	0.0285 (0.0229)	0.0246 (0.0177)	0.0394** (0.0178)	0.0356** (0.0176)
Observations	58,976	42,573	45,937	48,960	52,063
Adjusted R-squared	0.4187	0.4085	0.4078	0.4079	0.4079
Controls	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Sample Period	2019-2024	2019-2022Q1	2019-2022Q2	2019-2022Q3	2019-2022Q4

Notes: The table reports coefficient estimates from equation (2) using the matched sample. The dependent variable is the logarithm of price per square meter. The specifications correspond to: (1) the full sample period; (2) up to the first quarter of 2022; (3) up to the second quarter of 2022; (4) up to the third quarter of 2022; and (5) up to the fourth quarter of 2022. All specifications are estimated using OLS within the DiD framework described in the text. Standard errors (in parentheses) are clustered at the city level to allow for serial correlation in local housing market shocks. Variable Treat $\times$ Post captures the policy effect. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

When it comes to explore potential heterogeneity across regions, we examined whether or not the treatment effects - the policy-induced upward divergence in prices - were evenly distributed across control regions. Table 6 highlights regional heterogeneity in the policy effect, with a stronger and

statistically significant impact observed when comparing Flanders with Brussels (+6.95%), while no significant difference is detected relative to Wallonia. The widespread adoption of remote work since COVID-19 pandemic may have favoured relocations to Wallonia, as families seek larger homes with gardens. Nevertheless, these results on regional heterogeneity highlight the need for further investigation into potential diffusion or spillover effects into adjacent areas.

Table 6: Heterogeneity in Regional Price Responses

	(1)	(2)	(3)
	Log of Price per sqm	Log of Price per sqm	Log of Price per sqm
Treat $\times$ Post	0.0464** (0.0218)	0.0672** (0.0281)	0.0221 (0.0233)
Observations	58,976	58,976	58,976
Adjusted R-squared	0.4187	0.4189	0.4189
Controls	Yes	Yes	Yes
Region FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Control group	All	Brussels	Wallonia

Notes: The table reports coefficient estimates from equation (2) by control group using the matched sample. The dependent variable is the logarithm of price per square meter. The specifications correspond to: (1) baseline estimate; (2) baseline estimate vs. Brussels only; (3) baseline estimate vs. Wallonia only. All specifications are estimated using OLS within the DiD framework described in the text. Standard errors (in parentheses) are clustered at the city level to allow for serial correlation in local housing market shocks. Variable Treat $\times$ Post captures the policy effect. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

5.2 Spillover effects

The fact that baseline results reveals heterogeneity among control regions, with coefficients being significant only for Brussels, may suggest that cut in registration duty triggered a potential “spillover effect”, shifting housing demand from Brussels and surrounding Wallonian districts to neighbouring communes in Flanders. In housing market contexts, policies implemented in one locality can influence nearby markets through demand shifts or price signalling, potentially contaminating control groups. Failing to account for such spatial diffusion risks violating the stable unit treatment value assumption (SUTVA) and biasing estimated effects (Dube et al., 2010; Miguel & Kremer, 2004). Moreover, the presence of spillover may bias treatment effects estimates in DiD (Borusyak et al., 2024; Imbens, 2024) and potentially violating parallel trend (Dube et al., 2010; Egger & Kreickemeier, 2012). This requires disentangling the direct treatment effect on treated units, the spillover/diffusion effect on nearby (bordering/inside) units, and the baseline trend in unaffected (control) units. To ensure the validity of the causal estimates, we need to test for potential spillover effects from the treated region to neighbouring areas. Spatial DiD methods and contaminated control group designs are well-established tools for identifying and isolating these indirect effects (Chyn, 2018; Dell, 2010).

We identified the diffusion zone comprising control group’s districts bordering Flanders. Those nearby districts are Brussels and five Wallonia districts bordering Flanders, as portrayed in Fig.3a - Fig.3b in appendix. This allows us to implement a spatial DiD strategy by classifying control units into near (e.g., directly bordering the treated region) and far (non-adjacent) groups. This ring-based approach allows us to test whether housing price dynamics differ with geographic distance from the treatment zone. We then estimate a modified DiD model that clearly distinguish treated, near-treated (potentially exposed), and unaffected control areas, with a partial treatment indicator for near-treated areas to explicitly test for diffusion effects, as proposed by Chyn (2018).

We then estimate the spillover effect with this adjusted empirical model:

$$\begin{aligned}
\ln(\text{Price_Sqm}_{irt}) = & \alpha + \beta_1 \text{Treat}_r + \beta_2 \text{Post}_t \\
& + \delta(\text{Treat}_r \times \text{Post}_t) + \theta(\text{Near_Treat}_r \times \text{Post}_t) \\
& + \mathbf{X}'_{irt}\boldsymbol{\beta} + \mathbf{Z}'_t\boldsymbol{\gamma} + \gamma_r + \varepsilon_{irt}
\end{aligned} \tag{5}$$

where spatial term $\text{Near_Treat}_r = 1$ for Brussels and the five Walloon border districts and 0 otherwise, with θ capturing the potential diffusion or spillover effects.

Estimates presented in Table 7 indicate that, once spatial spillovers are accounted for, the previously observed price increase in the treated region is no longer statistically significant.

Table 7: Spillover effects

	(1)	(2)
	Log of Price per sqm	Log of Price per sqm
Treat $\times$ Post	0.0464** (0.0218)	0.0034 (0.0284)
NearTreat $\times$ Post		-0.0575** (0.0267)
Observations	58,976	58,976
Adj. R-squared	0.4187	0.4257
Controls	Yes	Yes
Region FE	Yes	Yes
Quarter FE	Yes	Yes
Spillover	No	Yes

Notes: The table reports coefficient estimates from equation (5) using the matched sample. The dependent variable is the logarithm of price per square meter. The specifications correspond to: (1) baseline DiD estimates ; (2) spatial DiD estimates. All specifications are estimated using OLS within the DiD framework described in the text. Standard errors (in parentheses) are clustered at the city level to allow for serial correlation in local housing market shocks. Variable NearTreat $\times$ Post captures the spillover effect. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. For the full table including all covariates, see Appendix A - Table 7: Spillover effects.

The baseline estimate of a 4.75% post-policy price increase appears to be upwardly biased, likely

reflecting a reallocation of housing demand rather than a true market expansion. Supporting this interpretation, property prices in near-treated areas declined by approximately 5.60%, consistent with a diffusion effect mechanism whereby demand shifted from adjacent regions into the treated market.

Now that the spatial DiD estimates suggest spillover, we are interested to assess the pricing reaction by property type in near treated areas Table 8 shows a significant treatment effect only for houses, with an estimated 5.40% price decrease post-policy. These heterogeneous effects by property type reveal Flanders' housing stock profile and relative affordability.

Table 8: Spillover effects by property Type

	(1)	(2)
	Apartment	House
Treat $\times$ Post	0.0266 (0.0406)	0.0007 (0.0203)
NearTreat $\times$ Post	-0.0152 (0.0405)	-0.0554*** (0.0198)
Observations	24,446	34,530
Adj. R-squared	0.3393	0.3571
Controls	Yes	Yes
Region FE	Yes	Yes
Quarter FE	Yes	Yes

Notes: The table reports coefficient estimates from equation (5) by property type using the matched sample. The dependent variable is the logarithm of price per square meter. The specifications correspond to: (1) spatial DiD estimates for apartment ; (2) spatial DiD estimates for house. All specifications are estimated using OLS within the DiD framework described in the text. Standard errors (in parentheses) are clustered at the city level to allow for serial correlation in local housing market shocks. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

5.3 Market activity

As shown in Table 9, while the policy effect was not discernible over the study period, quarterly analysis reveals a temporary boost in the treated region, with above-trend performance sustained for the first year before gradually dissipating in 2024. Further evidence for these findings is presented in appendix D (Fig. 4). The effectiveness of the policy was likely dampened by the tightening of monetary policy aimed at curbing inflation, particularly in response to the surge in energy prices following the Russian invasion of Ukraine, which acted as a countervailing force (Haider et al., 2016).

