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Exemption or illusion? The impact of a youth tax policy on house asking prices in Portugal

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Abstract

Affordable housing has become an increasing challenge for young individuals in Portugal, where rising house prices and precarious employment conditions hinder homeownership. To address this barrier, the Portuguese Government introduced a tax exemption for individuals under 35 purchasing their first home. However, while the policy aims to improve housing affordability, it may paradoxically drive house prices up by boosting demand and influencing sellers' pricing strategies. This study examines sellers' reactions to the policy announcement using the synthetic difference-in-differences methodology and monthly data on the asking price per square meter growth rate from January 2023 to December 2024, with Spain serving as a natural control. The findings show a significant rise in asking price growth, suggesting sellers priced in the tax exemption. This increase was strongest in Lisbon and Oporto, lower-income areas and where more buyers were eligible, pointing to an uneven market response shaped by local conditions. On average, the tax savings will be offset in 3 to 15 months depending on the housing price range. The unintended consequences of the tax exemption highlight the risks of implementing demand-side measures without corresponding supply-side adjustments.

KEYWORDS
house asking prices, regional effects, synthetic difference-in-differences, tax exemption, web scraping

1 | INTRODUCTION

House prices in Portugal have followed a nonlinear trajectory influenced by historical factors and market conditions (Rodrigues et al., 2022). After a period of low prices from the 1990s to the 2008 financial crisis, the housing market experienced declining investment and falling prices. Since 2014, economic recovery, rising incomes, favorable credit conditions, and limited housing supply have driven a sharp increase in prices, a trend that accelerated even further postpandemic. By 2023, Portugal had the highest price-to-income ratio among OECD countries (154.4 vs. an average of 116.6). However, this price growth has not been uniform across the country, with urban centers like Lisbon and Oporto seeing much steeper increases than rural areas, reflecting regional disparities in demand and housing supply constraints.

Rising house prices create significant barriers, particularly for young people, for whom homeownership feels out of reach due to lower wages and precarious jobs. This societal challenge hampers social mobility, discourages family formation, threatens economic stability, weakens Portugal's ability to retain skilled talent, and exacerbates the effects of demographic ageing.

In response to the housing crisis affecting younger generations, the Portuguese government introduced Law 30-A/2024, granting tax exemptions for first-time homebuyers under 35. Announced in late May 2024, this policy eliminates municipal real estate transaction taxes and stamp duties for first-time homebuyers purchasing a primary and permanent residence, fully exempting properties valued up to € 316,772. For properties priced between € 316,772 and € 633,453, the exemption applies only to the first € 316,772, with standard tax rates applied to the remaining value. This threshold-based structure is intended to support access to housing while deterring market speculation in higher-value segments. However, by lowering the cost of homeownership, the policy may inadvertently fuel housing demand, leading sellers to adjust prices in response to buyers' increased financial capacity. Economic theory suggests that reducing the user cost of housing capital drives up house prices, while empirical studies indicate that higher transaction taxes tend to suppress prices (Benjamin et al., 1993; Fritzsche & Vandrei, 2019; Gárate & Pennington-Cross, 2023). This means that its overall effect depends on how much of the subsidy is capitalized into higher prices. If the subsidy exceeds any resulting price increase, eligible buyers may still benefit, but noneligible buyers (who do not receive the subsidy) are fully exposed to any upward pressure on prices. The reform may therefore improve affordability for some while worsening it for others, raising important concerns about its distributional consequences.

This article investigates the supply-side response of real house prices to a demand-side, youth-targeted tax exemption, making several novel contributions. First, it is the first study to analyze such a policy, broadening the scarce literature on tax exemptions, which has primarily focused on the United States, the United Kingdom, and a few EU countries. Second, it applies the advanced synthetic difference-in-differences (SDiD) method using high-frequency monthly asking price data per square meter from Portugal and Spain, collected via web scraping. Third, it incorporates municipality-level data which allow to capture the spatial heterogeneity in treatment effects, distinguishing locations grounded on metropolitan status, average income, and the local prevalence of eligible young buyers. Lastly, as the first study of its kind, it contributes to a deeper understanding of the tax exemption's unintended consequences.

Using Spain to construct a synthetic control (SC) group, the results show that the tax exemption led to faster growth in real house asking prices per square meter in Portugal, especially in the metropolitan areas of Lisbon and Oporto. The impact also varied across municipalities: price increases were stronger in areas with below-average household incomes and in places with more eligible young buyers. In contrast, no significant effect was found in areas with fewer eligible individuals. These patterns point to a market response shaped by local economic and demographic factors. Robustness checks support the reliability of these findings. Overall, rather than improving access to homeownership, the tax exemption may have made housing less affordable for the very group it aimed to help, highlighting the need for a more targeted housing policy.

This article is organized as follows. Section 2 looks at how tax policies affect house prices according to existing research. Section 3 describes the methodology used in the study. Section 4 presents and discusses the results of the study. In Section 5, the policy implications of the findings are drawn. Finally, Section 6 concludes the article.

2 | LITERATURE REVIEW

Research on tax policies and housing markets has looked at different ways taxes can affect prices and whether people can afford homes. These include property transfer taxes, exemptions, and taxes on foreign buyers. While most of the studies look at the United States, the United Kingdom, and EU, they show that tax policies influence housing transactions. However, there is less information about how policy announcements affect market outcomes.

The impact of taxes on housing markets. Property taxes and related financial policies can have a big effect on housing markets. Many studies show that increasing property taxes usually leads to a drop in home values. For example, Benjamin et al. (1993) saw a drop in property prices in Philadelphia after a tax was increased, while Dachis et al. (2011) reported that Toronto's 1.1% land transfer tax caused a 15% drop in sales and prices. Fritzsche and Vandrei (2019) showed similar effects in Germany, with fewer transactions due to higher taxes. On the other hand, Rosenthal (1999) found that the United Kingdom's 1990 poll tax surprisingly increased housing prices. Gárate and Pennington-Cross (2023) showed that property taxes can make housing prices go down and can also make local amenities better.

The impact of tax reductions and exemptions is mixed. Besley et al. (2014) and Best and Kleven (2018) looked at short-term changes to a UK tax on transactions. They found that it led to a temporary increase in activity and prices, but these increased again when the tax was brought back. Kopczuk and Munroe (2015) showed that a 1% "mansion tax" in New York and New Jersey caused a sharp bunching of home sales just below the $1 million threshold, as sellers reduced prices to avoid the tax, sometimes by more than the tax amount, leading to fewer sales just above the threshold. Slemrod et al. (2017) found similar price manipulation in response to a transfer tax notch in Washington, DC, but unlike Kopczuk and Munroe (2015), did not observe broader reductions in sales volume, suggesting a limited lock-in effect. Elinder and Persson (2017) found that Sweden's tax cap increased prices for high-value homes but had limited broader effects.

Regulations on how land is used can indirectly increase housing costs by reducing the number of houses available. Cheung et al. (2009) and Eicher (2024) showed how these rules can make housing more expensive, especially for people on low incomes (Riley, 2011).

Taxes on capital gains affect how prices change over time and how much people want to buy houses. Aregger et al. (2013) found these taxes have a stronger impact than taxes on transactions, while Cunningham and Engelhardt (2008) showed that reducing capital gains taxes led to more people moving around and to more sales. Recent studies, such as Tian et al. (2024), highlight their role in limiting price increases in markets like Beijing.

Fiscal policies and young homebuyers. First-time buyers often represent the entry point into owner occupation in housing markets. Typically younger, with limited savings and lower incomes (Herbert & Belsky, 2008), their transition into homeownership is shaped by life-cycle factors such as household formation, employment stability, and long-term security goals (Dieleman & Everaers, 1994). In this context, tax exemptions and subsidies—aimed at reducing financial barriers—have gained policy attention.

Empirical evidence increasingly questions the effectiveness of such interventions. A recurring finding is that demand-side incentives often lead to price capitalization, whereby benefits to buyers are absorbed by sellers through higher prices. Bourassa and Yin (2008) and Hembre (2018) find that US tax credits expanded homeownership but also drove up prices, especially in supply-constrained markets.

In the United States, Goodwin and Zumpano (2011), Berger et al. (2020), and Dynan et al. (2013) emphasize that tax credits boosted short-term demand—particularly among younger, lower-income buyers—but did not sustainably improve ownership access. These effects are echoed in other contexts: Wang and Otsuki (2015) document similar patterns in urban China, while Biehl (2018) argues that such policies may primarily benefit sellers in thin markets, where increased demand is not met with sufficient supply.

More recently, Krolage (2022) finds that purchase subsidies in Germany significantly inflated prices in tight housing markets, raising concerns about the overall affordability impact. Collectively, these studies suggest that while tax incentives for first-time buyers can stimulate demand, their success depends on responsive housing supply.

Contribution to the literature. Building on existing literature on the effects of tax policies in the housing market, this study contributes by examining an unexplored context: the impact of a targeted tax exemption for young homebuyers in Portugal. While prior research suggests that transaction taxes tend to lower prices and that tax reductions can stimulate demand, thereby driving prices upward, some studies indicate that such benefits may be absorbed by sellers, diminishing their intended impact on affordability. The empirical analysis of this dynamic remains underdeveloped, especially in the Portuguese context. This study fills that gap by assessing whether the tax exemption was incorporated into asking prices, analyzing regional variations in its effects, and offering new evidence into the effectiveness of youth-focused housing affordability policies.

3 | EMPIRICAL METHODOLOGY

Our strategy to identify the effect of the tax exemption on homeowners' real house asking prices employs a panel of municipalities from Portugal and Spain. The key idea is to compare the evolution of real asking prices in the Portuguese municipalities, which benefited from the tax

TABLE 1 Socioeconomic and financial indicators.

Indicators	Portugal	Spain
– Units of observation: countries		
Gross fixed capital formation (% of GDP)	3.9	6.0
Short-term interest rate (3-month EURIBOR rate)	3.43	3.43
Long-term interest rates (10-year government bond yield traded in the secondary market)	3.24	3.49
Share of rejected loan applications for households [a]	7.5	6.25
Average construction cost index (2021 as base year)[a]	116.53	115.03
Arrivals of nonresidents as % of total arrivals, at tourist accommodation establishments	59	50
Share of people living in households as owners (%)	76	75.3
– Units of observation: municipalities		
Average real gross income *per capita* (€)[b]	11,450	11,634
Rate of total population change (per 1000 persons)	11.7	12.6
Age dependency ratio for young individuals (% working-age population)	20	21
Individuals between 20 and 35 years old (% population older than 20 years old)	19.86	20.20

[a] stands for authors' calculations using quarterly data.
[b] stands for 2022 as the last available period with data.
Source: Eurostat; OECD; European Central Bank; World Bank; *Instituto Nacional de Estatística* (Portugal); and *Instituto Nacional de Estadística* (Spain).

exemption, with the evolution of real asking prices in the Spanish municipalities. The new Portuguese tax policy is described in Appendix A.

