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## Who Bears the Burden of Real Estate Transfer Taxes? Evidence from the German Housing Market

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# Who Bears the Burden of Real Estate Transfer Taxes?

## Evidence from the German Housing Market

Mathias Dolls, Clemens Fuest, Carla Krolage, Florian Neumeier\*

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### Abstract

This paper examines the effects of real estate transfer taxes (RETT) on property prices using a rich micro dataset of roughly 17 million German properties for the period from 2005 to 2019. Our empirical analysis exploits variation in RETT rate hikes across German states and over time. Our monthly event study estimates indicate a price response that strongly exceeds the change in the tax burden for single transactions. Twelve months after a reform, a one percentage point increase in the tax rate reduces property prices by on average 3%. Price effects are larger for apartments (-4%) than for single-family houses (-2%). Exploring potential mechanisms, we provide evidence that different holding periods are the main driver of the differential price effect between property types. Moreover, effects are stronger in counties where properties sell quickly and price discounts are small as well as in growing housing market regions. Our results can be rationalized by a theoretical model that predicts larger price responses in sellers' markets and for properties with a high transaction frequency.

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**Keywords:** Real estate transfer taxes, property taxes, housing market

**JEL-Codes:** H22, H71, R32, R38

# 1 Introduction

In many countries, taxes on real estate transfers are an important source of public sector revenue. However, they are often criticized for preventing real estate transactions and for making housing unaffordable, in particular for the middle class.<sup>1</sup> This paper studies how changes in real estate transfer taxes (RETT) affect house prices. We focus on *permanent* tax changes, which are understudied in the literature. Most existing contributions focus on transitory tax changes or discontinuities in the tax schedule. Understanding effects of permanent increases in transfer taxes is important, not least because growing international mobility of both capital and people may increase pressure to raise more revenue from land and real estate. If that happens, a key question is who bears the tax burden. Is the tax capitalized into house prices so that those who own the house when the reform happens effectively pay the tax? Or do the buyers, who actually remit the tax, bear the burden? To provide answers to these questions, we develop a simple housing market model and test the model’s predictions using a large micro dataset of property listings in Germany.

In the first part of the paper, we consider a stylized overlapping generations model where the price effects of transfer taxes depend on (i) the distribution of bargaining power between the seller and the buyer as well as (ii) the likelihood that the buyer will resell the house in the future. To the best of our knowledge, the second channel has not been formally studied in previous studies. In the second part of the paper, we exploit variation in RETT rate hikes across German states and over time and employ an event study design to investigate the impact of the RETT on property prices.<sup>2</sup> To this end, we use a unique dataset covering roughly 17 million properties offered for sale over the period from 2005 to 2019. The data was collected by analyzing real estate advertisements from 140 different sources, including online property portals such as ImmobilienScout24.de, as well as regional and trans-regional newspapers. Our dataset includes information about asking prices and the date of

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<sup>1</sup>With 45%, Germany has the lowest homeownership rate in the Eurozone and the second lowest rate among OECD countries. Kaas et al. (2021) estimate that lowering real estate transfer taxes in Germany to the average level of the tax in the US would lead to an increase in the homeownership rate by 6–14 percentage points.

<sup>2</sup>A reform of the German federal fiscal system in 2006 gave the German states the right to set the rate of the real estate transfer tax (RETT) autonomously. Before the reform, there was a nationwide uniform tax rate of 3.5%. After the reform, most states increased their tax rates, some of them multiple times. Today, the highest tax rates are equal to 6.5%.

the listing (asking prices on the first and the last day the property was listed, time on the market), as well as a large number of property characteristics, such as the property type (apartment, single-family house, apartment building), floor size, the construction year, as well as several amenity features.

Our theoretical model predicts that the price effects of a RETT rate change can be larger than the associated change in the tax burden.<sup>3</sup> Such an overshifting of the tax burden to the sellers can occur if the bargaining power of the seller is high and the (expected) holding period of the property is not too long. Our empirical results are in line with these predictions. In our pooled sample that includes all property types, a one percentage point increase in the RETT rate is found to reduce property prices by roughly 3% within a year after the tax reform, indicating a strong overshifting of the tax burden.

Next, making use of high-quality administrative data on the number of transactions of apartments and single-family houses (available for the period 2009–19) as well as the composition of the housing stock in Germany (available for the year 2011), we document that transaction frequencies of apartments are roughly twice as large as those of single-family houses. In 2011, 3.2% of the stock of freely tradable apartments were sold in Germany, but only 1.5% of single-family houses. We also show that the transaction volume of apartments was higher throughout the period 2009–19, although the stock of freely tradable apartments is smaller than the stock of single-family houses.

Estimating the event study separately for the two property types, we find that price effects are about two times larger for apartments (-4%) than for single-family houses (-2%) and that the differential price effect is statistically significant. We do not find evidence that the larger price effect found for apartments is driven by sellers of apartments having stronger bargaining power than sellers of single-family houses, or that buyers of apartments are more likely to be crowded out of the market because of downpayment constraints (Best and Kleven, 2018). Instead, all our evidence points to transaction frequencies as the main driver of the differential price effect across property types. A simple back-of-the envelope calculation reveals that our theoretical model performs reasonably well in predicting the estimated price responses.

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<sup>3</sup>This finding refers to the tax liability for a single transaction. Since the tax increase will also apply to future transactions, the true burden implied by the tax increase is likely to be larger, as we will discuss further below. This point is also made by Petkova and Weichenrieder (2017).

Finally, we investigate the role of alternative channels that may also affect the magnitude of the price response. For both apartments and single-family houses, we find notably stronger price effects in growing housing market regions as well as in counties where properties sell quickly and discounts are rare, while the price effects in shrinking housing market regions as well as in counties where properties stay on the market for a longer time and are sold with larger discounts are statistically insignificant.<sup>4</sup> We interpret this finding as evidence that — as our theoretical model predicts — the price effect of a RETT rate change is indeed positively related to the bargaining power of the sellers. However, for apartments, we find that regions characterized by supposedly higher bargaining power of the seller also exhibit higher transaction frequencies than regions with lower bargaining power of sellers. In contrast, transaction frequencies do not vary much across regions for single-family houses. This suggests that the larger price effects found for apartments in regions with stronger bargaining power of the seller could potentially also be driven by the apartments’ higher transaction frequencies.

Moreover, exploring the role of downpayment constraints and assuming that they are less prevalent among institutional and business investors than among private households, we compare price effects in counties with a high and a low share of institutional and business investors. Neither for apartments nor for single-family houses we find evidence for heterogeneous price effects with respect to downpayment constraints.

Our results are robust to several modifications to our empirical specification. Moreover, we find that key assumptions for the identification of causal effects — that is, the common trend assumption and the absence of spillovers from the treatment to the control group — appear to be valid.

In contrast to the present paper, a large part of the existing literature on the effect of property transaction taxes on housing prices either focuses on temporary tax changes or uses discontinuities in property tax schedules (Besley et al., 2014; Kopczuk and Munroe, 2015; Slemrod et al., 2017; Best and Kleven, 2018). Besley et al. (2014) use the 2008-2009 UK stamp duty tax holiday and find that only 60% of the tax relief was passed on to property prices. Due to the temporary nature

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<sup>4</sup>Growing and shrinking housing market regions are defined by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) according to a set of criteria such as population growth, migration and unemployment. Information on price discounts and time on the market is retrieved from our listings data. See section 6.3.1 for details.

of the tax reduction, it is not surprising that their estimated price effect is notably smaller than ours, as permanent tax changes also affect the tax burden on all future transactions. Bunching estimates based on notches in the tax schedule, on the other hand, infer the price effects of property transfer taxes from the distribution of property prices in close proximity to the tax notch, thus estimating local effects. Interestingly, the estimates reported in this strand of the literature vary notably. Slemrod et al. (2017) exploit a notched transfer tax in Washington D.C. and find that the burden of the transfer tax is equally split between buyers and sellers. In contrast, Kopczuk and Munroe (2015) use a discontinuity in the so-called ‘mansion tax’ in New York and New Jersey that applies to residential transactions of US-\$ 1 million and more and report that the tax-induced decline in property prices is more than two times larger than the tax liability. Best and Kleven (2018) exploit notches in the UK stamp duty tax and even find a decline in property prices that exceeds the tax paid by four to five times.

The price effect of a permanent tax change is studied by Dachis et al. (2012), who exploit the introduction of a land transfer tax in Toronto in 2008. Their estimates suggest that the introduction of the tax led to a reduction of property prices that is roughly equal to the tax for a single transaction.

There are also studies analyzing the price effects of the German RETT. However, these studies use aggregated price or transaction indices. Petkova and Weichenrieder (2017) use annual property price indices and transaction volumes to evaluate the effects of RETT rate hikes on the German housing market. For houses and vacant lots, the authors find that the number of transactions declines, while prices are not significantly affected. For apartments, however, the authors report negative price effects, but no effect on the number of transactions. In a similar vein, Fritzsche and Vandreier (2019) find negative effects of RETT rate hikes on monthly transaction volumes for family homes. Their results also suggest that RETT rate hikes are anticipated by market participants. Focusing on commercial property, Baudisch and Dresselhaus (2018) document a reduction in the number of transactions and prices following a RETT rate hike. Christofzik et al. (2020) find negative price effects as well, but only in shrinking housing market regions. A problem with these analyses based on aggregated housing data is that they fail to control for regional differences in housing market conditions and property characteristics, which may result in biased estimates. Also, property price indices typically reflect the value of

some standardized properties rather than actual property values, or they are based on a single source like ImmobilienScout24, an online property portal in Germany. However, standardized properties are often not representative of the actual property stock, especially at the sub-national level. The same is true for properties posted on ImmobilienScout24 as we show in section 4.

Some of the studies discussed above also use theoretical models to analyze the importance of different channels through which transfer taxes may affect property prices. Most of them employ bargaining models similar to ours (Besley et al., 2014; Kopczuk and Munroe, 2015; Slemrod et al., 2017). A different approach is taken by Best and Kleven (2018), who consider a model with downpayment constraints to rationalize large prices responses. A novel feature of our model is that it highlights the importance of the likelihood that a property will be resold in the future for the magnitude of the price effect. As we will see later on, this channel has a notable impact on the RETT's price effects.

The remainder of the paper is structured as follows. The next section describes the institutional background of the RETT in Germany. In section 3, we present a simple housing market model that guides our empirical analysis and facilitates the interpretation of our results. Section 4 presents the data and descriptive statistics. In section 5, we describe our empirical approach. Results are presented in section 6. Section 7 concludes.

## 2 Institutional Background

The RETT is an important source of revenue for the German states. This is not primarily because of its share in overall revenue. With a revenue of 15.8 billion euros in 2019, which corresponds to 4.9% of state level tax revenues, its quantitative weight is limited. Its importance is rather due to fact that it is the only tax with significant revenue where the states can set the tax rate autonomously.<sup>5</sup> The RETT is charged on the purchase price of residential and non-residential property as well as vacant lots and is paid by the buyer.

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<sup>5</sup>In Germany, tax autonomy is higher at the local level. Local governments can set the property tax rate and the rate of the local business tax. The rates of the most important revenue sources, the income tax and the value added tax, are set at the federal level. Through the second chamber, the states participate in decisions regarding income and value added tax rates, and they receive a share of the revenue.



