Pricing in the Taxman: Corporate Tax Incidence and Commercial Real Estate*

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Abstract

This paper presents novel estimates on the incidence of corporate taxes by measuring the effect of local business tax increases on the welfare of commercial landowners. We use unique data on commercial real estate prices in Germany covering 1.1 million properties offered for sale and over 3 million properties offered for rent between 2008 and 2019. Empirically, we exploit the German institutional setting with over 17,000 municipal tax changes using an event study design. The estimates suggest that a 1 percentage point business tax increase reduces commercial real estate prices by 3 percent after 4 years on average, while commercial rents stay almost unchanged. This result is robust to the inclusion of a large set of controls and to estimators that account for heterogeneous treatment effects. We use the reduced-form estimates to update current incidence measures and find that commercial landowners bear a significant share of the tax burden ($\approx$ 40%) that increases over time, while workers ($\approx$ 9%) and residential landowners ($\approx$ 15%) are likely to bear a smaller burden than prior research suggests, with firm owners bearing around 36%.

Keywords: Corporate taxation, tax incidence, real estate markets.

JEL Classification: H22, H25, H71.

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1 Introduction

The incidence of corporate taxation is of great interest to policymakers, as it directly influences the progressivity of the tax system and has important distributional consequences. While macroeconomists traditionally distinguish between three key input factors of production – *capital*, *labor*, and *land* (Auerbach and Kotlikoff, 1998) – the existing literature has largely focused on the effects of corporate taxation on wages and firm profits, offering limited insights into its broader implications. More recently, research on corporate tax incidence has begun to incorporate local labor markets, worker mobility, and residential housing, providing evidence that landowners are also affected by corporate taxes (Suárez Serrato and Zidar, 2016). Nonetheless, both theoretical and empirical analyses of the effects of corporate taxes on *commercial* properties remain largely absent.

This paper addresses this gap by developing a framework for a broader assessment of the welfare effects of corporate taxation. In particular, we investigate how corporate taxes causally influence the sales prices and rental rates of commercial properties. We define commercial properties as any type of real estate utilized by firms as a production factor, including—but not limited to—restaurants, retail spaces, office buildings, and production or storage facilities. These properties may be either rented or owned by businesses.

To study the relationship between corporate taxes and commercial land price, we exploit the peculiarities of the German system of business taxation and make use of unique real estate data. The decentralized institutional setup of the German local business tax (henceforth, LBT) offers two important advantages for our research design. First, while municipalities can autonomously adjust the LBT every year via local scaling factors, the tax base and liability criteria are set by the federal government. This allows us to distinguish local tax rate variation from changes in the tax base. Second, there is substantial variation in LBT rates as the around 10,000 German municipalities adjust their taxes frequently providing the statistical power for robust identification.

The analysis combines administrative panel data on German municipalities and their LBT rates with property-level microdata from *F+B*, a real estate consulting firm. The dataset covers more than 17 million properties listed for sale and over 17 million properties listed for rent between 2008 and 2019. Among these, approximately 1.1 million (3.0 million) are commercial properties offered for sale (rent). Compared to prior research on real estate markets, our property data are unique in two key respects. First, whereas most existing studies do not distinguish between commercial and residential real estate, our dataset provides detailed information on *commercial* properties separately, in addition to residential listings. Second, unlike most previous studies, we do not rely on one single (type of) data source or advertising platform. Instead, the *F+B* data include ads from more than 140 different sources and cover various leading online platforms and newspapers. Therefore, our data likely encompass nearly the universe of commercial real estate transaction offers and are thus representative of the commercial real estate market as a whole, serving as a proxy for the cost of land as a factor of production.

We apply a series of non-parametric event study designs exploiting the across-municipality variation in LBT rates over time to estimate reduced-form effects of local business tax increases on offered commercial property sales prices and rents. This design allows us to identify the effects of LBT hikes on prices in the market for commercial properties. We then implement difference-in-differences (DiD) regressions and estimate the corresponding price elasticities.

Our main finding is that higher business tax rates significantly reduce commercial property prices. The negative effect of tax hikes on property prices is increasing over time. For the case of sales prices, the baseline event study estimates suggest that a one percentage point increase in local business taxes decreases the offered sales price of commercial properties by three percent after four years. Similarly, prices for residential properties decline by about one percent after four years. This result is robust to the inclusion of municipality and property level controls, as well as accounting for shocks at the state or commuting zone level. The findings are also robust to using the estimator developed by De Chaisemartin and d'Haultfoeuille (2024) to account for treatment heterogeneity.

Finally, we extend the theoretical model by Suárez Serrato and Zidar (2016) to include commercial real estate. We then estimate the welfare-relevant elasticities and the share of incidence borne by each factor of production. We report incidence shares applying the representative agent perspective of Suárez Serrato and Zidar (2016) and the shares when applying the income weighted approach developed in Suárez Serrato and Zidar (2024). The representative agent perspective reveals that firm owners and owners of commercial properties are most strongly affected by corporate tax changes. They each bear about 30 to 40 percent of the burden. Residential landowners bear between 15 and 20 percent of the burden. Only roughly 10 percent of the burden falls on workers. According to these estimates, landowners bear a larger burden than the previous literature suggests, while workers bear a smaller share. When taking the income-share-weighted perspective the shares borne by firm owners and workers increase moderately, while the shares falling on landowners decline. We also investigate how the incidence shares develop over time. While the incidence in the year of the tax change is almost entirely borne by the firm owners, it is slowly shifted to the other agents over time. Five years after a tax hike firm owners bear only 26 percent of the incidence, while landowners of commercial properties bear 43 percent.

The remainder of this paper is structured as follows. We present the institutional setting of business taxation in Germany and the data we use in our analysis in Section 2. The empirical model is presented in Section 3. In Section 4, we discuss the main results, explore heterogeneous effects, and perform the incidence calculation. Section 5 concludes.

2 Institutions and Data

To empirically estimate the effect of corporate tax increases on factor prices and real estate prices, we exploit the German institutional setting of local business taxation.

2.1 Business Taxation in Germany

Germany levies three types of taxes on business profits. Two of them are entirely set by the federal government – the corporate income tax (CIT, *Körperschaftsteuer*) which applies to corporate firms, and the personal income tax (PIT, *Einkommensteuer*) which applies to non-corporate firms. The third type of corporate tax is the local business tax (LBT, *Gewerbesteuer*) which is partly determined by German municipalities. Although the overall share of corporate taxes in total tax revenues in Germany is smaller than in many other OECD countries (5.2% compared to a 9.6% average revenue share in the OECD in 2019 (OECD, 2022)) local business tax revenues in Germany are comparatively high. In 2019, the LBT generated a revenue of 55 billion euros which corresponds to 48.9% of municipal tax revenues (Statistisches Bundesamt (Destatis), 2021). This corresponds to 65% of total profit tax revenues from corporate firms, making the LBT a substantial part of overall corporate taxation (Statistisches Bundesamt (Destatis), 2021).

The LBT is assessed on the operating profits of firms that are determined in the Local Business Tax Act (*Gewerbesteuergesetz*). The tax applies to both corporate and non-corporate firms, with some exceptions.[1] For firms that are active in multiple municipalities, taxable profits are divided between municipalities according to the payroll share of each establishment using formula apportionment.

Importantly for our research design, the LBT rate τ_{LBT} has two components: the basic rate (*Steuermesszahl*) t^{fed}_{LBT}, which is determined at the federal level and identical across all municipalities and firms, and a local scaling factor (*Hebesatz*) $\theta^{\text{mun}}_{LBT}$ set at the municipal level, such that:

$$\tau_{LBT} = t^{\text{fed}}_{LBT} \times \theta^{\text{mun}}_{LBT}. \tag{1}$$

At the end of each year, the municipal government sets (and announces) the $\theta^{\text{mun}}_{LBT}$ that applies

[1]Paragraphs 2 and 3 of the LBT Act regulate which firms are exempt including, for example, specific professions like lawyers or physicians. See `https://www.gesetze-im-internet.de/gewstg/BJNR009790936.html`, accessed 01/14/2023.

as of January 1st of the subsequent year, with a legal minimum of at least 200 percent (i.e. twice the basic rate). This implies that the exact level of the LBT is determined by the municipality, while – crucially for our identification strategy – both the liability criteria and the tax base are set by the federal government. In our empirical specification, we rely on changes in τ_{LBT} that are solely caused by $\theta^{\text{mun}}_{LBT}$ such that we can identify the effect of local tax changes only.[2]

Using tax increases arising from municipality-specific scaling factor changes as identifying variation has two advantages (see also the discussion in Fuest et al., 2018; Link et al., 2024). First, the more than 10,000 German municipalities can be regarded as small open economies within the integrated German national economy. In this setting, the parallel trend assumption is more likely to hold than in many previous studies on corporate tax changes – particularly those relying on cross-country variation. Second, the entire variation in LBT rates that we consider is driven by municipalities' tax decision, rather than by any factor on the federal level. Thus, the variation in tax rates that we exploit empirically does not depend on (current) firm choices. As also discussed in Fuest et al. (2018), this is especially advantageous compared to previous studies, as tax rate changes have been shown to often occur at the same time as changes in the tax base (Kawano and Slemrod, 2012). In addition to the LBT, the Local Property Tax Rate (LPT) is also determined at the municipal level. It will be crucial for us to control for LPT changes, as in many cases municipalities choose to change both these tax rates together at the same time. Therefore, in all TWFE-specifications we control of the scaled leads and lags of the LPT.

2.2 Municipal Variables and Business Tax Data

The information for our municipality-level dataset is retrieved from administrative data from the Statistical Offices of the German Federal States (*Statistische Landesämter)*. Most importantly, the dataset includes the annual municipal scaling factors. In addition, we have yearly data on economic indicators such as the gross domestic product (GDP) and unemployment rate at district level, and annual population figures at the level of municipalities to proxy and control for fluctuations in the local business cycle.[3] We add district-level indicators for the degree of urbanization (*Siedlungsstrukturelle Kreistypen*) using information from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) and match municipalities to one out of 258 commuting zones (CZs). We drop 648 municipalities that were subject to merger reforms as well as 803 municipalities that experienced a tax cut during the

[2]In 2008, after a tax reform, t^{fed}_{LBT} was lowered from 5.0 percent to 3.5 percent and remains unchanged ever since. Thus, from 2008 onward, every observed change in the LBT rate is caused by a change in the local scaling factor. As our sample period starts in 2008, for this year we only consider those increases in $\theta^{\text{mun}}_{LBT}$ that exceed the decrease in t^{fed}_{LBT} such that τ_{LBT} of a municipality increased compared to 2007.

[3]Due to a change in data reporting, measures of municipal public finances (public expenditure and revenues) are only available until 2014, such that we drop these variables in most of our analyses.

sample period resulting in a panel covering 9,288 German municipalities between 2004 and 2023.[4]

Figure A.1 in the Appendix presents the spatial variation in LBT rates across municipalities over the observation period. Panel A.1a shows that tax rates are relatively high in Northrhine-Westphalia and Saxony, and comparatively low in most parts of former East Germany and Bavaria. Municipal business tax rates range from 7 percent to 31.5 percent. Panel A.1b illustrates that municipalities in Northern Germany change their taxes slightly more frequently than those in the South. While 1,522 municipalities did not change their LBT rate during the sample period, 691 did so five or more times between 2004 and 2023. On average, per year around 10 percent of municipalities adjust their LBT rates. Figure A.2 in the Appendix shows the size of LBT changes and LPT changes. It shows that LBT cuts – which are excluded from the analysis – are both infrequent and small. Most LBT increases are under 2 percentage points; with the average increase (indicated by the red, vertical line) at 0.81 percentage points. In 2019, the mean local business tax rate was at 12.76%, the median was at 12.78%. By comparison, changes in the LPT are much smaller in magnitude, with most ranging between 0.05 and 0.5 percentage points and an average of 0.12 percentage points. The absolute level of the LPT is also substantially lower than that of the LBT.

2.3 Property Data

To measure the effect of local business taxes on commercial property prices, we employ a large and unique micro-dataset on the German real estate market. The data are provided by the real estate consultancy firm *F+B* and comprise information from real estate advertisements covering all types of buildings and facilities offered for rent or for sale. A key feature of the dataset is its coverage of both residential and commercial real estate. The data were collected using web scraping techniques from approximately 140 different sources, covering online real estate portals such as *ImmobilienScout24*, advertisements in (trans-)regional newspapers, as well as real estate agencies. The dataset includes the complete record of all sources in which each property was advertised and was thoroughly cleaned to ensure that properties listed across multiple platforms at the same time only appear only once in the final dataset. For each property, we observe the first and last day of listing, as well as a proxy for the actual selling or rental price.[5]

To validate the representativeness of the *F+B* listing price data, we compare the evolution of our residential price index with actual transaction prices from the *GREIX (German Real Estate*

[4]We also drop two municipalities with scaling factors lower than the legal minimum of 200 percent.

[5]This proxy corresponds to the offering price on the final day of the listing, adjusted by an estimated deduction. The deduction is derived by *F+B* through matching a subsample of the advertisements to actual transaction data.

Index) database – an administrative database based on transaction prices reported by municipal property valuation committees (Gutachterausschüsse) in major German cities. As shown in Figure A.11 in the Appendix, price trends in both datasets are highly aligned, suggesting that the listing prices used in our analysis reliably capture underlying market developments.

Importantly, we are among the first to have access to comprehensive information on commercial property prices, which enables us to assess both the residential real estate channel – previously explored in the literature (e.g. Suárez Serrato and Zidar, 2016) – and the effect of corporate taxation on commercial landowners, which is novel to the literature. For our analysis, we use data on real estate prices from January 2008 to December 2019, resulting in a dataset of over 35 million observations. Of these, approximately 31 million correspond to offering prices and rents for residential buildings, while around 4 million pertain to commercial properties. Within the commercial segment, our sample includes nearly 3 million rental listings and 1.2 million sale listings across Germany.

To the best of our knowledge, this micro-dataset constitutes the most comprehensive data source on the German real estate market, capturing close to the universe of all property transactions. While administrative records are available only at more aggregated levels – and microdata are not accessible for research – many previous studies rely on data from a single real estate platform. In contrast, our data allow us to aggregate the value of all properties offered for sale in each year, thereby enabling us to approximate annual transaction volumes. Appendix Figure A.13 presents the estimated transaction volumes for each year between 2008 and 2018, which range from 26 billion euros (in 2009) to over 70 billion euros (in 2019). These estimates closely align with those of Burkert et al. (2019), who report an average transaction volume of 35 billion euros between 2004 and 2018, and 54 billion euros between 2014 and 2018. This suggests that our dataset likely captures a substantial share of the German commercial property market.[6]

In our empirical analysis, we separately study the price effects of an increase in the LBT on properties offered for rent and those offered for sale. Therefore, our two outcome variables of interest are the rental and sales price per square meter (sqm) of a property on the final day a property was listed. As previously noted, we observe listing prices rather than actual transaction prices.[7] Besides price information, the dataset covers a wide range of property

[6]The annual real estate report by the expert committees for property valuation *(Gutachterausschüsse)* documents approximately 1 million transactions per year (Arbeitskreis der Oberen Gutachterausschüsse, 2019). Around 10% of these involve commercial properties, while about 70% relate to residential properties. The remainder comprises agricultural and other types of properties. For instance, in 2018, the committees reported approximately 727,000 residential and 84,000 commercial transactions. In our dataset, we observe an average of roughly 100,000 commercial and 1.3 million residential properties offered for sale per year. Thus, while our offering figures are somewhat higher than realized transaction counts, they are of a comparable order of magnitude.

[7]This implies that properties never listed on the market are excluded from the dataset. However, as discussed above, listing prices closely track transaction prices. Prior studies have treated final listing prices as a reliable

characteristics. For each property, we have information on floor space, the number of rooms, the construction year, as well as dummy indicators for amenities and locational characteristics. Commercial properties are categorized into five different types: offices, retail, storage, production, or restaurant spaces. Figure A.12 in the Appendix shows the distribution of property types for the commercial sales and the commercial rents sample. In both samples, offices constitute the most common type of properties offered, though there are some differences in the composition between the samples. The sales sample consists of comparatively more restaurant and production spaces. Importantly, the data contain information on the location of every advertised object, such that we can match every property to its corresponding municipality.

Besides price information, the dataset includes a wide range of property characteristics. For each listing, we observe details such as floor space, number of rooms, year of construction, and binary indicators for amenities and locational features. Residential properties are categorized as single-family houses, multi-family houses, or apartments. Commercial properties, in turn, are classified into five types: offices, retail, storage, production, and restaurant spaces. Figure A.12 in the Appendix shows the distribution of property types for the commercial sales and rental samples. In both samples, offices represent the most frequently offered property type, although there are notable differences in composition: the sales sample contains relatively more restaurant and production spaces, while the rental sample has a higher share of retail spaces. Importantly, the dataset includes precise location information for each advertised property, enabling us to match every listing to its corresponding municipality.

2.4 Estimation Sample

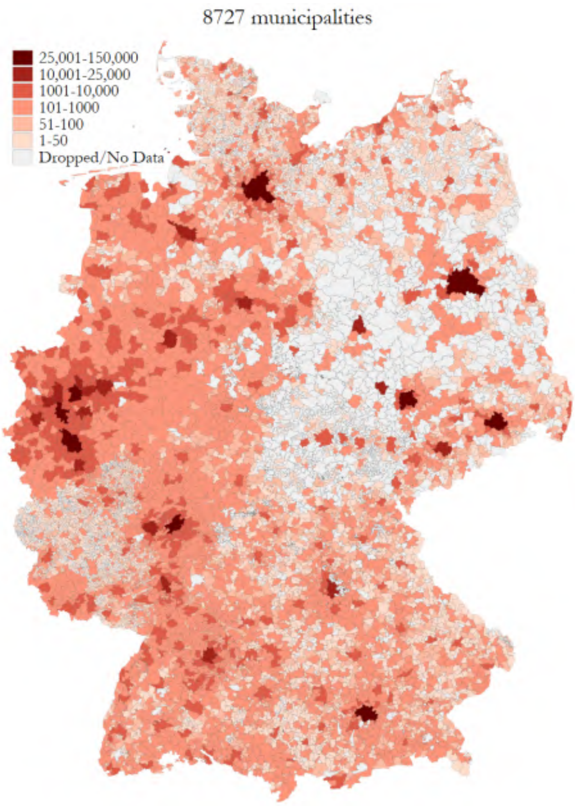
We combine and harmonize municipality and property data to construct an annual panel dataset, with four subsets categorized by property usage type (commercial vs. residential) and type of offer (for sale vs. for rent). These datasets cover all German municipalities, with municipality-year observations spanning from 2008 to 2019. To account for correct leads and lags in our event study estimation, we incorporate information on local scaling factors from 2004 to 2023.

In our baseline specification, we require at least one advertisement per municipality-year cell, resulting in a total sample of 8,727 municipalities, encompassing all property types. Figure 1 illustrates the spatial variation in the average number of postings per year between 2008 and 2019. As expected, the number of postings is highest in densely populated areas of West Germany and major agglomeration zones, where large municipalities report between 25,000

proxy for actual prices (e.g. Löffler and Siegloch, 2021), and in our case, using the estimated transaction price provided by F+B leads to virtually identical results. Moreover, Figure A.11 in the Appendix further corroborates the validity of the listing prices by showing that they closely follow transaction-based price indices from the GREIX database.

and 150,000 postings per year.

Figure 1: Average Number of Postings per Year (2008-2019)



Notes: The figure illustrates the spatial variation in the average annual number of advertising postings across 8,727 German municipalities between 2008 and 2019. Grey areas denote municipalities for which we do not observe at least one posting per sample year or which were excluded due to municipal merger reforms or tax cuts occurring during the sample period (2008–2019). *Source*: Own calculation based on data from *F+B* and the Statistical State Offices.

When disaggregating the data into our estimation samples, we retain 8,596 municipalities per year in the residential sales sample (over 16 million price observations) and 6,250 municipalities per year in the residential rental sample (over 14 million price observations). In the commercial market, we observe 3,329 municipalities per year in the sales sample (1,179,468 price observations) and 2,634 municipalities per year in the rental sample (2,996,770 price observations).[8] Figures A.7 and A.8 in the Appendix show the spatial distribution of the estimation samples.