3.1 | Data description

Spain as the natural control. To assess the impact of a tax exemption on homeowners' real house asking prices in Portugal, it is crucial to select a control group with an economic, social, demographic, and financial context as similar as possible to Portugal's. In this case, Spain emerges as the natural choice.

Several factors influence housing markets, including economic activity, interest rates, credit conditions, construction costs, and demographic trends. The literature suggests that higher income and GDP *per capita* are associated with increased house prices (Abelson et al., 2005; Andrews, 2010), while lower interest rates typically stimulate demand (Himmelberg et al., 2005). In addition, credit conditions and rising construction costs can play a significant role in driving prices (Adams & Füss, 2010; Duca et al., 2011). Demographic factors, such as population growth and shifts in age structure, also impact demand and contribute to regional price variability (Capozza et al., 2002; Egert & Mihaljek, 2007).

Table 1 presents a comparison of key socioeconomic and financial indicators for Portugal and Spain in 2023, at both national and municipal levels. As expected, Portugal and Spain share substantial characteristics in various indicators relevant to the housing market, such as a common monetary policy (thus equal interest rates), and similar construction costs and levels of homeownership. While Spain and Portugal differ somewhat in administrative structure, both countries are also subject to the same EU-level directives across all regions. These requirements are transposed into national law and applied uniformly to all the territory. In rural regions, administrative capacity and market activity are generally more limited, with implementation practices converg-

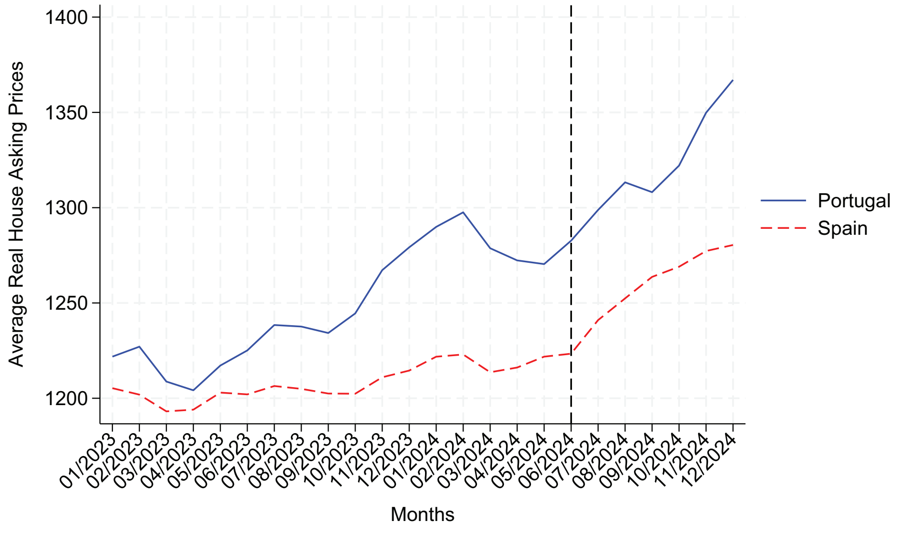

FIGURE 1 Evolution of real house asking prices in Portugal and Spain.

Note: The figure shows municipal averages of *Idealista*'s prices, deflated using the Harmonized Index of Consumer Prices (2015 = 100).

ing further, which reduces any meaningful divergence between both countries. As such, there are no substantial differences in building codes that would compromise the validity of using Spain as a control in our empirical analysis.

Portugal and Spain also exhibit similar behavior in real house asking price trends over time, as shown in Figure 1. During this period, both countries display consistent upward trajectories, which, alongside comparable socioeconomic and financial indicators, further corroborates Spain as Portugal's natural control group. Moreover, Portugal seems to have amplified the series' upward trend following the policy announcement in late May 2024 and further reinforced it after its implementation, in contrast to Spain. Hence, on a national scale, the tax exemption appears to have been translated into an increase in house asking prices.

Choosing the start of the treatment. Two key moments could be considered when determining the start of the treatment period: the announcement and the implementation of the policy. This study focuses on the announcement date, as it captures the immediate impact of the policy and reflects the initial effects on house asking prices. This approach aligns with the literature that views housing markets as highly sensitive to expectations, suggesting that policy announcements can trigger anticipatory behavior among market participants (Kuchler et al., 2023; Vilchez & Kucel, 2022). Buyers and sellers often adjust their decisions based on credible policy signals, which can lead to observable price changes even before the official implementation, as demonstrated by Case and Shiller (2003). Therefore, announcement dates represent crucial moments for market adjustment, with price shifts occurring in response to policy cues well before the new rules are enacted (Hendershott et al., 2020; Vilchez & Kucel, 2022). Given that the tax exemption was announced in late May 2024, our treatment period begins in June 2024.[1]

[1] We extend the analysis by considering the formal implementation of the policy as the beginning of the treatment. The results keep their sign but are generally smaller in magnitude, and in some cases statistical significance is lost, suggesting that defining the announcement as the starting point provides a more accurate representation of the policy's effect. The corresponding evidence and discussion are presented in Appendix B.

Choosing the pre- and posttreatment period of analysis. To ensure robust analysis and minimize bias in estimating effects, we use data ranging from January 2023 to December 2024. Egami and Yamauchi (2022) suggest that a longer pretreatment period enhances the accuracy of treatment effect estimates by minimizing bias and more effectively detecting potential anticipatory responses to the policy, enabling a clearer capture of underlying trends and fluctuations in house prices. Besides, given data availability, the posttreatment period allows us to quickly observe market reactions to the policy, without obscuring the immediate effects with long-term trends. Furthermore, the Spanish government announced 12 specific measures, at the beginning of January 2025, to address similar issues in the housing sector, including taxation policies. As a result, from then onward, Spain will no longer be suitable for use as a natural control.

House prices data. We rely on data for the list price of a square meter in Portugal and Spain, obtained from an online portal for house renting and sales.[2] Particularly, we collect data from *Idealista*,[3] covering solely houses that are announced through this portal.[4] These data have the advantage of being reported with monthly periodicity, to cover Portugal, Spain, and Italy, and to distinguish house asking prices across inner regions, enriching the type of analysis that can be done.

Idealista's calculation of the asking price per square meter involves a rigorous process: active and visible ads are selected, excluding errors, duplicates, and fraudulent listings. Anomalies are eliminated based on 95% confidence intervals within market segments, ensuring that extreme outliers do not skew the data. To address the potential endogeneity of house size, particularly the tendency for larger properties to exhibit lower unit prices, the platform segments listings into fine-grained “microsegments” defined by property type, transaction type, structural characteristics, and geographic location. This segmentation ensures that comparisons are made within internally homogeneous groups, controlling for variation in house size and other structural features. Within each microsegment, the median unit price is calculated to minimize the influence of extreme values, and the final average prices by geographic zone are obtained as weighted averages of these medians. The weight reflects the proportion of ads in each microsegment, ensuring that the final regional price metrics are not biased by compositional changes, such as variations in property sizes or the listing of larger or luxury homes. Prices for larger zones are then calculated iteratively as weighted averages of the subzones, and in cases of insufficient data, weighted moving averages of adjacent periods are used to capture trends and seasonality, ensuring the consistency and representativeness of the final values.

Using web scrapping techniques we were able to collect and process house asking prices data in Spain and Portugal. From a hierarchical base of autonomous communities, provinces,

[2] In theory, we could rely also on the house price index (HPI) from a standard database as Eurostat, OECD, or BIS, but we had to exclude it from our framework. First, the index has quarterly frequency, meaning that there is not enough data after the policy announcement and before January 2025 (period when the control group, Spain, will be influenced by new fiscal policies in the housing market) to capture the potential impact of the tax exemption. Second, the HPI is only available at the country level and more granular data together with our econometric methodology ensures that results are robust. Finally, we want to capture the supply-side reaction to a demand targeted policy and the HPI is based on actual market transactions.

[3] https://www.idealista.pt.

[4] *Idealista* is a reliable source as, besides being the leader marketplace of South Europe, its services are used by private individuals, investment funds, banks, real estate agencies, developers, and appraisers: at each quarter, more than 500,000 properties are listed as well as more than 300 million online searches are made by users. Historical data from the website are available since 2005 with a monthly periodicity, covering more than 5000 zones (from the country level to a census section).

TABLE 2 Descriptive statistics.

Countries	Period	Obs.	Mean	Std. Dev.	Min.	Max.
	Pretreatment	3213	1257.90	764.90	330.48	4671.84
Portugal	Posttreatment	1323	1320.27	793.36	335.05	4677.68
	Entire	4536	1269.01	773.92	330.48	4677.68
	Pretreatment	19,975	1208.09	697.57	280.76	8034.61
Spain	Posttreatment	8225	1258.17	746.26	308.46	6986.69
	Entire	28,200	1222.69	712.47	280.76	8034.61
	Pretreatment	23,188	1213.60	707.40	280.76	8034.61
Portugal and Spain	Posttreatment	9548	1266.77	753.23	308.46	6986.69
	Entire	32,736	1229.11	721.46	280.76	8034.61

Note: The entire period ranges from January 2023 to December 2024; it is divided into the pretreatment period (until May 2024) and the posttreatment period (from June 2024 onward).

and municipalities in the case of Spain and districts and municipalities in the case of Portugal, dynamic URLs were built to access specific information for each location on the online property platform. To convert them into real house asking prices, these were deflated using the harmonized index of consumer prices with a base year in 2015.

Asking prices and the housing market. To validate the use of house asking prices and show its consistency with other price indexes, we constructed a quarterly House Asking Price Index from the *Idealista* data (base year 2015 = 100), and we compare it to the official Eurostat HPI for Portugal in Figure C.2. Throughout the period, the two indicators maintain similar trends, with asking prices above market prices, suggesting a heated housing market, where sellers tend to overprice due to their expectations of fiercer buyer competition and indicating that Portuguese housing sellers are forward-looking. This behavior aligns with the literature that has been indicating the existence of a close relationship between house asking prices and market prices (Han & Strange, 2013) and the fact that a sizable portion of sales are made for less than the asking price (Case & Shiller, 2003).

Descriptive analysis. Given that our econometric methodology requires a balanced data set, we end up with information on the real house asking prices per square meter for 1175 Spanish municipalities and 189 Portuguese municipalities.[5] This means that our control group contains more than six times the number of municipalities in our treatment group, increasing the precision of the estimates and the statistical power of the analysis. In other words, it reduces the impact of random variations and helps detect smaller effects, making the results more reliable and robust. Our final data set contains a total of 32,736 observations, spanning for 24 months and 1364 cross sections. For the entire sample, the real house asking prices have a mean of 1229.11, with a within-group standard deviation of 70.30, and a larger between-group standard deviation of 718.28.