Before 2006, the RETT rate was uniform across all states. The rate was equal to 2% prior to 1997 and 3.5% between 1997 and 2006. In 2006, a constitutional reform permitted the states to set their RETT rates autonomously. With the exception of Bavaria and Saxony, all states have increased their tax rates over our sample period (2005–2019), often multiple times. Until today, RETT rates have been raised 30 times in total, with 27 of the tax changes falling into our sample period. So far, no state has ever reduced its tax rate. Table 1 provides an overview of all RETT rate changes in our sample period by state, along with the date of the tax increase, the new tax rate, as well as the publishing date of the first legal draft of the bill implementing the tax rate hike. The median time between a RETT rate hike and the first legal draft of the corresponding bill is 3.2 months. We take this time gap between the announcement of the RETT rate change and its implementation into account when choosing a reference period in our empirical analysis (cf. section 5).

Krause and Potrafke (2020) find that German RETT increases are driven by government ideology, with leftwing and center governments being more likely to raise the tax. As shown by Büttner and Krause (2021), the German fiscal equalization scheme also sets strong financial incentives for states to raise their RETT rates under certain circumstances. Moreover, the German public debt ceiling (‘debt brake’) required state governments to achieve structurally balanced budgets from 2020 onwards, which may explain why the need for budget consolidation is the most frequent official justification for RETT increases (Fritzsche and Vandrei, 2019).

Presumably, housing market conditions, e.g., whether regions are growing or declining, could also affect states’ incentives to increase RETT rates. Such a correlation between housing market conditions and RETT changes could bias our estimates of the price effects of RETT rate changes. To test for such a selection on observables, we build on the previous work by Krause and Potrafke (2020) and Büttner and Krause (2021). Starting with the model of Krause and Potrafke (2020), which relates RETT increases to government ideology and state budget variables, we add the key explanatory variable of Büttner and Krause (2021) indicating the degree of fiscal redistribution, capturing the fiscal incentives to raise state taxes due to peculiarities of the German fiscal equalization scheme. We subsequently add proxies for state housing market conditions. These encompass all variables that the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) considers in its classification of growing and shrinking housing regions, as

used in Section 6.3.1: state population growth, the migration balance, working-age population growth, the growth in the number of employees subject to social insurance contributions, the change in the unemployment rate, and the revenue growth from local business taxes, all in lags. In line with the previous literature, our results in Appendix Table 6 confirm that state RETT rates are mainly driven by two factors: government ideology, with left and center governments displaying a higher propensity to increase the RETT, and incentives driven by the fiscal equalization scheme. In contrast, housing market conditions do not exert a significant impact, indicating that our later findings are not driven by a selection on observables.<sup>6</sup>

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<sup>6</sup>As the variables used by the BBSR in its classification of housing market regions display a high degree of multicollinearity, with ensuing large standard errors, we separately add each variable to the model by Krause and Potrafke (2020). As Büttner and Krause (2021)’s variable measuring fiscal incentives is only available until 2016, we also estimate a specification covering the full sample period until 2019 without accounting for fiscal equalization (results available upon request). Over the longer time frame, none of the proxies for housing market conditions are significant either.

Table 1: Real estate transfer tax rate changes over the sample period

State	Initial Tax Rate	Date of Increase	New Tax Rate	First Legal Draft
Baden-Württemberg	3.5%	11/05/2011	5.0%	09/13/2011
Bavaria	3.5%	-	-	-
Berlin	3.5%	01/01/2007	4.5%	11/07/2006
		04/01/2012	5.0%	01/18/2012
		01/01/2014	6.0%	10/10/2013
Brandenburg	3.5%	01/01/2011	5.0%	09/13/2010
		07/01/2015	6.5%	03/04/2015
Bremen	3.5%	01/01/2011	4.5%	06/22/2010
		01/01/2014	5.0%	07/09/2013
Hamburg	3.5%	01/01/2009	4.5%	10/14/2008
Hesse	3.5%	01/01/2013	5.0%	09/25/2012
		08/01/2014	6.0%	05/13/2014
Lower Saxony	3.5%	01/01/2011	4.5%	08/31/2010
		01/01/2014	5.0%	09/17/2013
Mecklenburg-Western Pomerania	3.5%	07/01/2012	5.0%	02/14/2012
North Rhine-Westphalia	3.5%	10/01/2011	5.0%	05/10/2011
		01/01/2015	6.5%	10/28/2014
Rhineland-Palatinate	3.5%	03/01/2012	5.0%	11/23/2011
Saarland	3.5%	01/01/2011	4.0%	10/19/2010
		01/01/2012	4.5%	10/18/2011
		01/01/2013	5.5%	10/08/2012
		01/01/2015	6.5%	10/07/2014
Saxony	3.5%	-	-	-
Saxony-Anhalt	3.5%	03/02/2010	4.5%	09/30/2009
		03/01/2012	5.0%	09/28/2011
Schleswig-Holstein	3.5%	01/01/2012	5.0%	08/23/2010
		01/01/2014	6.5%	07/26/2013
Thuringia	3.5%	04/07/2011	5.0%	01/06/2011
		01/01/2017	6.5%	09/23/2015

*Notes:* This table shows the timing and the scope of state-level RETT rate increases over the sample period. Three recent RETT rate increases fall outside the sample period (2005–2019) and are therefore neither included in Table 1 nor in our empirical analysis. In January 2023, Hamburg and Saxony raised their RETT rates to 5.5%, respectively. Mecklenburg-Western Pomerania raised its RETT rate to 6% in July 2019. The latter tax increase is not included because our data cover only 6 post-reform months.

Date format: MM/DD/YYYY.

### 3 A Simple Model of a Housing Market with Transfer Taxes

To guide our empirical analysis, we consider a highly stylized model of a housing market with overlapping generations. There are two types of agents, the young ( $Y$ ) and the old ( $O$ ). All agents live for two periods; they are young in the first period and old in the second. The number of households in each generation is normalized to unity. There is a stock of two units of housing in the economy. For simplicity we abstract from depreciation of housing capital and construction.

The utility for the young of owning property while young is given by  $U^Y$ . Property ownership may or may not imply that a household actually occupies the property. There is a perfectly competitive rental market which makes sure that all households live somewhere. For the purposes of our analysis, we do not need to model this market explicitly.

We consider a housing market with frictions. At the beginning of each period, the young enter the housing market. They would all like to buy a house. The old generation owns all houses in the economy. Households experience a preference shock when they are old. For a fraction  $q$  with  $0 < q < 1$ , the utility of owning a second house when old takes a low value  $U^{OL} < U^Y$ . For a fraction  $1 - q$ , the utility of owning the house is high, with  $U^{OH} > U^Y$ .<sup>7</sup> Each young household in the market is matched with an old household.

If no trade takes place because a young household happens to be matched with an old household with high preferences for owning a second house ( $U^{OH} > U^Y$ ), the old agent keeps the property while old and passes it on to the next generation.<sup>8</sup> If a transaction takes place, the buyer pays a transfer tax equal to  $T$  percent of the property price. The reservation utility of the young households is equal to zero.

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<sup>7</sup>A limitation of our model is that we do not endogenize  $q$ , which implies that changes in transfer taxes in our model do not influence the number of transactions. We make this assumption because our empirical analysis focuses on effects of tax changes on prices, not on the number of transactions. The main objective of our theoretical analysis is to highlight the idea that prices of properties which are traded more often for reasons other than taxes are more affected by changes in real estate transaction taxes.

<sup>8</sup> $U^{OH}$  may or may not include the utility of the old from passing on a property to the next generation. Note that in equilibrium, at the beginning of each period before transactions take place, only the old households own houses, which is why only they can sell them. After transactions have taken place, the old households still own  $2 - q$  units of housing. At the end of period two, the old households die and the houses owned by the old are inherited by the next generation of old households.

It is straightforward to determine the equilibrium property price. When the young negotiate, they take into account that they will sell the property with probability  $q$  when they are old. With probability  $1 - q$  they will keep and use the property while old, so that the present value of the surplus from buying the property is given by

$$U^Y + q \frac{p_{t+1} - U^{OL}}{(1 + \rho)} + (1 - q) \frac{U^{OH}}{(1 + \rho)} - p_t(1 + T) \quad (1)$$

where  $t$  is the period index and  $\rho$  is the discount rate. The surplus of the old agent from selling is simply given by  $p_t - U^{OL}$ .

The equilibrium property price in period  $t$  is thus given by maximizing the Nash maximand

$$\beta \ln \left( U^Y + q \frac{p_{t+1} - U^{OL}}{(1 + \rho)} + (1 - q) \frac{U^{OH}}{(1 + \rho)} - p_t(1 + T) \right) + (1 - \beta) \ln(p_t - U^{OL}) \quad (2)$$

over  $p_t$ , which yields

$$p_t^*(1 + T) = \beta U^{OL}(1 + T) + (1 - \beta) \left( U^Y + q \frac{p_{t+1} - U^{OL}}{(1 + \rho)} + (1 - q) \frac{U^{OH}}{(1 + \rho)} \right). \quad (3)$$

where  $\beta$  refers to the bargaining power of the buyer. For the purpose of our analysis, which focuses on the property price effects of changes in the transfer tax rate  $T$  which are perceived as permanent, it is sufficient to consider the tax effect on prices in the steady state, where prices are the same in each period in this stationary model. The steady state property price is given by

$$p^* = \left( 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right)^{-1} \left[ \beta U^{OL}(1 + T) + (1 - \beta) \left( U^Y - q \frac{U^{OL}}{(1 + \rho)} + (1 - q) \frac{U^{OH}}{(1 + \rho)} \right) \right]. \quad (4)$$

Denote the semi-elasticity of the property price with respect to the tax rate by  $\varepsilon \equiv \frac{dp^*}{dT} \frac{1}{p^*}$ . Consider first the two polar cases  $\beta = 1$  (buyer has all the bargaining power) and  $\beta = 0$  (seller has all the bargaining power). If the buyer has all the bargaining power it follows directly from (4) that  $\varepsilon = 0$ . Since the seller is always reduced to her reservation utility and the property price is the net of tax price, changes in  $T$  are always fully borne by the buyer and the property price does not change. In the opposite polar case, where the seller has all the bargaining power

( $\beta = 0$ ), we get

$$\varepsilon = -\frac{1}{1 + T - \frac{q}{(1+\rho)}}. \quad (5)$$

Equation (5) yields various important insights. First, if  $q$  converges to zero, which implies that buyers do not expect further transactions during their lifetime, a one percentage point increase in the transfer tax ( $dT = 0.01$ ) reduces the price by approximately 1%. But if  $q$  is positive, the decline in the price will be larger than 1% because the tax increase is also expected to be a burden on future transactions. We may then observe that a one percentage point increase in the real estate transfer tax reduces property prices by more than 1%. Unsurprisingly, the impact of future transactions is stronger, the lower the discount rate.

Consider finally the general case  $0 < \beta < 1$ , where:

$$\varepsilon = \left[ 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right]^{-1} \left[ \frac{\beta U^{OL}}{p^*} - 1 \right] < 0 \quad (6)$$

As we show in Appendix A.2, equation (6) defines  $\varepsilon$  as a function of  $\beta$  and  $q$ , that is  $\varepsilon = \varepsilon(\beta, q)$ , with  $\frac{\partial \varepsilon}{\partial \beta} > 0$  and  $\frac{\partial \varepsilon}{\partial q} < 0$ . For our empirical analysis, this implies that we would expect to see (i) a smaller price reduction in response to a tax increase in case the bargaining power of the buyers is higher and (ii) a larger price reduction in case the traded property is expected to be traded more frequently in the future.<sup>9</sup>

In Figure 7 of Appendix A.2, we illustrate the relationship between  $q$  and  $\varepsilon$  graphically. The other parameters that appear in our model are held fixed. The figure demonstrates that the price reduction in response to a tax hike is close to 1% in case  $q$  approaches zero. In contrast, if  $q$  is sufficiently large, the price reduction can exceed the increase in the tax burden by far. To ease the comparison between the predictions of our theoretical model and the results of our empirical analysis, we transform the transaction probability  $q$  into an annual transaction frequency and plot its relationship with  $\varepsilon$  in Figure 8 of Appendix A.2. To compute an annual transaction frequency, we assume that a generation lasts for 25 years, so that the annual transaction frequency is obtained by dividing  $q$  by 25. As for the transaction probability, the association between the annual transaction frequency

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<sup>9</sup>Of course, the expected number of future transactions will itself be a function of the transfer tax. In the simple model considered here, the number of future transactions is exogenous because our empirical analysis focuses on price effects, not the quantity of property transactions.

and  $\varepsilon$  is strongly concave. We will get back to this Figure in Section 6.2, where we compare the results of this simulation to the findings from our empirical analysis.