Table A.1 in the Appendix shows the selection steps we follow to generate our baseline estimation samples. Municipalities that are dropped (as they were subject to merger reforms, have none or too little observations per year in either sample, or as they experienced a tax cut at some point during the sample period) are situated in the rural part of former East Germany

[8]Additionally, 2,242 municipalities have at least one posting per year across all real estate subsamples. Although this subset includes only around 20% of German municipalities, it still accounts for approximately 78% of all postings in our dataset. This concentration of listings is expected, as in 2018, the median German municipality had 1,810 inhabitants, and 25% of municipalities had a population of 653 or fewer.

(especially in Saxony-Anhalt and Mecklenburg-Western Pomerania), or are small jurisdictions in the South and West of the country. Tables A.2, A.3, A.4, and A.5 in the Appendix show descriptive statistics for property, tax, and municipal variables in our four estimation samples, respectively.

3 Identification and Empirical Strategy

3.1 Empirical Design

In the first part of our analysis, we employ an event study design to estimate the causal effect of changes in the local business tax rate on real estate prices and rents. Our baseline outcome variables are the logarithm of the rent and sales price per square meter $ln(p_{imt})$ of property i, in municipality m, and year t, where each municipality is nested in a commuting zone cz[9] and state s.

We set up our panel-event study design to allow for municipalities to experience multiple tax changes in the event window. In addition, instead of using dummys, we scale our event study indicators by the tax change, i.e. the actual change in the LBT rate caused by changes in the municipal scaling factor.[10] Thus, we allow for varying and continuous treatment intensities. Formally, all of our regressions are based on some form of the following empirical regression model:

$$ln(p_{imt}) = \sum_{j=-\underline{j}+1}^{\bar{j}} \beta_j \Delta LBT_{mt}^{t-j} + \sum_{j=-\underline{j}+1}^{\bar{j}} \gamma_j \Delta LPT_{mt}^{t-j} + \delta X_{imt} + \mu_m + \theta_{st} + \varepsilon_{imt}. \quad (2)$$

where $\hat{\beta}_j$ includes the estimates of interest that measure the dynamic causal effect before ($j < 0$) and after ($j \geq 0$) treatment with $\bar{j}$ lags and $\underline{j}$ leads of the treatment variable. The event study indicators in ΔLBT_{mt}^{t-j} capture the treatment as a change in the LBT rate in year t and municipality m relative to the year $t - j$ triggered by a change in the local scaling factor. The main control variables included in all specifications are the scaled leads and lags of local property tax rate changes ΔLPT_{mt}^{t-j} in municipality m at time t relative to the year $t - j$. It is important to control for changes in property tax rates for two reasons. First, the property tax has a direct effect on property prices (Oates, 1969; Löffler and Siegloch, 2021) such that our estimates can be biased if we do not control for it. Second, similar to the LBT

[9]There are several ways to define a commuting zone in Germany. The arguably most common way (see e.g. Fuest et al. (2018)) is to use the so-called definition of *Arbeitsmarktregionen* from the *BBSR* which leaves us with 258 CZs.

[10]Note that these changes can be negative in case of a tax drop. We omit tax drops as they are rare and exclude the corresponding observations.

rate, the LPT rate is also set by municipal governments each year, which suggests that there is a likely connection between changes in both of these tax rates.[11] Equation 2 also includes a set of time-varying controls, denoted by X_{imt}, which are incorporated in some specifications. These controls capture dynamic measures such as district GDP, district unemployment rate, municipal population, and the municipal share of income tax revenues (in logs and lagged by two periods) and serve to account for time-varying shocks that may occur shortly before or after a tax change.[12] Moreover, X_{imt} includes the property control variables described in Section 2.3. Unobserved municipal characteristics that are constant over time are captured in the municipal fixed effects, μ_m, while θ_{st} captures "state $\times$ year" fixed effects, thus controlling for time-varying trends and shocks at the state level. In some specifications we instead control for "CZ $\times$ year" fixed effects to capture shocks at lower geographical levels. ε_{mt} denotes the error term.

As we focus on property data between 2008 and 2019, we set $\underline{j}$ = 4 and $-\overline{j}$ = 5 allowing us to cover ten years around a tax reform occurring in period $t = 0$. Including four years in the pre-treatment period seems long enough to allow for detecting unequal pre-trends while a post-reform period of five years investigates both the short- and medium-run effects of tax changes. As proposed in Schmidheiny and Siegloch (2023), ΔT_{mt}^{t-j} are binned treatment indicators so that at the end points the coefficients deliver an estimate for all future and past tax hikes, respectively, that proceed or follow our chosen effect window. In this setup, the observation window of the tax change has to be set longer than the observation window of the dependent variable to account for the correct leads. Therefore, we track tax changes between 2004 and 2023. The varying treatment timing leads to an unbalanced panel in event time (even though the municipality panel is perfectly balanced from 2004 to 2023). We therefore drop the binned endpoints from our event study graphs following Fuest et al. (2018). The regressor for the pre-reform year is omitted from the regression, such that all coefficients have to be interpreted relative to the pre-reform year. Standard errors are clustered at the municipal level which comprises the level of the identifying variation in our model.

As described in Section 2.1, municipalities set the scaling factor individually each year, which implies that tax changes occur at different times and with different intensities. At the same time, some municipalities change their taxes frequently. In general, our event study design allows for all the above-mentioned features so that both municipalities that are never treated

[11] To further address potential concerns regarding the interaction between LBT and LPT, in Section 4.1, we present a robustness analysis that reports our results excluding property tax controls. Additionally, we provide results for a subset of municipalities that did not experience an LPT change during the sample period.

[12] Unfortunately, while Fuest et al. (2018) control for municipal expenditures as proxy for local public good provision, we only observe this variable until 2014 so that we exclude it from our estimations. The reason is that the accounting standard for local governments has changed over time and at different times for each state. Hence, expenditure statistics are not comparable over time and place.

and those that are not-yet treated function as control groups while they can receive treatment several times during our sample period. Still, the never-treated municipalities may well experience a tax change outside of the event window. The choice of the effect window can thus have a direct effect on identification through its influence on the control groups (Schmidheiny and Siegloch, 2023).

While Equation 2 identifies the semi-elasticity of the effect of LBT increases on sale prices and rents, most studies in the corporate tax incidence literature also report elasticities with respect to the net-of-tax rate (e.g. Suárez Serrato and Zidar, 2016; Fuest et al., 2018). To be able to compare the magnitudes of our estimates to previous findings on the effects of corporate taxes on other production factors, we also estimate a standard elasticity. To do this we substitute the treatment indicators in Equation 2 with the change in the log net-of-tax rate in each lag and lead. We then average the effects in the post-treatment period and report these averaged elasticities. These estimates deliver the needed measure of the elasticity of sales and rental prices with respect to the business tax rate. The regressions include the same controls and fixed effects as our event study specification in Equation 2.

3.2 Identification

The baseline event study regression in Equation 2 includes "state $\times$ year" fixed effects. Thus, we identify the effect of tax changes on rental and sale prices within municipalities and states over time. The identification of causal effects requires that there is neither reverse causality nor omitted variable bias and is based on several additional assumptions.

The first identifying assumption is the parallel trends assumption. In our model, we assume that untreated (and not-yet-treated) municipalities represent the corresponding counterfactual of the trends in property prices that treated municipalities would have followed if they had not been treated. This implies that our estimates are solely driven by tax hikes and not by other shocks in the observed municipalities. This assumption would be violated in case of any biasing trends or systematic shocks on municipality level that influence property prices or tax rates. While the event study setup allows for a visual test of parallel pre-trends – i.e., if the lead-coefficients are close to zero – we check for differential local shocks between treatment and control group in two ways similar to Fuest et al. (2018). First, we estimate the model in Equation 2 with municipal unemployment and district GDP as outcome variables. The results are presented in Figure B.27 in the Appendix. We find flat pre-trends suggesting that the taxes were not changed in response to worsening economic circumstances. Moreover, we control for local shocks on the level of 258 commuting zones. We do so by including more granular "CZ $\times$ year" fixed effects instead of "state $\times$ year" that we use in our baseline model. Thus, we account for any annual (labor-market) shock omitted at the state-level, such as municipal election years, which have recently been shown to affect LBT rates (Foremny and Riedel, 2014). In connection to this, we also assume no anticipation effects, i.e., landlords are assumed to not adjust offering prices after the announcement of a tax change and before it comes into effect. LBT changes are usually announced in December and go into force in January of the following year. Therefore, anticipation effects can only affect a small fraction of observations in our sample where the final date of advertisement falls into that time window. Finally, we assume the Stable Unit Treatment Value Assumption (SUTVA) holds. This assumption requires that the effect on sales and rental prices following a tax hike in a municipality does not depend on whether neighboring municipalities also experienced tax hikes (Imbens and Rubin, 2015). While it is not trivial to show that SUTVA is fulfilled, we demonstrate that tax increases in close-by municipalities do have a small negative effect on sales prices (see Figure B.21 in the Appendix). If anything this should bias our estimates downwards, since the spillovers are negative. In Figure B.20 in the Appendix we also show that controlling for spillovers leaves our estimates essentially unchanged. This is unsurprising as most municipalities are small, as are the tax changes.

Heterogeneous Treatment Effects. In the baseline estimation, we assume that the treatment effect is proportional to the treatment intensity, i.e., that the effects on property prices vary in the same proportion as the tax change, while there is no variation in the treatment effect between municipalities or for different years of treatment. However, treatments may be heterogeneous between different (groups of) municipalities.[13] Our model in Equation 2 cannot account for such heterogeneity. In addition, while some municipalities experience only one tax hike during our observation window, others are treated more frequently. Potentially, the price effect of a single tax increase differs from the effect following multiple tax increases. For instance, the treatment effect may vary between the first and the subsequent tax increases within a municipality. Finally, given the differences in treatment timing, some municipalities increase taxes at the beginning of the observation period, while others do so later. Dynamic effects of tax increases may depend on the year of their implementation.

To account for potentially heterogeneous treatment in our model, we implement the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024), i.e. the only two-way fixed effect robust estimator that can account for the complex multiple treatment in our complex setting of German local business taxation. It accounts for heterogeneous treatment and measures both immediate and dynamic treatment effects. Applied to our context, the estimator

[13] Consider the following example: Historically, some regions in Germany are home to a large number of big manufacturing firms, while in other region the service and information sectors are more developed. While firms in manufacturing require large and complex production spaces, companies that operate in other sectors mainly own or rent office buildings which, arguably, are significantly easier to substitute. In this setting, following a tax hike we would expect the price effects to be less elastic in municipalities with a higher share of manufacturing firms.

allows that groups (municipalities) may be exposed to multiple treatment changes (tax hikes), with the treatment event being defined as the period of time when a group first experiences a change in treatment. The estimator is based on three identifying assumptions required for unbiased estimation: (i) treatment applies at the group-level (*sharp design*), (ii) a group's current outcome does not depend on its future treatments (*no anticipation*), and (iii) treatments are exogenous to group-level shocks while treatment and control groups follow parallel trends (*independence*) (De Chaisemartin and d'Haultfoeuille, 2024). The control group consists of both not-yet-treated and never-treated municipalities.

3.3 Measuring Tax Incidence

The DiD estimate that we get when substituting the treatment indicators in Equation 2 with the leads and lags of the change in the log net-of-tax rate captures the elasticity of rent and sales prices with respect to the net-of-tax rate. We interpret this elasticity through the spatial equilibrium model developed in Suárez Serrato and Zidar (2016) and Suárez Serrato and Zidar (2024). We extend the model by adding a commercial properties market. Firms rent commercial real estate and use it as a factor of production. If an increase in business tax rates leads to lower rents or sales prices, part of the incidence is passed on to commercial landowners. Similarly, the stronger higher business taxes reduce wages, profits, and residential real estate prices/rents, the more burden if borne by workers, firm owner, and residential landowners, respectively. Table 1 shows how reduced form estimates map into welfare changes for workers, landowners and firm owners. The reduced form estimates are the elasticities of wages (γ^W), of residential rents (γ^{RH}), of rents for commercial properties (γ^{RG}) and of after-tax profits (γ^{Π}). These estimable parameters map into the incidence formulae obtained from the model. This makes it possible to estimate the share of incidence borne by each of the four groups of economic agents. In the next section we estimate γ^{RG}, γ^{RH} and γ^{Π}. We take γ^W from the literature on the corporate tax incidence on wages (Fuest et al., 2018). Suárez Serrato and Zidar (2016) do not directly observe net-of-tax profits and therefore infer it from other estimates (Firm entry, wages, intensive margin labor demand) and make assumptions about certain parameters (Product demand elasticity, capital to labor output elasticity). Instead we directly estimate the effect on net profits. From the business tax statistics we take the LBT base and multiply it by the net-of-tax rate. This yields a measure for net profits. We then run the same specifications as for the effects on rent and sales prices. This yields an elasticity of net profits with respect to the log net-of-business tax rate which corresponds to γ^{Π}.

Table 1: Incidence

Stakeholder	Incidence	Identified by
Workers (disposable income)	$\dot{w} - \alpha\dot{r}^H$	$\gamma^W - \alpha\gamma^{RH}$
Residential Landowners (housing costs)	$\dot{r}^H$	γ^{RH}
Commercial Landowners (rent of comm. property)	$\dot{r}^G$	γ^{RG}
Firm owners (after-tax profit)	$\dot{\pi}$	γ^{Π}

Notes: This table shows how estimable elasticities map into the incidence formulae obtained from a spatial equilibrium model in the style of Suárez Serrato and Zidar (2016). The parameter α refers to the housing expenditure share and has to be calibrated. In Germany housing expenditures make up between 26% and 31% of disposable income in the period from 2009 to 2019. Hence, we set $\alpha = 0.3$, keeping in line with Suárez Serrato and Zidar (2016).

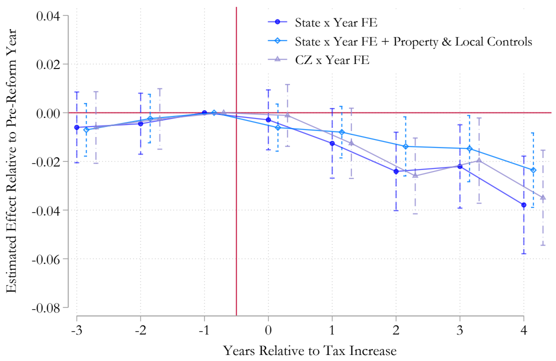
4 Results

4.1 Estimation Results

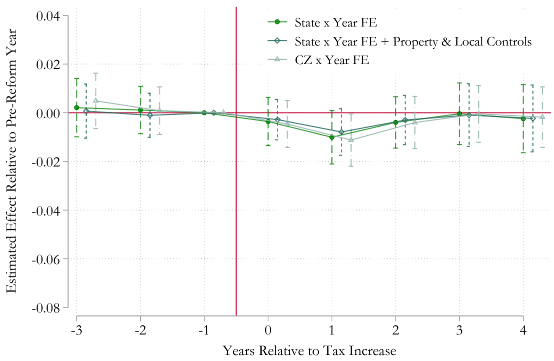
Figure 2 presents the baseline estimates separately for commercial sales prices (Panel 2a) and commercial rents (Panel 2b) as dependent variables, along with corresponding 95% confidence intervals. We report results from three specifications of Equation 2: (i) including only "state $\times$ year" fixed effects, (ii) adding property-level controls to "state $\times$ year" fixed effects, and (iii) replacing them with "commuting zone (CZ) $\times$ year" fixed effects. Panel 2a shows that, following a tax hike, the sales prices of commercial properties decline significantly. While the effect is small and not statistically significant in the first year after the tax increase, it becomes more pronounced over time, such that after four years, the estimated effect is strongly significant across all specifications. The estimated effect corresponds to a semi-elasticity of approximately 3 – that is, a one percentage point increase in the tax rate leads to a 3 percent decrease in commercial sales prices after four years, relative to the year prior to the reform. Importantly, pre-treatment trends are very flat across all three specifications, suggesting that treatment and control municipalities were evolving similarly prior to the tax change. This supports the validity of the parallel trends assumption.

The estimates for the effect of tax hikes on rental prices in Panel 2b are very flat before and after the tax change. While there appears to be a slight decrease in rents immediately after the tax hike, the later estimates are also negative but closer to zero and insignificant. All specifications lead to very similar results, suggesting only a very small effect on rents of commercial properties. There could be three reasons for a smaller effect on rents. First, rents are almost fully deductible from the tax base, while financing costs for a real estate purchase are not, which implies that tax changes affect buyers of properties more than renters. Second, the effects of a tax change might affect real estate markets over a long time, because firm location decisions are slow and gradual. The tax change might be priced in relatively quickly in

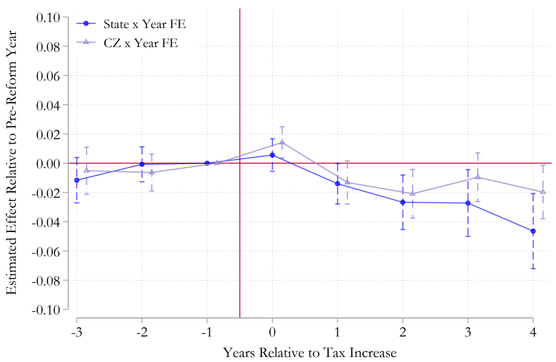
Figure 2: Baseline Effects on Commercial Properties



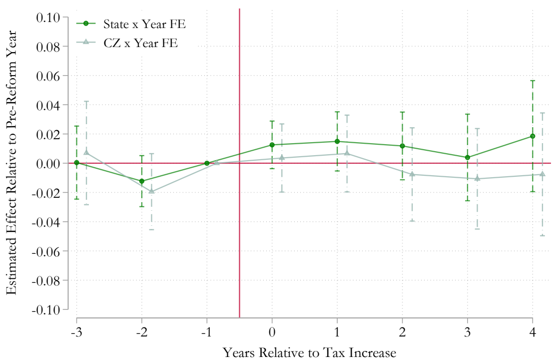
(a) TWFE: Sales Prices



(b) TWFE: Rents



(c) Heterogeneity Robust: Sales Prices



(d) Heterogeneity Robust: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log sales price per sqm (Panel 2a) and the log rental price per sqm (Panel 2b). Treatment variables are event study indicators scaled by the LBT change. We require at least one ad per municipality-year cell such that we have 3,329 (2,634) municipalities and 7,101 (5,481) tax hikes for the sales (rental) price sample. All regressions include municipal fixed effects and the scaled leads and lags of the municipal property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels 2c and 2d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as the discrete number of tax hikes with the first tax hike happening in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

sales prices, but it might take a longer time to reach rental markets as firms gradually relocate over multiple years. Third, there is some theoretical research and empirical evidence that rents are downward rigid while sales prices are not (Genesove, 2003; Gallin and Verbrugge, 2019). Hence, rents might remain stable after a tax hike, but vacancies might increase, leading to lower expected cash-flows and sales prices. For these reasons we consider the sales price effects the more reliable estimates of the economic incidence of the tax.

In Panels 2c and 2d of Figure 2 we also show the results of using the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). The estimator accounts for heterogeneous treatment and measures both immediate and dynamic treatment effects. Applied to our context, the estimator allows that groups (municipalities) may be exposed to multiple treatment changes (tax hikes) of varying intensities, with the treatment event being defined as the period of time when a group first experiences a change in treatment. When using this robust estimator, we find almost unchanged results for the point estimates on sales of commercial properties, but they are less precise than the TWFE results. This is not surprising since our setting is quite complex. However, the magnitude of the estimates remains very similar, which confirms that the results are not caused by heterogeneous treatment effects.

The results for the equivalent specifications on residential properties are shown in Appendix Figure B.14. We find that the reduction in prices extends to residential properties. Both rents and sales prices decline following a tax hike. Just as observed for commercial properties the effects build up over time. Four years after the taxhike the prices are about 1 percent lower than in the counterfactual. This suggests that residential prices respond less than commercial property prices, but the effects are still substantial. The TWFE results for sales prices suffer from slightly diverging trends in the pre-treatment period, suggesting that the TWFE estimates might be biased due to heterogeneous treatment timing or differential trends across groups. Conversely, the heterogeneity-robust estimators show flat pre-trends and significant effects after the tax hike. The post-treatment effects are relatively similar across all specifications, with estimates suggesting medium-run semi-elasticities of around 1. The differences between the TWFE and the heterogeneity-robust estimators in the pre-treatment period suggest that the TWFE model may not adequately capture the underlying treatment dynamics, potentially overstating pre-existing trends.