Table 2 shows the descriptive statistics for the real house asking prices in Portugal and Spain, before and after treatment. In Portugal, the average real house asking price rises from 1257.90

[5] The total number of municipalities in Spain and Portugal is 8131 and 308, respectively, according to their national statistics institutes. The discrepancy between the full list of municipalities and the number included in the final data set stems from two main factors. First, *Idealista* does not provide data for municipalities that do not meet their selection criteria, which is more likely for municipalities with fewer transactions and that are not representative of the housing market. Second, the panel needs to be balanced, meaning that municipalities missing data for any period within the study's time frame were excluded from the analysis.

pretreatment to 1320.27 posttreatment (4.96%), with high standard deviations in both periods. In Spain, a positive but smaller variation of the average also exists (4.15%). The difference between the countries' averages suggests that the intervention may have impacted the Portuguese property market.

The disparity in average real prices per square meter between Portuguese municipalities reflects the country's different economic and social dynamics. On one hand, not only the country's mainland coast appears to have higher real asking prices, but also these are more pronounced in the metropolitan areas of Lisbon and Oporto (see Figure C.1). These regions show strong real estate pressure due to their high population density and concentration of activities, where increasing housing demand (young buyers included) faces limited supply. On the other hand, the municipalities in the interior show significantly lower values since they are characterized by lower housing demand and less urban development. The Algarve (south of mainland Portugal) and the Madeira archipelago are two other regions where the real asking price has increased the most. House price inflation in these regions is, however, explained by the strong pressure from tourism.

Our analysis necessarily excludes a set of smaller municipalities with insufficient market data. To assess how this affects the generalizability of our findings, we compare included and excluded municipalities along four key dimensions: average real gross income *per capita*, population growth rate, youth dependency ratio, and the share of individuals aged 20–35. As shown in Table D.1, included municipalities are more demographically and economically dynamic, and more likely to exhibit measurable responses to a demand-side housing policy targeting first-time buyers. As such, our estimated effects may be interpreted as an upper bound of the policy's average impact across all municipalities. At the same time, our focus on active housing markets aligns with the empirical goal of identifying price effects where they are most likely to occur.

We will consider the growth rate of real house asking price per square meter as the interest variable, measured by the logged difference of the real house asking price per square meter between consecutive months (in %). This allows for greater comparability between municipalities, by controlling for structural differences in initial prices and eliminating scale effects. In addition, this metric focuses on relative asking price dynamics, which are more relevant to capture the marginal impact of the policy.

3.2 | Econometric methodology

To evaluate how the supply side of the housing market reacts to the demand-side tax exemption announcement, the standard approach in policy evaluation studies is to use a difference-in-differences (DiD) estimation. However, given the limitations of the standard DiD model in our setting, specifically the lack of strong evidence supporting the parallel trends assumption (presented in Appendix E, together with an event-study analysis), we adopt the novel SDiD method as our main identification strategy.

The approach developed by Arkhangelsky et al. (2021) consists in a combination of the SC and the DiD methodologies. It creates a synthetic version of the treated group by constructing a weighted version of the control group that most closely resemble the pretreatment characteristics of the first one, minimizing the difference between the pretreatment outcomes of the treated unit and the SC. By leveraging the untreated group's postpolicy real house asking price data and adjusting for preexisting trends, the SDiD approach not only replicates what real prices would have looked like in the absence of the treatment in the treated group but also allows for a more accurate estimation of the measure's effect, accounting for any structural differences that might exist between the two countries and improving the reliability of the conclusions drawn from the analysis. The joint methodology also overcomes the need for the treated group to fit perfectly within the control units' convex hull.

We consider the SDiD implementation for Stata by Clarke et al. (2024). Our attention is devoted to the case of a single adoption period for treated units (block treatment assignment), which can be described as follows. Given a balanced panel of N groups and T time periods, there is an outcome variable, P_{it}, the measure for house asking prices, observed for each pair $\{i, t\}$ and a treatment variable $W_{it} = 1$ if i receives a treatment in t, and $W_{it} = 0$ otherwise (untreated). If a unit is treated, it remains exposed to the treatment from that moment onward. The method requires at least two pretreatment periods to be considered to determine suitable controls. The average treatment on the treated, ATT, is estimated through a two-way fixed effect regression as:

$$(\hat{\tau}^{\text{sdid}}, \hat{\mu}, \hat{\alpha}, \hat{\gamma}) = \arg\min_{\tau,\mu,\alpha,\gamma} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} (P_{it} - \mu - \alpha_i - \gamma_t - W_{it}\tau)^2 \hat{\omega}_i^{\text{sdid}} \hat{\lambda}_t^{\text{sdid}} \right\}, \tag{1}$$

where γ_t and α_i are time fixed effects and unit fixed effects, respectively.

Unit fixed effects imply that the pretreatment trends match that of treated and control groups, and not that both pretreatment trends and levels will be matched. This allows for a systematic difference between the two groups. To avoid multicollinearity, one γ_t and one α_i are set to zero, as it is often done in fully saturated fixed-effect models. $\hat{\omega}_i^{sdid}$ and $\hat{\lambda}_t^{sdid}$ are optimally chosen unit and time weights, respectively. The first guarantees that both groups approximately followed parallel trends before the tax exemption was implemented. The second ensures that pretreatment periods that are more similar to posttreatment periods are given more weight, identifying a fixed difference between each control unit's posttreatment average and the pretreatment weighted averages across the chosen control units. The estimator's variance is computed using a bootstrap approach.[6]

4 | EMPIRICAL RESULTS AND DISCUSSION

This section evaluates the impact of the tax exemption on real house asking prices in Portugal, using Spain as a natural control group. We begin by calculating the nationwide effect of the housing policy across all 189 Portuguese municipalities, which are pooled into the treatment group and compared to the counterfactual based on the 1175 Spanish municipalities. Results for the main sample are presented in Figure 2, indicating a positive and significant effect of 0.26% on the monthly growth rate of real house asking prices as a result of the tax exemption (p-value below 5%), reflecting sellers' anticipation of increased demand.

The evidence also reveals a nonlinear pattern, namely, an initial spike in asking prices following the policy announcement (late May), a temporary inversion around the policy's implementation (August to mid-October), and a sustained increase in treated group prices thereafter. Anticipative behaviors, speculation, information frictions, and inelastic supply may have combined to cause the complex short-run dynamics that Figure 2 describes. The initial price increase indicates that the announcement effect was strong, so that sellers have adjusted housing prices in

[6] The SDiD does not require stationarity. Arkhangelsky et al. (2021) argue that stationarity is not needed for their approach, as the data can be deterministic with nonstationary errors, and the unexposed units are only used for efficiency rather than identification.

FIGURE 2 Results for the full sample.

Note: The figure shows outcome trends for treated (Portuguese) and synthetic control (Spanish) municipalities, with time weights (lambda). The dashed line marks the policy announcement.

anticipation of increased demand once the policy became active. This result has been underlined by Palmon and Smith (1998) while studying properties located in the suburbs of Houston, Texas, concluding that only unexpected tax changes can be passed from homeowners to new buyers of residential properties.

The inversion that followed, where treated prices fell below the control group, indicates a short-term market friction or correction, as buyers delayed transactions to benefit from the exemption. The postponement of purchases may have caused a temporary dip in market activity after the policy was enacted. Also, if sellers have initially overreacted with aggressive price hikes, buyers may have had to exit the market temporarily, forcing a readjustment of prices downward. Moreover, lower-priced homes that qualify for the exemption may have temporarily experienced excess demand and quick turnover, while more expensive properties stagnated—depressing the average asking price momentarily.

The recovery in October suggests that at that time the market must have fully digested the policy and demand materialized in the treated segment. Given that the supply of eligible properties is inelastic, the renewed and concentrated demand could have driven price growth, this time backed by real transaction data rather than expectations.

Tax relief and market speed. While the monthly average growth rate of asking prices may seem modest, its cumulative impact is substantial: a 0.26% monthly increase compounds to 3.17% over 12 months. At this pace, the monetary value of the tax exemption is quickly absorbed into rising house prices. Table A.1 presents estimated total savings for eligible buyers based on the midpoint of each property value range defined by the tax authorities. In the lowest price bracket, price growth erodes the benefit entirely within just 3 to 4 months. Even in higher segments, the exemption's value is typically exhausted within a year. These findings highlight that, in fast-appreciating or supply-constrained markets, tax relief provides only temporary affordability gains before being fully capitalized into prices—ultimately limiting its effectiveness, particularly for the most financially constrained buyers.

Possible reversed filtering and market spillovers. The structure of available data impedes us from analyzing the impact of the tax deduction along the price distribution, namely, considering the house price thresholds defined for eligible transactions. Nevertheless, our results point to the presence of spillover effects across the price distribution. Specifically, the policy might have induced a reverse filtering effect in which housing becomes less affordable or shifts to higher-income occupants over time, contrary to the typical filtering pattern. The tax exemption might have caused an increase in demand in the segment of entry-level homes or midmarket, leading to price appreciation, especially combined with inelastic supply. These homes become less accessible to lower-income buyers, crowding them out, so that the same homes now require higher incomes to access. This is in line with Spader (2024), who concluded that in tighter or high-demand markets, the policy can lead to reversed filtering, blocking the downward income transition that was intended, showing that local market conditions matter. In tandem, the policy may have acted as a market-wide price anchor where sellers adjust prices upward to match the increased perceived value of similar lower-tier properties, taking advantage of housing heterogeneity and converting it into hedonic prices. In this case, targeted exemptions, such as the one adopted in the Portuguese housing market, can create broad market distortions that even affect equilibrium conditions.

4.1 | Spatial heterogeneity in treatment effects

Research has consistently shown that there is no universally effective housing policy since its outcomes vary across regions and depend heavily on local market conditions (O'Sullivan, 2019). To explore whether the effects of the youth-targeted tax exemption vary across different contexts, we now turn to a subsample analysis based on three dimensions of spatial heterogeneity: metropolitan status, average income, and the local prevalence of eligible young buyers. Descriptive statistics for these subsamples are provided in Tables F.1, F.2, F.3, F.4, F.5 and F.6.

Metropolitan status. Oikarinen and Engblom (2015) point that the elasticity of housing supply differs considerably across space. In metropolitan areas, where supply is often inelastic, demand-side incentives such as tax benefits are more likely to be capitalized into higher prices, rather than increasing access to homeownership. In contrast, nonmetropolitan areas tend to have more elastic supply, allowing new construction to respond to demand pressures and thus mitigating price effects.