## 4 Data and Descriptive Statistics

In our empirical analysis we use a novel and large dataset on the German real estate market provided by F+B, a commercial real estate consultancy firm. The dataset covers roughly 17 million properties that were offered for sale in Germany during the period from January 2005 until December 2019. It was created by analyzing real estate advertisements from 140 different sources, including online property portals, regional and trans-regional newspapers, as well as real estate agencies, using web-scraping techniques. The raw data was thoroughly cleaned to make sure that properties that were listed in more than one source at the same time only appear once in the final dataset. For all properties included in the final dataset, we know the complete list of sources in which the property was advertised as well as both the first and last asking price and the date of the first and the last day the property was listed, respectively.<sup>10</sup> The dataset covers a wide range of property characteristics, such as floor space, the number of rooms, the construction year, as well as binary indicators for equipment and locational features, and the postal code of the property. It is available for three different property types: apartments, single-family houses, and apartment buildings.

Over the last decades, there was a very heterogeneous development of property prices in Germany. Some large German cities, such as Berlin, Frankfurt, Hamburg, and Munich, as well as areas in their vicinity, have experienced a rapid increase in property prices, considerably driven by a substantial growth in population size. At the same time, there are some predominantly rural areas in Germany that suffer from a population drain, leading to declining property prices. To mitigate concerns that our results might be affected by some outliers that have experienced extreme migration patterns during our sample period, we truncate our sample according to municipal population growth between 2005 and 2017.<sup>11</sup> More precisely, we drop all

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<sup>10</sup>We do neither observe whether the listed properties were actually sold nor the sale price. Moreover, the dataset does not contain a property identification number so that we do not observe how often a property was listed. This implies that in our empirical specification, we cannot exploit repeated listings by including property fixed effects.

<sup>11</sup>Administrative data on municipal population size was only available until 2017 at the time of writing the paper.

municipalities with a population growth rate that is smaller than the population-weighted 5% quantile or larger than the population-weighted 95% quantile of the population growth rate.<sup>12</sup>

Table 2 shows descriptive statistics for the full sample and the truncated sample separately for each property type. The figures suggest that the average realizations of important property characteristics are fairly stable across both samples. For apartments (single family houses/apartment buildings), the average final asking price per square meter is EUR 2,135 (EUR 1,729/EUR 1,161) in the full sample compared to EUR 2,067 (EUR 1,767/EUR 1,205) in the truncated sample. The average number of days a property is posted amounts to 114-115 for apartments, 97-98 for single-family houses, and to 102-105 for apartment buildings. The average apartment in both samples has a floor size of around 97 square meters, three rooms, and was built in 1980. For single-family houses, the average floor size is around 152 to 153 square meters and the average number of rooms is five. Our final sample comprises roughly 7.6 million apartments, 8.6 million single-family houses, and 830,000 apartment buildings.

Table 3 shows the sample means separately for three different time periods: 2005-2009, 2010-2014, and 2015-2019. A glance at the price variables suggests that property prices have increased notably over the past years. I.e., between 2015 and 2019, the average price per square meter for an apartment (single-family house) was roughly EUR 780 (EUR 490) higher than it was between 2010 and 2014. This corresponds to a price increase of about 42% (30%).

A closer inspection of our data reveals that a large fraction of the properties included in our dataset was advertised on the online property portal ImmobilienScout24.de, the largest online property portal in Germany. To check whether properties listed on ImmobilienScout24.de differ from those advertised in other outlets, we compare the characteristics of properties listed on ImmobilienScout24.de to the characteristics of properties that were solely listed in other outlets. The descriptive statistics in Table 7 of Appendix A.3 indicate that properties listed on ImmobilienScout24.de do not appear to be representative of the German property market. On average, properties listed on ImmobilienScout24.de are more expensive than

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<sup>12</sup>Dropping properties offered for sale in areas that experienced particularly large increases and declines in population growth, rather than directly truncating based on property prices, ensures that the selection of our sample is not endogenous, that is, related to price changes induced by changes in RETT rates.



Table 2: Real estate data: Full vs. truncated sample

	Apartments		Single-family Houses		Apartment Buildings	
	Full sample	Truncated	Full sample	Truncated	Full sample	Truncated
First price	2,146	2,078	1,739	1,778	1,169	1,213
Last price	2,135	2,067	1,729	1,767	1,161	1,205
Days posted	115	114	98	97	105	102
Floor size	96.5	96.6	151.9	152.6	329.3	328.8
Rooms	3.1	3.1	5.0	5.0	8.5	8.5
Constr. year	1980	1980	1980	1981	1953	1955
Kitchen	0.35	0.35	0.21	0.22	0.18	0.19
Parking spot	0.63	0.63	0.59	0.60	0.64	0.65
Garden	0.25	0.25	0.36	0.37	0.34	0.35
Balcony	0.42	0.42	0.32	0.32	0.33	0.34
Basement	0.40	0.41	0.39	0.40	0.40	0.41
Obs. (max.)	8,687,213	7,618,910	10,157,288	8,644,239	980,249	829,039

*Notes:* The table shows the average realizations of different property characteristics for different property types separately for the full sample and a sample that is truncated based on municipal population growth rates. Asking prices refer to the price per square meter. Discount is the difference between the first and the last asking price of a property (measured in percent of first price). Floor space is measured in square meters. Note that the construction year is missing for some properties, which is why the number of observations for this variable is smaller than it is for the others.

properties solely listed in other outlets. Also, the characteristics of properties advertised on ImmobilienScout24.de differ from the characteristics of properties listed in other sources. For instance, apartments advertised on ImmobilienScout24.de appear to be smaller, but are more likely equipped with a kitchen, a parking spot, a garden, a balcony, and a basement. This underlines our dataset’s higher degree of representativity compared to web-scraped Immobilienscout24.de data used by other studies on the German real estate market.

Finally, for the analysis of heterogeneous price effects with respect to transaction frequencies, bargaining power and downpayment constraints, we rely on further data sources which are described in detail in sections 6.2–6.3.

Table 3: Real estate data: Summary statistics by time period

	Apartments			Single-family Houses			Apartment Buildings		
	2005-09	2010-14	2015-19	2005-09	2010-14	2015-19	2005-09	2010-14	2015-19
First price	1701	1857	2635	1614	1636	2123	1013	1056	1446
Last price	1683	1849	2629	1598	1628	2116	998	1049	1439
Days posted	148	130	66	119	107	60	142	125	65
Floor size	93.2	94.3	102.4	150.7	152.6	154.5	387.1	312.4	328.0
Rooms	3.1	3.1	3.2	4.8	5.1	5.1	7.5	8.5	8.9
Constr. year	1979	1978	1982	1982	1979	1981	1955	1953	1956
Kitchen	0.32	0.40	0.31	0.17	0.22	0.26	0.12	0.17	0.23
Parking spot	0.62	0.63	0.65	0.58	0.61	0.60	0.58	0.65	0.67
Garden	0.25	0.22	0.27	0.33	0.32	0.46	0.31	0.28	0.42
Balcony	0.47	0.37	0.44	0.31	0.24	0.42	0.34	0.24	0.44
Basement	0.28	0.47	0.46	0.35	0.41	0.43	0.31	0.40	0.45
Obs. (max.)	2,286,182	2,879,276	2,453,452	2,959,107	3,070,274	2,614,858	109,095	370,830	349,114

*Notes:* The table shows the average realizations of different property characteristics for different property types and across different time periods. Asking prices refer to the price per square meter. Discount is the difference between the first and the last asking price of a property (measured in percent of first price). Floor space is measured in square meters. Further variables indicate the fraction of properties that come with amenities, such as a fully equipped kitchen or a parking spot. Note that the construction year is missing for some properties, which is why the number of observations for this variable is smaller than it is for the others.

## 5 Empirical Strategy

We employ an event study design to assess the impact of changes in RETT rates on residential property prices. All of our regressions are based on some form of the following equation:

$$\ln(p)_{i,c,t} = \sum_{j=-18}^{23} \beta_j \Delta\tau_{c,t-j} + \gamma_p + \mu_c + \varsigma_{c,t} + \epsilon_{i,c,t} \quad (7)$$

Index  $i$  refers to the property,  $c$  to the postal code area the property is located in, and  $t$  to the month it was offered for sale. The dependent variable is the log of the last asking price per square meter, i.e., the asking price observed on the last day the property was posted.<sup>13</sup> As event study indicator, we use  $\Delta\tau$  which depicts the size of the tax rate change. The event window runs from 18 months prior to the tax change to 24 months after the tax change.<sup>14</sup> Note that the generalized event study approach represented by Equation 7 accommodates setups where multiple events (here: RETT rate changes) occur during the event window, as it is the case in Berlin, Hesse, and Saarland. End points are adjusted in line with Schmidheiny and Siegloch (2023).

We control for property type fixed effects  $\gamma_p$  in all event studies estimated on the pooled sample including apartments, single-family houses and apartment buildings. Postal code area fixed effects  $\mu_c$  are included to account for time-invariant local characteristics that influence property prices.  $\varsigma_{c,t}$  is a time-fixed effect for months and years which we interact with a set of four different dummy variables indicating the degree of urbanization. Indicators for the degree of urbanization (*Siedlungsstrukturelle Kreistypen*) are provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR). Through these interaction terms, we account for the fact that property prices have experienced a stronger increase in urban areas over the last years (Baldenius et al., 2019). Standard errors are clustered at the postal code level.<sup>15</sup>

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<sup>13</sup>We also explore the effect on the number of days the properties are listed. Arguably, RETT hikes may affect the time on the market, for example because transactions are brought forward so that they take place before the tax hike. We do not find any effects.

<sup>14</sup>The final month of the event window is indicated by the number 23 as the month of the tax change is coded as 0.

<sup>15</sup>An alternative would be clustering standard errors at the state level. However, in that case, we would only have 16 clusters, which is problematic since clustered standard errors are biased in case the number of clusters is small (Cameron and Miller, 2015).

Deviating from the standard event study setting, we choose  $t - 4$  as the reference period relative to which the change in property prices is measured. We do so for two reasons. First, the price of a property offered for sale shortly before a tax reform might already reflect the upcoming tax rate change. As it may take several months to complete a property transaction, setting an earlier reference period ensures that prices are compared to a time period in which the preceding tax rate still applies. Second, the median time between the first legal draft of the bill through which a tax rate hike was implemented and the day the reform became effective amounts to 3.2 months (Table 1). The 4-month window hence ensures that the pre-treatment development of property prices is not affected by announcement effects.