These discrepancies underscore the importance of accounting for treatment heterogeneity. The heterogeneity-robust estimators, by isolating treatment effects without conflating them with timing variations, provide a more reliable assessment of causal impacts. Consequently, the results indicate that relying solely on TWFE could lead to incorrect inferences in this case, particularly in the pre-treatment period. However, consistency in post-treatment semi-elasticities across specifications supports the robustness of the observed tax impact, as indicated by the heterogeneity-robust models.

To estimate price elasticities for sales prices and rents, we run the same regressions but replace the tax rate changes by changes in the log net-of-tax rate as explained in Section 3.1.[14] Panel A of Table 2 shows the corresponding elasticity estimates for commercial properties. The estimate of the sales price elasticity for our baseline specification (with no controls except for leads and lags of property tax changes and "State $\times$ Year" fixed effects) reveals an elasticity of 2.4 and decreases slightly when adding controls and using "CZ $\times$ year" fixed effects. For the most demanding specification we estimate an elasticity of about 2.2. That is, following a one percent increase in the net-of-tax rate, sales prices of commercial properties increase by 2.2 percent. The estimated elasticities for rents are smaller in magnitude and statistically insignificant and lie between 0.6 and 0.8.

Panel B of Table 2 shows the estimated elasticities for residential properties. The elasticities are generally smaller than for commercial properties. For sales prices it ranges from 1.2 (only "State $\times$ year" fixed effects) to 0.87 (full controls). For rents the elasticity stays almost constant when including more demanding sets of control variables (always between 0.81 and 0.86). According to our estimates residential properties are less affected by corporate tax changes than commercial properties. Finally, Panel C displays the estimated elasticities for net profits. The elasticity ranges from 1.8 to 2 depending on the exact specification. The event study graphs for these specifications are displayed in Figure B.15 in the Appendix. They show that there is an immediate effect of the tax change on profits. Over time this effect decreases. This shows that firm owners are able to shift part of the incidence of the tax on other market participants. These estimates are within the range of point estimates obtained by Suárez Serrato and Zidar (2024) for their specifications relying on productivity and intensive margin labor demand to quantify effects on firm profits.

While the estimated pre-trends are reassuringly flat in our baseline specification in Figure 2, we further test the robustness of our estimates. We run five major robustness tests and present the results of these tests in Figure 3 for the commercial samples and Figure B.18 for the residential samples. To improve readability, we summarize pre-treatment effects (leads of up to three years prior to a tax change) and medium-run effects (two to four years after a policy change) by calculating the average over the respective estimates.[15]

First, there might be concerns about complexities introduced by municipalities being treated by multiple tax hikes. Therefore, we restrict the sample to municipalities that experience only

[14] The corresponding event study graphs are presented in Figure B.16 (for commercial properties) and Figure B.17 (for residential properties) in the Appendix.

[15] The corresponding event study graphs are presented in the Appendix in Figures B.22 , B.23, and B.24. We also present event study graphs excluding the states of North-Rhine Westphalia and Rhineland Palatine from the analysis in the Appendix in Figures B.25 and B.26.

Table 2: Elasticity Estimation

Panel A:	Commercial Properties					
	Ln Sales Price sqm			Ln Rent Price sqm		
Δ Ln Net-of-Tax Rate	2.410***	1.557**	2.207***	0.673	0.569	0.780
	(0.647)	(0.493)	(0.634)	(0.456)	(0.460)	(0.419)
Property Controls		✓	✓		✓	✓
State x Year FE	✓	✓		✓	✓	
CZ x Year FE			✓			✓
Observations	959,065	959,065	959,065	2,328,881	2,328,881	2,328,881

Panel B:	Residential Properties					
	Ln Sales Price sqm			Ln Rent Price sqm		
Δ Ln Net-of-Tax Rate	1.225**	1.230**	0.874**	0.826**	0.860**	0.809***
	(0.460)	(0.418)	(0.300)	(0.310)	(0.293)	(0.208)
Property Controls		✓	✓		✓	✓
State x Year FE	✓	✓		✓	✓	
CZ x Year FE			✓			✓
Observations	13,519,952	13,519,952	13,519,952	11,305,157	11,305,157	11,305,157

Panel C:	Ln Net Profit		
Δ Ln Net-of-Tax Rate	1.812**	1.809**	2.019**
	(0.736)	(0.737)	(0.631)
State x Year FE	✓	✓	
CZ x Year FE			✓
Observations	117,967	90,537	90,477

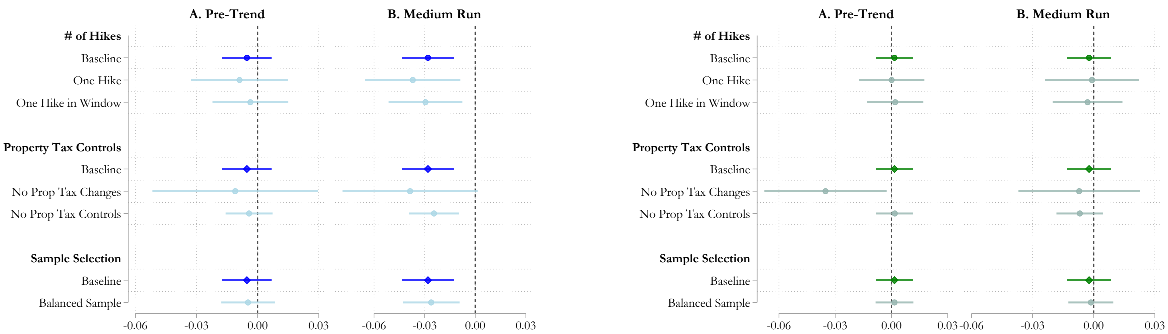
Notes: This table presents the DiD estimates, $\hat{\gamma}$, of the regression model in Equation 2. The coefficients measure the rental price elasticity with respect to the net-of-local business tax rate. Panel A displays the elasticities for commercial properties. Panel B displays the elasticities for residential properties. Panel C displays the net-profit elasticity. All regression models include municipal fixed effects and account for the local property tax rate. Additional control variables and fixed effects (year, "state × year", or "commuting zone (CZ) × year") vary depending on the specification (as indicated at the bottom of the table). The estimation sample is restricted to non-merged municipalities and municipalities that experience no tax cuts between 2008 and 2019. Standard errors are clustered at the municipal level. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

one tax change in the event window. The estimates are slightly less precise but the point estimates are remarkably similar to our baseline estimates. Second, we restrict the sample further to only municipalities that only experienced one tax hike between 2004 and 2023, i.e. the window of years for which we consider leads and lags of municipal tax changes. Again, we lose some precision but the coefficient estimates remain very similar.

Third, since both LBT and LPT are set by municipal governments, there may be important interrelations between the two. To account for this, we estimate the model separately for municipalities that did not experience an LPT change between 2004 and 2023. Given that LPT is adjusted even more frequently than LBT (albeit with small tax changes), this specification again results in a loss of precision. Nevertheless, the point estimate of the medium-run effect remains very similar to our baseline estimate. Fourth, and relatedly, to address concerns that the leads and lags of LPT may serve as a 'bad control,' we also conduct the event study analysis without any property tax controls. Again, we find very similar results.

Finally, as explained in Section 2.4, the set of municipalities used in the estimations differ between our four estimation samples due to differences in posting numbers. To show that results are not driven by differences between municipality coverage between samples, we also report the results for the sample of 2,242 municipalities that we observe for all four estimation samples with estimates remaining virtually unchanged. This is not surprising, as we mainly loos municipalities with few observations if we restrict the analysis to the balanced sample retaining about 77% of price observations.

Figure 3: Robustness Analysis – Commercial Samples



(a) Sales Sample

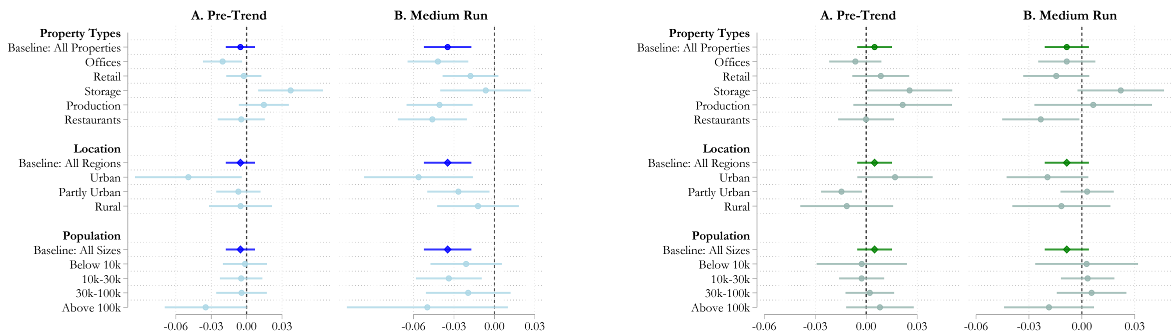
(b) Rental Sample

Notes: This figure presents the results of five major robustness tests assessing the estimated treatment effect of a one percentage point increase in the LBT rate on offered commercial sales and rental prices. To improve readability, we summarize pre-treatment effects (the average coefficient for up to three years before a tax change) and medium-run effects (the average estimate for two to four years after a policy change). The first section presents estimates for municipalities that experienced only a single tax increase between 2004 and 2023 or within the (shorter) event window. The second section reports results for the subsample of municipalities that did not experience a local property tax change during the sample period, as well as estimates from regressions excluding property tax controls. The final section presents results for the subset of 2,242 municipalities included in all four estimation samples. Unless stated otherwise, all regressions control for the scaled leads and lags of the local property tax rate. Horizontal bars indicate 95% confidence intervals. All specifications include municipal and "state × year" fixed effects, with no additional controls. Standard errors are clustered at the municipality level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

4.2 Heterogeneity

Turning to the heterogeneity analysis, we group properties and municipalities according to different indicators and inspect whether our estimates are driven by certain subgroups. Results are presented in Figure 4. We investigate heterogeneous effects for five different property types: offices, retail, storage, production and restaurants. Certain types of businesses are more mobile and less dependent on a specific location than others. Therefore, we might expect larger elasticities for these less location-dependent businesses. Regarding sales prices we find that the effects are particularly large for offices, production properties and restaurants. This is in line with the basic theoretical intuition. The location of offices or production facilities has a much smaller impact on success than for shops. Other factors (such as taxes) play a larger role in these more location-independent sectors. The heterogeneity for the rent sample is substantially different. While the point estimates for retail, restaurants and offices are negative and marginally significant. These are also the types of properties that are more likely to be rented and probably more representative of the rental market.

Figure 4: Heterogeneity Analysis



(a) Sales Sample

(b) Rental Sample

Notes: This Figure presents the results for different subsamples of observations according to property and municipal level variables. Estimates depict the estimated treatment effect of a one percentage point increase in the LBT rate on the offered sales price (Panel 4a) and rent (Panel 4b) of commercial properties relative to the pre-reform year. The baseline results for the sales sample correspond to the blue estimates in Figure 2a; baseline results for the rental sample correspond to the green estimates in Figure 2b. Estimates from alternative specifications are depicted in lighter colors. Subpanel A presents summary estimates of pre-treatment trends, i.e., the average coefficient in the three years prior to a tax reform. Subpanel B shows the medium-run effect measured as the average estimate of the third, fourth, and fifth lag in the LBT rate. All regressions account for scaled leads and lags of the local property tax rate. Horizontal bars indicate 95% confidence intervals. All regressions include municipal and "state $\times$ year" fixed effects. Standard errors are robust to clustering at the municipality level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

When investigating heterogeneity by the degree of urbanization, we observe different effects on rents and on sales prices. While the sales price effects seem to be driven mostly by urban municipalities, rents reduce somewhat in urban and rural areas. This could be due to the different distributions of the observations in both samples. Rental observations are more concentrated in big municipalities, and barely available in rural jurisdictions such that the effect on rents in rural areas can be driven by few outliers, as it is quite imprecisely estimated.

Finally, we inspect heterogeneous effects over municipality size. The effects are strongest in municipalities above a population of 100,000. Estimates for the other groups are smaller but still significantly negative for commercial properties.

4.3 Incidence Analysis

Table 3: Incidence Estimates

A. Incidence		Baseline		Short-Run	Medium-Run
Landowners (Residential)	1.225**	1.230**	0.874**	0.313**	0.951**
	(0.460)	(0.418)	(0.300)	(0.151)	(0.428)
Landowners (Commercial)	2.410***	1.557**	2.207***	0.591	2.353***
	(0.647)	(0.493)	(0.634)	(0.528)	(0.755)
Workers	0.633***	0.631***	0.738***	0.316**	0.722***
	(0.099)	(0.155)	(0.189)	(0.131)	(0.196)
Firm owners	1.812**	1.809**	2.019**	2.305***	1.443*
	(0.736)	(0.737)	(0.631)	(0.611)	(0.798)
B. Share of Incidence					
Landowners (Residential)	21%	24.8%	15.7%	8.9%	17.4%
Landowners (Commercial)	41.4%	31.3%	39.6%	16.8%	43.0%
Workers	6.4%	7.5%	8.6%	9.0%	13.2%
Firm owners	31.1%	36.4%	36.2%	65.4%	26.4%
Rent or Sales	Sales	Sales	Sales	Sales	Sales
Property Controls		✓			
State x Year FE	✓	✓			
CZ x Year FE			✓	✓	✓

Notes: This table presents the incidence estimates for landowners, workers and firm owners. Panel A displays the welfare-relevant elasticities described in Table 1. Panel B displays the share of incidence borne by economic agent. Each column displays a different specification for estimating the elasticities. All regression models include municipal fixed effects and account for the local property tax rate. Additional control variables and fixed effects (year, "state × year", or "commuting zone (CZ) × year") vary depending on the specification (as indicated at the bottom of the table). The fourth column shows incidence estimates for the first two years after the tax hike. The fifth column shows the incidence estimates for the medium-run (four and five years after the tax hike). The estimation sample is restricted to non-merged municipalities and municipalities that experience no tax cuts between 2008 and 2019. Standard errors are clustered at the municipal level. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

In this section we use the point estimates from the previous sections to estimate the incidence of the local business tax on landowners (residential and commercial real estate), workers, and firm owners. As explained in Section 4.1 we consider the effects on prices the more reliable estimate for the economic incidence of the tax change on landowners. Therefore, we focus on the estimates obtained form the sales price regressions in the following analysis. Panel A of Table 3 displays the incidence estimates for each factor of production following the derivations from Suárez Serrato and Zidar (2016) that we summarize in Table 1 and in Appendix C. Panel B then shows the share of the incidence borne by each type of agent.[16]

[16] We follow Suárez Serrato and Zidar (2016) and show the unweighted shares of incidence. This approach takes

The shares borne by landowners of commercial real estate, workers and firm owners are relatively stable through the three main specifications, while the shares of burden that falls on residential landowners vary slightly more. Commercial landowners and firm owners bear the largest burdens of the tax with incidence shares varying between 30 and 40 percent each. These groups together bear a burden of between 65 and 75 percent. Between 16 and 25 percent of the incidence falls on residential landowners. Finally, between 6 and 9 percent of the incidence falls on workers. In the fourth and fifth columns of Table 3 we also show incidence estimates for the short-run (first and second year after tax hike) and the medium-run (fourth and fifth year). In the short-run the incidence is mostly borne by the firm owners because of the mechanical tax increase. In the medium-run the markets adjust and the incidence is shifted on the other agents. This leads to larger incidence shares for workers, residential and commercial landowners, while the shares of firm owners decline over time.

Our incidence estimates for firm owners are smaller than the updated estimates reported by Suárez Serrato and Zidar (2024). This is unsurprising as we include the owners of commercial real estate, which might be a somewhat similar and overlapping group. Adding both groups together we obtain similar estimates as Suárez Serrato and Zidar (2024) for firm owners only. Our estimates for residential landowners vary from specification to specification, but are generally of similar or slightly smaller magnitudes than their estimates. Our estimate on workers (which we take from Fuest et al. (2018)) is lower than the estimate by Suárez Serrato and Zidar (2024). This is potentially unsurprising due to institutional differences. The German LBT is set on the municipal level, while Suárez Serrato and Zidar (2024) rely on variation at the state level. Workers are likely more mobile between municipalities (with a median population of about 1,500) than they are between US states. This could explain the smaller incidence falling on workers in our setting.

5 Conclusion

This paper studies the effect of local business taxes on commercial property prices. For identification, we exploit the German institutional setting of business taxation in which we observe more than 17,000 business tax reforms between 2008 and 2019. We combine administrative data on tax rates and municipalities with real estate micro data that is unique in two dimensions. First, it functions as a convincing proxy for the cost of commercial land as a production factor, as it specifically covers only commercially used properties. Second, the dataset lever-

the perspective of trying to understand which type of agent is most affected by the tax change. The approach does not take into account that the groups have different shares of overall income. Suárez Serrato and Zidar (2024) extend their incidence estimates to include income share weighted estimates, which captures the different income shares of the groups. This mostly decreases the share borne by landowners and slightly increases the share falling on workers and firm owners. We show income share weighted estimates in Table B.6 in the Appendix.

ages information from 140 different sources on over 2.9 million offered rents and 1 million offered sales observations of commercial properties.

We implement an event study setup and find that business tax increases capitalize into lower commercial property prices. Following a one percentage point local business tax increase, the sales prices of commercial buildings reduce by about three percent after four years. To alleviate concerns that the results are driven by heterogeneous treatment effects across municipalities and over time we apply the estimator by De Chaisemartin and d'Haultfoeuille (2024) that is robust to treatment effect heterogeneity. This is particularly important as, in our baseline estimations, we allow for dynamic, continuous, and multiple treatments. The analysis confirms our results, both in sign and magnitude.

Expanding the model developed by Suárez Serrato and Zidar (2016), we obtain incidence shares for commercial landowners, workers, residential landowners, and firm owners. Commercial landowners bear a substantial part of the burden with about 40 percent that increases over time. Between 16 and 25 percent of the incidence falls on residential landowners, while workers bear between 7 and 9 percent. Finally, about 36 percent of the incidence falls on firm owners. Our analysis shows that it is crucial to account for both commercial and residential landowners in an evaluation of corporate taxation. We also show that there is a dynamic response to tax changes. While firm owners bear most of the burden immediately after the tax hike, the adjustment of real estate and labor markets leads to a shift towards the other agents over time.