Table 9: Policy impact on real estate transactions

	(1)	(2)	(3)	(4)	(5)
Treat $\times$ Post	0.0020 (0.0100)	0.1450*** (0.0261)	0.0840*** (0.0204)	0.0686*** (0.0163)	0.0504*** (0.0139)
Observations	90,638	49,511	53,921	58,065	62,836
Pseudo R-squared	0.2430	0.2647	0.2638	0.2605	0.2578
Controls	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Sample Period	2019-2024	2019-2022q1	2019-2022q2	2019-2022q3	2019-2022q4

Notes: The table reports coefficient estimates from equation (3). The dependent variable is the number of real estate transactions (parcel number), aggregated at the panel level. Panel fixed effects correspond to unique cells defined by age group, district (*niscode*), transaction type, parcel nature, buyer type, and price range. The specifications correspond to: (1) the full sample period; (2) up to the first quarter of 2022; (3) up to the second quarter of 2022; (4) up to the third quarter of 2022; and (5) up to the fourth quarter of 2022. Regressions are estimated using Poisson pseudo-maximum likelihood (PPML) within the DiD framework described in the text. Standard errors are clustered at the district level to allow for serial correlation in local housing market shocks. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

5.4 Policy effectiveness

To evaluate the policy's effectiveness in improving housing affordability for first-time buyers, we examined real estate acquisition patterns among individuals aged 20 - 29 and 30 - 39, benchmark-

ing these groups against relevant control cohorts. Table 10 shows no significant relative increase in young buyers in Flanders following the reform, consistent with aggregate market patterns. However, quarterly analysis reveals a temporary surge of roughly 16% in early 2022, which fades out by year-end (see Appendix D, Figures 5 - 8).

Table 10: Policy impact on real estate transactions by 20s 30s age groups

	(1)	(2)	(3)	(4)	(5)
Treat $\times$ Post	-0.0167 (0.0124)	0.1583*** (0.0406)	0.0996*** (0.0320)	0.0602** (0.0272)	0.0353 (0.0239)
Observations		13,051	14,181	15,262	16,468
Pseudo R-squared	0.2668	0.2938	0.2929	0.2882	0.2853
Controls	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Sample Period	2019-2024	2019-2022q1	2019-2022q2	2019-2022q3	2019-2022q4

Notes: The table reports coefficient estimates from equation (4). The dependent variable is the number of real estate transactions (parcel number) by buyers of 20s 30s age groups, aggregated at the panel level. Panel fixed effects correspond to unique cells defined by age group, district (*niscode*), transaction type, parcel nature, buyer type, and price range. The specifications correspond to: (1) the full sample period; (2) up to the first quarter of 2022; (3) up to the second quarter of 2022; (4) up to the third quarter of 2022; and (5) up to the fourth quarter of 2022. Regressions are estimated using Poisson pseudo-maximum likelihood (PPML) within the DiD framework described in the text. Standard errors are clustered at the district level to allow for serial correlation in local housing market shocks. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

6 Robustness

In this section, we present a series of robustness checks on our DiD analysis, focusing on the policy's impact on transaction volumes, prices, regional heterogeneity, and its effectiveness in attracting our proxy group of first-time buyers into the housing market.

6.1 Is the trend parallel?

The DiD design hinges on the parallel trends assumption, meaning that pre-treatment trends in treated and control groups must be comparable for post-treatment differences to serve as valid counterfactuals. To assess the plausibility of the parallel-trend assumption underlying our Spatial DiD estimate, we follow the framework proposed by Bilinski and Hatfield (2020). Rather than relying on conventional pre-trend significance tests, this approach evaluates whether violations of parallel trends of economically meaningful magnitude can be ruled out - a necessary condition to sustain a causal interpretation of the estimated policy effect.

In the first step, we allow either treated region (Model B.1.1 in appendix B) or near-treated regions (Model B.1.2 in appendix B) to follow their own linear trends prior to treatment, while pure control region define the counterfactual trend. We then relax this restriction by permitting both treated and near-treated regions to follow distinct linear pre-treatment trends, which serves as an unrestricted benchmark against which the restricted specifications are evaluated. The pre-existing trends are normalised to zero at treatment quarter. Then, we conduct non-inferiority (NI) tests using two margins ($\delta_1 = 0.0125$ and $\delta_2 = 0.0072$). The first threshold correspond to quarterly average of house price growth rate in the pre-treatment period of our sample (2019-2021), consistent with covid-19 period average, and the second is the quarterly long-term average (2011-2021).

As depicted in Table 11 in appendix B, the NI tests indicate that treated region exhibits economically meaningful pre-treatment trend differences relative to pure control, exceeding both non-inferiority margins ($\delta_2 < \delta_1 < [0.0124, 0.0177]$). In contrast, near-treated regions satisfy non-inferiority, with the upper bound of the 90% confidence interval laying below both NI margins ($UB_{90\%} = -0.0014 < \delta_2 < \delta_1$), providing strong evidence in favour of parallel trends.

Finally, we estimate step-up DiD models following Bilinski and Hatfield (2020), allowing for region-specific pre-treatment price trends. Table 12 in appendix B summarises the price estimates obtained from the baseline DiD, spatial DiD and the corresponding step-up models. Once treated-specific pre-trends are accounted for, the baseline DiD estimate for the treated region - previously

suggesting a price increase - becomes statistically insignificant, indicating that the initial price rise was driven by differential pre-treatment dynamics rather than the tax cut itself. In the spatial DiD model, prices in near-treated regions exhibit a stable and statistically significant decline even after adjusting for treated-specific pre-trends. Although the treated coefficient turns negative and significant in this specification, this price decline should be interpreted with caution, as treated units fail the non-inferiority test and therefore do not satisfy the identifying assumptions required for causal interpretation. Overall, the results indicate that the price increase observed for treated region in the baseline DiD is not credible due to violations of parallel trends, whereas the price decline in near-treated regions is robust across specifications and supported by non-inferiority tests. The findings are consistent with spillover effects - demand reallocation from Brussels and Wallonian districts bordering Flanders - rather than a positive direct price impact of the registration duty reduction in Flanders.

6.2 Bootstrapping, anticipation effect and placebo test

First, we re-estimate the spatial Difference-in-Differences (DiD) models using bootstrapped standard errors with 5,000 replications. The main treatment effects remain statistically significant at the 5% level. Second, to account for potential anticipation effects, we drop observations from 2021Q4 and find that the negative spillover effect in near-treated areas becomes only marginally stronger (by about 1%), reinforcing our baseline conclusion. Third, we reassign the treatment to Wallonia and Brussels as placebo tests. The estimated coefficients are small and statistically insignificant for Wallonia, while for Brussels they are significant and consistent with our spatial DiD findings. Finally, when restricting the sample to the pre-treatment period and artificially setting 2021 as the treatment year, the estimated effects are non-significant. Together, these checks summarised in (Table 13 in appendix C) confirm the stability of our results and support the credibility of the identification strategy.

Furthermore, it should be emphasised that Brussels is not only geographically enclosed within Flanders but also constitutes a bilingual region (French and Dutch), with a non-negligible share

of Flemish households. These linguistic and cultural ties may encourage residents of the Capital-Region to purchase property in Flanders, particularly under a more favourable tax regime. Such factors strengthen the interpretation of the spillover effects identified in the results section.