In our baseline specification, we estimate Equation 7 for our pooled sample including all property types (section 6.1). We then check for heterogeneous price effects in order to test the theoretical predictions of our model. First, we test the importance of transaction frequencies for the price effects by estimating Equation 7 separately for apartments and single-family houses (section 6.2). As we will discuss below, transaction frequencies systematically vary across property types, enabling us to discern effects of varying holding periods. Second, we test for alternative mechanisms driving price responses: seller bargaining power and downpayment constraints. For the former, we exploit information in our listings data on time on the market and price discounts which serve as measures for sellers' bargaining power (Carrillo, 2013) and estimate Equation 7 separately for counties which rank high and low with regard to seller's bargaining power, respectively (section 6.3.1). Moreover, we estimate separate event studies for growing and shrinking housing market regions which may also differ in the bargaining power of the seller. For the latter, we assess heterogeneous effects by county-level business and institutional investor shares, as downpayment constraints predominately affect credit constrained households, rather than investors (section 6.3.2).

We conduct various robustness checks (section 6.4). First, we employ the change in the log net-of-tax rate as event study indicators as in Fuest et al. (2018). The coefficient estimates of these indicators measure the elasticity of property prices with respect to the net-of-tax rate  $\eta = (\frac{\Delta p}{p}) / (\frac{\Delta(1-\tau)}{1-\tau})$ . Second, we include several property characteristics that may affect property prices per square meter to our empirical model. We include the floor space, the number of rooms, dummy variables for construction year groups, as well as dummy variables indicating whether the

property comes with a kitchen, a parking spot, a garden, a balcony, or a basement (see Table 2). Third, we include regional control variables that are related to the state’s fiscal position and local property market developments. These variables include state debt per capita, the log of per-capita GDP, and the unemployment rate.<sup>16</sup>

Furthermore, we address the concern that effects in border regions may be partially driven by spillover effects. An increase in a state’s tax rate may shift demand to border regions in neighboring states, which might result in higher prices in the control group. Such a spillover effect from the treatment to the control group would represent a violation of the SUTVA assumptions for the identification of a causal treatment effect (Imbens and Rubin, 2015) and could lead to an overestimation of price effects. We test for the presence of spillover effects in two different ways. First, we estimate a specification without observations in the vicinity of a border. More precisely, we exclude postal code areas that either directly adjoin a state border, or for which the postal code’s centroid is located at a distance of up to 10 kilometers to the border. Figure 1 indicates which postal codes areas are excluded in this specification. Second, we restrict the sample to border regions and define the event as a RETT tax increase taking place in the neighboring state.

## 6 Results: The Effect of RETT Rate Hikes on Property Prices

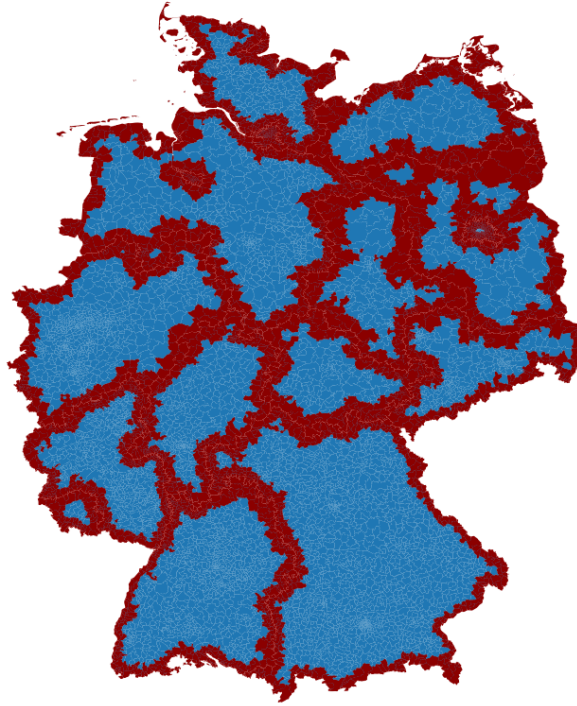
### 6.1 Price Effects for the Pooled Sample

Figure 2 displays the results for the baseline specification in which we employ the change in the RETT rate  $\Delta\tau$  as event study indicator. We estimate our empirical model based on a pooled sample including apartments, houses, and apartment buildings. The solid vertical line at  $t = -4$  indicates our reference period (i.e., the median time gap between the presentation of the first legal draft of the bill implementing the tax rate hike in the state parliament and the month the reform became effective, cf. section 5). The dashed vertical line at  $t = 0$  indicates the month of the tax rate change taking effect. Our results show that prices start to

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<sup>16</sup>The latter two variables are measured at the county-level. Note that all regional variables are only available at an annual frequency. The data are provided by the German Federal and the German States’ Statistical Offices.

Figure 1: Postal codes in the vicinity of state borders



*Notes:* This figure shows all German postal code areas, distinguished by their distance to state borders. Red areas indicate postal codes that either directly adjoin a state border or whose centroid is located at a distance of up to 10 kilometers to a border.

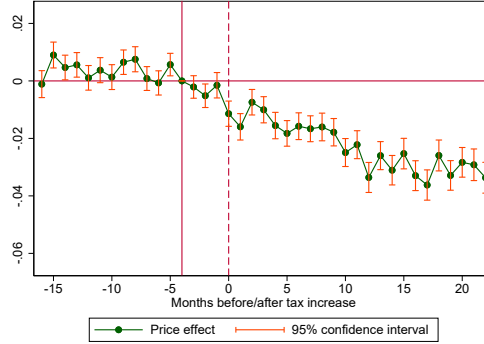
drop immediately after the tax rate hike becomes effective. There is a gradual decline in prices until the price response reaches a minimum at around  $-0.03$  after one year. This indicates that an increase in the RETT rate by one percentage point reduces prices by up to 3%, implying that the reduction in the property price exceeds the increase in the tax burden for a single transaction.<sup>17</sup>

While our results stand in contrast to previous findings for Germany, the observed overshifting is consistent with Best and Kleven (2018), Kopczuk and Munroe (2015) and Davidoff and Leigh (2013), who also find a reduction in real estate prices that exceeds the increase in the tax burden by far. In the following subsections, we investigate the role of transaction frequencies and bargaining power, two factors that may contribute to this overshifting as highlighted in our theoretical model, as well as the role of downpayment constraints (Best and Kleven, 2018).

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<sup>17</sup>We find very similar results when we estimate the same event study on a pooled sample of apartments and single-family houses, but exclude apartment buildings. Results are available upon request. In the next sub-section, we will focus on the differential price effects of single-family houses

Figure 2: Joint estimation for apartments, houses, and apartment buildings



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands for a pooled sample including apartments, single-family houses, and apartment buildings. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . The specification includes property type, postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level.  $N=17,092,188$ .

## 6.2 Price Effects by Transaction Frequency

Our theoretical model predicts that the price effect of a RETT rate change is larger when a property is expected to be traded more frequently in the future (see section 3). As described in section 4, we do not observe in the F+B listings data how often a property was listed. However, we compiled administrative data on the number of property transactions and the composition of the housing stock in Germany which indicate that transaction frequencies differ notably across single-family houses and apartments. This allows us to test the importance of the transaction frequency channel at least indirectly by estimating separate price effects for different property types.

The data that allow us to shed light on the transaction frequencies of different property types come from two sources. First, we acquired data on the universe of transactions of single-family houses and apartments in Germany for the period from 2009 to 2019. These data were provided to us by the working group of the higher-level expert committees (*Arbeitskreis der Oberen Gutachterausschüsse, Zentralen Geschäftsstellen und Gutachterausschüsse in der Bundesrepublik Deutschland, Immobilienmarktberichte Deutschland*) and are based on notarized sales contracts.

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and apartments for which, in contrast to apartment buildings, we do have detailed, administrative transaction frequencies.

They therefore constitute the most comprehensive and reliable source of property transactions in Germany.

Second, we compiled data on the universe of single-family houses as well as owner-occupied and rented apartments in Germany from the German Statistical Office for the year 2011. The data come from a complete survey of all property owners in Germany and were collected as part of the census in 2011. They provide the most up-to-date information covering the complete housing stock in Germany.<sup>18</sup> Importantly, the census data obtained by us only include dwellings that are traded freely on the market, i.e., dwellings such as public-housing, co-operative flats, residential homes and student hostels are excluded.

We proxy the transaction frequency of single-family houses and apartments by dividing the number of transactions of each property type by the corresponding stock. When doing so, we find that in 2011, 1.5% of all single-family houses and 3.2% of all apartments were sold in Germany.<sup>19</sup> This, in turn, suggests that the average holding period for single-family houses is roughly twice as large as the average holding period for apartments. Over the whole time period 2009–2019, the transaction volume of apartments is constantly higher than the transaction volume of single-family houses (cf. Figure 9 of Appendix A.3), and the relation between the transaction volumes of both property types remained fairly stable, which indicates that the average holding period for apartments is considerably lower than for single-family houses during our whole sample period. One explanation might be that single-family houses are mainly bought by families who plan to live in the property for many years (and may even have a bequest motive), while apartments (and apartment buildings) are more frequently rented out or bought by institutional investors who often resell the property at some point in time.<sup>20</sup>

We then estimate the event study specification separately for apartments and

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<sup>18</sup>The next census collecting information on the housing stock in Germany was carried out in 2022. However, at the time of writing, the data was not yet published.

<sup>19</sup>According to the data from the working group of the higher-level expert committees, 236,100 single-family houses and 287,700 apartments were sold in 2011. According to census data, the number of single-family houses was around 15.6 million and the number of owner-occupied and rented apartments 9 million in 2011.

<sup>20</sup>As shown by Table 8 in the Appendix, only 12% of privately owned houses are owned for investment purposes and rented out, while 58% of privately owned apartments are rented out. Moreover, the 2011 census data reveal that the apartment share at the county-level is positively associated with the share of institutional and business investors at the county-level. The correlation coefficient amounts to 0.53.



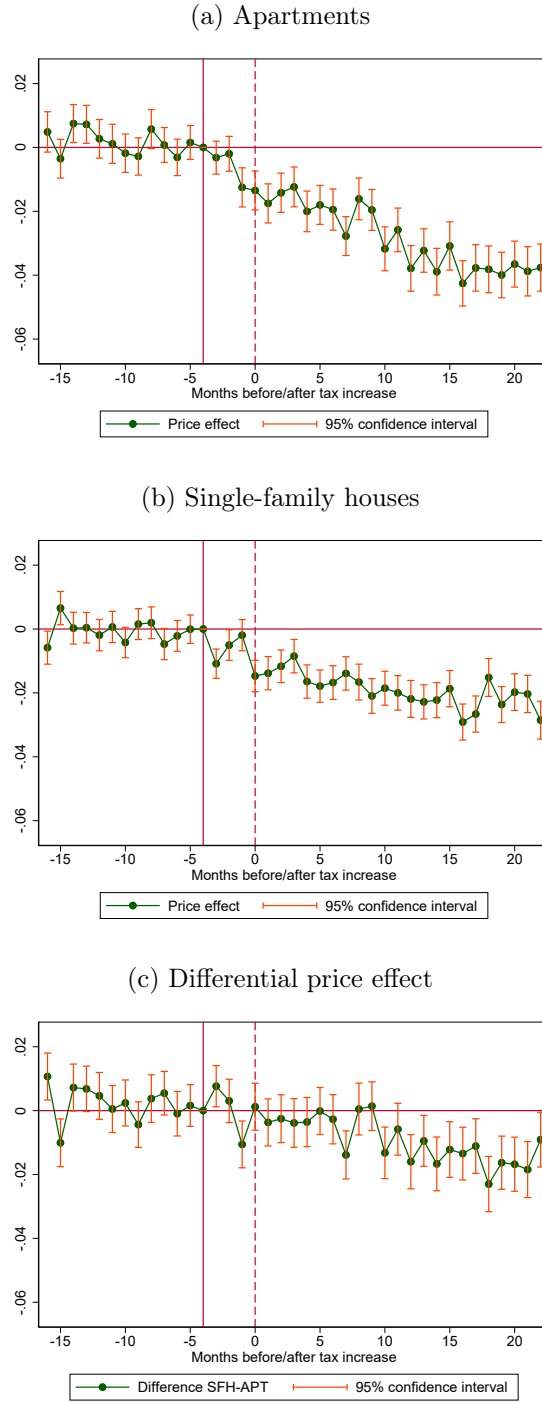
single-family houses in order to empirically test the theoretical prediction of our model. The results are presented in Figure 3. In line with the prediction of our model, we find larger price effects for apartments than for single-family houses. For apartments, the price response reaches a minimum of roughly  $-0.04$  (see panel a) which suggests that an increase in the RETT rate by one percentage point reduces the prices of apartments by up to 4%. In contrast, with a minimum coefficient of roughly  $-0.02$ , the price response is smaller for single-family houses for whom an increase in the tax rate by one percentage point leads to a price reduction of 2% (see panel b).