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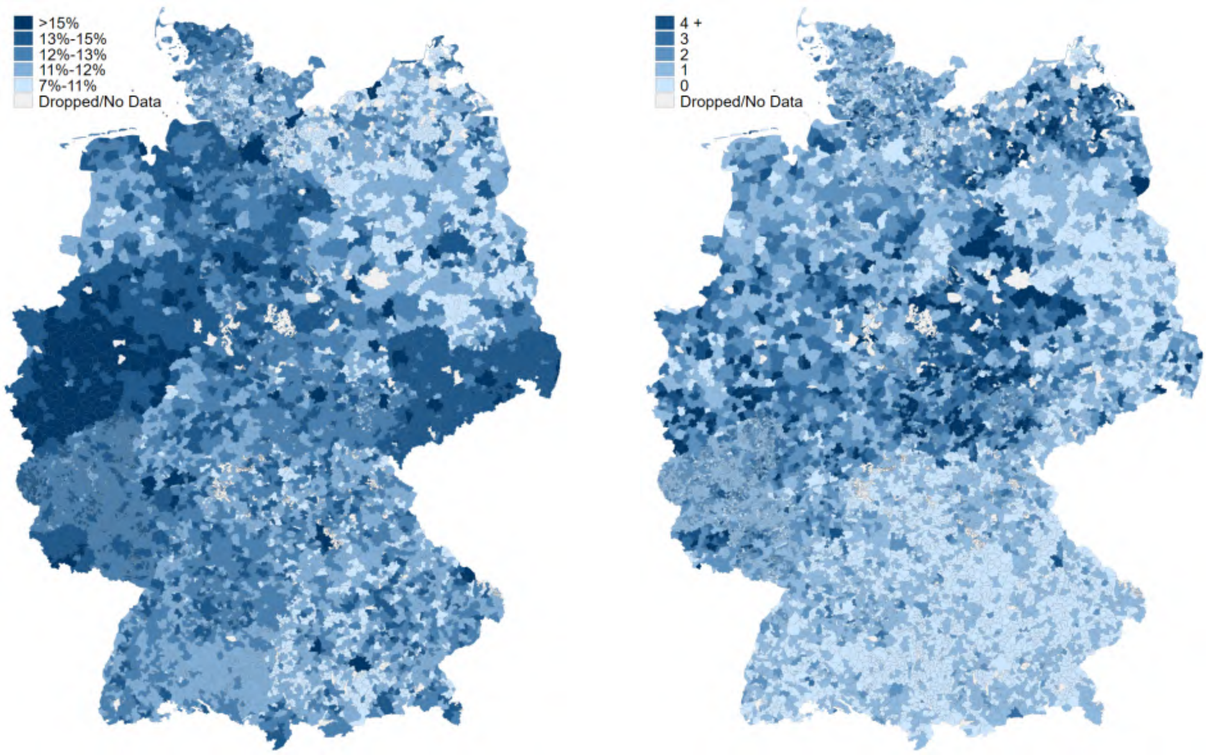
Appendix

A Descriptive Statistics

Figure A.1: Tax Variation - All municipalities

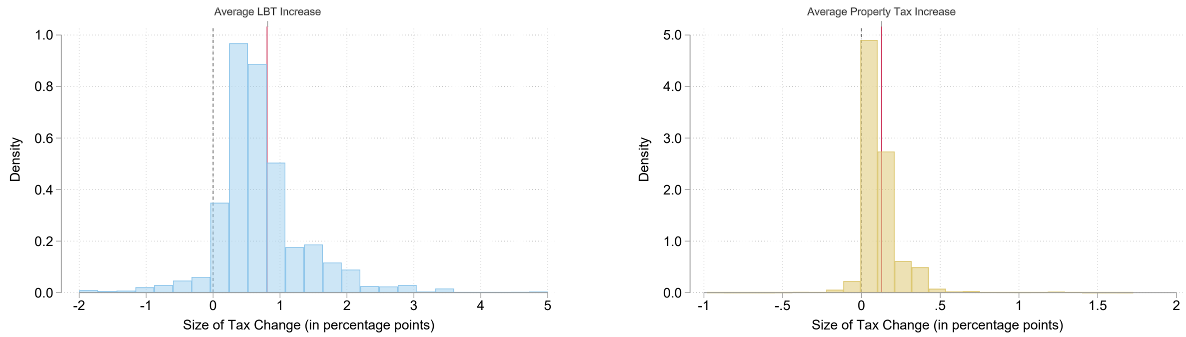
(a) Average LBT Rates

(b) Number of Taxhikes



Notes: The figure shows the number of municipal LBT increases in Germany for a sample of 10,775 municipalities. Panel A.1a shows the average LBT rate (in percent) for each municipality between 2008 and 2019. Panel A.1b shows the number of taxhikes for each municipality between 2008 and 2019.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.2: Histogram of Tax Increases



(a) Local Business Tax Changes

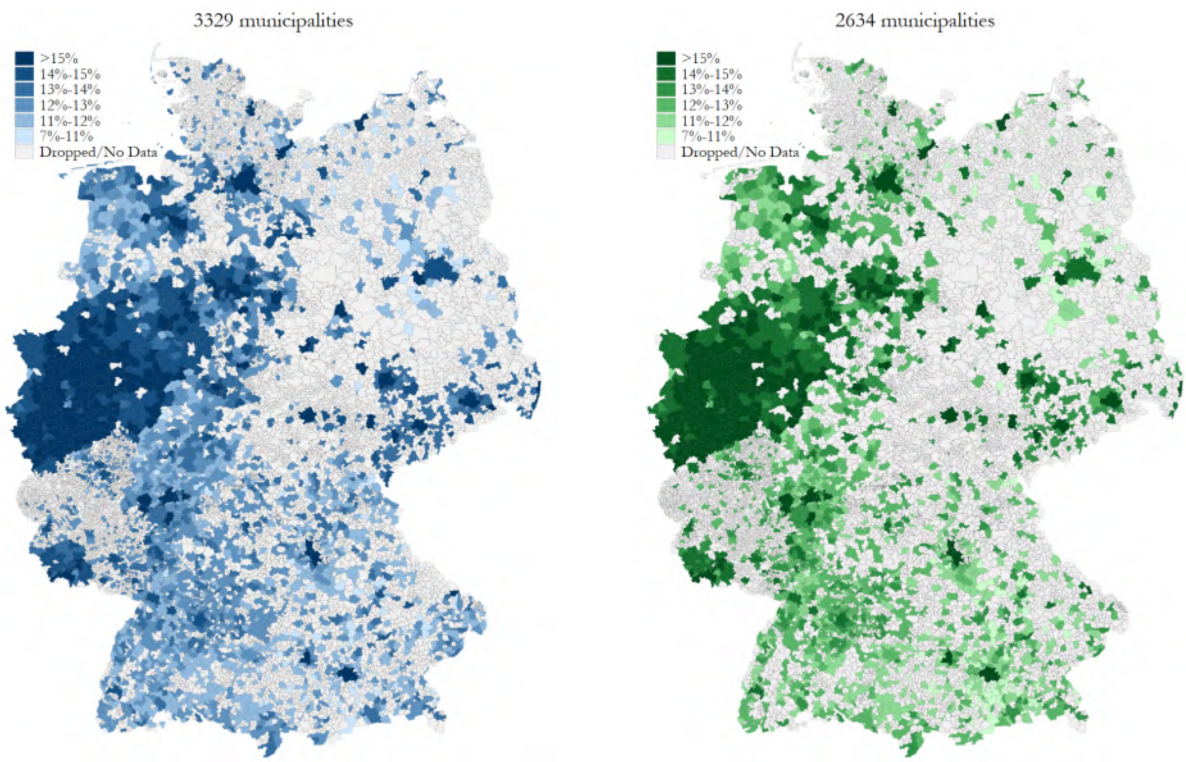
(b) Local Property Tax Changes

Notes: The histogram displays changes in the LBT rate, induced by municipal scaling factor changes between 2004 and 2023 for the 10,724 municipalities that were not subject to merger reforms. The average increase in the local business tax (excluding tax drops) is 0.806 percentage points, the 75th percentile increase is at 1.5 percentage points. The average increase in the property tax (excluding tax drops) is 0.127 percentage points, the 75th percentile increase is at 0.157 percentage points. The number total tax changes between 2003 and 2023 amounts to 22,916. For illustrative reasons, around 0.1 percent of observations with increases greater than 5 (2) or smaller than -2 (-1) percentage points for the LBT (LPT) are omitted.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.3: Average LBT Rates (2008-19)

(a) Commercial Sales Sample

(b) Commercial Rents Sample

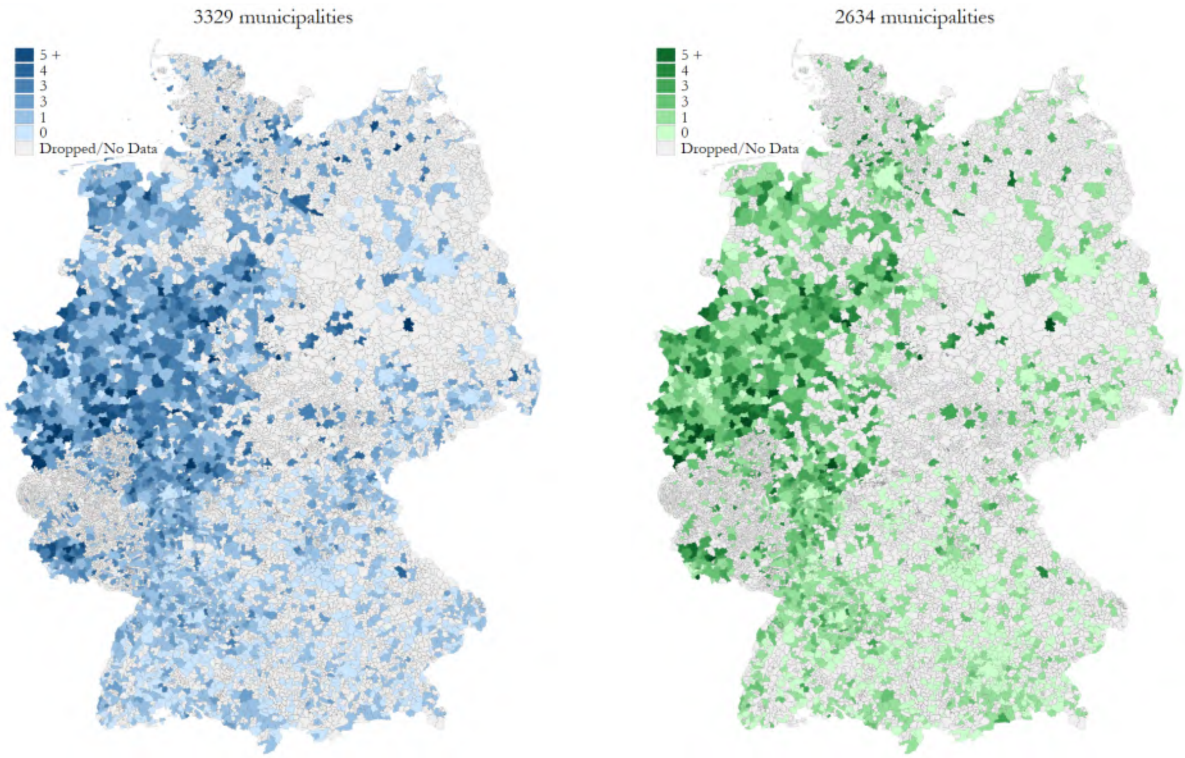


Notes: The figure shows the number of municipal LBT increases in Germany for a sample of 10,775 municipalities. Grey areas indicate municipalities that experienced municipal merger reforms or tax cuts during the sample period (2008-2019) and are consequently dropped from the sample. Panel A.2b shows the average LBT rate (in percent) for each municipality between 2008 and 2019.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.4: Number of LBT Hikes (2008-19)

(a) Commercial Sales Sample

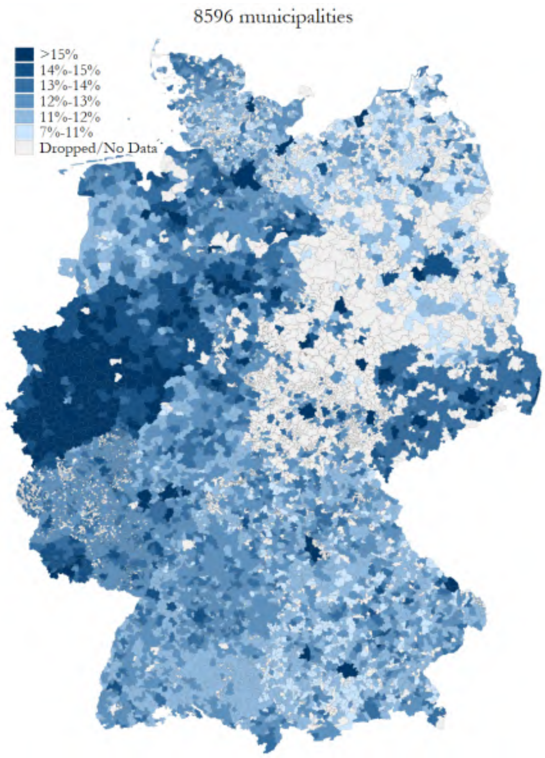
(b) Commercial Rents Sample



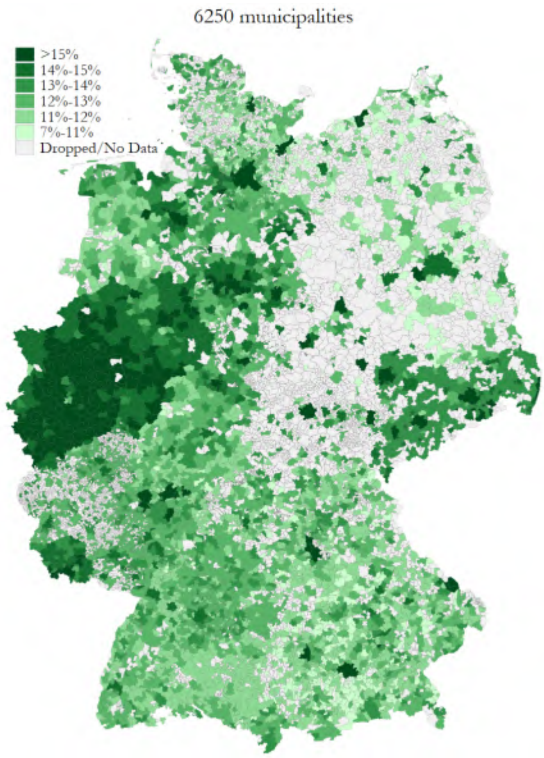
Notes: The figure shows the number of municipal LBT increases in Germany for a sample of 10,775 municipalities. Grey areas indicate municipalities that experienced municipal merger reforms or tax cuts during the sample period (2008-2019) and are consequently dropped from the sample. Panel A.5a shows the average LBT rate (in percent) for each municipality between 2008 and 2019. Panel A.5b plots the number of tax increases (defined as a scaling-factor induced LBT increase) between 2008 and 2019.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.5: Average LBT Rates (2008-19)

(a) Residential Sales Sample



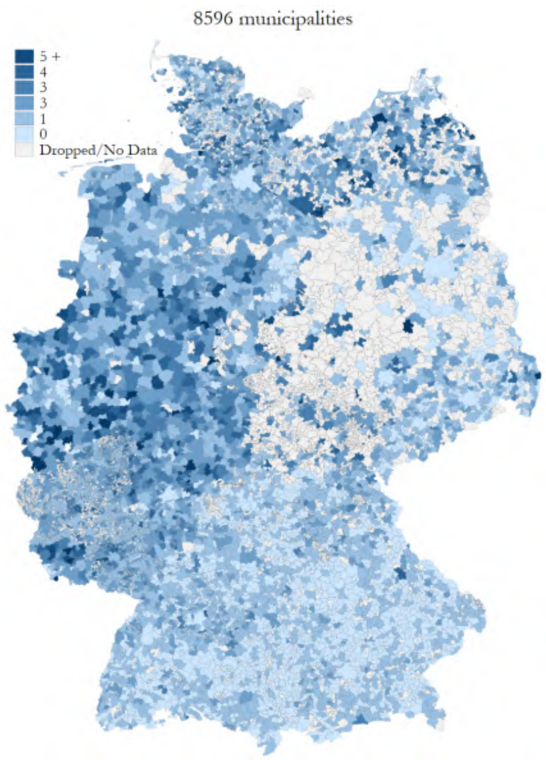
(b) Residential Rents Sample



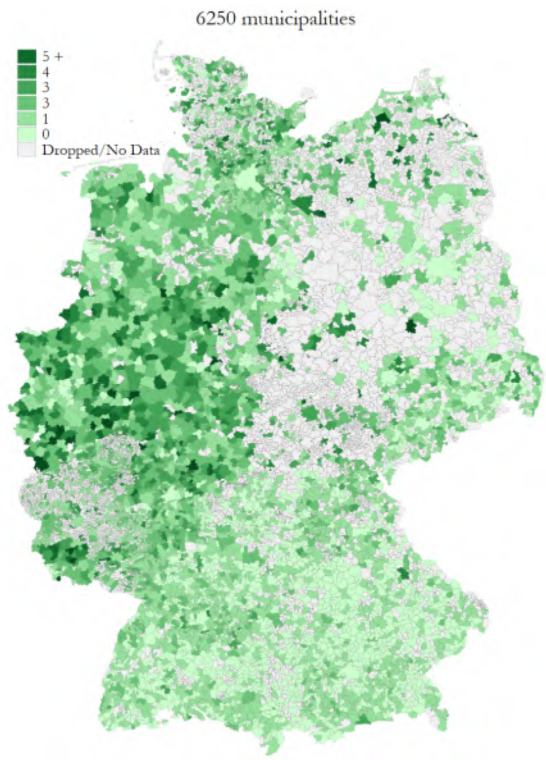
Notes: The figure shows the number of municipal LBT increases in Germany for a sample of 10,775 municipalities. Grey areas indicate municipalities that experienced municipal merger reforms or tax cuts during the sample period (2008-2019) and are consequently dropped from the sample. Panel A.2b shows the average LBT rate (in percent) for each municipality between 2008 and 2019.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.6: Number of LBT Hikes (2008-19)

(a) Residential Sales Sample

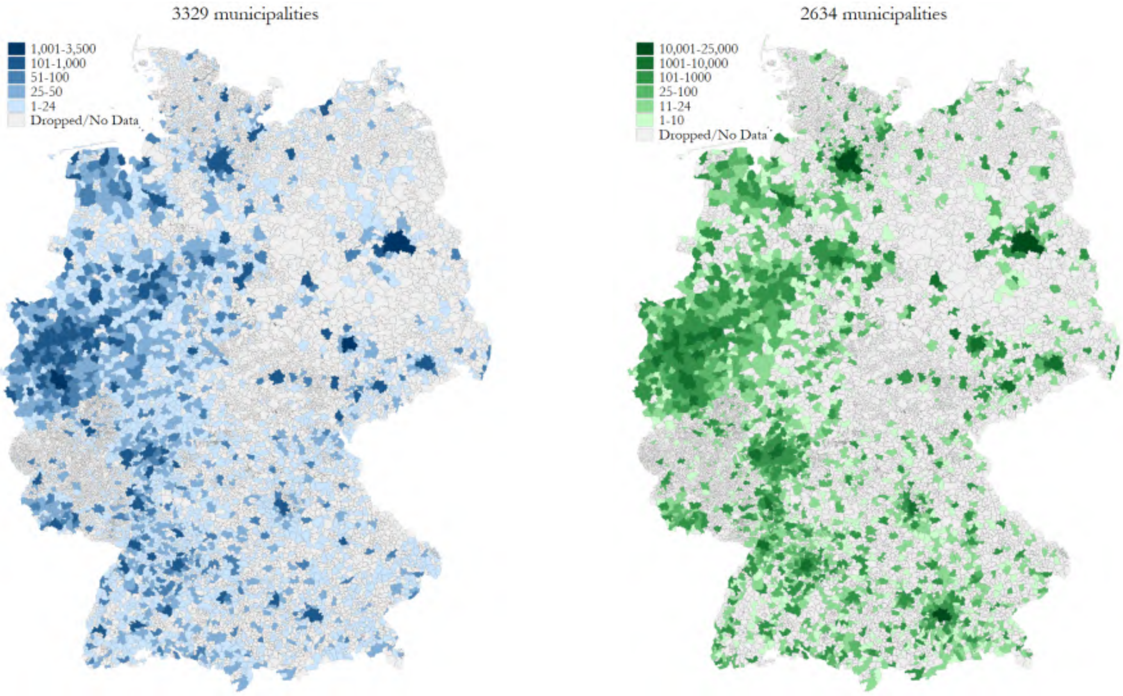


(b) Residential Rents Sample



Notes: The figure shows the number of municipal LBT increases in Germany for a sample of 10,775 municipalities. Grey areas indicate municipalities that experienced municipal merger reforms or tax cuts during the sample period (2008-2019) and are consequently dropped from the sample. Panel A.6a shows the average LBT rate (in percent) for each municipality between 2008 and 2019. Panel A.6b plots the number of tax increases (defined as a scaling-factor induced LBT increase) between 2008 and 2019.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.7: Average Number of Postings per Year (2008-2019) – Commercial Samples



(a) Sales Sample

(b) Rents Sample

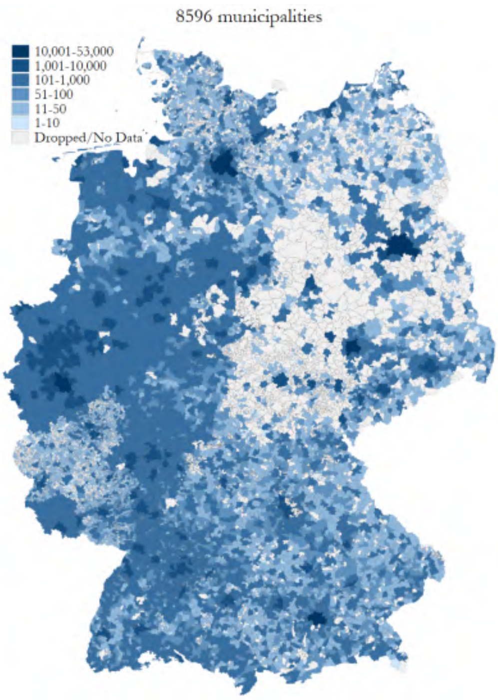
Notes: The figure shows spatial variation in the average number of advertising postings in Germany for a sample of roughly 3,329 municipalities in the commercial sales sample (Panel A.7a) and roughly 2,634 municipalities in the commercial rents sample (Panel A.7b) between 2008 and 2019. Grey areas indicate municipalities for which we observe not at least one posting per sample year or that we drop as they either experienced municipal merger reforms, tax cuts during the sample period (2008-2019)), or for which we do not observe at least one postings per year. *Source*: Own calculation based on data from *F+B* and the Statistical State Offices.