6.3 Other robustness

Demand displacement inferred with spatial DiD implies that Flandres should see relatively higher transactions volumes than near treated area. Indeed, the policy reform boosted transaction volumes in the short run, but its impact quickly faded as the market corrected, as shown in Fig.4 in appendix. Similar dynamics are observed for buyers in their 20s and 30s (Fig.5 - Fig.8). While these age groups are typically the most active across regions, their house purchases rose markedly in Flanders following the 2022 reform, highlighting the policy's effectiveness, though its impact appears short-lived. As expected, joint buyers were more active and at higher price range than singles (Fig.9 - Fig.11), alike buyers in their 30s who purchased more properties at higher price ranges (Fig.13) than those in their 20s (Fig.12). One plausible explanation is that the former group is at a more advanced stage in their professional careers, benefiting from higher compensation packages and greater accumulated savings.

7 Conclusion

This paper evaluates the impact of a major real estate transaction tax cut in the Flanders region of Belgium on housing market dynamics, using daily property-level platform data as well as administrative data, and applying a combination of propensity score matching, Difference-in-Differences (DiD), and spatial DiD estimation strategies. Our baseline DiD results indicate that prices have increased on average approximately 4.75% in Flanders relative to the rest of the country. However, the spatial DiD analysis reveals that this price effect is likely overstated: there was no a statistically meaningful prices rise in Flanders once cross-regional spillovers are accounted for. Instead, we observe a 5.60% price decline in neighbouring regions, including Brussels and adjacent

Walloon districts, consistent with a displacement of demand rather than a broad-based price surge.

Further heterogeneity analysis reveals that significant price increases are observed for houses, but not for apartments. This pattern suggests that the policy disproportionately benefited house buyers, likely reflecting the predominance of houses in Flanders' housing stock and their relatively greater affordability compared to Brussels. As a result, houses constitute the preferred option for budget-constrained first-time buyers - the policy's primary target group. Moreover, buyers in their 30s and joint purchasers display greater purchasing power, allowing them to move further up the price ladder relative to single buyers and those in their 20s.

When examining the policy's impact on real estate transactions, we observe a marked increase in market activity in early 2022; however, this positive response proved temporary. Transaction volumes gradually weakened throughout 2023 and fully corrected by 2024, underscoring the transitory nature of the policy's effect on overall housing market dynamics.

Similarly, we evaluate the policy's effectiveness in stimulating home purchases among buyers in their 20s and 30s - our proxy for first-time buyers - relative to control cohorts. Although the reform generated a noticeable uptick in purchases by these groups, the effect was short-lived and dissipated after three quarters. Nevertheless, the initial response suggests that transaction tax cuts can temporarily facilitate entry into the housing market, even if the impact is not sustained over time.

Our findings are consistent with the broader literature documenting the negative relationship between real estate transaction taxes and house prices, as well as with the evidence that rapid fading of market activity likely reflects the interplay of the reform with concurrent macroeconomic shocks. Importantly, the results highlight the need to account for spatial diffusion when evaluating such policies. This is particularly relevant for regions with open borders, where the outflow of financially stronger households toward neighbouring regions can erode the local tax base and potentially trigger dynamics of tax competition across regions.

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Appendix A: Tables

Table 2: EPC Label Thresholds by Region (kWh/m²/year)

EPC Label	Flanders (kWh/m ² /year)	Brussels (kWh/m ² /year)	Wallonia (kWh/m ² /year)
A+	-100 – 0	≤ 0	≤ 45
A	0 – 100	0 – 45	45 – 85
B	100 – 200	46 – 95	85 – 170
C	200 – 300	96 – 150	170 – 255
D	300 – 400	151 – 210	255 – 340
E	400 – 500	211 – 275	340 – 425
F	500 +	276 – 345	425 – 510
G	N/A	> 345	> 510

Source: Certinergie.

Table 4: Variable Definitions

Variable	Definition
<i>A. Pooled cross-sectional data on real estate listing from Immoweb</i>	
<i>Time</i>	
Year	The year of the listing.
Month	The time the listing was last seen.
Start Date	The time the listing first appeared.
End Date	The month the listing first appeared.
<i>Location</i>	
Region ID	The id of the region the listing was geolocated at.
Region	The name of the region the listing was geolocated at.
Subregion ID	The id of the subregion the listing was geolocated at.
Subregion	The name of the subregion the listing was geolocated at.
District ID	The id of the district the listing was geolocated at.
District	The name of the district the listing was geolocated at.

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Table 4: Variable Definitions – continued from previous page

Variable	Definition
Municipality	The name of the municipality the listing was geolocated at.
<i>Property Characteristics</i>	
Build Year	Year the property was built.
Number of rooms	Total number of rooms in the property.
Garden	Dummy equal to 1 if the property has a garden, 0 otherwise.
Cellar	Dummy equal to 1 if the property has a cellar, 0 otherwise.
Terrace	Dummy equal to 1 if the property includes a terrace.
Swimming Pool	Dummy equal to 1 if the property includes a swimming pool.
Indoor Parking Places	The number of parking spaces that are outside, not in a garage.
Outdoor Parking Places	The number of parking spaces that are inside, in a garage or underground.
Non-Shared Walls	The number of non-shared walls.
Condition: Mint Condition	Property is in perfect or like-new condition, either recently built or fully renovated to a high standard.
Condition: Renovated	Property has undergone significant modernization (e.g., kitchen, bathroom, systems) but is not newly built.
Condition: To Refurbish	Property is functional and habitable but requires minor repairs or cosmetic improvements.
Condition: To Renovate	Property shows signs of wear or outdated features and needs major interior or system upgrades.
Condition: To Restore	Property is in poor or derelict state; full restoration is required, often involving structural work.
Condition: Well-kept	Property has been regularly maintained and remains in good condition without urgent renovation needs.
<i>Energy Performance</i>	
EPC Label	Energy performance label, ranked from A (efficient) to G (inefficient).
Maximum Electrical Power	This is the primary energy consumption kWh/year that defines EPC label.
EPC Label A	Energy Performance Certificate Label A
EPC Label B	Energy Performance Certificate Label B

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Table 4: Variable Definitions – continued from previous page

Variable	Definition
EPC Label C	Energy Performance Certificate Label C
EPC Label D	Energy Performance Certificate Label D
EPC Label E	Energy Performance Certificate Label E
EPC Label F	Energy Performance Certificate Label F
EPC Label G	Energy Performance Certificate Label G
<i>Economic Indicators</i>	
Price (€)	The price of the listing in euros.
Price per m ² (€)	The price per square meter, computed from price and area.
Log of Price per m ²	The natural logarithm of the price per sqm of the property.
Economic Sentiment Indicator	A leading indicator of economic condition generated through textual analysis by Sentometrics BV
Economic Policy Uncertainty Indicator	A leading indicator of economic policy uncertainty generated through textual analysis by Sentometrics BV
Interest Rate	Daily 10-year Belgium Government Bond yield, used as proxy for mortgage lending rate, sourced from Bloomberg
<i>B. Completed real estate transactions - buyer profile data from Federal Ministry of Finance</i>	
NIS Code	The INS code of the administrative or statistical unit.
Name FR	Official French name of the commune.
Parcel Nature	Cadastral nature of the purchased parcels, corresponding to their morphological purpose. Examples: 200 (House), 533 (Room), 534 (Studio) and 537 (Apartment).
Buyer Type	Type of real estate buyer. This type defines three categories of buyers: those buying alone (Alone), in pairs (Two), or in groups larger than two (Other).
Age Group	The age group of individuals, regardless of gender, who have purchased parcels of the specified cadastral nature located within the described commune, for a price within the specified range. Examples: 20s: age group ranging from 20 to 29 years; 30s: age group ranging from 30 to 39 years.
Price Range	Purchase price bracket for the concerned real estate. Example: Range50000: Total purchase price between €0 and €50,000. - Range>750000: Total purchase price greater than €750,000.