These differential price effects are in line with the predictions from our theoretical model. According to Figure 8 of Appendix A.2 – which illustrates the relationship between the annual transaction frequency and the semi-elasticity  $\varepsilon$  of the RETT based on the results of our theoretical model –, our model predicts that an increase in the transaction frequency from 1.5% (single-family houses) to 3.2% (apartments) shifts the semi-elasticity of the RETT from  $-1.4$  to  $-3.1$ . These predicted values are somewhat smaller (in absolute terms) than our event study estimates, but still close. Panel c of Figure 3 reveals that the differential price effect is also statistically significant from month 10 after the tax rate change onwards until the end of the event window.

Moreover, we observe that prices start to react already prior to the reform. This may reflect anticipation effects. If a property is offered for sale shortly before the RETT rate change becomes effective, it is unlikely that the transaction will be completed before the implementation date, implying that the higher RETT rate will apply. Therefore, we already observe a decrease in property prices before the implementation of the reform.

In the next subsection, we analyze two alternative mechanisms that may also affect the price effect of a RETT rate change: the bargaining power of the seller and downpayment constraints. The analysis of these alternative mechanisms will demonstrate that all our evidence points to transaction frequencies as the main channel underlying the differential price effects found for apartments and single-family houses.

Figure 3: Effects of changes in the RETT rate  $\Delta\tau$  across property types



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. a)  $N=7,618,910$  (apartments). b)  $N=8,644,239$  (single-family houses). c)  $N=16,263,132$  (differential price effect).

## 6.3 Alternative Mechanisms

Two alternative explanations could conceivably affect the price effects of RETT rate changes: the strength of sellers' bargaining power and the prevalence of downpayment constraints. This section investigates to what extent these alternative channels may underlie the differential overshifting of the tax burden into prices between the two property types. Moreover, we explore heterogeneous price effects across regions which differ in terms of sellers' bargaining power and the prevalence of downpayment constraints.

### 6.3.1 Bargaining Power

Our theoretical model predicts that the distribution of bargaining power between sellers and buyers is a key determinant of the magnitude of the price response. Since bargaining power is not directly observable, we use two approaches to proxy the bargaining power of buyers and sellers. In the first approach, we follow Carrillo (2013) and compute a measure of bargaining power exploiting information in our listings data on time on the market (number of days a property was posted) and price discount (percentage difference between first and last asking price) in the pre-reform years when the RETT rate was uniform across states. We hypothesize that the shorter the time on the market and the smaller the price discount, the stronger one can expect the bargaining power of the seller to be.

One should note that the theoretical model introduced in section 3 is static and therefore does not include variables like time spent on the market or price discounts. The bargaining power parameter  $\beta$  is a summary indicator of how the surplus from a transaction is divided between the buyer and the seller. Using the time a property is on the market as a proxy for  $\beta$  is correct, for instance, if sellers get desperate over time and make more concessions or sellers disregard the fact that leaving a property on the market for a long time may signal to buyers that it is easy to get. Of course, a long time on the market may also be the result of sellers making no concessions at all. Similarly, price discounts may reflect that sellers make concessions to buyers, but they may also reflect that the first asking price was set at a very high level. These limitations should be taken into account in the interpretation of the results.

We use this first approach to investigate whether the differential price effects of apartments and single-family houses documented in the previous section could also be driven by sellers of apartments having a higher bargaining power than sellers

Table 4: Time on the market and price discounts in the pre-reform period

	Single-family houses	Apartments
Average time on the market	111	134
Average price discount	1.3%	1.6%

*Notes:* This table shows the average number of days single-family houses and apartments were listed as well as the average price discount (percentage difference between first and last asking price in the F+B data) in the two pre-treatment years 2005–2006.

of single-family houses. In order to do so, we calculate mean values of time on the market and price discount during the pre-treatment years 2005–2006 separately for apartments and single-family houses. Table 4 shows that on average single-family houses were 23 days shorter on the market and their price discount was 0.3 percentage points smaller compared to apartments.<sup>21</sup> These numbers suggest that the larger price effects of apartments compared to single-family houses are likely not driven by a higher bargaining power of sellers of apartments.

Second, we study whether there are differential price effects for apartments and single-family houses in regions with a high and a low degree of seller bargaining power, respectively. For each of the 401 counties in Germany, we calculate mean values of time on the market and price discount during the pre-treatment years 2005–2006, construct an index based on these two (equally-weighted) variables and rank the counties according to this index. In the next step, we estimate our event study specification separately for counties with a high and a low degree of sellers’ bargaining power. That is, we pool listings from the top and bottom quartile of the county ranking. Figure 4 shows that in line with the predictions derived from our theoretical model, we observe larger price effects – both for apartments (panels a and b) and single-family houses (panels c and d) – in counties ranked in the top quartile of sellers’ bargaining power. Price responses in the bottom quartile of sellers’ bargaining power are mostly insignificant. In other words, in line with our model, we only observe the seller to bear the burden of the RETT in markets where her bargaining power is expected to be high.

One might object that transaction frequencies may vary across regions that differ with regard to sellers’ bargaining power so that ultimately, it remains unclear which of the two factors is actually driving the differential price effects. To address this concern, we compare transaction frequencies of apartments and single-family

<sup>21</sup>We find similar patterns if we calculate average values over the whole sample period 2005–2019.

houses in counties ranked in the top and the bottom quartile of sellers' bargaining power, respectively, in order to explore potential interaction effects of the distribution of bargaining power of buyers and sellers with transaction frequencies. For apartments, the average transaction frequency in 2011, the year for which we have both transaction numbers and information on the housing stock from the census, amounts to 2.7% in the bottom and to 3.6% in the top quartile of sellers' bargaining power. The fact that transaction frequencies of apartments are higher in the top quartile of sellers' bargaining power implies that the larger price effects in that quartile are possibly driven both by sellers having higher bargaining power and by higher transaction frequencies. In contrast, for single-family houses, we observe similar transaction frequencies in the top (1.6%) and the bottom quartile (1.5%) of sellers' bargaining power, suggesting that the larger price effects in the top quartile are likely driven by the bargaining power channel.

In the second approach, we compare price effects across housing market regions which differ in their demographic and socio-economic structures and, hence, very likely also in the way how bargaining power is distributed between buyers and sellers. The BBSR classifies housing market regions as 'strongly growing', 'growing', 'with no clear trend', 'shrinking', or 'strongly shrinking', depending on the realizations of a number of indicators.<sup>22</sup> In particular, growing (shrinking) housing market regions are characterized by relatively high demand for (supply of) apartments and houses. We interpret this as going along with a higher bargaining power for sellers (buyers) (Bundesinstitut für Bau-, Stadt- und Raumforschung, 2015).

In our posting data, most listings are in growing and strongly growing regions (79% of single-family houses and 86% of apartments). For the comparison of growing and shrinking housing market regions, we therefore pool strongly shrinking and shrinking regions with regions which do not exhibit a clear trend, and compare price effects to those in growing and strongly growing regions. As shown in Table 5, growing housing market regions where sellers' bargaining power is supposed to be higher are characterized by smaller discounts (single-family houses) and shorter

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<sup>22</sup>These are a) the average yearly population growth (in %), b) the average yearly total migration balance (per 1,000 inhabitants), c) the average yearly change in the working age population (i.e., persons between 20–64 years; in %), d) the average yearly change in the number of employees subject to social insurance contributions (in %), e) the average yearly change in the unemployment rate (in percentage points), and f) the average yearly change in local business tax revenue per inhabitant (in %). Most of these indicators are measured over the period 2011–16 which is only a sub-period of our sample period. However, a comparison with previous years reveals that there are relatively little changes in the classifications over time.

Table 5: Time on the market and price discounts in the pre-reform period and transaction frequencies by housing market regions

	Single-family houses	Apartments
<u>Shrinking Housing Market Regions</u>		
Average time on the market	122	147
Average price discount	1.6%	1.6%
Transaction frequencies	1.5%	2.5%
<u>Growing Housing Market Regions</u>		
Average time on the market	109	133
Average price discount	1.3%	1.6%
Transaction frequencies	1.6%	3.3%

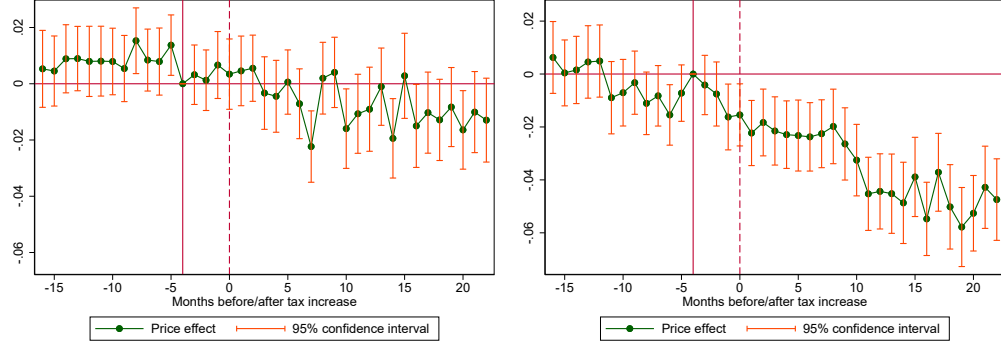
*Notes:* This table shows the average number of days single-family houses and apartments were listed and the average price discount (percentage difference between first and last asking price in the F+B data) in the two pre-treatment years 2005–2006 as well as average 2011 transaction frequencies by housing market region. Shrinking housing market regions include the BBSR classifications 'strongly shrinking', 'shrinking', and 'with no clear trend'. Growing housing market regions include the BBSR classifications 'growing' and 'strongly growing'. Looking at the whole sample period (2005–2019), the average time on the market for single-family houses (apartments) is 105 (123) days in shrinking and 95 (113) days in growing housing market regions. The average price discount for single-family houses (apartments) is 1.2% (1.2%) in shrinking and 0.9% (1.1%) in growing housing market regions.

posting periods (apartments and single-family houses), which is in line with our first approach.