Table A.1: Sample Selection - Sales Data

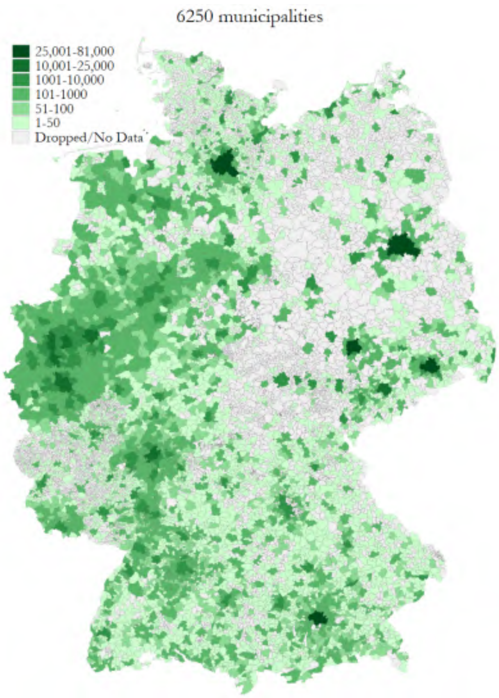
	# Municipalities	# Tax Hikes	# Properties
Municipality Data (2003–23)	10,775	22,916	-
Dropped mergers	10,091	20,686	-
No tax drops	9,288	18,809	-
Merge with Real Estate Data	8,727	17,941	35,225,148
(i) Commercial Sales Data	3,329	7,101	1,179,468
(ii) Commercial Rent Data	2,634	5,481	2,996,770
(iii) Residential Sales Data	8,596	17,350	16,326,103
(iv) Residential Rent Data	6,250	12,476	14,722,807
Balanced Data	2,242	4,987	27,674,326

Notes: The table shows the number of municipalities, tax hikes, and property price observations per sample selection step for the sales property price samples used in the analysis. *Source:* Statistical State Offices.

Figure A.8: Average Number of Postings per Year (2008-2019) – Residential Samples



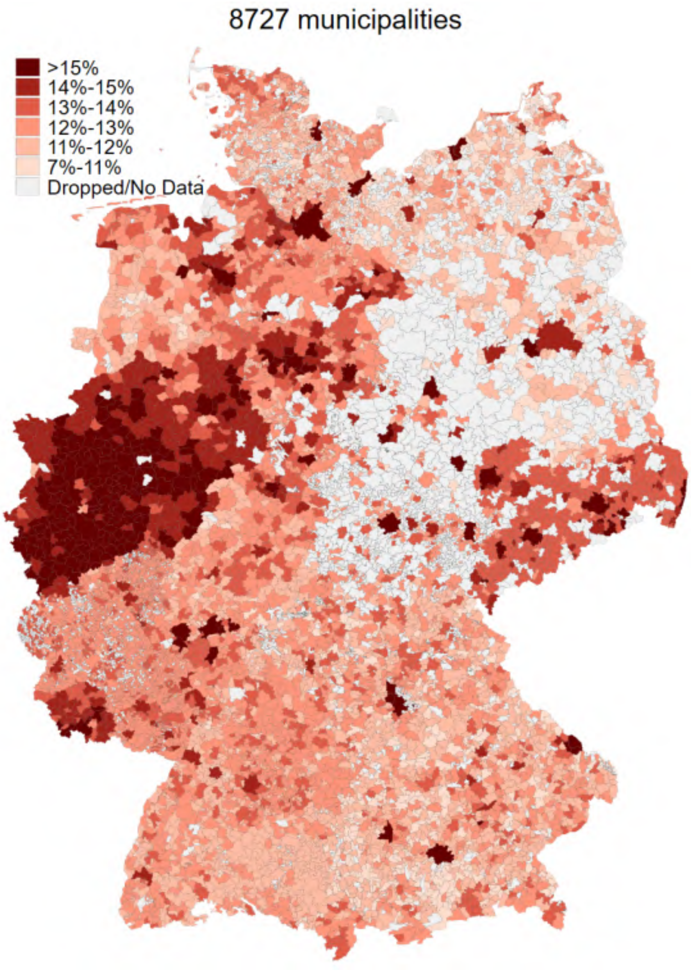
(a) Sales Sample



(b) Rents Sample

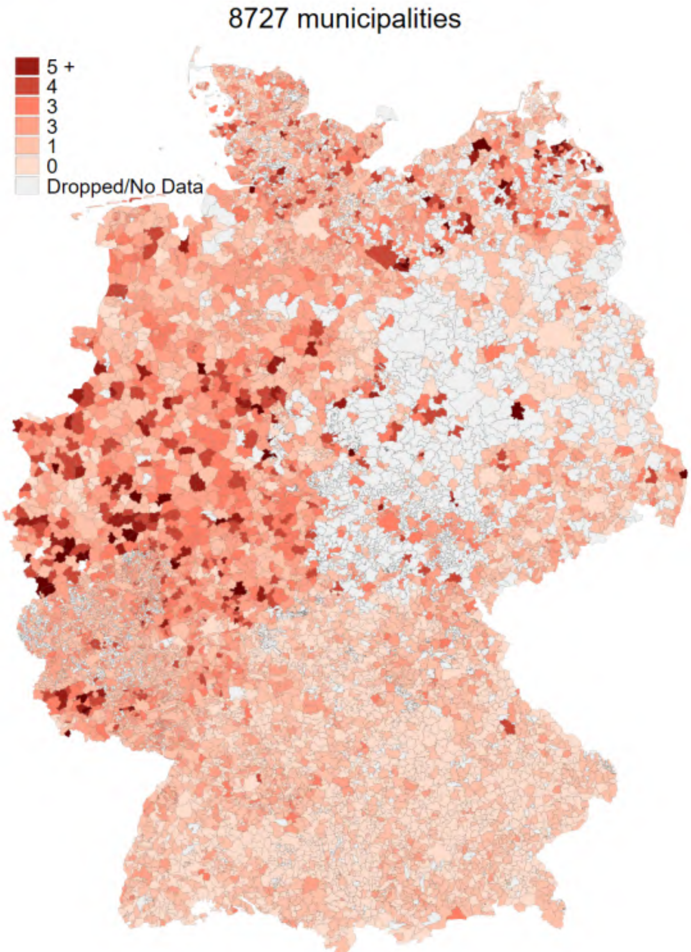
Notes: The figure shows spatial variation in the average number of advertising postings in Germany for a sample of roughly 8,600 municipalities in the residential sales sample (Panel A.8a) and roughly 6,250 municipalities in the residential rents sample (Panel A.8b) between 2008 and 2019. Grey areas indicate municipalities for which we observe not at least one posting per sample year or that we drop as they either experienced municipal merger reforms, tax cuts during the sample period (2008-2019)), or for which we do not observe at least one postings per year. *Source*: Own calculation based on data from *F+B* and the Statistical State Offices.

Figure A.9: Average LBT Rate in Real Estate Sample (2008-2019)



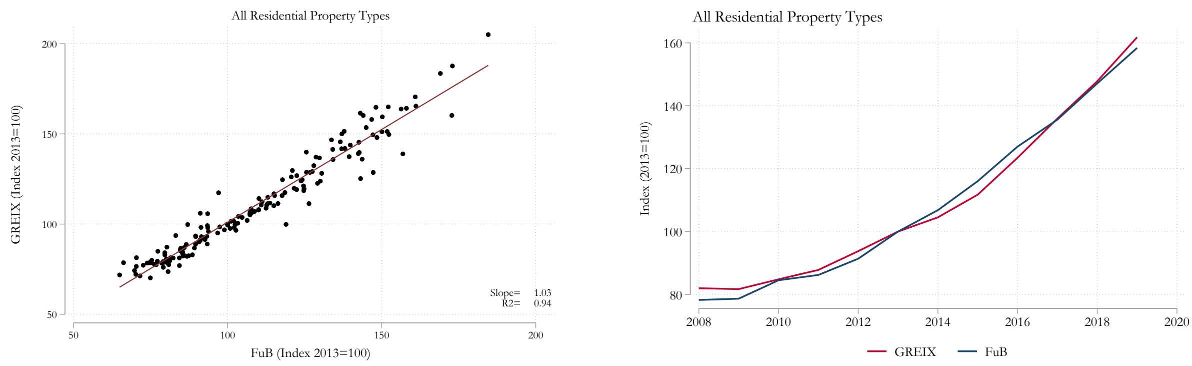
Notes: The figure shows spatial variation in the average LBT in Germany for a sample of 8727 municipalities for which we observe property prices between 2008 and 2019. Grey areas indicate municipalities for which we observe not at least one posting per sample year or that we drop as they either experienced municipal merger reforms or tax cuts during the sample period (2008-2019). *Source*: Own calculation based on data from *F+B* and the Statistical State Offices.

Figure A.10: Number of LBT Hikes in Real Estate Sample (2008-2019)



Notes: The figure shows spatial variation in the number of LBT hikes in Germany for a sample of 8727 municipalities for which we observe property prices between 2008 and 2019. Grey areas indicate municipalities for which we observe not at least one posting per sample year or that y<we drop as they either experienced municipal merger reforms or tax cuts during the sample period (2008-2019). *Source*: Own calculation based on data from *F+B* and the Statistical State Offices.

Figure A.11: Data Validation of Property Offering Prices: Evolution of *F+B* Property Offerings Price Index and German Real Estate Index (*GREIX*) 2008-2019

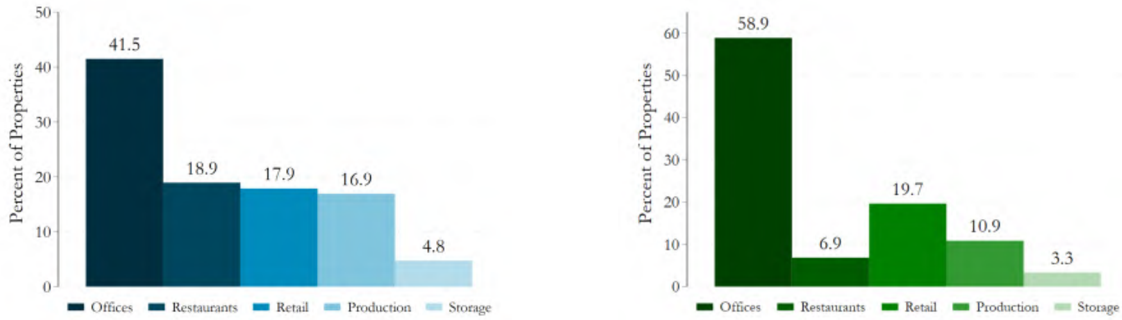


(a) Scatter Plot

(b) Line Plot

Notes: This figure illustrates the evolution of residential real estate price indices in 19 of Germany's largest counties/cities (Berlin, Hamburg, Munich, Cologne, Frankfurt, Stuttgart, Dusseldorf, Bonn, Chemnitz, Dortmund, Dresden, Duisburg, Erfurt, Karlsruhe, Leipzig, Lübeck, Münster, Potsdam and Wiesbaden which together represent roughly 16% of the German population) from 2008 to 2019. Panel A.11a illustrates the correlation between the *F+B* property offering price indices, which are used in the analyses of this paper (on the X-axis). The y-axis depicts the *GREIX*, an administratively compiled index based on actual transaction prices reported by municipal property valuation committees (Gutachterausschüsse). Both indices track prices of residential sales of apartments, single- and multi-family houses and are normalized to an index value of 100 in 2013. Panel A.11b shows a corresponding line plot over time where the blue line represents the *F+B* property offering price indices, while red lines depicts the *GREIX* index.
Source: Own calculation based on data from *F+B* and *GREIX*.

Figure A.12: Distribution of Commercial Property Types

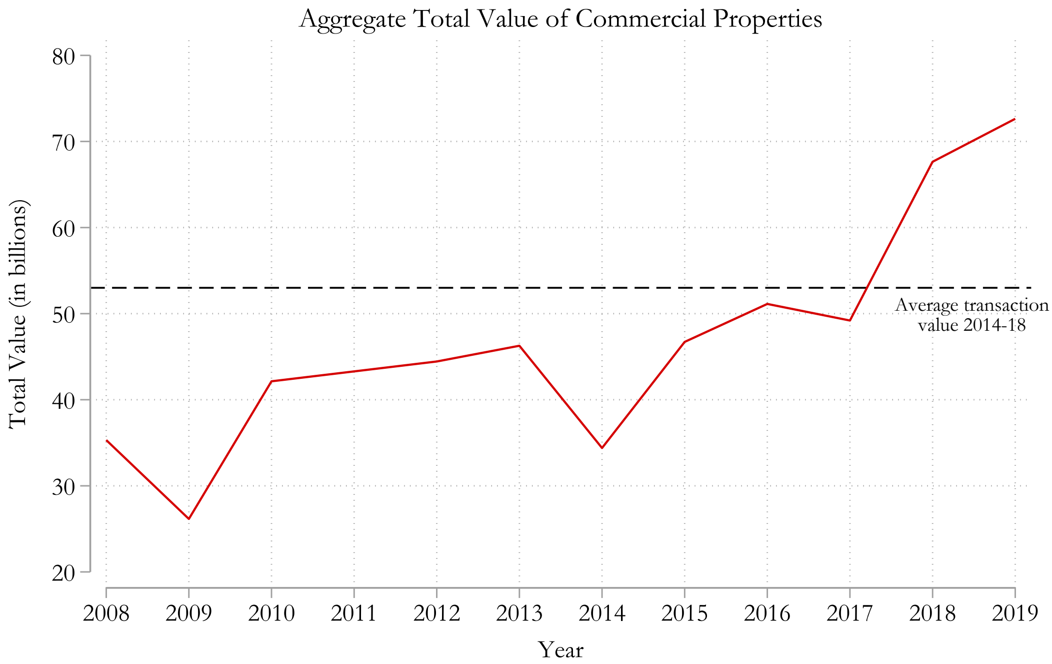


(a) Sales Sample

(b) Rents Sample

Notes: The figure shows the distribution of property types for both the commercial sales sample (Panel A.12a) the commercial rents sample (Panel A.12b) between 2008 and 2019. Every municipality in the data comes with one of the five property type labels listed in the figure such there are no missings. The number of properties in the sales sample is 1,002,272, the number of properties in the rents sample is 2,347,571. *Source*: Own calculation based on data from *F+B*.

Figure A.13: Aggregate Value of Commercial Properties offered for Sale



Notes: This figure shows the development of the aggregate value of commercial properties offered for sale in our data. *Source*: Own calculation based on data from *F+B*.

Table A.2: Summary Statistics: Commercial Sales Price Baseline Sample

	Mean	Std. Dev.	Min	Max	N	Years
Panel A – Property Variables						
Price (in €/m^2)	1,520	1,125	59.50	6,000	965,582	2008-19
First price (in €/m^2)	1,504	1,053	86.81	5,273	965,582	2008-19
Estimated transaction price (in €/m^2)	1,520	1,125	60	6,000	965,582	2008-19
Construction year	1962	54	1500	2022	791,926	2008-19
Floor size (in m^2)	585.8	1,640	1	99,329	965,582	2008-19
# Rooms	3.430	5.569	0	99	965,582	2008-19
Basement dummy	0.255	0.436	0	1	965,582	2008-19
Parking spots dummy	0.497	0.500	0	1	965,582	2008-19
Web portal dummy	0.743	0.437	0	1	965,582	2008-19
Panel B – Tax Variables						
LBT rate (in %)	13.86	1.807	7	20.30	965,582	2008-19
LBT change (in %p)	0.0732	0.335	-6.400	3.850	965,582	2008-19
LBT hike dummy	0.123	0.328	0	1	965,582	2008-19
# total tax changes	2.038	1.682	0	14	965,582	2008-19
Property tax rate (in %)	1.533	0.446	0.700	3.675	965,582	2008-19
Panel C – Economic Indicators						
Muni. Population	263,485	736,157	191	3,669,000	965,582	2008-19
Dist. GDP per capita (in €)	35,227	13,647	14,065	184,312	965,582	2008-19
Dist. Unemployment rate (in %)	7.06	3.25	1.40	21.24	965,582	2008-19

Notes: This table provides descriptive statistics for the baseline sales price estimation sample after merging the municipality data with commercial property sales price data. Observations in municipalities that are subject to merger reforms or experience tax cuts during the sample period (2008-2019) are excluded. The sample covers 9,556 municipalities and 8,094 tax increases (see Table A.1 for more context on the number of municipalities).
Source: Statistical State Offices and *F+B* .

Table A.3: Summary Statistics: Residential Sales Price Baseline Sample

	Mean	Std. Dev.	Min	Max	N	Years
Panel A – Property Variables						
Price (in €/m^2)	1,959	1,270	100	18,657	13,612,822	2008-19
First price (in €/m^2)	1,919	1,080	275.9	6,945	13,340,559	2008-19
Estimated transaction price (in €/m^2)	1,872	1,234	93.92	17,559	13,612,822	2008-19
Construction year	1980	39	1500	2022	11,795,165	2008-19
Floor size (in m^2)	137.5	105.6	12	12,000	13,612,822	2008-19
# Rooms	4.399	2.805	0	95	13,612,822	2008-19
Basement dummy	0.426	0.494	0	1	13,612,822	2008-19
Parking spots dummy	0.620	0.485	0	1	13,612,822	2008-19
Web portal dummy	0.798	0.401	0	1	13,612,822	2008-19
Panel B – Tax Variables						
LBT rate (in %)	13.63	1.853	7	20.30	13,612,822	2008-19
LBT change (in %p)	0.0610	0.362	-6.400	6.300	13,612,822	2008-19
LBT hike dummy	0.107	0.310	0	1	13,612,822	2008-19
# total tax changes	1.930	1.639	0	14	13,612,822	2008-19
Property tax rate (in %)	1.489	0.450	0.700	3.675	13,612,822	2008-19
Panel C – Economic Indicators						
Muni. Population	290,848	775,086	38	3,669,000	13,612,822	2008-19
Dist. GDP per capita (in €)	35,227	13,647	14,065	184,312	13,612,822	2008-19
Dist. Unemployment rate (in %)	7.06	3.25	1.40	21.24	13,612,822	2008-19

Notes: This table provides descriptive statistics for the baseline sales price estimation sample after merging the municipality data with commercial property sales price data. Observations in municipalities that are subject to merger reforms or experience tax cuts during the sample period (2008-2019) are excluded. The sample covers 9,556 municipalities and 8,094 tax increases (see Table A.1 for more context on the number of municipalities).
Source: Statistical State Offices and *F+B* .

Table A.4: Summary Statistics: Residential Rental Price Baseline Sample

	Mean	Std. Dev.	Min	Max	N	Years
Panel A – Property Variables						
Price (in €/m^2)	7.467	3.206	2	59.93	11,404,748	2008-19
First price (in €/m^2)	7.363	2.718	3.556	20.49	11,177,229	2008-19
Estimated transaction price (in €/m^2)	7.467	3.206	2	59.93	11,404,748	2008-19
Construction year	1975	38	1500	2021	7,010,850	2008-19
Floor size (in m^2)	79.56	37.08	12	970	11,404,748	2008-19
# Rooms	2.687	1.263	0	15	11,404,748	2008-19
Basement dummy	0.452	0.498	0	1	11,404,748	2008-19
Parking spots dummy	0.481	0.500	0	1	11,404,748	2008-19
Web portal dummy	0.804	0.397	0	1	11,404,748	2008-19
Panel B – Tax Variables						
LBT rate (in %)	14.42	1.849	7	20.30	11,404,748	2008-19
LBT change (in %p)	0.0591	0.342	-6.400	6.300	11,404,748	2008-19
LBT hike dummy	0.102	0.303	0	1	11,404,748	2008-19
# total tax changes	1.683	1.577	0	14	11,404,748	2008-19
Property tax rate (in %)	1.667	0.494	0.700	3.675	11,404,748	2008-19
Panel C – Economic Indicators						
Muni. Population	478,993	937,432	42	3,669,000	11,404,748	2008-19
Dist. GDP per capita (in €)	35,227	13,647	14,065	184,312	11,404,748	2008-19
Dist. Unemployment rate (in %)	7.06	3.25	1.40	21.24	11,404,748	2008-19

Notes: This table provides descriptive statistics for the baseline residential rents estimation sample after merging the municipality data with residential rents data. Observations in municipalities that are subject to merger reforms or experience tax cuts during the sample period (2008-2019) are excluded. The sample covers 9,556 municipalities and 8,094 tax increases (see Table A.1 for more context on the number of municipalities).
Source: Statistical State Offices and *F+B* .