Empirical works support this view. Evidence in Glaeser et al. (2005) shows that urban areas typically respond more rapidly and intensely to policy changes due to supply constraints and regulatory barriers that limit new construction. In Claessens and Schanz (2019), greater price volatility in urban areas is linked to speculative behavior and limited land availability, while Fan et al. (2019) show that metropolitan housing markets are more sensitive to macroeconomic conditions. Oikarinen et al. (2018) emphasize the cyclical nature of urban markets, and other studies further highlight the role of local factors in driving spatial heterogeneity (Nicodemo & Raya, 2012).

Lourenço et al. (2024) find that in Portugal, recent price increases are mainly demand-driven and reflect weak supply responsiveness, indicative of an inelastic market. In Spain, by contrast, supply has responded more flexibly, resulting in more moderate price growth. Although their study does not differentiate between regions, it is likely that these national patterns are even more pronounced in metropolitan contexts. Cunha and Lobão (2022) also analyze Iberian metropolitan statistical areas and show that the coastal ones present inelastic housing supply, while inner regions tend to exhibit more elastic responses. These results suggest that the housing market dynamics between spatially analogous areas across the Iberian Peninsula are suf-

FIGURE 3 Results for metropolitan areas.

Note: The figure shows outcome trends for treated (Portuguese) and synthetic control (Spanish) municipalities, with time weights (lambda). The dashed line marks the policy announcement.

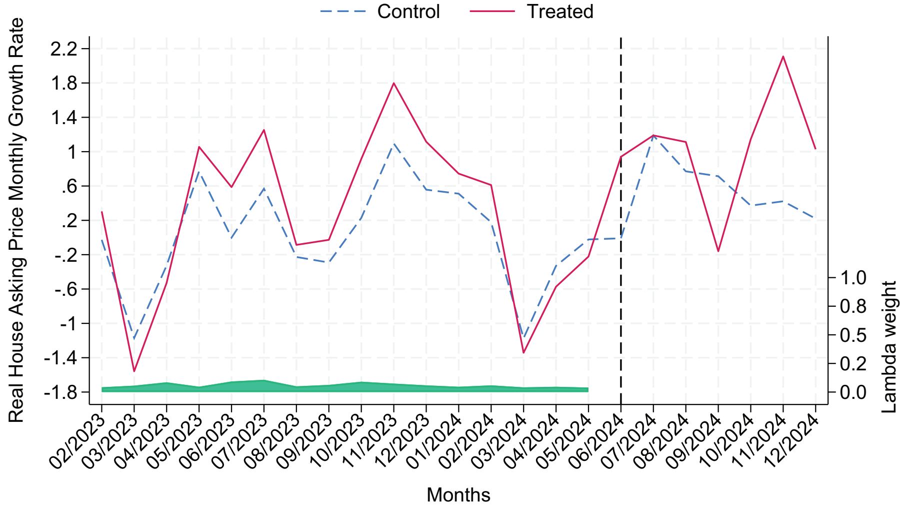

FIGURE 4 Results for nonmetropolitan areas.

Note: The figure shows outcome trends for treated (Portuguese) and synthetic control (Spanish) municipalities, with time weights (lambda). The dashed line marks the policy announcement.

ficiently aligned. Therefore, although we lack estimates of housing supply elasticity for urban and rural regions within Spain and Portugal, existing evidence supports the comparability of the two countries, with Spanish metropolitan and nonmetropolitan municipalities offering a credible counterfactual framework.

Results for metropolitan and nonmetropolitan areas are presented in Figures 3 and 4, respectively.

Again, as presumed, there is a positive and significant effect of the announcement on the growth rate of real house asking prices in both subsamples. Particularly, the estimation results for metropolitan areas indicate a statistically significant average treatment effect of 0.34%, higher than the one verified for the entire sample. In contrast, the municipalities in nonmetropolitan areas appear to suffer a lower impact of the policy announcement (0.23%) when compared to the full sample (0.26%). Both are statistically significant at the 5% level. Therefore, compared to their counterfactual, homeowners raised their asking prices more significantly in the municipalities that belong to the Portuguese metropolitan areas, reflecting the supply adjustment and validating the increased pressure on the housing market in Portugal's major urban centers. This pattern therefore aligns with evidence from other housing markets.

The similarity between metropolitan and nonmetropolitan areas results can be explained by factors such as housing market interdependencies, including spillover effects, spatial arbitrage by buyers, information diffusion, and a policy that does not distinguish metropolitan from nonmetropolitan markets. According to the ripple effect hypothesis (Cook, 2003), house price changes that occur in central or high-demand areas (often large urban centers) tend to spread outward over time to surrounding or peripheral areas. When demand increases or prices rise in metropolitan areas, spillovers can emerge from displacement of demand if eligible buyers are priced out of metropolitan areas and redirect their search to adjacent areas, putting price pressure in nearby nonmetropolitan municipalities. Sellers and agents in nonmetropolitan areas adjust their asking prices accordingly. Buyers faced with the widening of price differentials may engage in spatial arbitrage, opting for nonmetropolitan areas. This mobility pressure contributes to equalizing prices across regions. Thus, metropolitan housing markets tend to act as anchors in the regional housing hierarchy. On top of this, the tax exemption may be perceived as a national housing subsidy, leading to generalized expectations of rising prices across the territory.

It is also important to notice that the inflexion in asking prices at the time of the policy implementation is more pronounced in metropolitan municipalities than in nonmetropolitan ones, even if our results confirm that it is in the metropolitan areas that the policy most increases prices. This reflects a sharper, faster behavioral adjustment to the timing of the policy's implementation in urban markets indicating how they tend to be at once more responsive to information, and more sensitive to demand timing and liquidity dynamics. Market expectations adjust quickly given that supply is relatively inelastic, and eligible buyers are more concentrated. As a result, despite the initial pronounced dip, house prices in metropolitan areas end up increasing more over the full period.

Average income. Recognizing that the impact of financial incentives may vary with local economic context, we next assess whether the tax exemption had differential effects in municipalities with above- versus below-average real gross real income *per capita*, which is the variable available for both countries' municipalities, as presented in Table 1. Results for municipalities above and below-average income are presented in Figures 5 and 6, respectively. The estimated average treatment effect is 0.23% in municipalities with above-average income and 0.29% in those below the national average, statistically significant at the 10% and 5% levels, respectively.

The finding that the tax exemption for first-time homebuyers led to larger increases in asking prices in municipalities with below-average income *per capita* (also in comparison with the full sample) suggests a heterogeneous market response tied to local economic conditions. Although initially counterintuitive—given that higher-income areas often exhibit greater housing turnover and market responsiveness—the result aligns with a set of reinforcing mechanisms. In lower-income municipalities, the fiscal exemption represents a larger proportion of both income and transaction costs, amplifying its salience and making it more likely to trigger marginal buyers' entry into the market (Fuster & Zafar, 2021). This is particularly relevant for credit-constrained

FIGURE 5 Results for municipalities with above-average income.

Note: The figure shows outcome trends for treated (Portuguese) and synthetic control (Spanish) municipalities, with time weights (lambda). The dashed line marks the policy announcement.

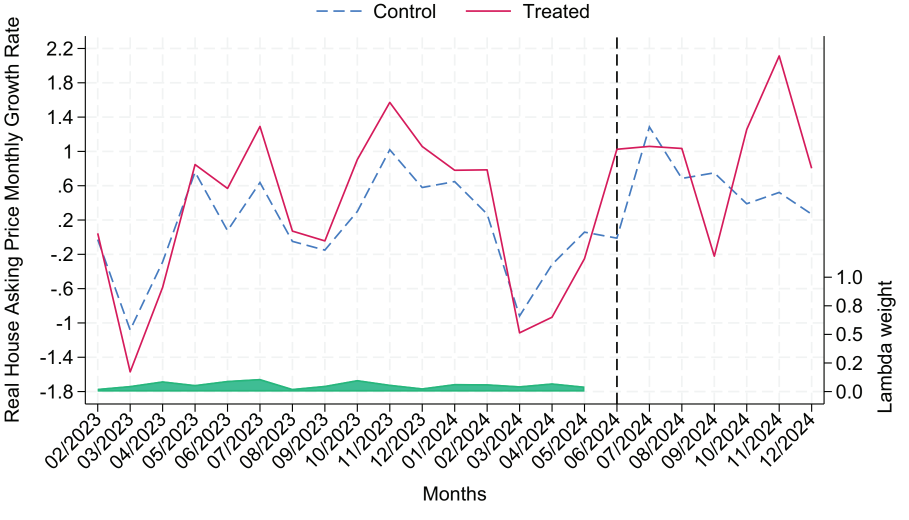

FIGURE 6 Results for municipalities with below-average income.

Note: The figure shows outcome trends for treated (Portuguese) and synthetic control (Spanish) municipalities, with time weights (lambda). The dashed line marks the policy announcement.

or liquidity-limited young households, for whom even fixed nominal subsidies have greater marginal utility.

Moreover, in thin or illiquid markets, small increases in perceived demand can lead to disproportionate changes in pricing behavior. Sellers in these contexts often set asking prices based not on realized transactions, but on expectations shaped by signals such as policy changes, buyer inquiries, or word-of-mouth about heightened interest. This anticipatory behavior, particularly under conditions of imperfect information, can contribute to overadjustment in prices by sellers.

FIGURE 7 Results for municipalities above-average share of eligible young individuals.

Note: The figure shows outcome trends for treated (Portuguese) and synthetic control (Spanish) municipalities, with time weights (lambda). The dashed line marks the policy announcement.

This effect is further amplified in municipalities with a higher concentration of young residents, where demand pressure from similarly targeted buyers is more direct, and competition more visible.

Given the often limited short-run elasticity of housing supply in lower-income regions (Hilber & Turner, 2014), especially where new construction is slow or constrained, such shifts in effective demand are more likely to be capitalized into prices rather than absorbed through increases in housing stock. This mirrors the findings in the work of Best and Kleven (2018), showing that reductions in transaction taxes are frequently priced in, particularly in lower-priced market segments.

Together, these dynamics illustrate how buyer-side tax incentives, while expanding access to credit-constrained populations, can also unintentionally fuel localized price pressures in more economically vulnerable areas, raising important concerns about the distributional and affordability implications of such measures.