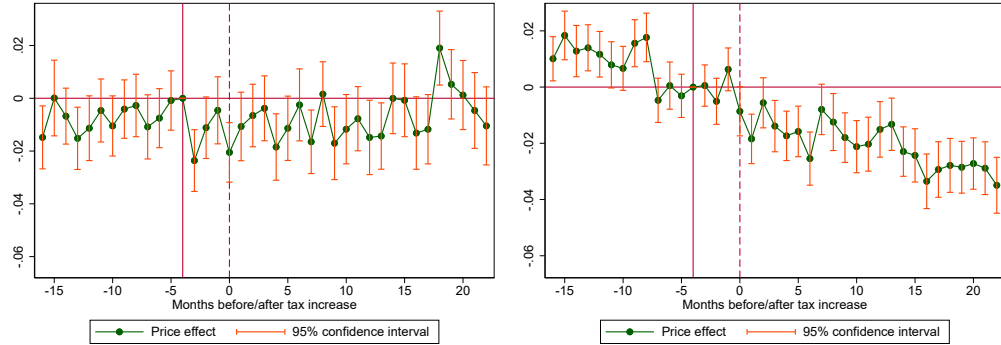
Similar to our first approach, we estimate our event study specification separately for shrinking and growing housing market regions. Results are shown in Figure 5. Again, we find larger price effects in regions where the bargaining power of the seller is supposed to be higher. While transaction frequencies of apartments are higher in growing than in shrinking housing market regions, they do not differ much for single-family houses. These findings indicate that similar to the analysis focusing on the top and bottom quartile of sellers' bargaining power, differential price effects in growing and shrinking housing market regions can be attributed to both the transaction frequency and the bargaining power channel in case of apartments, but only to the bargaining power channel in case of single-family houses.

Figure 4: Effects of changes in the RETT rate  $\Delta\tau$  across counties

(a) Bottom quartile of seller's bargaining power (apartments) (b) Top quartile seller's bargaining power (apartments)



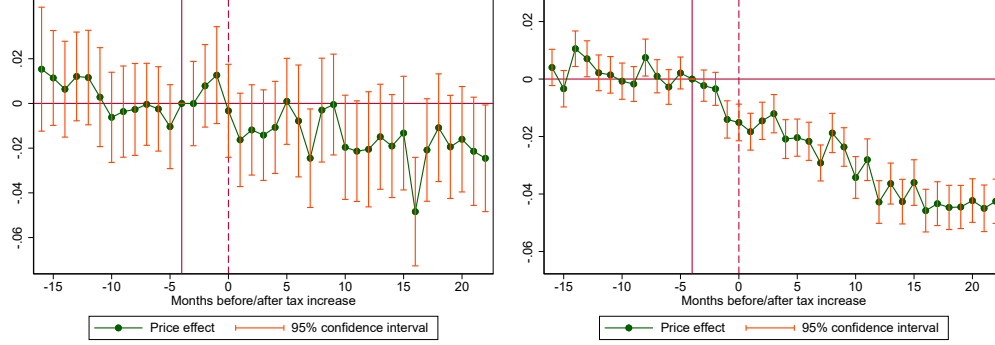
(c) Bottom quartile seller's bargaining power (single-family houses) (d) Top quartile seller's bargaining power (single-family houses)



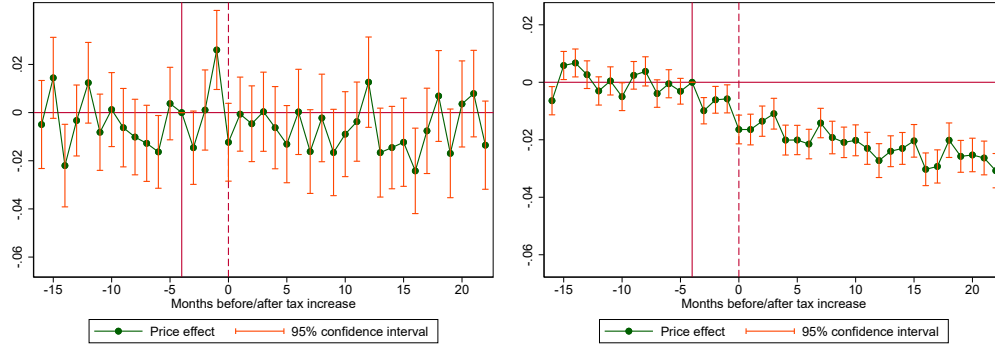
*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands for properties across counties in the top and bottom quartile of the county ranking of seller's bargaining power. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. Panel (a): N=1,851,879. Panel (b): N=1,924,076. Panel (c): N= 2,031,967. Panel (d): N=2,281,597.

Figure 5: Effects of changes in the RETT rate  $\Delta\tau$  across housing market regions

(a) Shrinking Housing Market Regions (apartments) (b) Growing Housing Market Regions (apartments)



(c) Shrinking Housing Market Regions (single-family houses) (d) Growing Housing Market Regions (single-family houses)



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands for properties across shrinking and growing housing market regions. Shrinking housing market regions encompass strongly shrinking and shrinking regions as well as those with no clear trend. Growing housing market regions encompass growing and strongly growing housing market regions. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. Panel (a):  $N=925,484$ . Panel (b):  $N=6,692,988$ . Panel (c):  $N=1,421,605$ . Panel (d):  $N=7,221,060$



### 6.3.2 Downpayment Constraints

Best and Kleven (2018) provide an alternative explanation for an overshifting of transfer taxes into property prices. Their theoretical and empirical analysis shows that prices respond more than one-for-one if households are liquidity constrained and highly leveraged: if households cannot borrow to pay the tax, stronger price effects arise. We undertake several steps to test the relevance of the liquidity channel and to disentangle its effect on the semi-elasticity of the RETT from the transaction frequency channel we are interested in.

In a first step, we analyze descriptive microdata on property ownership from the German Income and Consumption Panel (*Einkommens- und Verbrauchsstichprobe 2018*; EVS) to assess whether mortgage spending differs across property types. The EVS provides a representative overview of the finances of German households. We find no indication that apartment owners are more credit constrained than owners of houses (see Appendix Table 8 for details). Conditional on owning at least one property, those who own a house spend on average 8.4% of their net household income on mortgage payments, while those who own an apartment spend 8.6%. 48% of house and apartment owners have a mortgage, respectively. These numbers suggest that the larger price effects for apartments are likely not driven by stronger credit constraints of apartment buyers.

While the EVS looks at the housing stock, a similar picture emerges for new mortgages. Mortgage data from the Association of German Pfandbrief Banks, whose member banks have a market share of roughly 40% of German mortgages, indicates no substantial differences in leverage between apartments and houses (Verband der Pfandbriefbanken, 2020), with average leverage rates of 83% for apartments and 81% for houses for new mortgages in 2019.<sup>23</sup>

Second, our analysis differentiates between households on the one hand and businesses and institutional investors such as housing firms or real estate investment trusts on the other hand. Arguably, downpayment constraints are particularly relevant for liquidity-constrained households who cannot borrow to pay the tax, especially if they do not possess any other assets to be used as collateral for a loan. In contrast, businesses and institutional investors, who usually possess enough

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<sup>23</sup>Similarly, only small differences arise by degree of urbanization: both rural areas and major cities display an average leverage rate of 80%. As the VdP only reports information on owner-occupiers with a mortgage, we can neither infer information on investors nor on households who purchase without a mortgage.

equity or collateral to secure a loan or who can issue bonds to finance real estate investments, should not be as much affected by liquidity constraints. If the observed overshifting was driven by downpayment constraints, one would thus expect stronger price effects in markets with a lower share of institutional investors.

Following section 6.3.1, we test this channel by separately estimating our event study for apartments and houses in counties in the bottom and top quartile of institutional and business investor shares. This classification is based on county-level administrative ownership shares in all residential property according to the census 2011. As shown in Figure 6, apartment prices drop faster in markets with high institutional investor shares, with prices in low institutional investor share counties partly catching up a year after the tax increase. The slightly larger pass-through in the medium run in the top compared to the bottom quartile - albeit the differential effect not being statistically significant - may again be driven by higher transaction frequencies in markets with higher institutional investor shares. Furthermore, the larger anticipation effects and swifter pass-through for apartments may be attributable to professional investors having a better overview of market developments and hence responding faster to tax increases. In contrast, the pass-through for houses appears to be uncorrelated with investor shares. These findings run counter to the initial hypothesis: if downpayment constraints lead to a larger pass-through into prices, we would expect to see a lower price effect in markets with high investor shares rather than a higher, or a zero effect. Taken together, these findings indicate that the differential effects between houses and apartments in Section 6.2 cannot be explained by liquidity constraints, but rather by the consideration of expected transaction frequencies.

## 6.4 Robustness Checks

To check the robustness of our results, we modify our empirical specification in several additional ways. Results for apartments and single-family houses as well as the differential price effect for the two property types are displayed in Appendix A.4 and should be compared to the baseline results presented in section 6.2. As a first robustness check, we replace the change in the RETT rate by the log net-of-tax rate. A glance at Figure 10 reveals that our results remain qualitatively unchanged. For apartments, the estimated price elasticity with respect to the log net-of-tax rate reaches a maximum of 4, while the estimated elasticity for houses is roughly 2. The

differential price effect shown in panel c) of Figure 10 is significantly different from 0 from month 10 after the RETT rate change onwards.

Second, we add property-specific control variables to Equation 7. Specifically, we control for the floor space in square meters, the number of rooms, construction year categories, as well as various amenities (see Table 2). Controlling for property characteristics ensures that our findings are not driven by composition effects. This concern would be relevant if a change in the RETT has an effect on the pool of properties that are offered for sale. As shown in Figure 11, the coefficients' magnitudes slightly decrease, while the differential price response across property types persists.

In a third robustness check, we add control variables to our empirical model that cover local housing market conditions (per-capita GDP, the unemployment rate) and the state's fiscal space (state debt per capita). The results are illustrated in Figure 12. Again, we find somewhat smaller price responses compared to our baseline specification, but results remain qualitatively unchanged.

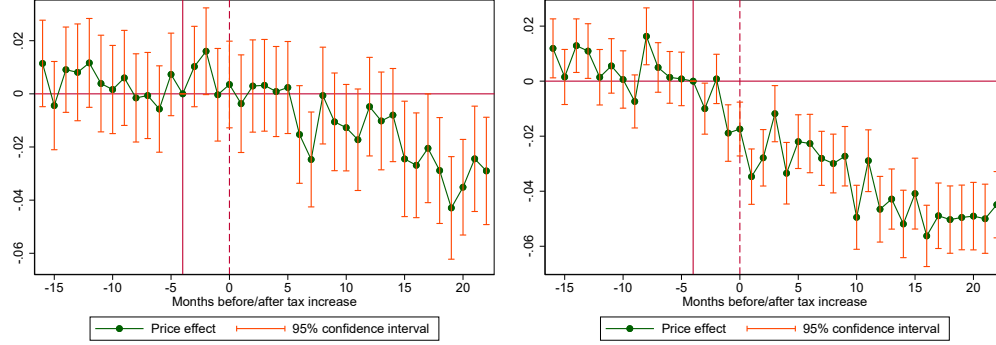
In a final robustness check, we investigate to what extent our results may be driven by spillover effects. The existence of spillover effects would constitute a violation of the SUTVA assumptions for causal inference and may result in biased estimates of the price effects (Imbens and Rubin, 2015). We test for the existence of spillover effects in two different ways. Our first approach is to exclude properties located in postal code areas in the vicinity of a state border, as described in section 5. The reason is that there may be spillover effects of RETT changes into regions that are located close to the border of a state that has implemented the RETT change. Suppose there is a region located in state A bordering state B. If state B increases the RETT, but state A does not, we may observe an increase in the demand for properties located in that region because of its proximity to state B. Figure 13 shows the results. While the estimated price effect for single-family houses is virtually identical to the one obtained in our baseline estimation, the magnitude of coefficient estimates slightly increases for apartments. This alleviates the concern that the rather large effect measured in the baseline specification is attributable to spillover effects into border regions of tax-increasing states.