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Table 4: Variable Definitions – continued from previous page

Variable	Definition
Parcels Number	Number of cadastral parcels of the specified nature, located in the described commune, purchased by the specified buyer type, and within the specified price range.
<i>C. Policy Exposure and Spillovers</i>	
Control Brussels	Control group Brussels.
Control Wallonia	Control group Wallonia
Treat	Treatment variable (dummy) that takes value equal to 1 if region is Flanders and Year is equal or greater to 2022, and 0 otherwise.
Treat Post	It is a binary indicator equal to 1 for observations in the treatment group after the policy intervention, and 0 otherwise.
Near Treat	Treatment variable (dummy) that takes value equal to 1 region Brussels and 5 districts of Wallonia bordering the treated region Flanders and Year is equal or greater to 2022, and 0 otherwise.
Near Treat Post	It is a binary indicator equal to 1 for observations in the near treated group after the policy intervention, and 0 otherwise. Captures spillover effects.

Table 7: Spillover Effects

	(1) Log of Price per sqm	(2) Log of Price per sqm
Treat x Post	0.0464** (0.0218)	0.0034 (0.0284)
quarter=236	0.0000 (.)	0.0000 (.)
quarter=237	0.0122 (0.0095)	0.0122 (0.0096)
quarter=238	0.0054 (0.0134)	0.0062 (0.0133)
quarter=239	0.0263** (0.0132)	0.0247* (0.0132)
quarter=240	0.0438***	0.0450***

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Table 7: Spillover Effects (continued)

	(1) Log of Price per sqm	(2) Log of Price per sqm
	(0.0148)	(0.0145)
quarter=241	0.0850** (0.0340)	0.0839** (0.0338)
quarter=242	0.0782*** (0.0240)	0.0766*** (0.0240)
quarter=243	0.0490** (0.0218)	0.0482** (0.0218)
quarter=244	0.0698*** (0.0232)	0.0677*** (0.0231)
quarter=245	0.0957*** (0.0170)	0.0941*** (0.0169)
quarter=246	0.1102*** (0.0173)	0.1095*** (0.0172)
quarter=247	0.0914*** (0.0209)	0.0915*** (0.0208)
quarter=248	0.0555* (0.0288)	-0.0650 (0.0500)
quarter=249	0.0959*** (0.0262)	-0.0223 (0.0480)
quarter=250	0.0964*** (0.0253)	-0.0213 (0.0498)
quarter=251	0.1003*** (0.0298)	-0.0182 (0.0524)
quarter=252	0.0815*** (0.0282)	-0.0379 (0.0505)
quarter=253	0.1126*** (0.0331)	-0.0082 (0.0556)
quarter=254	0.1910*** (0.0587)	0.0712 (0.0828)
quarter=255	0.0951 (0.0584)	-0.0110 (0.0671)
quarter=256	0.0806	-0.0366

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Table 7: Spillover Effects (continued)

	(1) Log of Price per sqm	(2) Log of Price per sqm
	(0.0543)	(0.0712)
quarter=257	0.1501*** (0.0548)	0.0480 (0.0631)
quarter=258	0.1705*** (0.0556)	0.0358 (0.0519)
quarter=259	0.1000* (0.0590)	0.0000 (.)
region_id=1	0.0000 (.)	0.0000 (.)
region_id=2	0.2263* (0.1270)	0.0092 (0.1299)
region_id=3	-0.2565*** (0.0726)	-0.3788*** (0.0723)
Construction Year	0.0011*** (0.0002)	0.0011*** (0.0002)
Passenger Lift	0.2307*** (0.0563)	0.2311*** (0.0565)
Floor	0.0020 (0.0015)	0.0019 (0.0015)
Number of Floors	-0.0035 (0.0044)	-0.0036 (0.0044)
Number of Bedrooms	-0.0574*** (0.0058)	-0.0572*** (0.0058)
Number of Bathrooms	0.0641*** (0.0225)	0.0633*** (0.0226)
Number of Toilets	-0.0064 (0.0106)	-0.0064 (0.0106)
Cellar	0.0300 (0.0294)	0.0291 (0.0295)
Number of Terraces	0.0663*** (0.0122)	0.0638*** (0.0122)
Number of Non-shared Walls	0.0207*	0.0220*

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Table 7: Spillover Effects (continued)

	(1) Log of Price per sqm	(2) Log of Price per sqm
	(0.0121)	(0.0122)
Number of Indoor Parking Spaces	0.0001 (0.0030)	-0.0003 (0.0030)
Number of Outside Parking Spaces	-0.0006 (0.0026)	-0.0007 (0.0026)
Garden	0.0578 (0.0363)	0.0554 (0.0351)
Swimming Pool	0.1380*** (0.0307)	0.1267*** (0.0292)
EPC Label A	0.1730*** (0.0332)	0.1767*** (0.0326)
EPC Label B	0.1366*** (0.0242)	0.1354*** (0.0242)
EPC Label C	0.1052*** (0.0166)	0.1028*** (0.0164)
EPC Label D	0.0605*** (0.0114)	0.0600*** (0.0112)
EPC Label E	0.0433*** (0.0097)	0.0432*** (0.0096)
EPC Label F	0.0240** (0.0101)	0.0252** (0.0100)
EPC Label G	0.0000 (.)	0.0000 (.)
Condition: Mint/As New	0.1092*** (0.0323)	0.1091*** (0.0324)
Condition: Renovated	0.1589*** (0.0518)	0.1577*** (0.0521)
Condition: To Refurbish	-0.0907*** (0.0091)	-0.0911*** (0.0091)
Condition: To Renovate	-0.1394*** (0.0175)	-0.1416*** (0.0175)
Condition: To Restore	-0.1990***	-0.1857***

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Table 7: Spillover Effects (continued)

	(1) Log of Price per sqm	(2) Log of Price per sqm
	(0.0557)	(0.0578)
Condition: Well-kept	0.0000 (.)	0.0000 (.)
Economic Sentiment Indicator	0.0289* (0.0160)	0.0316** (0.0160)
Economic Policy Uncertainty	0.0001 (0.0001)	0.0001 (0.0001)
Interest Rate	0.0001 (0.0001)	0.0001 (0.0001)
post=0		0.0000 (.)
post=1		0.1636*** (0.0630)
Near Treat=0		0.0000 (.)
Near Treat=1		0.2263*** (0.0591)
post=0 × Near Treat=0		0.0000 (.)
post=0 × Near Treat=1		0.0000 (.)
post=1 × Near Treat=0		0.0000 (.)
post=1 × Near Treat=1		-0.0575** (0.0267)
Constant	5.2603*** (0.3924)	5.3314*** (0.3844)
Observations	58,976	58,976
Adj. R-squared	0.4187	0.4257
Controls	Yes	Yes
Region FE	Yes	Yes
Quarter FE	Yes	Yes
Spillover	No	Yes

Notes: This is the full version of Table 7

Appendix B: Parallel Trend Assessment

This appendix presents the full set of econometric models used to assess the plausibility of the parallel trends assumption, following the non-inferiority framework of Bilinski and Hatfield (2020), together with the implementation of the “Step-Up” pre-trend adjustment applied in the robustness analysis.

B.1 Econometric Models

B.1.1 Restricted Pre-Trend Model (Treated vs Control)

$$y_{irt} = \alpha + \beta t + \theta \text{Treat}_i + \delta (t \times \text{Treat}_i) + \gamma_r + \varepsilon_{it}, t < 2022\text{q1}) \quad (6)$$

Notes. The outcome variable y_{it} denotes the log price per square meter. The variable t is a linear quarterly time trend. Treat_i is an indicator equal to one for treated region. γ_r denotes region fixed effects. The coefficient δ captures the differential pre-treatment trend slope between treated and control regions and constitutes the non-inferiority parameter of interest.

B.1.2 Restricted Pre-Trend Model (Near-Treated vs Control)

$$y_{irt} = \alpha + \beta t + \theta^{NT} \text{NearTreat}_i + \delta^{NT} (t \times \text{NearTreat}_i) + \gamma_r + \varepsilon_{it}. \quad (7)$$

B.1.3 Unrestricted Flexible Trend Model

$$y_{irt} = \alpha + \beta t + \theta^T \text{Treat}_i + \theta^{NT} \text{NearTreat}_i + \delta^T (t \times \text{Treat}_i) + \delta^{NT} (t \times \text{NearTreat}_i) + \gamma_r + \varepsilon_{it}, \quad t < T_0. \quad (8)$$

This model relaxes the assumption that both groups share the same linear trend in the pre-period.