Local prevalence of eligible young buyers. The policy's effects may also depend on local demographic composition, particularly the concentration of young adults who are most likely to benefit. Areas with a higher share of eligible individuals may experience stronger price or market effects due to greater uptake of the exemption. To assess whether the policy's impact varies with the local presence of young adults, we analyze municipal-level demographic data for Portugal and Spain. We focus on individuals aged 20–34, who are most likely to benefit from the tax exemption. Although the policy applies up to age 35, we exclude those aged 18–19 because the data are grouped in 5-year intervals, and including the 15–19 group would capture many minors unlikely to participate in the housing market. To better reflect potential demand, we calculate the share of 20–34-year olds relative to the adult population (more than 20 years old), avoiding distortion from younger, nonrelevant age groups. Results for municipalities with above- and below-average shares are shown in Figures 7 and 8.

The results reveal significant heterogeneity in the policy's impact across municipalities. In areas with an above-average share of eligible individuals, the estimated ATT is 0.34%, statistically sig-

FIGURE 8 Results for municipalities below-average share of eligible young individuals.

Note: The figure shows outcome trends for treated (Portuguese) and synthetic control (Spanish) municipalities, with time weights (lambda). The dashed line marks the policy announcement.

nificant at the 1% level, a change greater than the one verified in the full sample. In contrast, municipalities with a below-average share of eligible individuals show a no significant effect, indicating that the price response is concentrated in areas where the policy has greater relevance. As expected, sellers adjusted their asking prices more in municipalities where a larger share of potential buyers stood to benefit from the exemption, consistent with a supply-side reaction to increased potential demand pressure.

This should reflect also the results for the metropolitan and nonmetropolitan areas, being in line with the evidence provided by the Eurostat. Specifically, the distribution of older adults varies across urban, intermediate, and rural areas in the EU-27: the data used (2020) indicate that older individuals are disproportionately concentrated in predominantly rural and intermediate regions, while they are underrepresented in urban areas, especially when compared to younger age groups.

4.2 | Robustness analysis

We conduct three robustness exercises to assess the credibility of the estimated treatment effects. Results are presented in Appendix G.

First, we implement a role-reversal falsification test by assigning a fictitious treatment to Spain, treating it as if it had implemented the Portuguese tax exemption policy. This exercise tests whether the observed effects in Portugal could be attributed to broader macroeconomic shocks or regional housing trends common to both countries. If that were the case, we would expect to observe statistically significant effects in Spain, albeit in the opposite direction. This approach parallels the spatial falsification logic of Dube et al. (2010), but applies it across national boundaries, leveraging institutional and policy discontinuities. The results support the validity of the identification strategy: significant placebo effects emerge only in the full sample, nonmetropolitan areas, and among below-average income municipalities—groups more exposed to macroregional volatility. In contrast, effects are statistically insignificant in metropolitan areas and among the youth-based and high-income subsamples, where local policy mechanisms and more stable housing markets likely dominate. These patterns suggest that the true treatment effects identified in Portugal are not confounded by regional shocks or shared dynamics with Spain.

Second, we test the sensitivity of our results to the choice of pretreatment window. While the baseline specification uses one full year of data to estimate counterfactual trends, we reestimate the effects using shorter pretreatment periods starting in February, March, and April 2023. This approach follows the logic in Autor (2003), who proposes temporal variation as a means of testing for robustness and reducing exposure to short-term noise. The results are consistent across the entire sample and all subsamples: policy effects remain statistically significant in metropolitan areas and in all demographic subgroups, including municipalities with above- and below-average youth and income. This temporal stability suggests that the results are not driven by arbitrary design choices and reinforces the reliability of the estimated effects. Only the nonmetropolitan sample exhibits a modest decline in precision, possibly consistent with greater heterogeneity in smaller or more rural housing markets.

Third, we explore the possibility that market participants may have anticipated the policy. Drawing on Abadie et al. (2014), who advocates for anticipatory tests in SC designs, we simulate early treatment dates in April and May 2024—1 to 2 months before the actual rollout. April 2024 is particularly salient given the change in government, a plausible moment for policy expectations to emerge. In both placebo anticipation scenarios, treatment effects vanish across all samples and subsamples. This includes metropolitan and nonmetropolitan areas, as well as municipalities grouped by youth share and income level. The consistent absence of significant results confirms that the observed effects are not driven by pretreatment signaling or anticipatory behavior, reinforcing the temporal validity of the research design.

Taken together, these robustness checks provide compelling evidence that the main results are not driven by unobserved common shocks, sensitivity to temporal specification, or behavioral anticipation. They collectively reinforce the internal validity of the identification strategy, especially in metropolitan areas and among key demographic subgroups where policy relevance is highest.

5 | POLICY IMPLICATIONS

Demand-side policies must be complemented by robust supply-side interventions, especially in high-demand urban areas, to avoid unintended price surges and persistent shortages. As Glaeser et al. (2008) emphasize, in markets with constrained supply, demand shocks tend to inflate prices rather than stimulate new construction. As also pointed by Oikarinen and Engblom (2015), given that supply elasticity differs markedly across regions, policy responses must be tailored to local housing dynamics. Therefore, from a policy standpoint, our study outlines several key recommendations to improve housing affordability.

A well-balanced strategy should combine short-term actions—such as curbing speculative activity and protecting vulnerable groups—with long-term structural reforms that promote regional development and alleviate pressure on urban centers. In the short run, transparent and comprehensive price-monitoring systems are essential to detect and deter speculative behavior. Over the longer term, expanding supply should be prioritized through incentives for private-sector development, streamlined zoning and permitting procedures, and targeted support for construction in less-saturated regions.

Besides, tax exemptions with eligibility thresholds can create incentives for sellers to strategically set asking prices just below the cap to attract eligible demand. This bunching behavior could distort price signals and undermine the intended targeting of the policy. While our data lack the listing-level granularity needed to detect such effects directly, the possibility of strategic pricing remains a relevant concern for policy design. Nonetheless, several factors may limit the extent of bunching in the Portuguese housing market. Appraisal-based valuations used in mortgage processes, credit constraints faced by young buyers, and a generally heated market environment reduce sellers' incentives to adjust prices. In practice, sellers may opt to target higher-income buyers rather than reprice to meet eligibility criteria, especially given the narrow scope of the exemption. These dynamics suggest that policymakers should carefully calibrate price caps and consider complementary measures to ensure that benefits effectively reach the intended population without encouraging market distortions.

Finally, the market's anticipatory response to policy announcements must be considered. Speculative activity and price adjustments can occur even before a policy takes effect, as observed in other contexts (Janssen et al., 2018). Thus, both the timing and communication of policy initiatives are crucial for managing expectations and ensuring the effectiveness of interventions.

While our analysis suggests that the policy may present challenges to affordability, we emphasize that this conclusion is contingent upon the relationship between price increases and transaction prices. Future research will need to conduct a comprehensive cost/benefit analysis to fully assess the net effects of the policy, comparing the expected benefits for eligible buyers to the increase in prices.

6 | CONCLUSION

Can a tax exemption for young people make housing more affordable? This study employs recent panel econometric techniques to examine how Portuguese house sellers adjust their asking prices in response to a tax exemption for young first-time homebuyers. Our empirical analysis indicates that, contrary to its intended objective, the policy measure (announced in late May 2024) led to a significant increase in real house asking prices. Rather than improving affordability, the exemption appears to have fuelled speculative behavior among sellers, ultimately disadvantaging not only young buyers but also those ineligible for the exemption, who must now contend with inflated prices.

The announcement of the tax exemption resulted in a 0.26% increase in the monthly growth rate of real house asking prices across Portuguese municipalities. The impact was more pronounced in municipalities in metropolitan areas, with below-average household incomes, and higher share of eligible first-time buyers. This translates to an annualized growth rate of 3.17%, meaning that any financial relief provided by the exemption is effectively recouped by sellers within less than 15 months. These results remain robust across multiple robustness checks. This suggests that a policy aimed at alleviating demand-side pressures, by reducing the "user cost of housing capital," without accounting for supply constraints, may have unintended and counterproductive effects.

Our findings are in line with recent studies that have shown that levying taxes or increasing taxes on real estate transfers helps control home prices; therefore, their removal may have a positive impact. Portuguese real estate sellers, who operate in a housing market with limited supply, seem to be aware of this possibility and have attempted to reverse the tax benefit to themselves by increasing the price they are asking per square meter. A lower supply elasticity in metropolitan areas could have amplified this effect, as demand pressures in these geographies are typically higher. In particular, the limited availability of housing in major urban centers, such as Lisbon and Oporto, creates a scenario where sellers are more responsive to even slight policy changes.

To effectively address housing affordability, demand-side policies must be matched with strong supply-side measures, especially in urban areas where housing supply is most constrained. A one-size-fits-all approach is unlikely to succeed, as regional differences in supply responsiveness require locally tailored interventions. In areas with limited supply, targeted actions, such as tighter price monitoring and incentives for new construction, are essential to prevent demand-side measures from driving up prices. Broader reforms should focus on expanding housing supply through private-sector incentives, faster permitting, and development in less-saturated regions. In addition, policymakers must consider how markets react to policy announcements, as anticipation alone can fuel speculative behavior and price increases, affecting the policy's intended outcomes.

Our study has certain limitations. First, the short posttreatment window limits our ability to observe longer-term supply responses, particularly new construction, which is often delayed by planning and regulatory processes. While our results indicate that sellers quickly adjusted prices, mainly for existing housing, this likely reflects anticipatory behavior in response to increased buyer demand. Extending the analysis period is not feasible, as a similar housing policy was announced in Spain in January 2025, compromising the validity of our control group and introducing potential bias.

Second, the data set includes only municipalities in Portugal and Spain with sufficient observations, meaning some areas are excluded due to data constraints. Nonetheless, the selected municipalities cover a broad range of regions, including those that continue to attract young populations, ensuring a representative sample for both treated and counterfactual units.

Third, the data set captures asking prices rather than actual selling prices. Yet, in addition to *Idealista* validating property listings, most asking prices are set with guidance from real estate agencies that actively monitor market conditions, making them a reliable proxy for market trends.

Lastly, we analyze an overall HPI rather than focusing exclusively on affordable housing, which limits our ability to isolate the policy's impact on properties directly eligible for the tax exemption. This limitation is compounded by the absence of transaction-level price data, which prevents us from identifying or excluding above-threshold properties and from constructing a credible comparison group of units just above the eligibility cutoff. Nevertheless, given that house prices reflect hedonic pricing dynamics, an increase in the asking price of lower-priced properties tends to drive up prices across all market segments. This suggests that the tax exemption's effects extend beyond the targeted properties, influencing overall housing affordability.