Our second approach is a more direct test for spillover effects. We estimate an event study specification for border regions with the event being a RETT rate hike in the neighboring state, provided that there is no change in the RETT rate

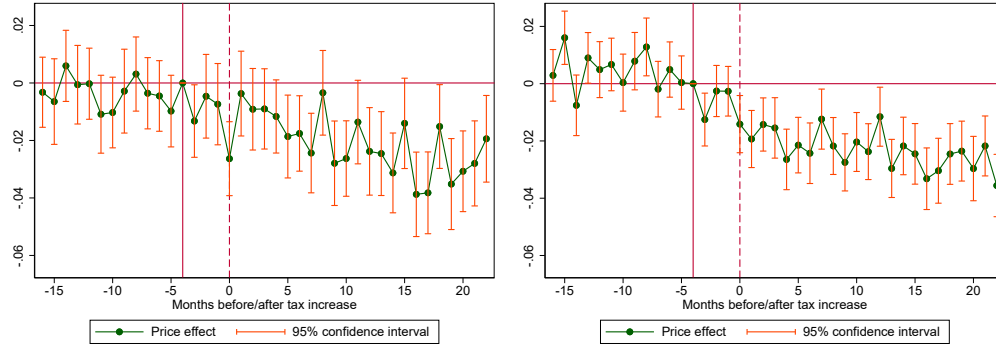
during the event window in the state the postal code area is located in. That is, we exclusively focus on those postal code areas close to state borders that were excluded in the first approach. Moreover, we vary the maximum distance to the border and estimate specifications with maximum distances of either 10 or 20 kilometers to the border. Significant treatment effects would indicate that RETT rate changes in the neighboring state affect property prices in close proximity to the tax-increasing state. We do not find any evidence for spill-over effects as all event study coefficients are insignificant (results available upon request).

Figure 6: Effects of changes in the RETT rate  $\Delta\tau$  by investor shares

- (a) Bottom quartile of investor share (apartments) (b) Top quartile of investor share (apartments)



- (c) Bottom quartile of investor share (single-family houses) (d) Top quartile of investor share (single-family houses)



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands for properties across counties in the top and bottom quartile of 2011 business and institutional investor ownership shares. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. Panel (a):  $N=1,510,757$ . Panel (b):  $N=3,080,142$ . Panel (c):  $N=2,263,741$ . Panel (d):  $N=2,179,610$ .

## 7 Conclusion

This paper proposes a stylized theoretical model of the housing market and tests its predictions exploiting variation in RETT rates across German states and over time. One of the main insights of the model is that an increase in the RETT rate may result in a decline in property prices that exceeds the tax increase. Our model predicts that the semi-elasticity of house prices with respect to the RETT may be larger than one if the bargaining power of the seller is high and if a property is expected to be traded frequently in the future. In the empirical analysis, we make use of a large micro dataset covering roughly 17 million properties (apartments, single-family houses, apartment buildings) offered for sale over the period 2005 to 2019 and employ an event study design to estimate the effects of RETT rate hikes on property prices.

Our findings lend support to our theoretical model and have important policy implications. We find that a one percentage point increase in the RETT reduces prices in the pooled sample by 3% in the 12 months after the reform. Price effects are larger for apartments (-4%) than for single-family houses (-2%). These results confirm a positive association between the magnitude of the price effect and the expected transaction frequency of a property as average holding periods are higher for single-family houses than for apartments. We further show that negative price effects are predominantly found in counties where properties sell quickly and price discounts are small, as well as in growing housing market regions. This indicates that the tax burden of the RETT mainly falls on the sellers, especially if their bargaining power is high. Our results are robust to several modifications to our empirical specifications. In terms of policy implications, our results suggest that making greater use of real estate transaction taxes does not make housing less affordable for first time buyers because tax hikes are capitalized in property prices. Accordingly, tax cuts may give rise to windfall profits for property sellers.

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## Appendix

### A.1 Determinants of RETT Increases

Table 6: State-level determinants of RETT increases

	(1)	(2)	(3)	(4)	(5)	(6)
Leftwing government (t-1)	0.264*** (0.081)	0.232** (0.081)	0.264*** (0.084)	0.262** (0.122)	0.243** (0.086)	0.258*** (0.083)
Center government (t-1)	0.190* (0.104)	0.182* (0.098)	0.193* (0.102)	0.161* (0.080)	0.168 (0.108)	0.189* (0.103)
Degree of fiscal redistribution (t-1)	1.292*** (0.397)	1.321*** (0.394)	1.285*** (0.392)	1.639*** (0.522)	1.277*** (0.368)	1.306*** (0.383)
Share rightwing voters in federal elections	0.018 (0.015)	0.022 (0.016)	0.022 (0.014)	0.012 (0.021)	0.022 (0.016)	0.018 (0.016)
Debt per capita tsd (t-1)	2.309 (18.179)	1.573 (17.770)	1.296 (18.203)	-20.637 (35.462)	7.435 (19.180)	2.232 (18.153)
Shared taxes per capita (t-1)	0.051 (0.100)	0.031 (0.102)	0.042 (0.098)	-0.033 (0.162)	0.051 (0.100)	0.052 (0.104)
Population growth (t-1)	0.048 (0.074)					
Migration balance (t-1)		-0.026 (0.019)				
Working-age population growth (t-1)			0.060 (0.056)			
Growth in socially insured employees (t-1)				0.028 (0.085)		
Change in unemployment rate (t-1)					-0.179 (0.151)	
Local business tax revenue growth (t-1)						0.001 (0.004)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	176	176	176	128	176	176
R2 within	0.273	0.280	0.276	0.265	0.283	0.272
R2 between	0.162	0.082	0.203	0.002	0.161	0.150
R2	0.127	0.101	0.121	0.069	0.128	0.127

*Notes:* This table shows results of a state-level regression of tax rate changes on lagged explanatory variables at the state level between 2006-2016 (2009-2016 for the specification shown in column 4 as the growth in the number of socially insured employees is not available for earlier years). Following Krause and Potrafke (2020), leftwing government is an indicator for state governments composed of either SPD, SPD/Greens, Greens/SPD, SPD/LINKE, SPD/LINKE/Greens, SPD/FDP/Greens or LINKE/SPD/Greens, and center government is an indicator for CDU/SPD, SPD/CDU, CDU/Greens, CDU/SPD/Greens, CDU/Greens/FDP, Greens/CDU. The reference category encompasses state governments by CDU, CSU, CDU/FDP or CSU/FDP. The degree of fiscal redistribution captures state incentives through the fiscal equalization scheme to raise taxes, based on Büttner and Krause (2021). The share of rightwing voters in federal elections refers to the voting shares of CDU, CSU, FDP and AfD. Shared taxes per capita encompass state revenues from taxes with a revenue split between the federal and state level, i.e., states' tax revenues from taxes for which states cannot set the tax rate. These include income and corporate taxes, the withholding tax and VAT. Levels of significance: \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

## A.2 Theoretical Model: General Case

In this appendix we show that  $\frac{\delta \varepsilon}{\delta \beta} > 0$  and  $\frac{\delta \varepsilon}{\delta q} < 0$ , as claimed in the main text.

From equation (6) we can derive

$$\begin{aligned} \frac{\delta \varepsilon}{\delta \beta} = & - \left[ 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right]^{-2} \frac{q}{(1 + \rho)} \left[ \frac{\beta U^{OL}}{p^*} - 1 \right] \\ & + \left[ 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right]^{-1} \frac{U^{OL}}{p^{*2}} \left[ p^* - \beta \frac{\delta p^*}{\delta \beta} \right] \end{aligned} \quad (\text{A.1})$$

Note that the first term on the right hand side of equation (A.1) is positive because  $\frac{\beta U^{OL}}{p^*} - 1 < 0$ . The second term on the right hand side of equation (A.1) is also positive because the price declines with increasing bargaining power of the buyers, i.e.

$$\frac{\delta p^*}{\delta \beta} = - \left[ 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right]^{-1} \left[ \frac{p^* q}{(1 + \rho)} - U^{OL}(1 + T) + U^Y - q \frac{U^{OL}}{(1 + \rho)} + (1 - q) \frac{U^{OH}}{(1 + \rho)} \right] < 0 \quad (\text{A.2})$$

From equation (6) we can also derive

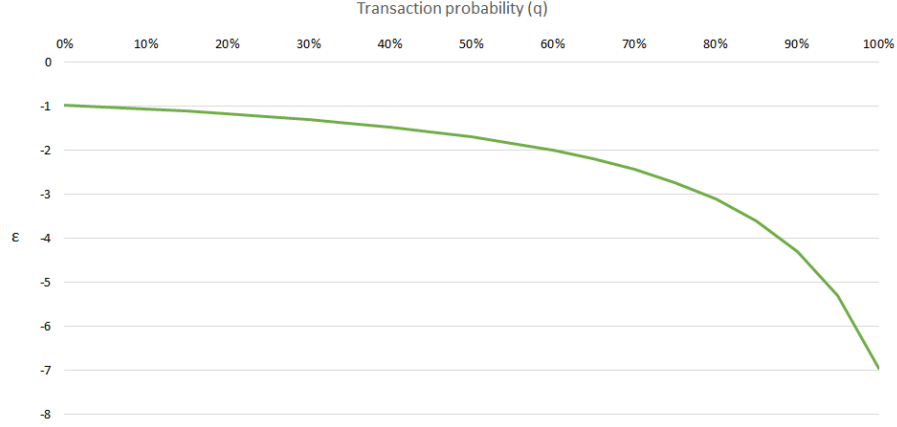
$$\begin{aligned} \frac{\delta \varepsilon}{\delta q} = & \left[ 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right]^{-2} \frac{(1 - \beta)}{(1 + \rho)} \left[ \frac{\beta U^{OL}}{p^*} - 1 \right] \\ & - \left[ 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right]^{-1} \frac{\beta U^{OL}}{p^{*2}} \frac{\delta p^*}{\delta q} \end{aligned} \quad (\text{A.3})$$

The first term on the right hand side of equation (A.3) is negative because  $\frac{\beta U^{OL}}{p^*} - 1 < 0$ . The second term is also negative because

$$\frac{\delta p^*}{\delta q} = \left[ 1 + T - \frac{(1 - \beta)q}{(1 + \rho)} \right]^{-1} \frac{(1 - \beta)}{(1 + \rho)} (p^* - (U^{OL} + U^{OH})) > 0. \quad (\text{A.4})$$

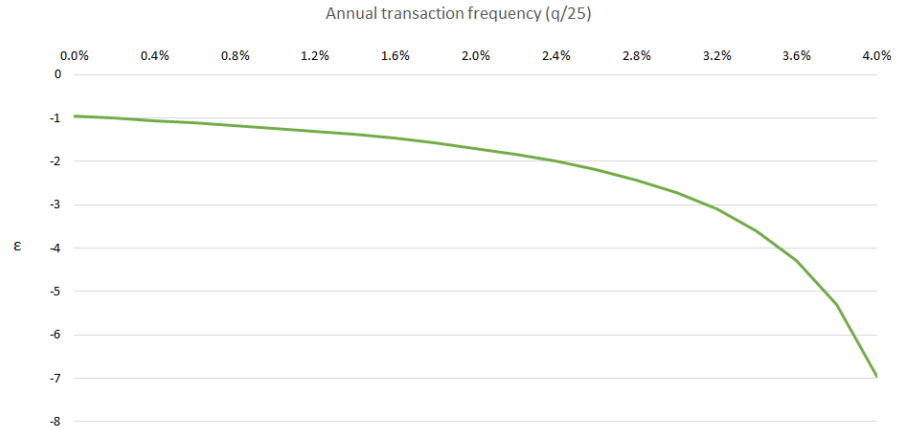
q.e.d.