B.2 Non-Inferiority Testing

We test whether pre-period trend differences are “small enough” to be considered economically negligible. Non-inferiority holds if: $\text{Upper}_{90\%}(\hat{\delta}) < \delta_0$.

Non-inferiority is assessed using two economically meaningful thresholds. The first threshold is $\delta_1 = 0.0125$, corresponding to the medium-term (2019-2021) average quarterly appreciation rate observed in the housing market. The second, more conservative threshold is $\delta_2 = 0.0072$, corresponding to the long-run (2011-2021) average quarterly appreciation rate. Non-inferiority is satisfied when the upper bound of the confidence interval for δ lies below the relevant threshold.

B.3 Joint F-Tests for Differential Pre-Trends

The null hypothesis of equal trends: $H_0 : \delta = 0$.

Table 11: Non-Inferiority Pre-Trend Checks and Joint F-Tests

Group Comparison	Slope Diff.	90% CI Upper	NI Verdict	Joint F-test p
Treated vs Control	0.0124	0.0177	Fail ($> \delta_1$)	0.000
Near-treated vs Control	-0.0165	-0.0014	Pass ($< \delta_1$)	0.0516

B.5 Step-Up Difference-in-Differences (Pre-Trend Adjusted)

The step-up specification augments the baseline DiD model by allowing for group-specific linear trends in the pre-treatment period. The parameter ρ adjusts away differential pre-period trends, ensuring that post-treatment estimates reflect deviations from pre-existing growth paths rather than trend extrapolation. Because treated region failed NI strongly, it will receive a pre-trend adjustment.

$$\begin{aligned}
\ln(\text{Price_Sqm}_{irt}) = & \alpha + \beta_1 \text{Treat}_r + \beta_2 \text{Post}_t \\
& + \delta (\text{Treat}_r \times \text{Post}_t) + \theta (\text{Near_Treat}_r \times \text{Post}_t) \\
& + \rho (t \times \text{Treat}_r \times (1 - \text{Post}_t)) \\
& + \mathbf{X}'_{irt} \boldsymbol{\beta} + \mathbf{Z}'_t \boldsymbol{\gamma} + \gamma_r + \varepsilon_{irt}.
\end{aligned} \tag{9}$$

Table 12: Baseline, Spatial, and Step-Up Difference-in-Differences Estimates

	Baseline DiD	Step-Up (Baseline)	Spatial DiD	Step-Up (Spatial)
Panel A: Treated Region				
Treat $\times$ Post	0.0464** (0.0218)	-0.0144 (0.0210)	0.0034 (0.0284)	-0.0664** (0.0210)
Treated pre-trend		0.0091*** (0.0033)		0.0098*** (0.0033)
Panel B: Near-Treated Region				
NearTreat $\times$ Post			-0.0575** (0.0267)	-0.0636** (0.0260)
Observations	58,976	58,976	58,976	58,976
Adjusted R-squared	0.4187	0.4192	0.4257	0.4263
Controls	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes

Notes: The table reports spatial and step-up DiD estimates from equations (5) and (9). The spatial model includes region and quarter fixed effects and a full set of property and macroeconomic controls. The step-up specifications additionally control for differential linear pre-treatment trends in treated region following the approach of Bilinski and Hatfield (2020). Standard errors are clustered at the city level and reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix C: Robustness tests

Table 13: Anticipation effect and placebo test

	(1)	(2)	(3)	(4)
NearTreat $\times$ Post	-0.0655** (0.0279)			
Treat $\times$ Post (Brussels)		-0.0620** (0.0266)		
Treat $\times$ Post (Wallonia)			-0.0043 (0.0229)	
NearTreat $\times$ Post 2021				-0.0320 (0.0242)
Observations	55,977	58,976	58,976	39,588
Adj. R-squared	0.4264	0.4188	0.4183	0.4162
Controls	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes
Quarter FE				
Spillover	Yes	No	No	Yes

Notes: The table reports coefficient estimates from equation using the matched sample. The dependent variable is the logarithm of price per square meter. The specifications correspond to: (1) anticipation effect (2021q1 dropped); (2) Brussels as the treated region; (3) Wallonia as the treated region; (4) year 2021 as treatment year. All specifications are estimated using OLS within the DiD framework described in the text. Standard errors (in parentheses) are clustered at the city level to allow for serial correlation in local housing market shocks. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Appendix D: Figures

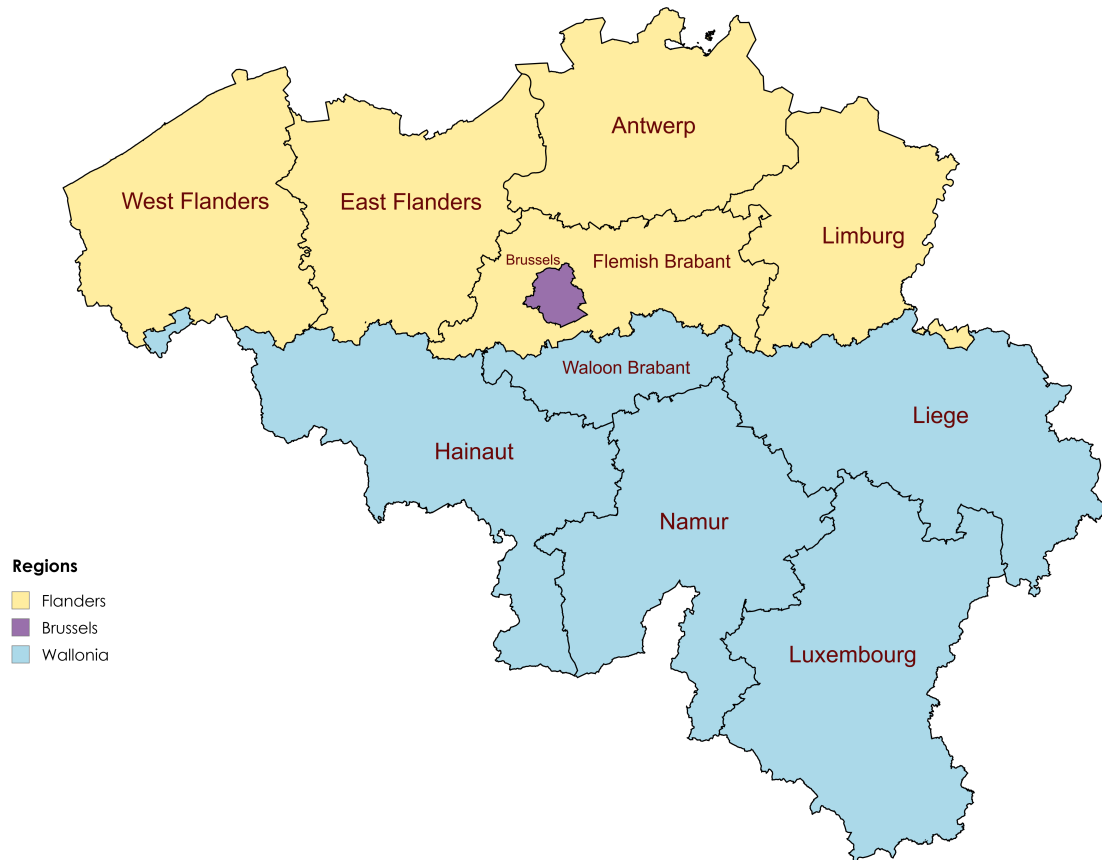
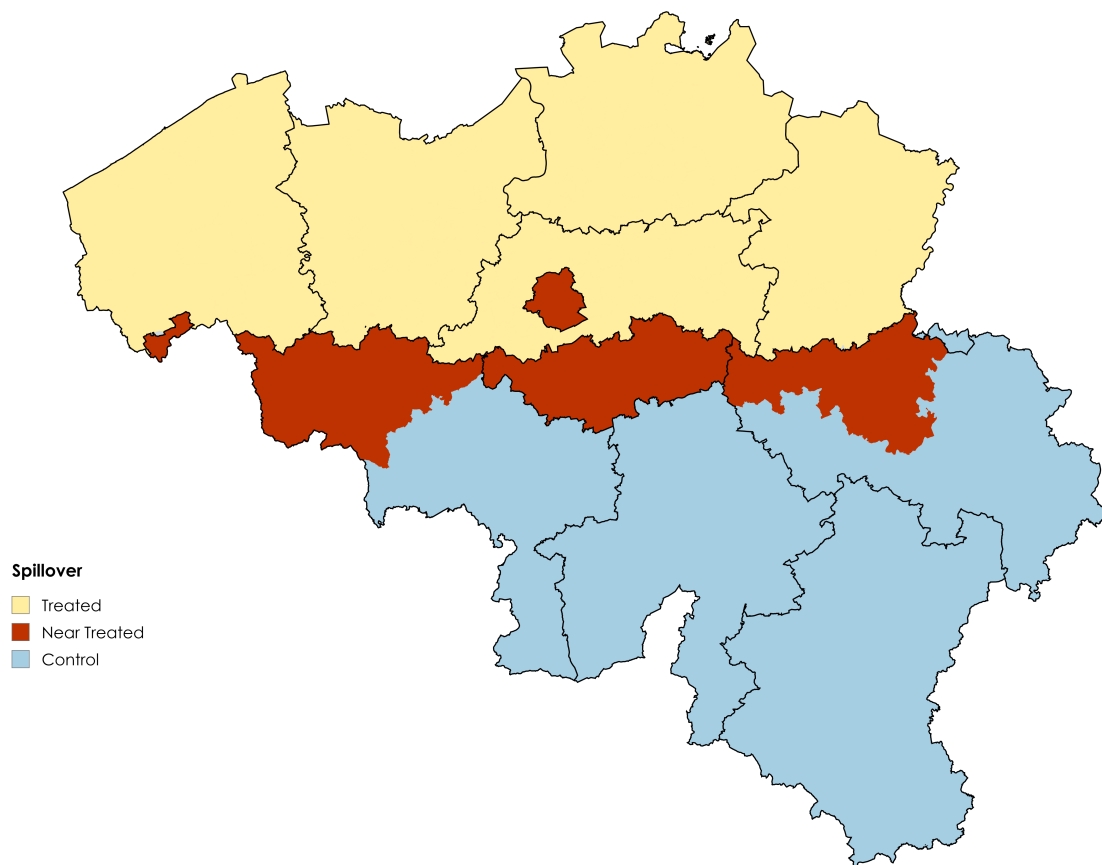