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APPENDIX A: THE TAX EXEMPTION

The Portuguese Government's tax exemption policy applies to two taxes: the municipal tax on real estate transactions (IMT) and the stamp tax (IS). The IMT is a tax on property transfers, whether the property is new or preowned. Although it is formally a "municipal" tax, it is defined and regulated at the national level, with a uniform rate structure across all municipalities in mainland Portugal. The only exceptions are the Azores and Madeira, for which the lower limit subject to the tax is higher. The IS is a tax on various contracts, documents, and legal situations, as specified in the General Stamp Tax Table. Both taxes are paid only by the buyers. The exemption discussed in this article is available to people under 35 years of age, no matter what their nationality is. It only applies to buildings that have already been constructed. To qualify, the person must not be a dependent for IRS purposes at the time of the transaction and must not have owned property in the past 3 years.

The exemption fully applies to properties valued up to € 316,772, if the property is the buyer's primary and permanent residence for at least 6 years. For properties valued between € 316,772 and € 633,453, the exemption applies only to the first € 316,772, with taxes due on the remaining value. No exemption is given for properties above € 633,453. In joint purchases, where one partner is over 35, the exemption applies to half the taxes due. There are no income limits to qualify for this tax exemption. IMT taxes to be paid according to the property value to general homebuyers and to young eligible buyers are presented in Table A.1. As an example, we also compute the total tax savings for first-time homebuyers considering the middle point in each property value range.

TABLE A.1 Tax rates and example of savings for eligible buyers (2023).

Ranges for property values (€)	Tax rates (%) > 35 years old	Tax rates (%) ≤ 35 years old	Total tax savings (€) Ranges midpoint
Up to 101,917[a]	0.0	0.0	407.67
101,917[a]–139,412	2.0	0.0	1340.27
139,412–190,086	5.0	0.0	3334.74
190,086–316,772	7.0	0.0	9745.04
316,772–633,453	8.0	8.0	12,151.62
633,453–1,102,920	6.0	6.0	0
Over 1,102.920	7.5	7.5	0

Note: Total savings include both IMT and Stamp Duty (flat rate of 0.8%) and are calculated at the midpoint of each property value range.

[a] This amount corresponds to € 127,396 in the Autonomous Regions of Madeira and the Azores.

APPENDIX B: ANNOUNCEMENT VERSUS IMPLEMENTATION

In the baseline specification, the treatment begins in June 2024, right after the tax exemption announcement (late May). This choice is based on the idea that housing market agents react in advance to information, adjusting prices and sales decisions as soon as new policies are made public. Alternatively, we now reestimate the model considering August 2024, the month of the law's formal implementation, as the start of the treatment. This redefinition allows us to determine any changes in the results due to the moment considered as the start of the treatment: by contrasting these two definitions we evaluate their relative importance in shaping expectations and driving price dynamics. Table B.1 presents the results.

A comparison between the two definitions of the treatment start date reveals a systematic reduction in magnitude across all subsamples, alongside some variation in statistical significance. In

TABLE B.1 Announcement versus implementation results.

Sample	Announcement (Benchmark)	Implementation
Full sample	**0.26 ** (0.10)**	0.24 ** (0.10)
Metropolitan areas	**0.34 ** (0.14)**	0.28 (0.20)
Nonmetropolitan areas	**0.23 ** (0.11)**	0.22 * (0.13)
Above-average income	**0.23 * (0.13)**	0.21 (0.14)
Below-average income	**0.29 ** (0.14)**	0.28 (0.17)
Above-average young	**0.34 *** (0.10)**	0.32 *** (0.10)
Below-average young	**0.13 (0.12)**	0.10 (0.14)

Note: The growth rate of real house asking price per square meter is the variable of interest. The estimated coefficients are presented with robust bootstrap standard errors (with 50 repetitions) in parentheses.
*, **, and *** stand for statistical significance at 10%, 5% and 1%, respectively.

several cases, the effects remain positive and significant, indicating that the main findings are robust to the temporal redefinition and that the policy continued to exert an influence after its formal entry into force. In other cases, however, a loss of statistical significance is observed when the treatment is anchored in August, suggesting that the price adjustment occurred primarily between the announcement and the implementation of the measure.

For the full sample, the effect remains positive and significant, though slightly smaller, indicating that the market began to react immediately after the announcement. In metropolitan areas and among higher-income groups, significance vanishes under the alternative definition, consistent with a largely anticipated reaction to the policy announcement. By contrast, in nonmetropolitan areas and lower-income segments, the effects remain stable, reflecting a more gradual diffusion of information. In areas with a larger share of young population, the estimates remain strong and significant under both definitions, suggesting a higher sensitivity of this group to the policy and the expectations it generated.

The attenuation of effects when the treatment is defined from August can be explained by the shift in the reference period implied by this alternative specification. By moving the start of the treatment forward, the months immediately following the announcement become part of the pre-treatment window. Consequently, part of the true impact is absorbed by the baseline, leading to smaller or statistically insignificant estimates.

These results indicate that, while the formal implementation of the policy may still have exerted a positive influence on prices, the announcement played the decisive role as an informational event, triggering most of the initial market adjustment. Defining the treatment only from the implementation date would thus omit an essential part of the anticipatory response and lead to a partial underestimation of the policy's overall impact. The findings are even more meaningful considering that, when the treatment is artificially brought forward to months preceding the announcement (see Appendix G), the estimated effect completely disappears. This reinforces the evidence that the announcement marks the true onset of the policy's impact.

Another important aspect is that these results are consistent with the nature of the data. Since the analysis relies on asking prices, which reflect sellers' expectations and tend to adjust almost immediately to new information, it is natural that the market reaction was more pronounced at the time of the policy announcement, rather than upon its formal implementation.

In summary, defining June 2024 as the start of the treatment allows us to capture the genuine informational shock that triggered the adjustment in prices and provides a more accurate assessment of the measure's full effect.

APPENDIX C: ADDITIONAL FIGURES

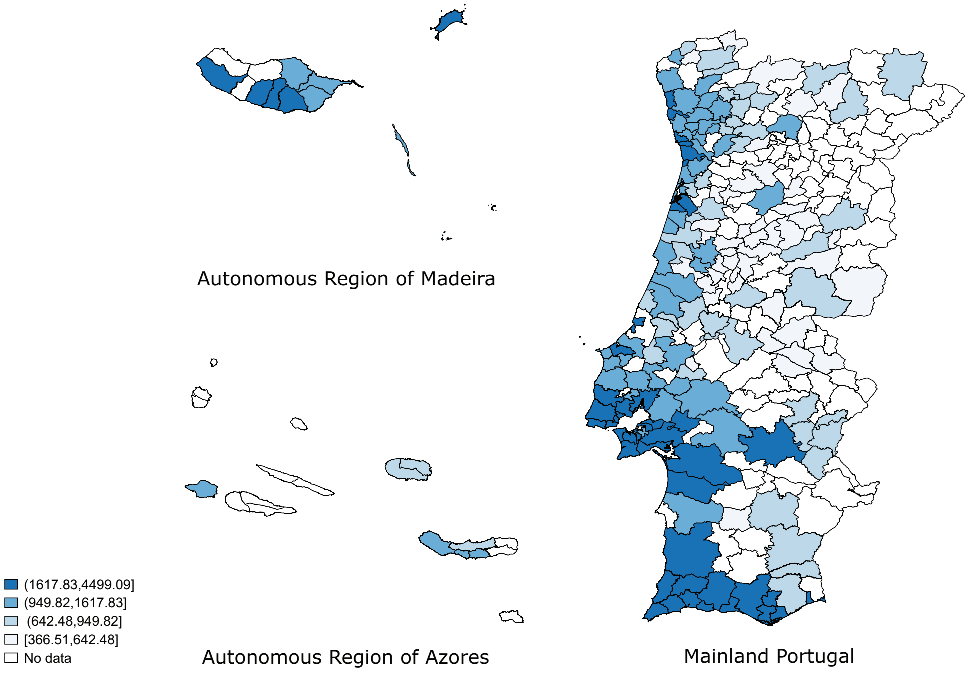

FIGURE C.1 Average real house asking price per Portuguese municipality (pretreatment).

Note: Pretreatment period—January 2023 to May 2024; darker blue indicates higher prices; figure not to scale.

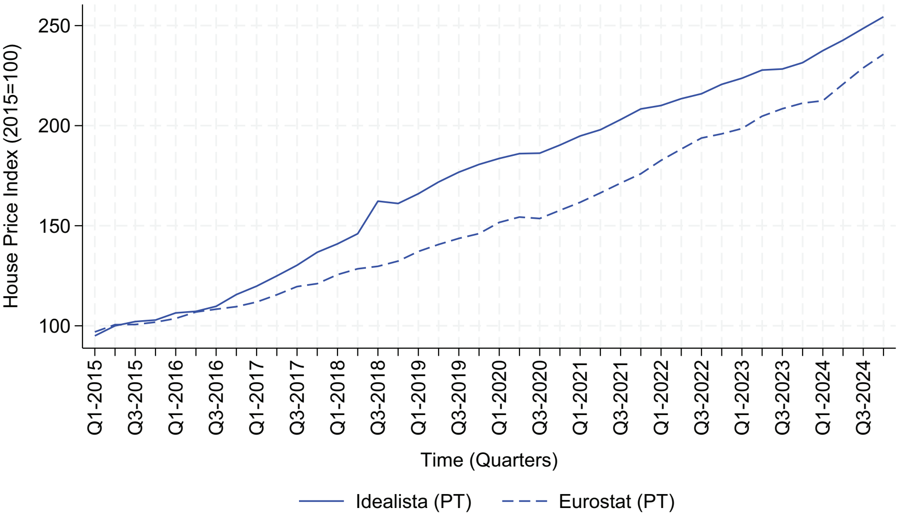

FIGURE C.2 House asking price index and house transaction price index (base 2015 = 100).

Note: The quarterly house asking price index using *Idealista* data is computed by the authors.

APPENDIX D: COMPARISON BETWEEN INCLUDED AND EXCLUDED MUNICIPALITIES

The exclusion of a subset of municipalities in both countries, specifically those for which *Idealista* does not provide sufficient data to construct consistent house price series, is critical for interpreting the scope of our findings. To address this, we conducted a systematic comparison between the included and excluded municipalities based on four key indicators for which we now also present descriptive statistics at the municipal level in Table 2, as they capture essential dimensions of economic and demographic context that are plausibly related both to housing market activity and to the likely effectiveness of demand-side housing policies. The results, reported in Table D.1, show clear and consistent differences across all four dimensions.

In both countries, included municipalities exhibit higher levels of average real gross income *per capita*. In Portugal, the difference is particularly pronounced, indicating that our sample tends to cover relatively better-off areas. Included municipalities also show significantly stronger population growth. For example, in Spain, the rate of population change in included areas is nearly twice that of excluded ones, suggesting more dynamic demographic conditions. Excluded municipalities tend to have higher youth dependency ratios, reflecting a higher burden of non-working-age individuals relative to the working-age population. This points to more aged or stagnant demographic structures in these areas. Finally, included municipalities have a larger share of individuals aged 20–35, a demographic segment particularly relevant for first-time homebuyers and thus for the mechanism of the tax policy under study.