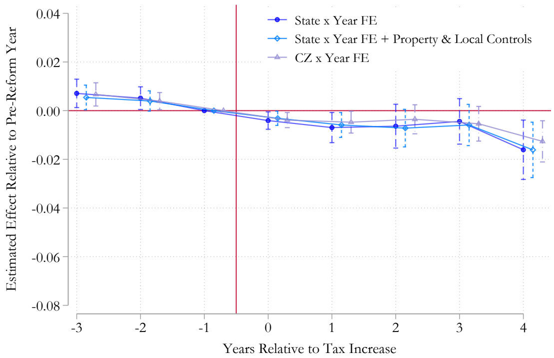
Table A.5: Summary Statistics: Commercial Rental Price Baseline Sample

	Mean	Std. Dev.	Min	Max	N	Years
Panel A – Property Variables						
Price (in €/m^2)	11.07	16.17	0.221	600	2,347,571	2008-19
First price(in €/m^2)	9.999	7.481	1.083	75	2,300,948	2008-19
Estimated transaction price (in €/m^2)	10.68	15.53	0.199	600	2,347,571	2008-19
Construction year	1,973	43.97	1,500	2,021	1,212,028	2008-19
Floor size (in m^2)	663.9	2,594	1	99,999	2,347,571	2008-19
# Rooms	0.570	1.504	0	15	893,259	2008-19
Basement dummy	0.158	0.365	0	1	2,347,571	2008-19
Parking spots dummy	0.370	0.483	0	1	2,347,571	2008-19
Web portal dummy	0.779	0.415	0	1	2,347,571	2008-19
Panel B – Tax Variables						
LBT rate (in %)	14.78	1.794	7	20.30	2,347,571	2008-19
LBT change (in %p)	0.0489	0.271	-6.400	3.500	2,347,571	2008-19
LBT hike dummy	0.0786	0.269	0	1	2,347,571	2008-19
# total tax changes	1.294	1.473	0	14	2,347,571	2008-19
Property tax rate (in %)	1.774	0.515	0.700	3.675	2,347,571	2008-19
Panel C – Economic Indicators						
Muni. Population	794,933	1.130e+06	179	3,613,495	2,309,905	2008-19
Dist. GDP per capita (in €)	46,343	20,366	14,065	184,312	2,343,232	2008-19
Dist. Unemployment rate (in %)	8.23	3.49	1.40	21.24	2,343,232	2008-19

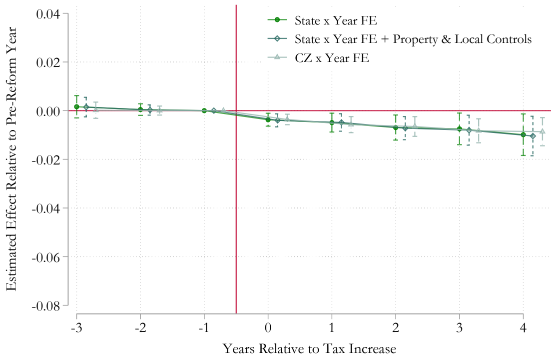
Notes: This table provides descriptive statistics for the baseline rental price estimation sample after merging the municipality data with commercial property rental price data. Observations in municipalities that are subject to merger reforms or experience tax cuts during the sample period (2008-2019) are excluded. The sample covers 8,099 municipalities and 5,852 tax increases (see Table A.1 for more context on the number of municipalities).
Source: Statistical State Offices and *F+B* .

B Additional Results

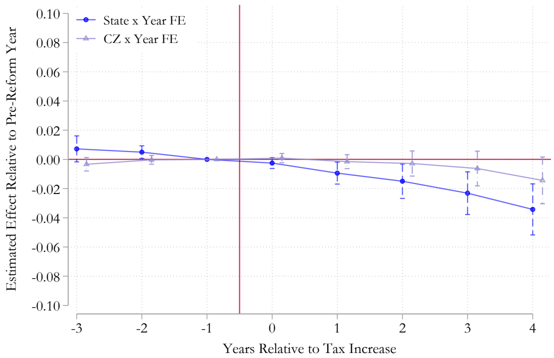
Figure B.14: Baseline Effects on Residential Property Prices



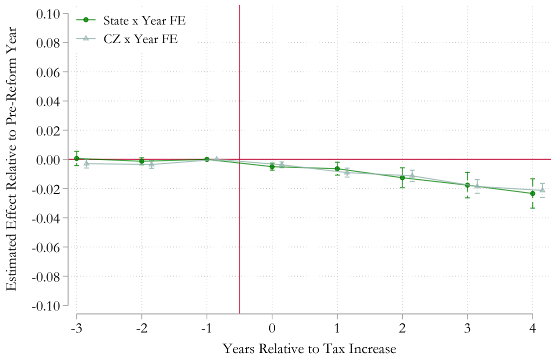
(a) TWFE: Sales Prices



(b) TWFE: Rents



(c) Heterogeneity Robust: Sales Prices



(d) Heterogeneity Robust: Rents

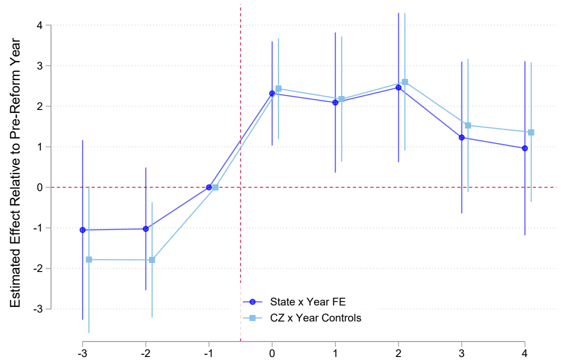
Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log sales price per sqm (Panel B.14a) and the log rental price per sqm (Panel B.14b). Treatment variables are event study indicators scaled by the LBT change. We require at least one ad per municipality-year cell. All regressions include municipal fixed effects and the scaled leads and lags of the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.14c and B.14d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as the discrete number of tax hikes with the first tax hike happening in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.*Source:* Own calculation based on data from *F+B* and Statistical State Offices.

Table B.6: Incidence Estimates - Weighting by Income Shares

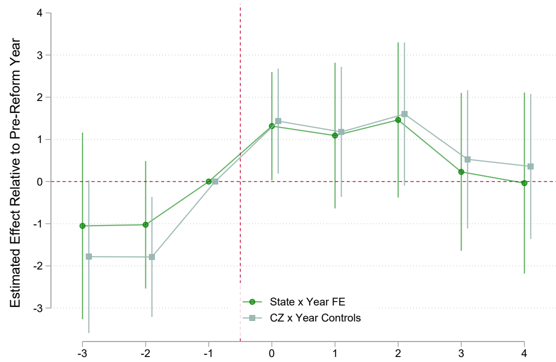
A. Incidence						
Landowners (Residential)	0.874**	0.874**	0.874**	0.809***	0.809***	0.809***
	(0.300)	(0.300)	(0.300)	(0.208)	(0.208)	(0.208)
Landowners (Commercial)	2.207***	2.207***	2.207***	0.780	0.780	0.780
	(0.634)	(0.634)	(0.634)	(0.419)	(0.419)	(0.419)
Workers	0.738***	0.738***	0.738***	0.757***	0.757***	0.757***
	(0.189)	(0.189)	(0.189	(0.217)	(0.217)	(0.217)
Firm owners	2.019**	2.019**	2.019**	2.019**	2.019**	2.019**
	(0.631)	(0.631)	(0.631)	(0.631)	(0.631)	(0.631)
B. Parameters						
ε^{PD}	-2.5	-5	-2.5	-2.5	-5	-2.5
θ	0.24	0.24	0.14	0.24	0.24	0.14
α	0.3	0.3	0.3	0.3	0.3	0.3
δ	0.2	0.2	0.2	0.2	0.2	0.2
C. Share of Incidence						
Landowners (Residential)	6.6%	9.9%	7.3%	6.7%	10.4%	7.2%
Landowners (Commercial)	13.1%	19.5%	9.2%	5.1%	7.9%	3.5%
Workers	12.1%	18.0%	13.4%	13.7%	21.4%	14.8%
Firm owners	68.2%	52.7%	70.1%	74.5%	60.3%	74.6%
Rent or Sales	Sales	Sales	Sales	Rent	Rent	Rent
CZ x Year FE	✓	✓	✓	✓	✓	✓

Notes: This table presents the incidence estimates for landowners, workers and firm owners. Panel A displays the welfare-relevant elasticities described in Table 1. Panel B displays the share of incidence borne by economic agent. Each column displays a different specification for estimating the elasticities. The first three columns use sales prices, and the last three columns use rent prices. All regression models include municipal fixed effects, account for the local property tax rate and "commuting zone (CZ) $\times$ year" fixed effects. The columns display the weighted incidence shares for different values of ε^{PD} and θ. More information about the income shares is provided in Section C.8. The estimation sample is restricted to non-merged municipalities and municipalities that experience no tax cuts between 2008 and 2019. Standard errors are clustered at the municipal level. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

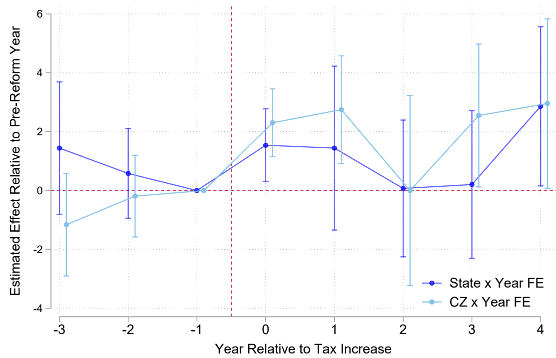
Figure B.15: Baseline Effects on Firm Profits - Elasticities



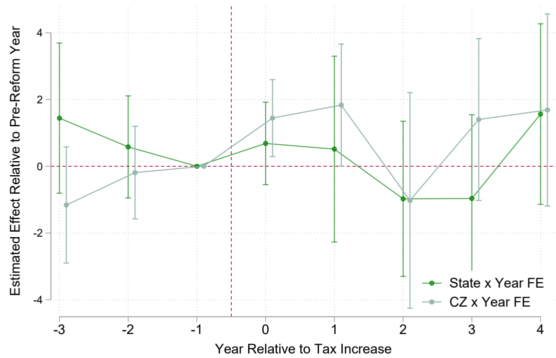
(a) TWFE: Net Profits



(b) TWFE: Gross Profits



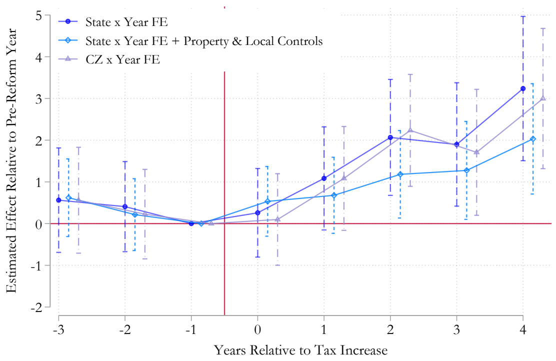
(c) Robust: Net Profits



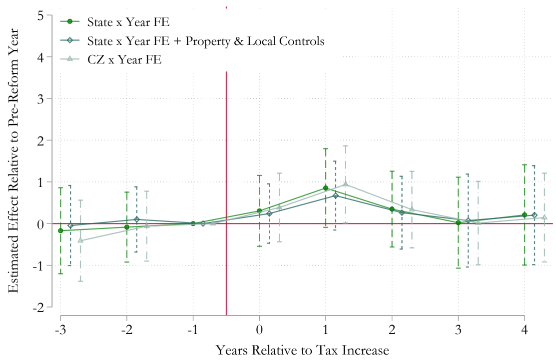
(d) Robust: Gross Profits

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log net profits. Treatment variables are event study indicators scaled by the log net of LBT change. All regressions include municipal fixed effects and the scaled leads and lags of the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. *Source:* Own calculation based on data from *F+B* and Statistical State Offices.

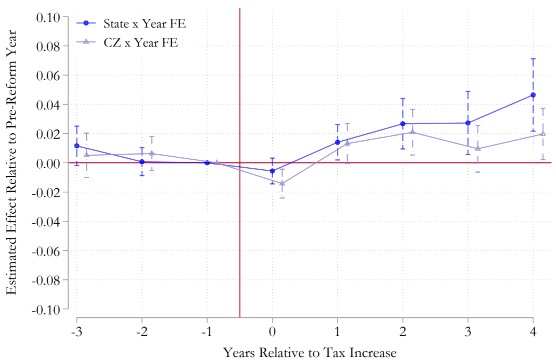
Figure B.16: Baseline Effects on Commercial Properties - Elasticity Measure



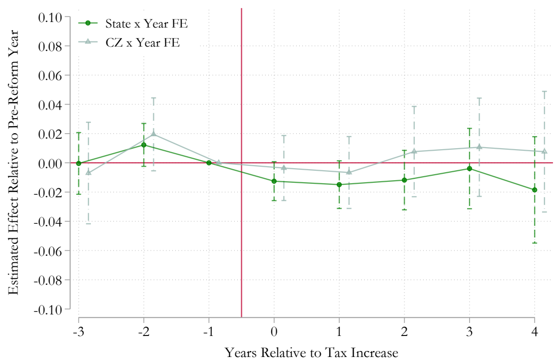
(a) TWFE: Sales Prices



(b) TWFE: Rents



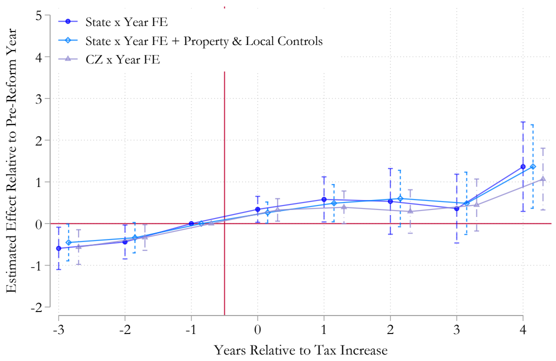
(c) Robustness TWFE: Sales Prices



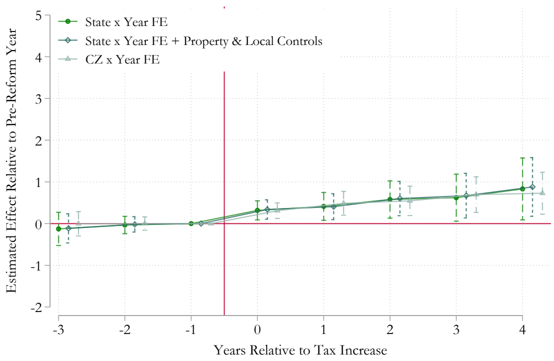
(d) Robustness TWFE: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log sales price per sqm (Panel B.16a) and the log rental price per sqm (Panel B.16b). Treatment variables are event study indicators scaled by the LBT change.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

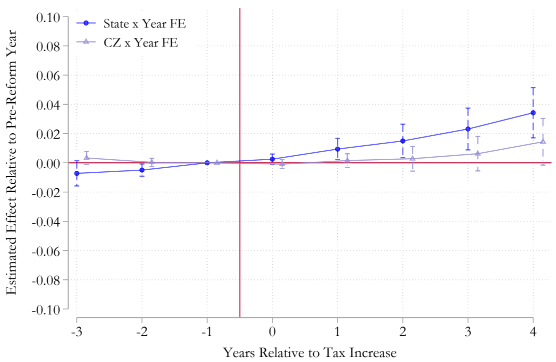
Figure B.17: Baseline Effects on Residential Properties - Elasticity Measure



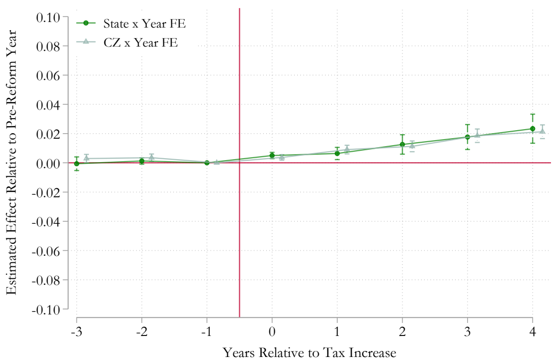
(a) TWFE: Sales Prices



(b) TWFE: Rents



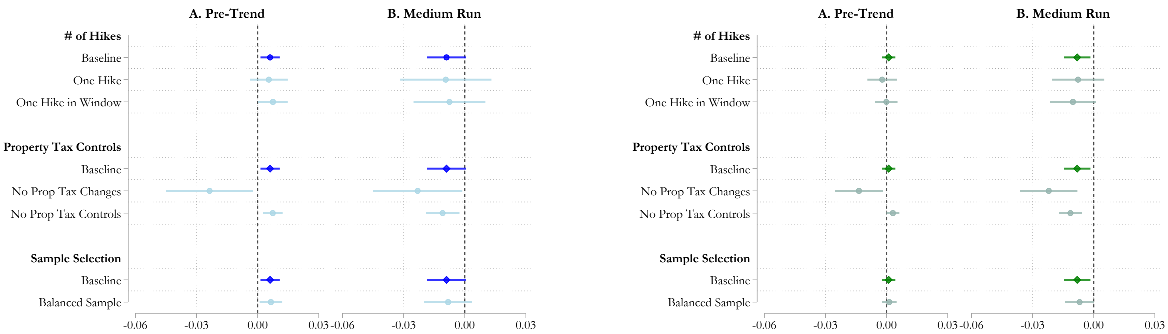
(c) Robustness TWFE: Sales Prices



(d) Robustness TWFE: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log sales price per sqm (Panel B.17a) and the log rental price per sqm (Panel B.17b). Treatment variables are event study indicators scaled by the LBT change.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

Figure B.18: Robustness Analysis – Residential Samples

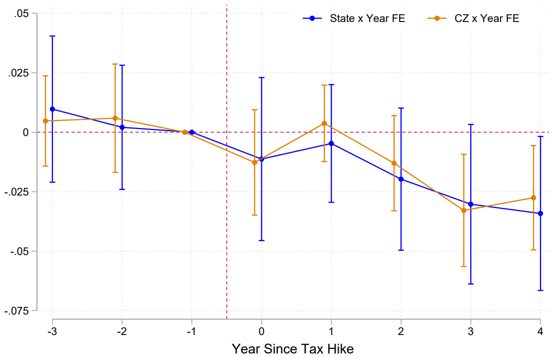


(a) Sales Sample

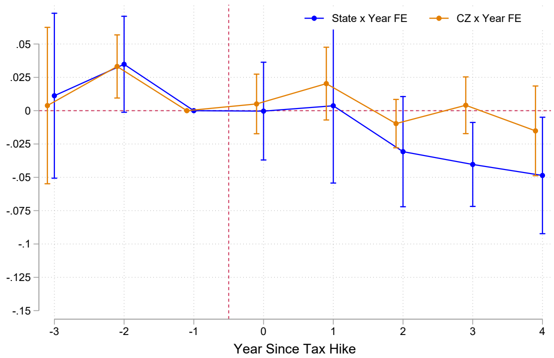
(b) Rental Sample

Notes: This figure presents the results of five major robustness tests assessing the estimated treatment effect of a one percentage point increase in the LBT rate on offered residential sales and rental prices. To improve readability, we summarize pre-treatment effects (the average coefficient for up to three years before a tax change) and medium-run effects (the average estimate for two to four years after a policy change). The first section presents estimates for municipalities that experienced only a single tax increase between 2004 and 2023 or within the (shorter) event window. The second section reports results for the subsample of municipalities that did not experience an LPT change during the sample period, as well as estimates from regressions excluding property tax controls. The final section presents results for the subset of 2,242 municipalities included in all four estimation samples. Unless stated otherwise, all regressions control for the scaled leads and lags of the local property tax rate. Horizontal bars indicate 95% confidence intervals. All specifications include municipal and "state × year" fixed effects, with no additional controls. Standard errors are clustered at the municipality level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