Fig.3a: Map of Belgium showing the three administrative regions: Flanders, Brussels and Wallonia.



Created with mapchart.net

Fig.3b: Map of Belgium showing the Near Treated (Brussels and Wallonian districts bordering Flanders).

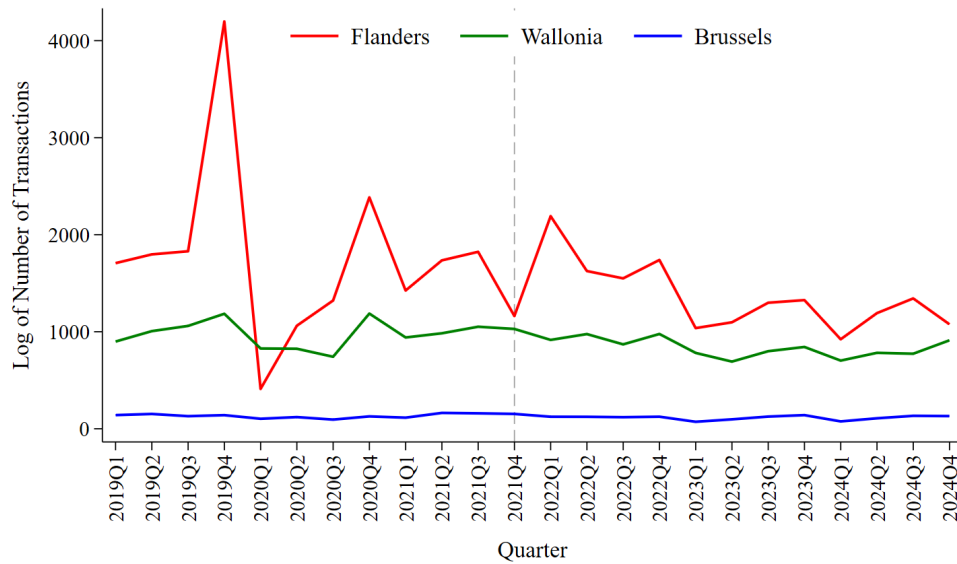


Fig.4: The figure depicts the log of completed real estate transactions across Belgian regions on a quarterly basis between 2019 and 2024. The vertical dashed line indicates the introduction of the registration duty reform in early 2022. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.

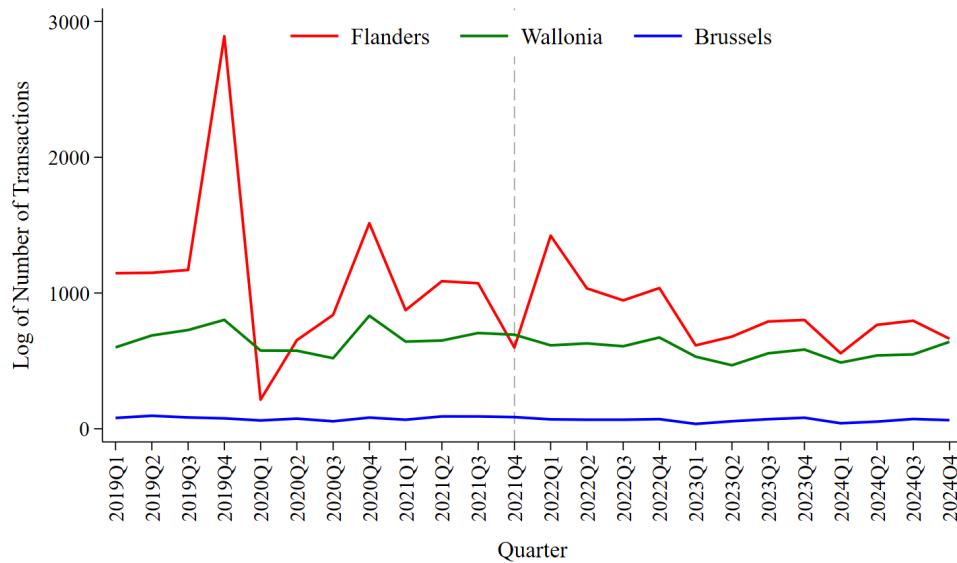


Fig.5: The figure depicts the log of completed real estate transactions for age groups 20s and 30s across Belgian regions on a quarterly basis between 2019 and 2024. The vertical dashed line indicates the introduction of the registration duty reform in early 2022. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.