These patterns support the view that the excluded municipalities are, on average, less dynamic, less affluent, and less demographically aligned with the target population of the policy intervention. This reinforces our interpretation that the estimated effects are likely to represent an upper bound of the average impact across all municipalities. It also highlights that our empirical focus is concentrated in precisely those areas where a demand-side housing policy is most likely to produce measurable effects on housing demand and prices.

TABLE D.1 Comparison between municipalities included and excluded from the analysis, 2023.

	Portugal		Spain	
Variables	**Included**	**Excluded**	**Included**	**Excluded**
Average real gross income *per capita* (€)[a]	11,958.48	10,643.67	11,293.41	11,692.42
Rate of total population change (per 1000 persons)	10.87	71	13.88	6.91
Age dependency ratio for young individuals (% working-age population)	63.33	71.49	50.86	68.48
Individuals between 20 and 35 years old (% population older than 20 years old)	18.63	17.05	19.32	14.89

[a] stands for 2022 as the last available period with data.

Source: *Instituto Nacional de Estatística* (Portugal) and *Instituto Nacional de Estadistica* (Spain).

APPENDIX E: PARALLEL TRENDS' ASSUMPTION DIAGNOSIS

Difference-in-differences (DiD). The DiD approach using a fixed-effects panel model, with i representing a cross-sectional unit and t denoting a time period, is formulated as follows:

$$P_{it} = \theta + \alpha C_i + \phi T_t + \tau(T_t \times C_i) + \epsilon_{it}, \tag{E.1}$$

where T_t is an indicator variable which assumes the value of 1 in the posttreatment period and 0 otherwise, C_i is an indicator variable equal to 1 for the treated units and 0 otherwise, $T_t \times C_i$ is an interaction term capturing the treatment effect, ϵ_{it} is the idiosyncratic error term, and τ measures the average causal impact of the policy's announcement on the real house asking prices measure, P_{it}, relative to the control group. Thus, a positive value of τ implies a positive impact, while a negative value implies the opposite, relative to the control group. This methodology relies on the parallel trends assumption.

To check whether this assumption is verified, we first visualize the average outcome changes over time for both treatment and control groups. Besides, we plot the predicted outcomes from the DiD approach, which incorporates interactions between time and the treatment group indicator. If the pretreatment trends are similar and begin to diverge after the treatment, it suggests a treatment effect. If the trends do not align before treatment, this could indicate a violation of the assumption. Figure E.1 presents these plots for the main sample of municipalities.

The graphs of the observed means reveal some divergence between the treatment and control groups before the intervention, suggesting a potential violation of the parallel trends assumption. In contrast, the linear-trends model shows more aligned pretreatment trajectories, providing visual support for the assumption and indicating that controlling for linear trends may help justify the use of the DiD approach. To reinforce this, we conduct a statistical test for linear parallel trends by extending the model to include group-specific time trends in the pre and posttreatment periods. In this specification, the key coefficient captures the difference in trends between groups before the intervention. A coefficient close to zero supports the validity of the parallel trends assump-

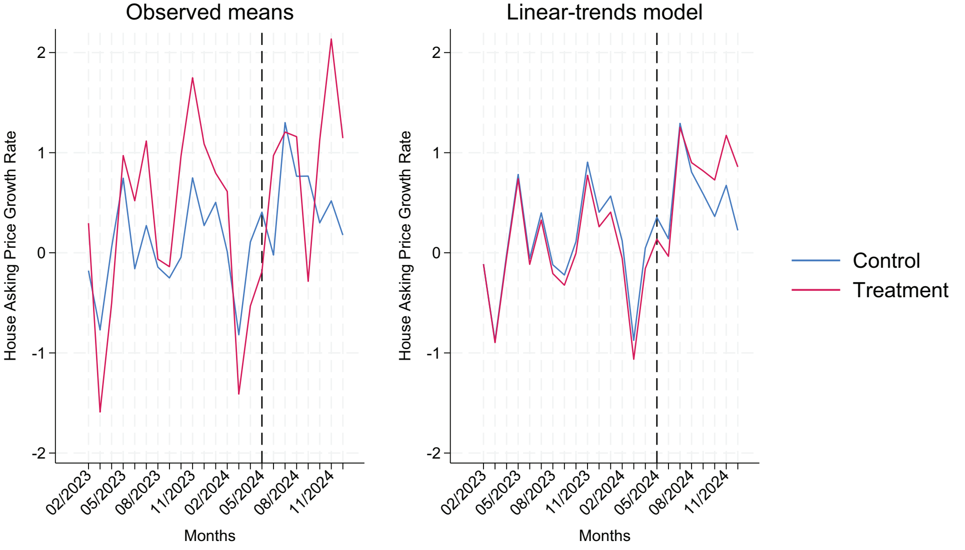

FIGURE E.1 Graphical diagnostics for parallel trends—full sample of municipalities.

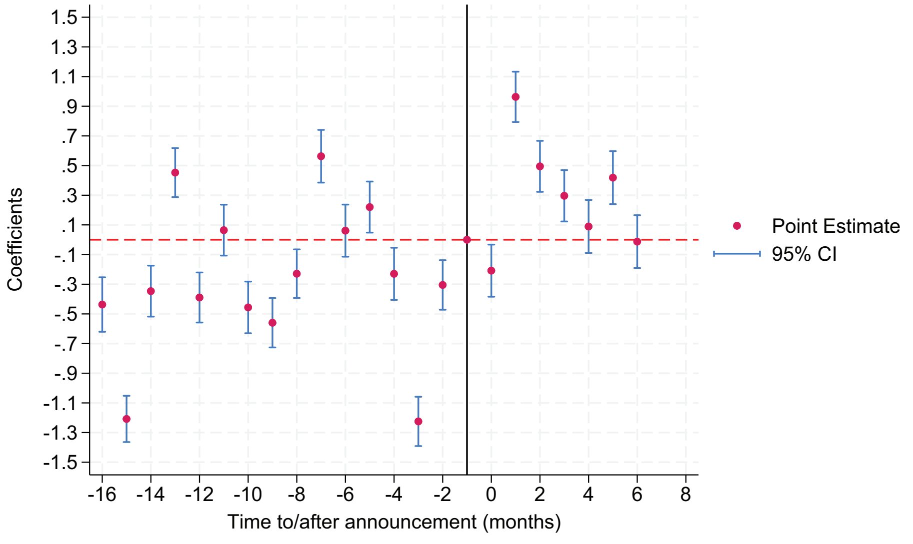

FIGURE E.2 Event study—full sample of municipalities.

tion. Our results show no significant pretreatment trend differences, lending further support to the DiD strategy. The same occurs for the smaller samples of municipalities.

Event-study analysis. Even with the previous evidence, it is important to emphasize that linearity is not a necessary condition for the assumption to hold. Therefore, we implement an event-study analysis which allows us to examine the dynamics of treatment effects over time without imposing a functional form on the trends, following the methodology developed by Clarke and Tapia-Schythe (2021). We consider the following panel event-study specification:

$$P_{it} = \theta + \sum_{j=2}^{J} \beta_j (\text{Lead } j)_{it} + \sum_{k=1}^{K} \psi_k (\text{Lag } k)_{it} + \alpha_i + \gamma_t + \varepsilon_{it}, \tag{E.2}$$

with $(\text{Lead } j)_{it}$ and $(\text{Lag } k)_{it}$ being binary indicators equal to 1 if $t = \text{Event}_i - j$ (pretreatment) or $t = \text{Event}_i + k$ (posttreatment), respectively.

The model is nonparametric in event time, as it estimates a separate coefficient for each period before and after the event, allowing for full flexibility in the timing of effects. Following standard practice in event-study designs, we omit the first lead (i.e., one period prior to the reform, where $j = 1$) as the baseline category, and interpret all estimated coefficients relative to this reference period. This approach allows us to visualize the dynamic effects of the event over time: we examine the coefficients in the pretreatment period. If the pretreatment coefficients deviate significantly from zero, it could indicate a violation of the parallel trends' assumption. Figure E.2 presents the evidence for the main sample used in our analysis.

The plot reveals that several pretreatment coefficients differ substantially from zero, and they do not follow a stable or flat pattern around the reference period. To further reinforce this evidence, we conduct a joint significance test of all pretreatment coefficients. If these coefficients are close to zero and statistically insignificant, it supports the assumption that the treatment and control groups would have followed similar trends in the absence of the treatment. A significant result, on the other hand, would suggest that the groups were diverging before treatment, making causal interpretation difficult. The results show that there are no parallel trends. These deviations, even under a flexible event-study specification, suggest that the control and treatment groups may have followed different trajectories prior to the intervention, casting doubt on the validity of the DiD identification strategy. Again, the same occurs for the smaller subsamples considered.

Conclusion. There is mixed evidence regarding the validity of the parallel trends assumption: the visual and statistical indications of the pretreatment trend divergence limit its reliability for causal inference. As a result, our analysis is focused on the synthetic difference-in-differences (SDiD) method, which is specifically designed to address violations of the parallel trends assumption by constructing a synthetic control (SC) that better aligns with the treatment group's pretreatment trajectory. This approach ensures more credible identification by design.

APPENDIX F: ADDITIONAL TABLES

TABLE F.1 Descriptive statistics for real house asking prices in metropolitan areas.

Metropolitan areas	Period	Obs.	Mean	Std. Dev.	Min.	Max.
	Pretreatment	527	1984.01	784.60	817.97	4671.84
Portuguese	Posttreatment	217	2086.03	804.06	926.62	4677.30
	Entire	744	2013.77	791.15	817.97	4677.30
	Pretreatment	4743	1439.01	694.60	401.84	5024.32
Spanish	Posttreatment	1953	1510.59	748.10	393.83	5298.10
	Entire	6696	1459.89	711.31	393.83	5298.10
	Pretreatment	5270	1493.51	722.77	401.84	5024.32
Portuguese and Spanish	Posttreatment	2170	1568.14	773.22	393.83	5298.10
	Entire	7440	1515.28	738.57	393.83	4677.30

Note: The entire period ranges from January 2023 to December 2024; it is divided into the pretreatment period (until May 2024) and the posttreatment period (from June 2024 onward). This subsample covers 31 municipalities from Portugal's two metropolitan areas, Lisbon and Oporto, and 279 municipalities from Spain's metropolitan areas, which include Madrid, Barcelona, Valencia, Seville, Malaga, Bilbao, Zaragoza, Alicante-Elche, Asturias, Murcia, Bahía de Cádiz-Jerez, Palma de Mallorca, Las Palmas de Gran Canaria, Granada, Vigo, Castellón, La Coruña, San Sebastián, Valladolid, Santa Cruz de Tenerife, Pamplona, Córdoba, Tarragona, and Santander.