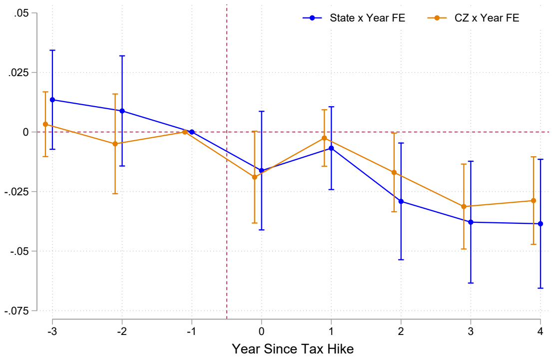
Figure B.19: Effects on Land Prices



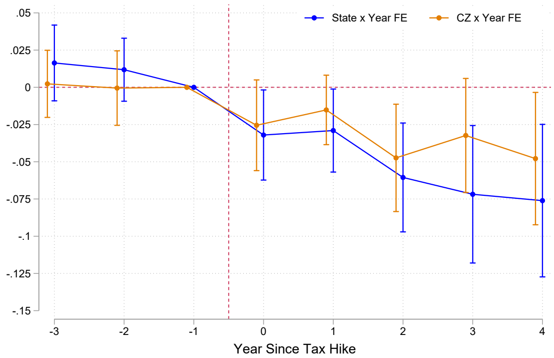
(a) County



(b) Municipality - Bavaria Only



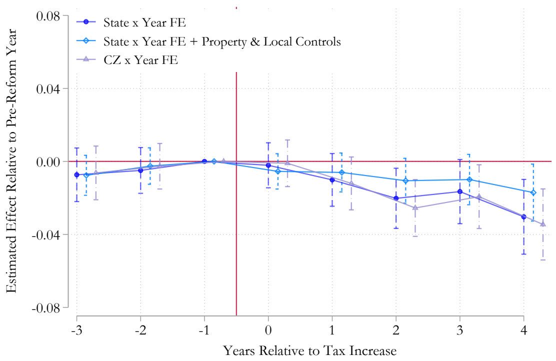
(c) County - Buildable Land



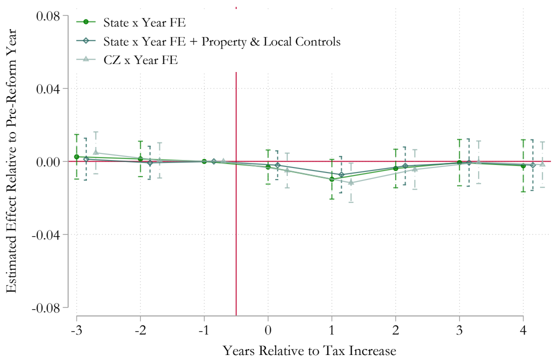
(d) Municipality - Buildable Land - Bavaria Only

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log sales price per sqm. Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state $\times$ year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

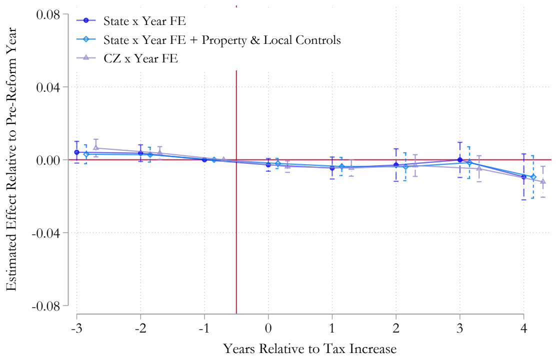
Figure B.20: Baseline Effects: Controlling for Spillovers



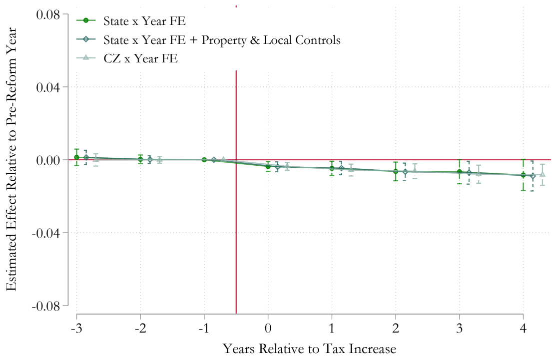
(a) Commercial: Sales Prices



(b) Commercial: Rents



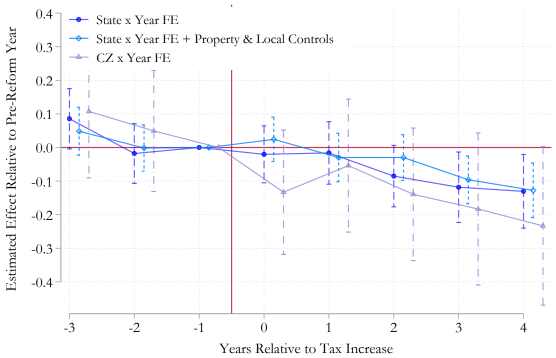
(c) Residential: Sales Prices



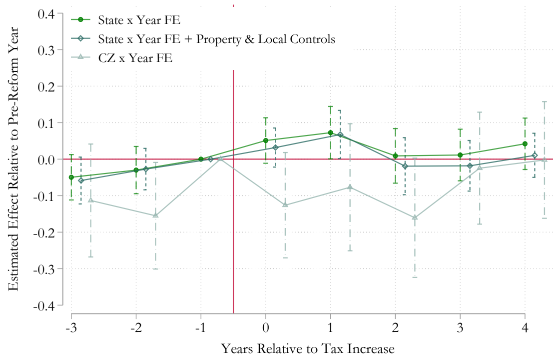
(d) Residential: Rents

Notes: This graph plots the event study estimates $(\hat{\beta}_j, j \in [-3, 4])$ and associated 95% confidence intervals of the event study model from Equation 2. In this specification, we control for weighted average leads and lags of tax changes in municipalities in surrounding areas. The dependent variables are the log sales price per sqm (Panel B.20a) and the log rental price per sqm (Panel B.20b). Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state $\times$ year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.20c and B.20d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as 1 in the year of the first (and only) tax hike in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

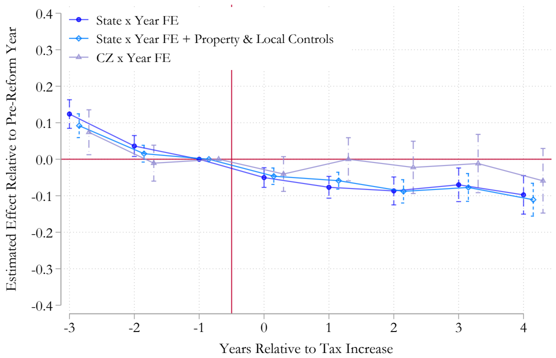
Figure B.21: Effect of Tax Changes in Surrounding Municipalities



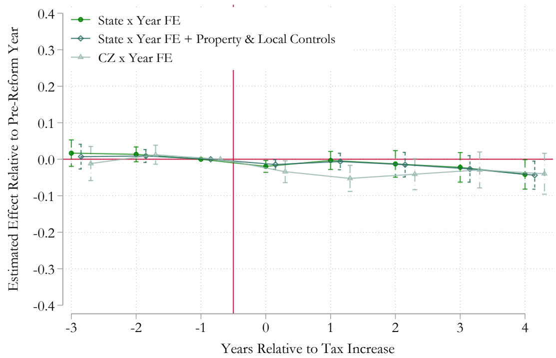
(a) Commercial: Sales Prices



(b) Commercial: Rents



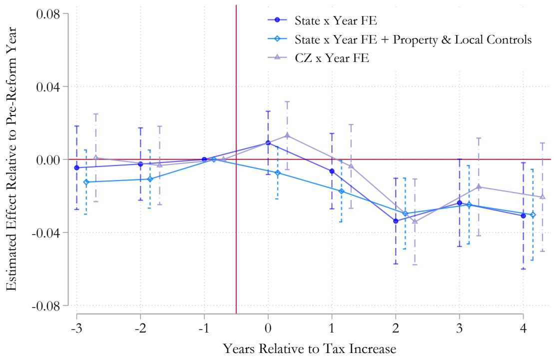
(c) Residential: Sales Prices



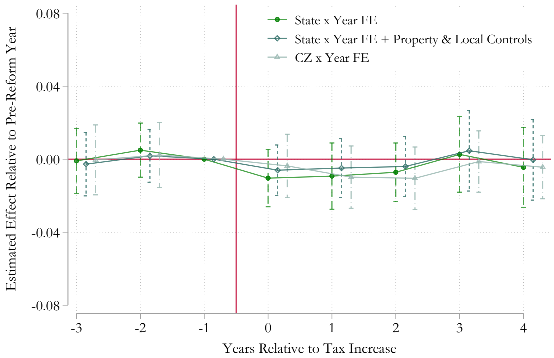
(d) Residential: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals for an event study model from Equation 2 that includes also weighted average leads and lags of tax changes in surrounding municipalities. The Figures plot the coefficient estimates for these weighted average leads and lags of the tax changes to analyse spillover effects of tax changes on other municipalities. The dependent variables are the log sales price per sqm (Panel B.21a) and the log rental price per sqm (Panel B.21b). Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state $\times$ year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.21c and B.21d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as 1 in the year of the first (and only) tax hike in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

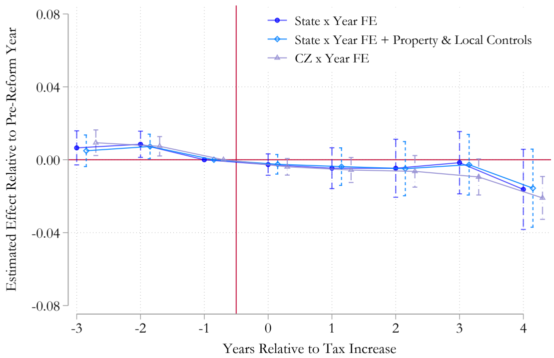
Figure B.22: Baseline Effects: Only One Tax Hike in Event Window



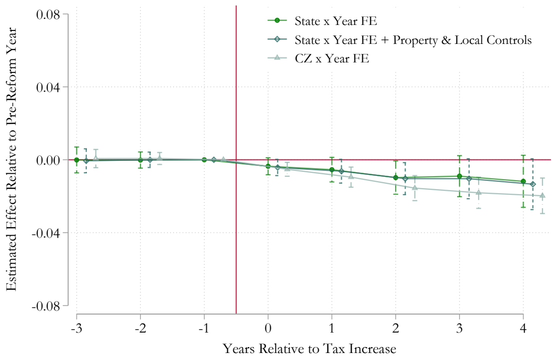
(a) Commercial: Sales Prices



(b) Commercial: Rents



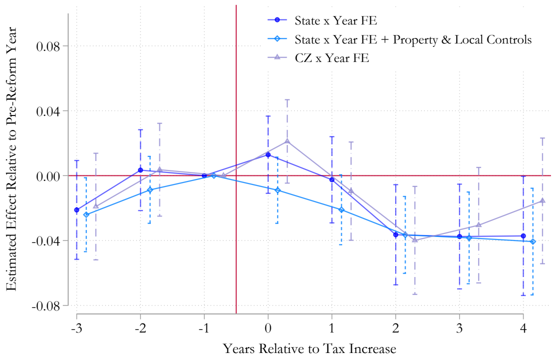
(c) Residential: Sales Prices



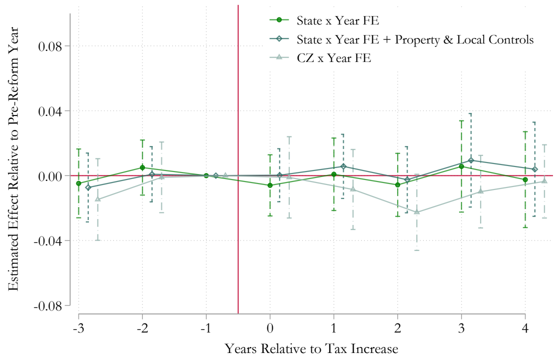
(d) Residential: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. In this specification, we only keep municipalities that experience no more than one tax hike during the sample period. The dependent variables are the log sales price per sqm (Panel B.22a) and the log rental price per sqm (Panel B.22b). Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state $\times$ year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.23c and B.23d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as 1 in the year of the first (and only) tax hike in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

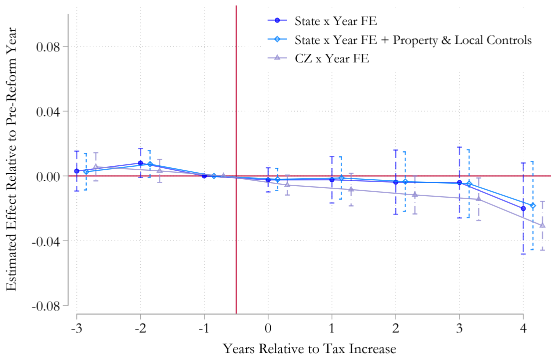
Figure B.23: Baseline Effects: Only One Tax Hike between 2004 and 2023



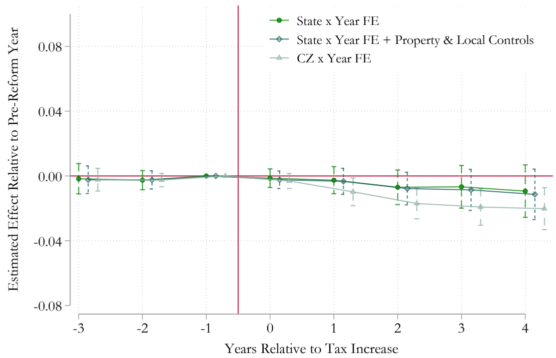
(a) Commercial: Sales Prices



(b) Commercial: Rents



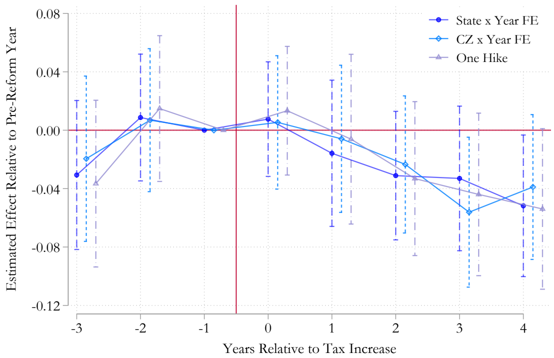
(c) Residential: Sales Prices



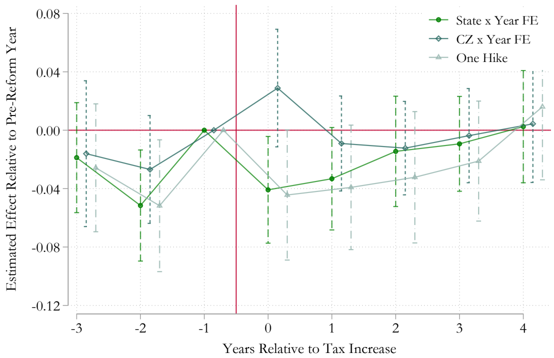
(d) Residential: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. In this specification, we only keep municipalities that experience no more than one tax hike during the sample period. The dependent variables are the log sales price per sqm (Panel B.23a) and the log rental price per sqm (Panel B.23b). Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state $\times$ year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.23c and B.23d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as 1 in the year of the first (and only) tax hike in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

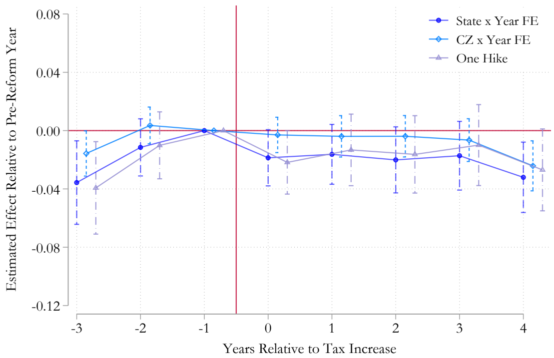
Figure B.24: Baseline Effects: No Property Tax Change in Event Window



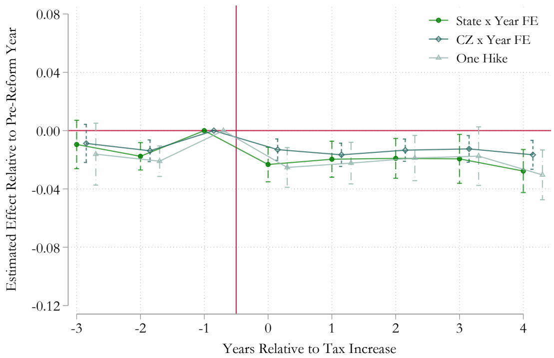
(a) Commercial: Sales Prices



(b) Commercial: Rents



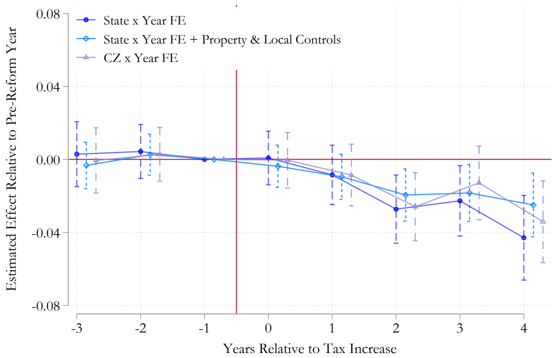
(c) Residential: Sales Prices



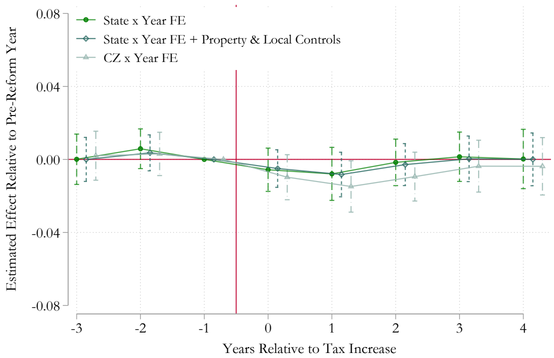
(d) Residential: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. In this specification, we only keep municipalities that experience no property tax changes during the event window. The dependent variables are the log sales price per sqm (Panel B.24a) and the log rental price per sqm (Panel B.24b). Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state × year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.24c and B.24d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as 1 in the year of the first (and only) tax hike in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

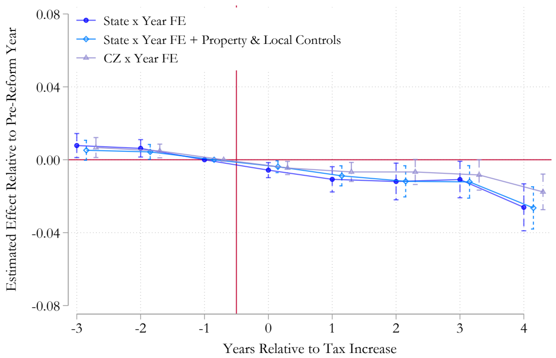
Figure B.25: Baseline Effects: Removing North-Rhine Westphalia



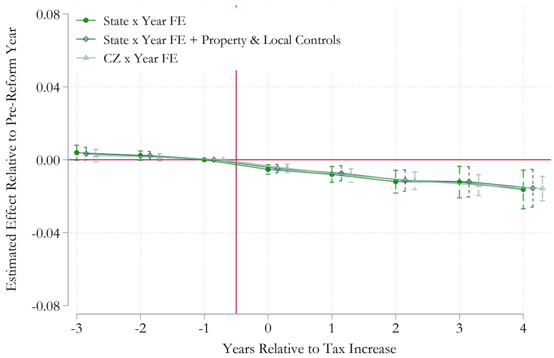
(a) Commercial: Sales Prices



(b) Commercial: Rents



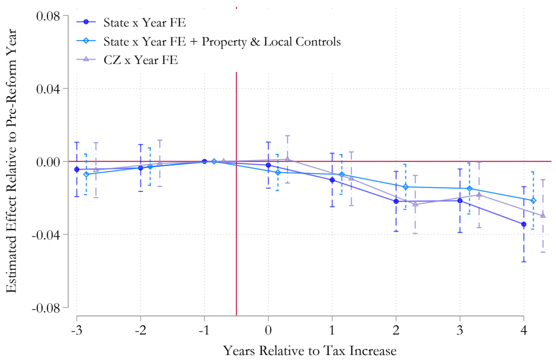
(c) Residential: Sales Prices



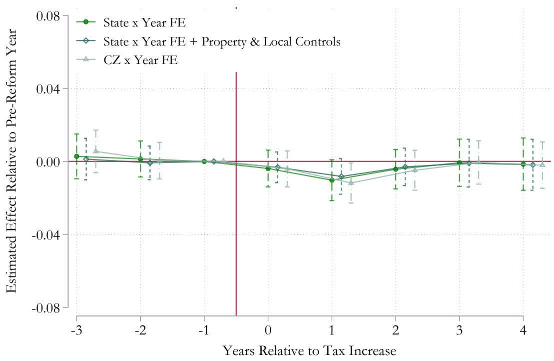
(d) Residential: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. In this specification, drop all municipalities in the state of North-Rhine Westphalia. The dependent variables are the log sales price per sqm (Panel B.25a) and the log rental price per sqm (Panel B.25b). Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state $\times$ year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.25c and B.25d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as 1 in the year of the first (and only) tax hike in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