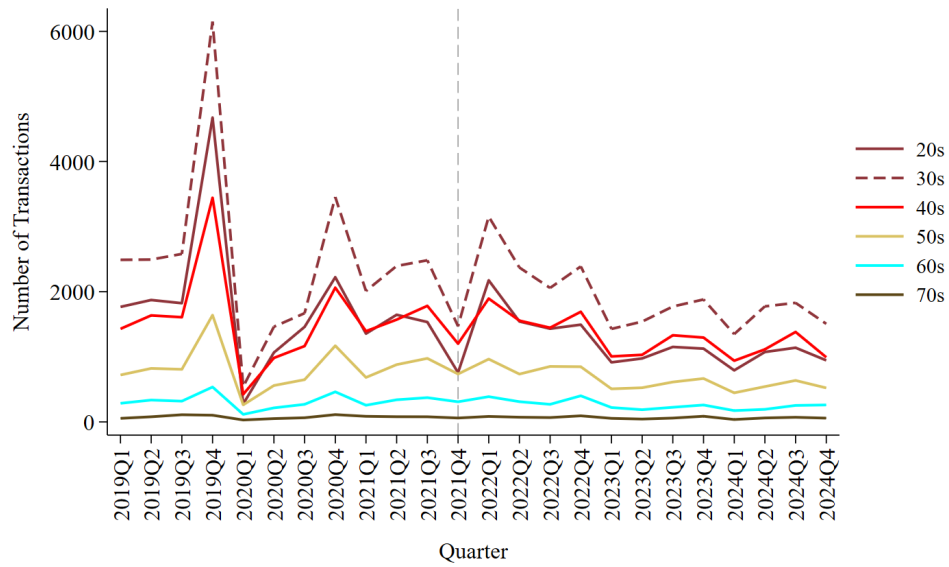


Fig.6: The figure depicts the number of completed real estate transactions by age groups in Flanders on a quarterly basis between 2019 and 2024. The vertical dashed line indicates the introduction of the registration duty reform in early 2022. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.

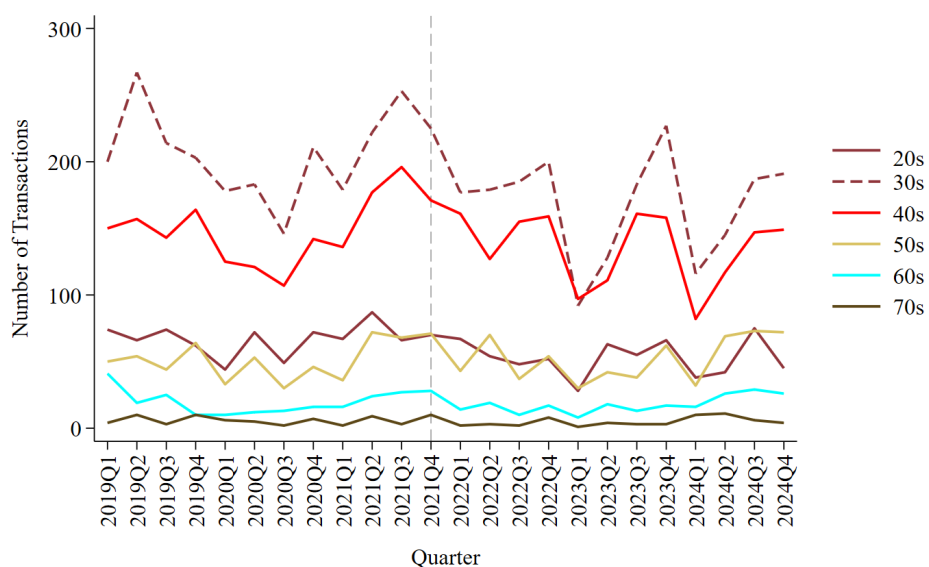


Fig.7: The figure depicts the number of completed real estate transactions by age groups in Brussels on a quarterly basis between 2019 and 2024. The vertical dashed line indicates the introduction of the registration duty reform in early 2022. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.

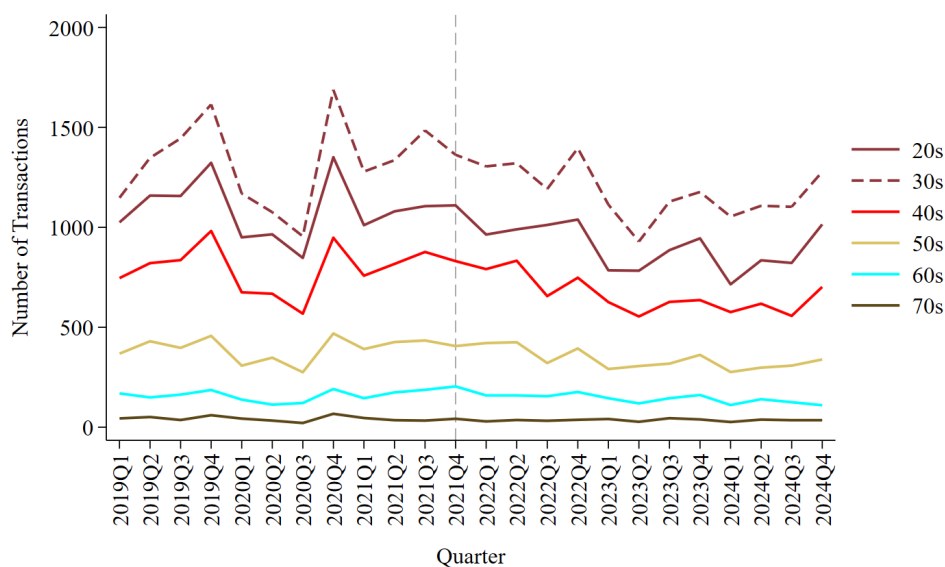


Fig.8: The figure depicts the number of completed real estate transactions by age groups in Wallonia on a quarterly basis between 2019 and 2024. The vertical dashed line indicates the introduction of the registration duty reform in early 2022. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.

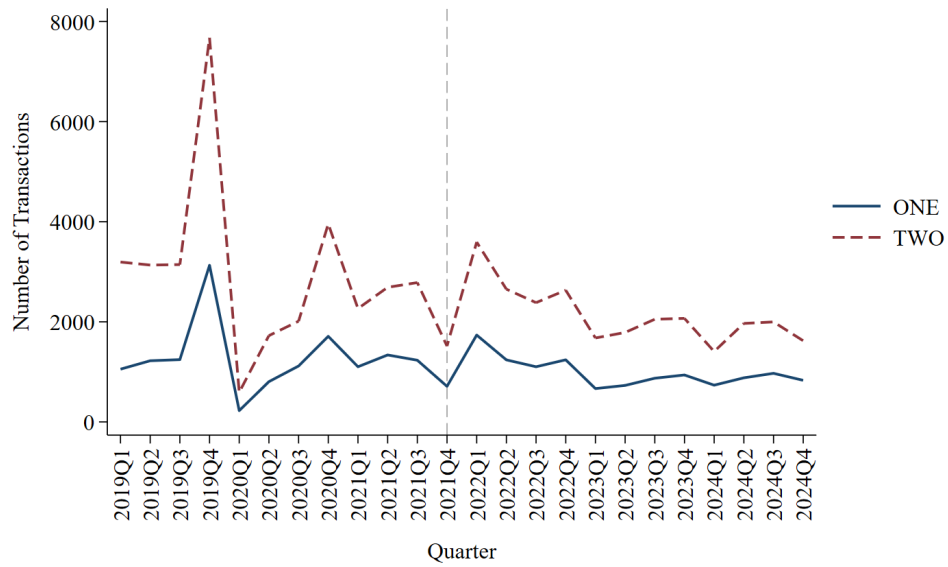


Fig.9: The figure depicts the number of completed real estate transactions by buyer type for age groups 20s and 30s in Flanders on a quarterly basis between 2019 and 2024. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.



Fig.10: The figure depicts the number of completed real estate transactions for buyer type ALONE and age groups 20s and 30s in Flanders on a quarterly basis between 2019 and 2024. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.



Fig.11: The figure depicts the number of completed real estate transactions for buyer type TWO and age groups 20s and 30s in Flanders on a quarterly basis between 2019 and 2024. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.