TABLE F.2 Descriptive statistics for real house asking prices in nonmetropolitan areas.

Nonmetropolitan areas	Period	Obs.	Mean	Std. Dev.	Min.	Max.
	Pretreatment	2686	1103.47	672.38	330.48	3913.19
Portuguese	Posttreatment	1106	1170.02	699.20	335.05	3779.57
	Entire	3792	1122.88	680.89	330.48	3913.19
	Pretreatment	15,232	1136.18	682.75	280.76	8034.61
Spanish	Posttreatment	6272	1179.58	728.10	308.46	6986.69
	Entire	21,504	1148.84	696.55	280.76	8034.61
	Pretreatment	17,918	1131.27	681.29	280.76	8034.61
Portuguese and Spanish	Posttreatment	7378	1178.15	723.80	308.46	6986.69
	Entire	25,296	1144.95	694.27	280.76	8034.61

Note: The entire period ranges from January 2023 to December 2024; it is divided into the pretreatment period (until May 2024) and the posttreatment period (from June 2024 onward). This subsample covers 158 municipalities in Portugal and 896 in Spain.

TABLE F.3 Descriptive statistics for real house asking prices in municipalities with above-average income.

Above-average income	Period	Obs.	Mean	Std. Dev.	Min.	Max.
	Pretreatment	1326	1648.53	80211	569.85	4671.84
Portuguese	Posttreatment	546	1741.70	820.03	600.19	4677.68
	Entire	1872	1675.71	808.27	569.85	4677.68
	Pretreatment	8976	1522.4	768.44	337.19	8034.61
Spanish	Posttreatment	3696	1575.16	815.09	334.30	6986.69
	Entire	12,672	1537.79	782.67	334.30	8034.61
	Pretreatment	10,302	1538.64	773.97	337.19	8034.61
Portuguese and Spanish	Posttreatment	4242	1595.59	817.54	334.30	6986.69
	Entire	14,544	1555.39	787.34	334.30	8034.61

Note: The entire period ranges from January 2023 to December 2024; it is divided into the pretreatment period (until May 2024) and the posttreatment period (from June 2024 onward). This subsample covers 78 municipalities in Portugal and 528 in Spain.

TABLE F.4 Descriptive statistics for real house asking prices in municipalities with below-average income.

Below-average income	Period	Obs.	Mean	Std. Dev.	Min.	Max.
	Pretreatment	1887	966.38	593.53	330.48	3165.18
Portuguese	Posttreatment	777	1024.13	622.28	335.05	3172.45
	Entire	2664	983.22	602.51	330.48	3172.45
	Pretreatment	10,999	951.58	505.41	280.76	3794.27
Spanish	Posttreatment	4529	999.48	566.02	308.46	4056.55
	Entire	15,528	965.55	524.25	280.76	4056.55
	Pretreatment	12,886	953.75	519.25	280.76	3794.27
Portuguese and Spanish	Posttreatment	5306	1003.09	574.31	308.46	4056.55
	Entire	18,192	968.14	536.44	280.76	4056.55

Note: The entire period ranges from January 2023 to December 2024; it is divided into the pretreatment period (until May 2024) and the posttreatment period (from June 2024 onward). This subsample covers 111 municipalities in Portugal and 647 in Spain.

TABLE F.5 Descriptive statistics for real house asking prices in areas with above-average young individuals (≤35 years old).

Above-average young individuals	Period	Obs.	Mean	Std. Dev.	Min.	Max.
	Pretreatment	1700	1512.76	719.13	520.62	4671.84
Portuguese	Posttreatment	700	1617.43	742.47	579.21	4677.69
	Entire	2400	1543.32	727.43	520.62	4677.68
	Pretreatment	11,067	1213.03	764.01	280.76	8034.61
Spanish	Posttreatment	4557	1270.74	817.79	311.67	6986.69
	Entire	15,624	1229.86	780.49	280.76	8034.61
	Pretreatment	12,767	1252.94	764.97	280.76	8034.61
Portuguese and Spanish	Posttreatment	5257	1316.92	816.65	311.67	6986.69
	Entire	18,024	1271.60	780.91	280.76	8034.61

Note: The entire period ranges from January 2023 to December 2024; it is divided into the pretreatment period (until May 2024) and the posttreatment period (from June 2024 onward). This subsample covers 100 municipalities in Portugal and 651 in Spain.

TABLE F.6 Descriptive statistics for real house asking prices in areas with below-average young individuals (≤35 years old).

Below-average young individuals	Period	Obs.	Mean	Std. Dev.	Min.	Max.
	Pretreatment	1513	950.31	703.06	330.48	3913.19
Portuguese	Posttreatment	623	986.27	712.09	335.05	3779.57
	Entire	2136	960.80	705.73	330.48	3912.19
	Pretreatment	8908	1201.94	604.65	310.75	4483.23
Spanish	Posttreatment	3668	1242.55	646.23	308.46	4573.62
	Entire	12,576	1213.78	617.52	308.46	4573.62
	Pretreatment	10,421	1165.40	626.42	310.45	4483.26
Portuguese and Spanish	Posttreatment	4291	1205.34	662.30	308.46	4573.62
	Entire	14,712	1177.05	637.33	308.46	4573.62

Note: The entire period ranges from January 2023 to December 2024; it is divided into the pretreatment period (until May 2024) and the posttreatment period (from June 2024 onward). This subsample covers 89 municipalities in Portugal and 524 in Spain.

APPENDIX G: ROBUSTNESS ANALYSIS

To guarantee the validity of our benchmark results, we perform a set of robustness analysis, namely: a role-reversal falsification check; a shrinking of the pretreatment period; and an anticipation of the treatment period (placebo test).

Role-reversal falsification check. Here, we check if the observed effect in Portugal is real or if it could be due to unobserved factors affecting both Portugal and Spain. It involves simulating a fake intervention in Spain, treating it as if it had the tax exemption policy. The logic of this exercise is as follows: if the estimated policy effect were merely the result of symmetric macroeconomic trends, common shocks, or unobserved factors affecting both countries similarly, then reversing the treatment assignment should yield a mirrored effect of similar magnitude but opposite sign. In contrast, if the original treatment had a true, asymmetric impact specific to Portugal, then the reversal should produce a qualitatively different result (not simply a mirror image of the original estimate).

Changing the pretreatment period. We test different pretreatment periods to ensure that the results are not sensitive to the specific time window chosen. In our baseline analysis, we start with data for real house asking prices from January 2023, calculating the first growth rate for February 2023. We then adjust the pretreatment period to start in February, March, and April, with the first growth rates for each period corresponding to March, April, and May 2023. We limit the pretreatment period to 1 year before the treatment to capture relevant trends while avoiding noise from too much historical data.

Anticipating the start of the treatment period. Anticipating the treatment period allows us to assess whether the observed effects are truly due to the policy or if they might result from preexisting trends or random fluctuations. In our benchmark analysis, we first bring the treatment backward by 1 month, to May 2024, when no effect should occur. This helps verify the stability of the results and strengthens the causal identification of the policy's impact. Then, we extend this exercise by simulating a fictitious treatment in April 2024, 2 months before the policy was announced. This earlier window is selected carefully: we limit anticipation tests to April 2024 (inclusive), as this marks the installation of the new Government. Going further back would risk capturing the effects of previously announced or ongoing policies from the outgoing government, particularly those intended to continue in the event of their expected reelection, given their absolute majority at the time, thereby confounding a clean placebo identification. By ensuring the results are not dependent on arbitrary date choices, this approach enhances the credibility of the conclusions.

Results. The results presented in Table G.1 provide strong support for the robustness and causal interpretation of the main findings. The role-reversal falsification test produces significant effects with the expected opposite sign for the full sample, nonmetropolitan areas, and the below-average income group, indicating that the identification strategy does not pick up spurious relationships when treatment is falsely assigned. In contrast, no significant effects are observed for metropolitan areas or for groups with below-average youth shares and above-average income, suggesting greater robustness in these segments, potentially due to more stable underlying market dynamics.

When varying the pretreatment window, results remain stable across most subsamples, including both metropolitan and nonmetropolitan areas, and all four demographic groups. This consistency across different pretreatment start dates confirms that the main estimates are not driven by arbitrary choices in model specification and that the identified effects are robust to plausible variation in temporal structure. Besides, when the treatment is artificially anticipated to a future period, no statistically significant effects are detected in any subsample, reinforcing the causal validity of the estimates and showing that the method does not erroneously detect treatment effects where none should exist. It further rules out the influence of preexisting trends, seasonal patterns, or random variation.

TABLE G.1 Robustness results.

Sample	Benchmark	Role-reversal		Pretreatment		Treatment anticipation	
Full sample	**0.26 ** (0.10)**	−0.47*** (0.13)	0.27** (0.10)	0.21** (0.09)	0.14 (0.09)	−0.04 (0.10)	0.12 (0.09)
Metropolitan areas	**0.34 ** (0.14)**	−0.12 (0.19)	0.38*** (0.14)	0.33** (0.14)	0.31** (0.13)	0.05 (0.14)	0.21 (0.14)
Nonmetropolitan areas	**0.23 ** (0.11)**	−0.51*** (0.13)	0.23 ** (0.10)	0.17 * (0.10)	0.10 (0.10)	−0.08 (0.10)	0.09 (0.09)
Above-average income	**0.23 * (0.13)**	−0.23 (0.15)	0.28** (0.12)	0.21* (0.11)	0.15 (0.12)	0.08 (0.12)	0.11 (0.11)
Below-average income	**0.29 ** (0.14)**	−0.52*** (0.18)	0.29** (0.14)	0.23* (0.14)	0.17 (0.14)	−0.13 (0.13)	0.14 (0.13)
Above-average young	**0.34 *** (0.10)**	−0.35** (0.16)	0.34 *** (0.10)	0.27 ** (0.11)	0.23 * (0.12)	0.09 (0.09)	0.24 (0.09)
Below-average young	**0.13 (0.12)**	−0.39 (0.17)	0.14 (0.12)	0.10 (0.13)	0.03 (0.13)	−0.20 (0.12)	−0.03 (0.12)

Note: The growth rate of real house asking price per square meter is the variable of interest. The estimated coefficients are presented with robust bootstrap standard errors (with 50 repetitions) in parentheses. The pretreatment results consider the case where the pretreatment data start in February 2023 (left-hand side) and in April 2023 (right-hand side). Anticipating the treatment is done for April 2024 (left-hand side) and May 2024 (right-hand side).

*, **, and *** stand for statistical significance at 10%, 5% and 1%, respectively.