Figure 7: Relationship between the tax semi-elasticity  $\epsilon$  and transaction probability  $q$



*Notes:* The figure illustrates the relationship between the semi-elasticity  $\epsilon$  of the real estate transfer tax (y-axis) and the transaction probability  $q$  (x-axis). Realizations of the other model parameters are chosen as follows:  $T = 0.035$ ,  $\rho = 0.01$ ,  $\beta = 0.1$ ,  $U^{OH} = 150000$ ,  $U^{OL} = 0$ ,  $U^Y = 100000$ .

Figure 8: Relationship between the tax semi-elasticity  $\epsilon$  and annual transaction frequency  $\frac{q}{25}$



*Notes:* The figure illustrates the relationship between the semi-elasticity  $\epsilon$  of the real estate transfer tax (y-axis) and the annual transaction frequency  $\frac{q}{25}$  (x-axis). Realizations of the other model parameters are chosen as follows:  $T = 0.035$ ,  $\rho = 0.01$ ,  $\beta = 0.1$ ,  $U^{OH} = 150000$ ,  $U^{OL} = 0$ ,  $U^Y = 100000$ .

### A.3 Additional descriptive statistics

Table 7: Real estate data: Summary statistics by data source

	Apartments		Single-family Houses		Apartment Buildings	
	IS24	Other sources	IS24	Other sources	IS24	Other sources
First price	2162	2012	1840	1731	1280	1177
Last price	2149	2003	1828	1721	1269	1171
Days posted	110	118	96	97	101	102
Floor size	94.7	98.1	149.6	154.8	311.5	339.0
Rooms	3.2	3.0	5.3	4.8	10.4	7.4
Constr. year	1979	1980	1983	1978	1955	1955
Kitchen	0.38	0.33	0.15	0.27	0.11	0.23
Parking spot	0.67	0.60	0.62	0.58	0.68	0.63
Garden	0.28	0.22	0.32	0.40	0.31	0.36
Balcony	0.48	0.38	0.29	0.35	0.31	0.35
Basement	0.50	0.33	0.46	0.35	0.51	0.35
Obs. (max.)	3,420,820	4,198,090	3,812,550	4,831,689	306,565	522,474

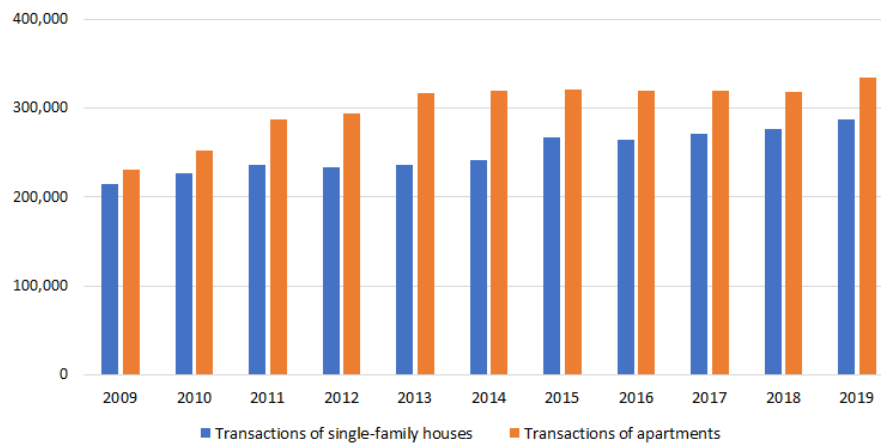
*Notes:* The table shows the average realizations of different property characteristics for different property types separately for properties listed on immobilien Scout24.de (IS24) vs. properties listed in other sources. Asking prices refer to the price per square meter. Discount is the difference between the first and the last asking price of a property (measured in percent of first price). Floor space is measured in square meters.

Table 8: Financing of privately owned real estate

	Houses	Apartments
Share owned for investment purposes	12%	58%
Share of properties with a mortgage	48%	48%
Average mortgage payments relative to net household income	8.4%	8.6%

*Notes:* This table shows summary statistics on financing of privately-held real estate in Germany based on administrative survey data from the Einkommens- und Verbrauchsstichprobe 2018 (EVS), RDC of the Federal Statistical Office and Statistical Offices of the Federal States. The share owned for investment purposes indicates the share of privately-held properties that are rented out rather than owner-occupied.

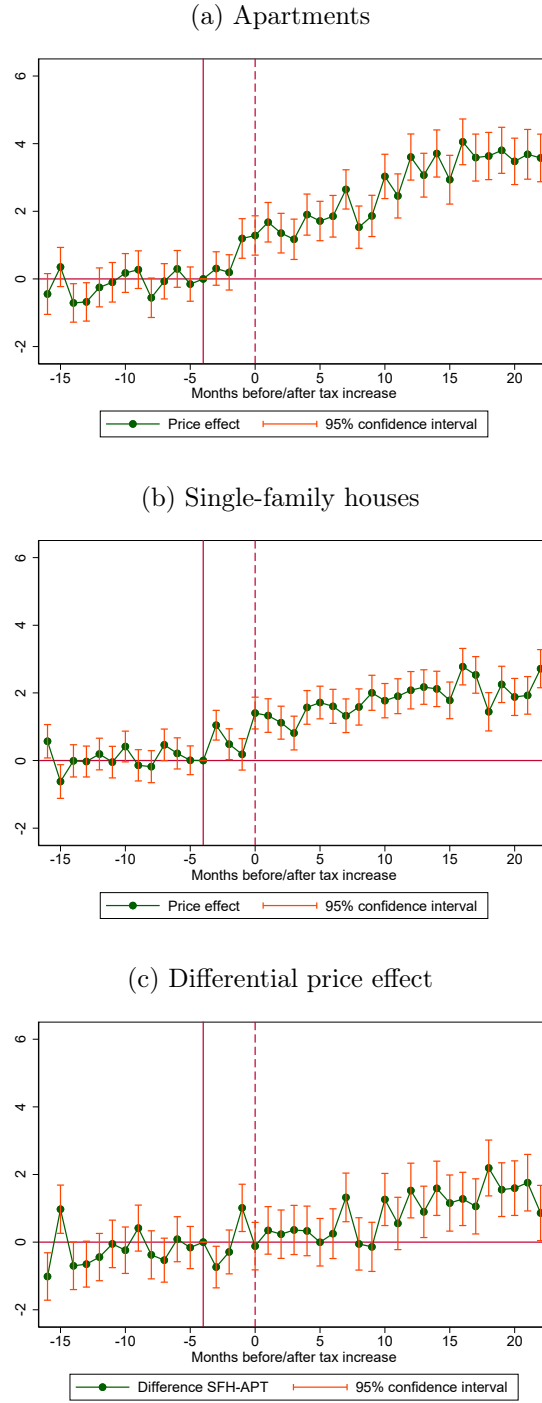
Figure 9: Number of Transactions of Single-Family Houses and Apartments in Germany 2009-2019



*Notes:* The figure shows the number of transactions of single-family houses (blue bars) and apartments (orange bars) in Germany per year over the period from 2009–2019. The data come from the working group of the higher-level expert committees (*Arbeitskreis der Oberen Gutachterausschüsse, Zentralen Geschäftsstellen und Gutachterausschüsse in der Bundesrepublik Deutschland, Immobilienmarktberichte Deutschland*).

## A.4 Robustness checks

Figure 10: Robustness check: Effects of changes in the log net-of-tax rate



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the log net-of-tax rate. Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. a)  $N=7,618,910$  (apartments). b)  $N=8,644,239$  (single-family houses). c)  $N=16,263,132$  (differential price effect).

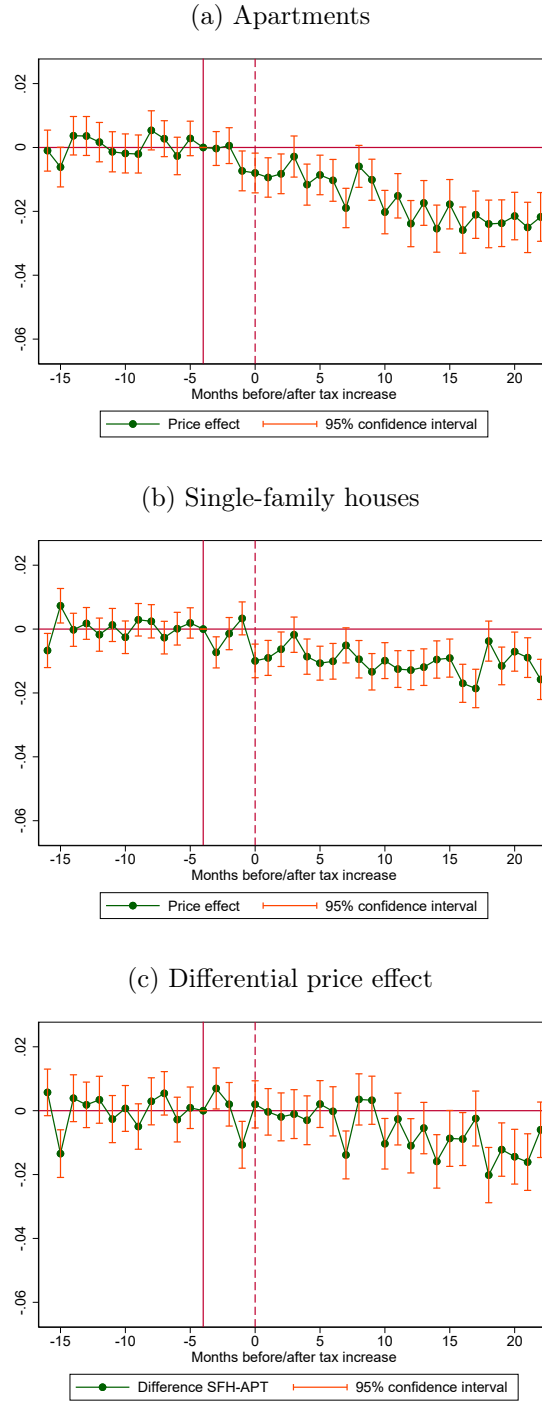


Figure 11: Robustness check: Property-specific control variables



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. Specifications control for area in square meter, the number of rooms, the construction year, as well as whether the property has a basement, a parking spot, a garden, and a kitchen. a)  $N=7,618,910$  (apartments). b)  $N=8,644,239$  (single-family houses). c)  $N=16,263,132$  (differential price effect).

Figure 12: Robustness check: Regional control variables



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. Specifications control for annual county-level GDP, the unemployment rate, and state debt per capita. a)  $N=7,618,910$  (apartments). b)  $N=8,644,239$  (single-family houses). c)  $N=16,263,132$  (differential price effect).

Figure 13: Robustness check: Without postal codes within 10 km of the border



*Notes:* The figure plots monthly event study estimates and corresponding 95% confidence bands. The dependent variable is a property's log price per square meter, and event study indicators correspond to the change in the tax rate  $\Delta\tau$ . Specifications include postal code and month-year  $\times$  urbanization level fixed effects. Standard errors are clustered at the postal code level. Specifications exclude postal codes that either directly adjoin a border or whose centroid is located at a distance of up to 10 kilometers to the border. a)  $N=4,931,489$  (apartments). b)  $N=5,522,226$  (single-family houses). c)  $N=10,453,706$  (differential price effect).