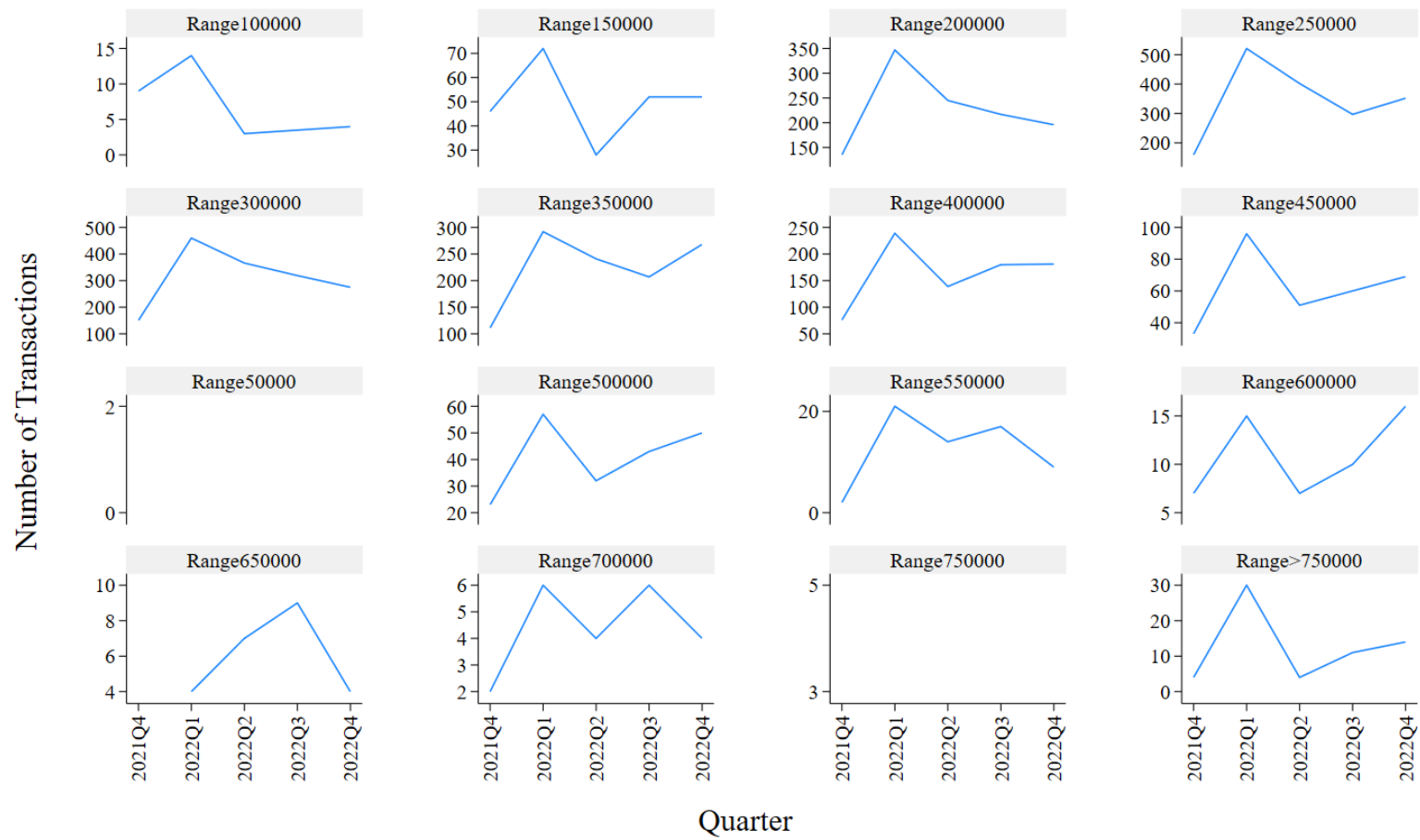


Fig.12: The figure depicts the number of completed real estate transactions by price range for age group 20s in Flanders on a quarterly basis between 2021Q1 and 2022Q4. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.

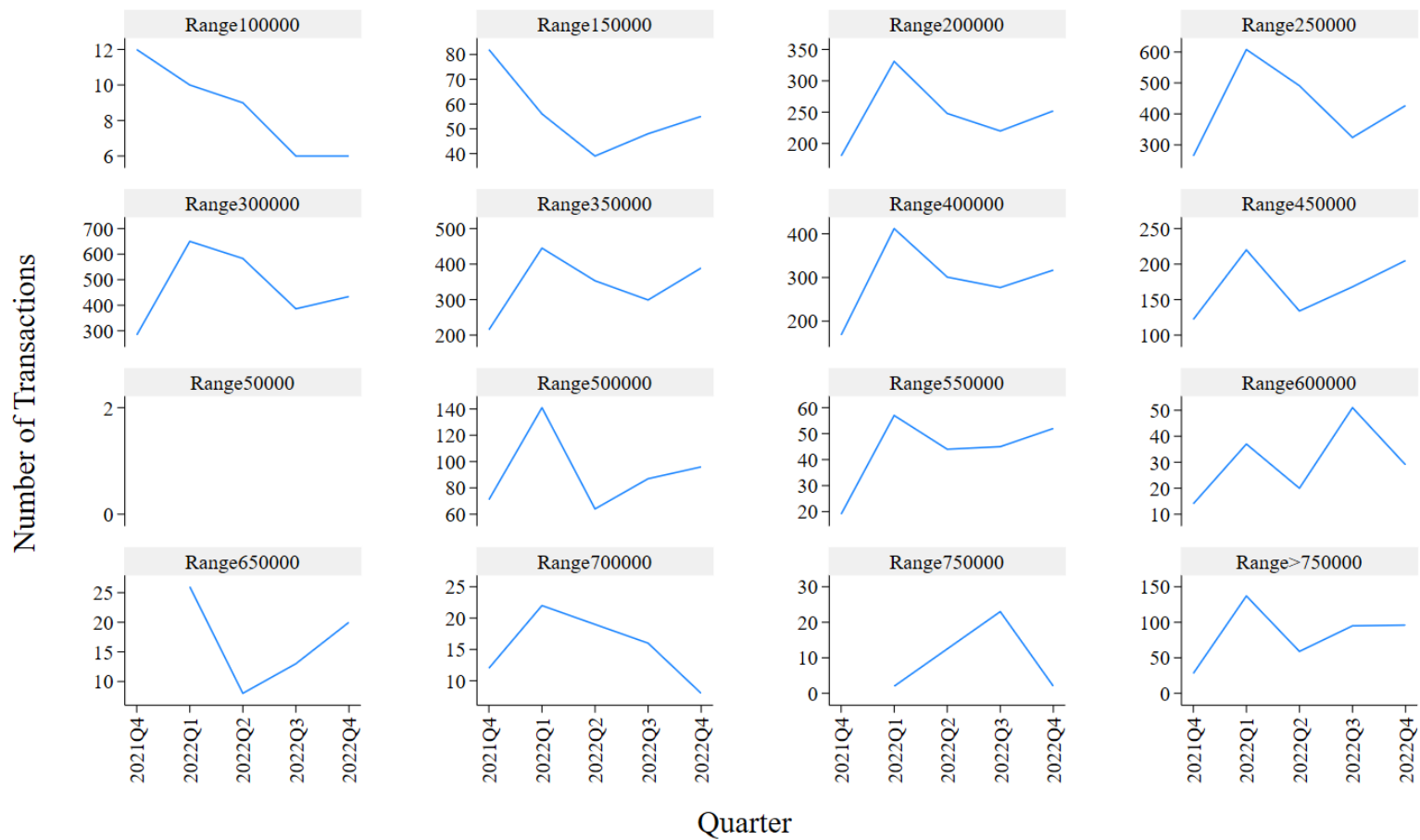


Fig.13: The figure depicts the number of completed real estate transactions by price range for age group 30s in Flanders on a quarterly basis between 2021Q1 and 2022Q4. Source: Authors' calculations based on completed real estate transactions - buyer profile data from Federal Ministry of Finance.