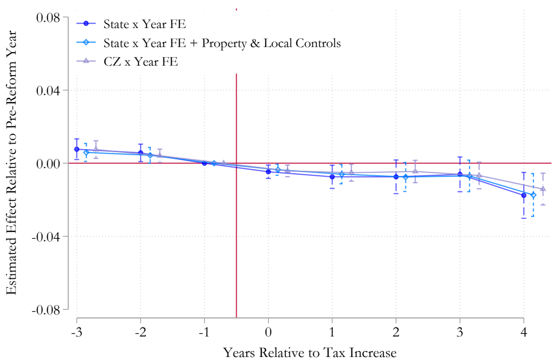
Figure B.26: Baseline Effects: Removing Rhineland Palatine



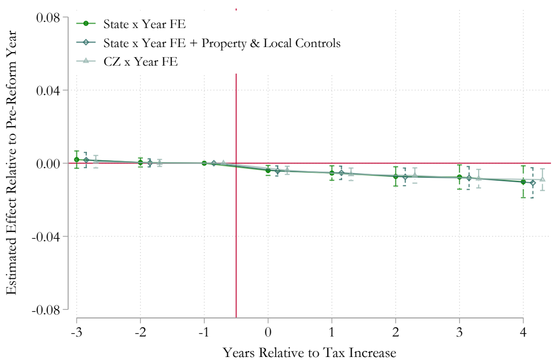
(a) Commercial: Sales Prices



(b) Commercial: Rents



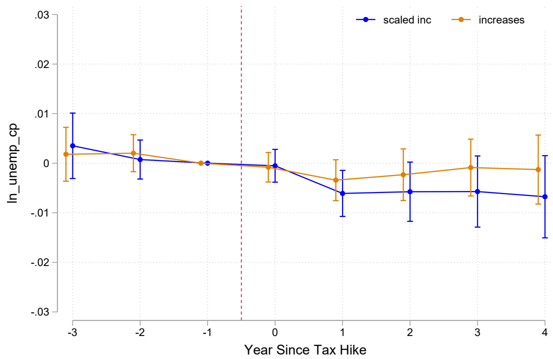
(c) Residential: Sales Prices



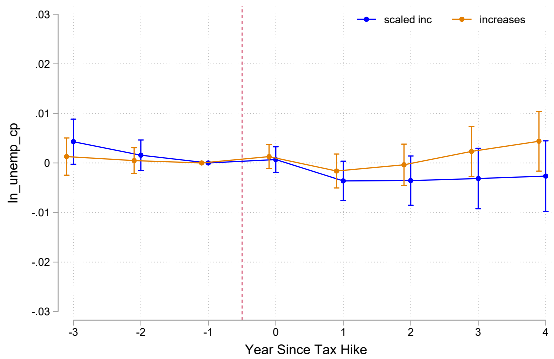
(d) Residential: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. In this specification, drop all municipalities in the state of Rhineland Palatine. The dependent variables are the log sales price per sqm (Panel B.26a) and the log rental price per sqm (Panel B.26b). Treatment variables are event study indicators scaled by the LBT change including only municipalities which experience a maximum of one tax increase in the effect window period (2008-19). All regressions include municipal and "state $\times$ year" fixed effects and the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.26c and B.26d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2024). Treatment there is defined as 1 in the year of the first (and only) tax hike in period 0.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

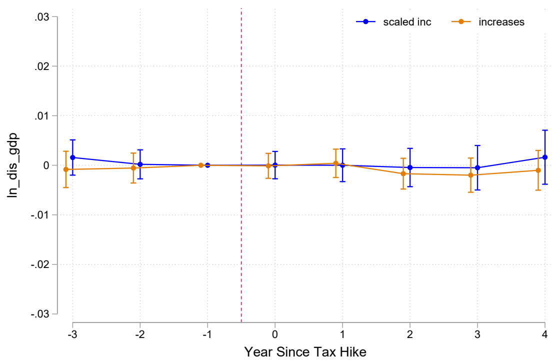
Figure B.27: Local Business Cycle Effects



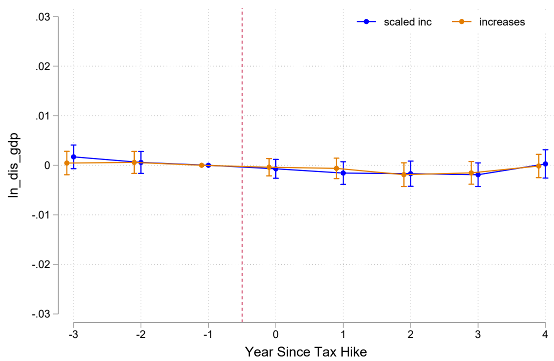
(a) Unemployment Rate: State-Year FE



(b) Unemployment Rate: CZ-Year FE



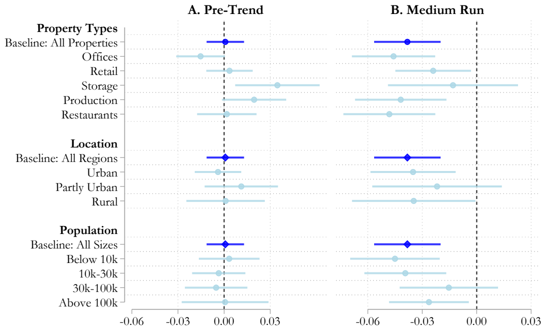
(c) District GDP: State-Year FE



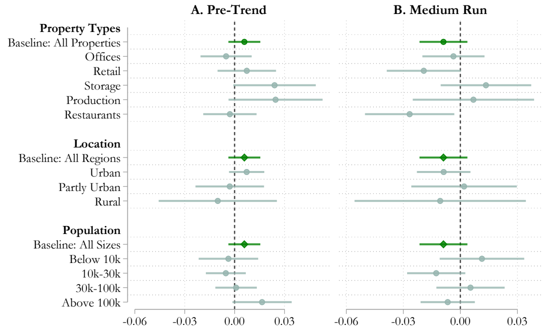
(d) District GDP: CZ-Year FE

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-3, 4]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log unemployment rates and district GDP. Treatment variables are event study indicators scaled by the LBT change. We require at least one ad per municipality-year cell. All regressions include municipal fixed effects and the scaled leads and lags of the local property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. *Source:* Own calculation based on data from Statistical State Offices.

Figure B.28: Heterogeneity Analysis: Commercial Real Estate



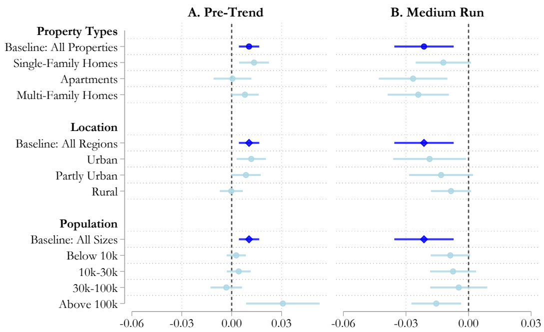
(a) Sales Sample



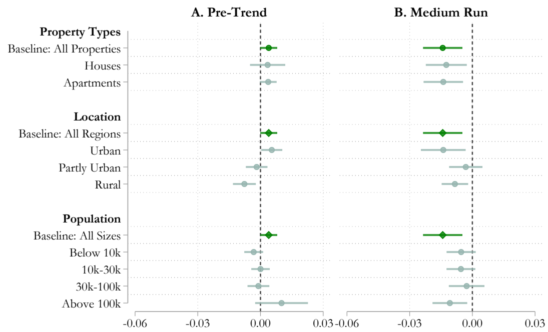
(b) Rental Sample

Notes: This Figure presents the results for different subsamples of observations according to property and municipal level variables. Estimates depict the estimated treatment effect of a one percentage point increase in the LBT rate on the offered sales (Panel B.28a) and rental (Panel B.28b) price of commercial properties relative to the pre-reform year. The baseline results for the sales sample correspond to the blue estimates in Figure 2a; baseline results for the rental sample correspond to the green estimates in Figure 2b. Estimates from alternative specifications are depicted in lighter colors. Subpanel A presents summary estimates of pre-treatment trends, i.e., the average coefficient in the three years prior to a tax reform. Subpanel B shows the medium-run effect measured as the average estimate of the third, fourth, and fifth lag in the LBT rate. All regressions also account for the scaled leads and legs of the local property tax rate. Horizontal bars indicate 95% confidence intervals. All regressions include municipal and “state × year” fixed effects and no further controls Standard errors are robust to clustering at the municipality level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

Figure B.29: Heterogeneity Analysis: Residential Real Estate



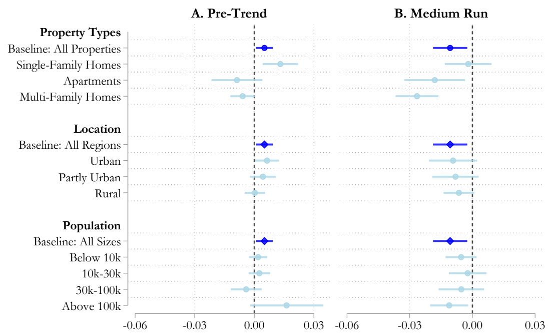
(a) Sales Sample



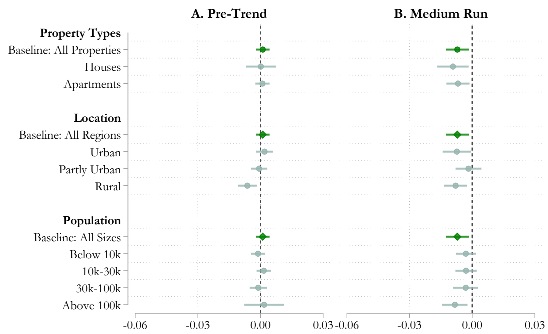
(b) Rental Sample

Notes: This Figure presents the results for different subsamples of observations according to property and municipal level variables in the residential samples. Estimates depict the estimated treatment effect of a one percentage point increase in the LBT rate on the offered sales (Panel B.29a) and rental (Panel B.29b) price of commercial properties relative to the pre-reform year. The baseline results for the sales sample correspond to the blue estimates in Figure 2a; baseline results for the rental sample correspond to the green estimates in Figure 2b. Estimates from alternative specifications are depicted in red. Subpanel A presents summary estimates of pre-treatment trends, i.e., the average coefficient in the three years prior to a tax reform. Subpanel B shows the medium-run effect measured as the average estimate of the third, fourth, and fifth lag in the LBT rate. All regressions also account for the scaled leads and lags of the local property tax rate. Horizontal bars indicate 95% confidence intervals. All regressions include municipal and “state × year” fixed effects. Standard errors are robust to clustering at the municipality level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

Figure B.30: Heterogeneity Analysis: Residential Real Estate with Property and Local Controls



(a) Sales Sample



(b) Rental Sample

Notes: This Figure presents the results for different subsamples of observations according to property and municipal level variables in the residential samples. Estimates depict the estimated treatment effect of a one percentage point increase in the LBT rate on the offered sales (Panel B.30a) and rental (Panel B.30b) price of commercial properties relative to the pre-reform year. The baseline results for the sales sample correspond to the blue estimates in Figure 2a; baseline results for the rental sample correspond to the green estimates in Figure 2b. Estimates from alternative specifications are depicted in red. Subpanel A presents summary estimates of pre-treatment trends, i.e., the average coefficient in the three years prior to a tax reform. Subpanel B shows the medium-run effect measured as the average estimate of the third, fourth, and fifth lag in the LBT rate. All regressions also account for the scaled leads and lags of the local property tax rate. Horizontal bars indicate 95% confidence intervals. All regressions include municipal and "state $\times$ year" fixed effects as well as local and property controls. Standard errors are robust to clustering at the municipality level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

C Theoretical Model

To estimate the incidence of corporate taxation we rely on the spatial equilibrium model developed in Suárez Serrato and Zidar (2016) and ?. SZ characterize the incidence of local corporate tax increases on workers, landowners and business owners as a function of estimable parameters. They focus only on residential property markets. We add commercial properties to the model.

C.1 Basic Outline

Both workers and plants are mobile across locations. We consider a small location c in an open economy with many locations. There are four types of agents workers, business owners and (commercial and residential) landowners. There is no population growth or establishment entry on a global level, but firms and workers can change their location. Workers choose the location that maximizes their utility, establishments choose the location that maximizes their profits and landowners choose the supply of residential and commercial real estate to maximize their profits. Capital and goods markets are global, while the labor and housing markets are local. These markets clear in each location. In equilibrium N_c households locate in location c and earn wage w_c and pay housing costs r_c^H. E_c establishments are active and they earn after-tax profits π_c. There is a representative commercial and residential landowner, earning rents r_c^G and r_c^H respectively.

C.2 Household Problem and Residential Real Estate Market

The solution to the household problem and the equilibrium in the residential real estate market are exactly equivalent to the derivations in Suárez Serrato and Zidar (2016). Households will locate in location c if their indirect utility there is higher than in any other location c'. Assuming $\xi'_{nc}s$ are i.i.d. type I extreme value, the share of households for whom that is true determines local population N_c :

$$N_c = P\left(V_{nc}^W = \max_{c'}\left\{V_{nc'}^W\right\}\right) = \frac{\exp\frac{u_c}{\sigma^W}}{\sum_{c'}\exp\frac{u'_c}{\sigma^W}}, \tag{C.1}$$

where σ^W is the dispersion of location-specific preferences and u_c is the indirect utility in location c.

The housing market clearing condition, $H_c^S = H_c^D$, determines the rents r_c in location c and is given in log-form by the following expression:

$$\ln r_c^H = \frac{1}{1+\eta_c^H} \ln N_c + \frac{1}{1+\eta_c^H} \ln w_c - \frac{\eta_c}{1+\eta_c^H} B_c^H + a_1 \tag{C.2}$$

where B_c^H is exogenous local housing productivity and a_1 is a constant. η_c^H is the local housing supply elasticity and governs the strength of the price response. We can then get an elasticity of labor supply that includes the effect of increased wages on residential housing market. This yields the first key elasticity: the effective elasticity of labor supply,

$$\frac{\partial \ln L_c^S}{\partial \ln w_c} = \left(\frac{1+\eta_c^H - \alpha}{\sigma^W \left(1+\eta_c^H\right) + \alpha} \right) \equiv \varepsilon^{LS} \tag{C.3}$$

where σ^W is the dispersion of location-specific preferences and α is the housing expenditure share.

C.3 Establishment Problem

Establishments j are monopolistically competitive and have productivity B_{jc} that varies across locations. Establishments combine labor l_{jc}, capital k_{jc}, commercial real estate g_{jc}, and a bundle of intermediate goods M_{jc} to produce output y_{jc} with the following technology:

$$y_{jc} = B_{jc} l_{jc}^{\gamma} k_{jc}^{\delta} g_{jc}^{\eta} M_{jc}^{1-\gamma-\delta-\eta}$$

where $M_{jc} \equiv \left(\int_{v \in J} \left(x_{v,jc}\right)^{\frac{\varepsilon^{PD}}{\varepsilon^{PD}}} dv \right)^{\frac{\varepsilon^{PD}}{\varepsilon^{PD}+1}}$ is establishment j 's bundle of goods of varieties v. Goods of all varieties can serve as either final goods for household consumption or as intermediate inputs for establishment production.

In a given location c, establishments maximize profits over inputs and prices p_{jc} while facing a local wage w_c, local rents for commercial real estate r_c^R, national interest rates ρ, national prices p_v of each variety v, and local business taxes τ_c^b subject to the production technology in equation (3) :

$$\pi_{jc} = \max_{l_{jc}, k_{jc}, x_{v,jc}, p_{jc}} \left(1-\tau_c^b\right) \left(p_{jc} y_{jc} - w_c l_{jc} - r_c^R g_{jc} - \int_{v \in J} p_v x_{v,jc} dv \right) - \rho k_{jc},$$

After deriving the first order conditions, input factor demands and solving the establishment location choice, we can obtain the macro elasticity of labor demand:

$$\frac{\partial \ln L_c^D}{\partial \ln w_c} = \underbrace{\gamma - 1}_{Substitution} + \underbrace{\gamma \varepsilon^{PD}}_{Scale} - \underbrace{\frac{\gamma}{\sigma^F}}_{Firm-Location} = \varepsilon^{LD} \tag{C.4}$$

This elasticity combines the average firm's elasticity and the effect of firm entry and exit on labor demand. We can also derive the effect of a business tax change on local labor demand:

$$\frac{\partial \ln L_c^D}{\partial \ln(1-\tau_c)} = \frac{1}{-(\varepsilon^{PD}+1)\sigma^F} = \frac{\mu - 1}{\sigma^F} \tag{C.5}$$

C.4 Commercial Real Estate Market

From the establishment problem demand for commercial real estate is given by:

$$G_c^D = E_c \times \frac{y_{ijc}}{B_{ijc}} \left[\tilde{w}_c^{\gamma} \left(\tilde{\rho}_c\right)^{\delta} \tilde{r_c}^{\eta - 1} \gamma^{-\gamma} \delta^{-\delta} \eta^{1-\eta} (1-\gamma-\delta-\eta)^{-(1-\gamma-\delta-\eta)} \right]$$

Substituting for y_{ijc}:

$$G_c^D = \left(\frac{1}{C\pi} \exp\left(\frac{v_c}{\sigma^F} \right) \right) \times \tilde{w}_c^{(1+\varepsilon^{PD})\gamma} \tilde{\rho}_c^{(1+\varepsilon^{PD})\delta} \tilde{r_c}^{(\eta \varepsilon^{PD} + \eta - 1)} \omega_0 e^{(-\varepsilon^{PD}-1)\bar{B}_c} z_c$$

where $\omega_0 = \frac{\mu}{I} \gamma^{-\gamma(\varepsilon^{PD}+1)} \delta^{-\delta(\varepsilon^{PD}+1)} \eta^{(-\eta(\varepsilon^{PD}+1)+1)} (1-\gamma-\delta-\eta)^{-(1-\gamma-\delta-\eta)(\varepsilon^{PD}+1)}$.

Taking logs and simplifying yields the (log) demand curve for commercial real estate:

$$\ln G_c^D = \omega_2 - \frac{\ln(1-\tau_c)}{(\varepsilon^{PD}+1)\sigma^F} - \ln \pi + (\gamma(\varepsilon^{PD} + 1 - \frac{1}{\sigma^F})) \ln \tilde{w}_c - \frac{\ln \mu_{ic}}{(\varepsilon^{PD}+1)\sigma^F} +$$
$$(\delta(\varepsilon^{PD} + 1 - \frac{1}{\sigma^F})) \ln \tilde{\rho}_c + (\eta(\varepsilon^{PD} + 1 - \frac{1}{\sigma^F}) - 1) \ln \tilde{r}_c + (-(\varepsilon^{PD}+1) + \frac{1}{\sigma^F}) \bar{B}_c + z_c$$

Substituting $\ln N_c$ yields[17] :

$$\ln G_c^D = \ln N_c + \ln \tilde{w}_c - \ln \tilde{r}_c^G$$

The local supply of commercial real estate, $G_c^S = G\left(r_c^G; B_c^G\right)$, is upward-sloping in both the rental price r_c^G, which allows landowners to benefit from higher rental prices, and ex-

[17] In Equilibrium $\ln N_c = \ln L^D = \kappa_2 - \frac{\ln(1-\tau_c)}{(\varepsilon^{PD}+1)\sigma^F} - \ln \pi + (\gamma(\varepsilon^{PD} + 1 - \frac{1}{\sigma^F}) - 1) \ln \tilde{w}_c - \frac{\ln \mu_{ic}}{(\varepsilon^{PD}+1)\sigma^F} + (\delta(\varepsilon^{PD} + 1 - \frac{1}{\sigma^F})) \ln \tilde{\rho}_c + (\eta(\varepsilon^{PD} + 1 - \frac{1}{\sigma^F})) \ln \tilde{r}_c + (-(\varepsilon^{PD}+1) + \frac{1}{\sigma^F}) \bar{B}_c + z_c$

ogenous local real estate productivity B_c^G. The marginal landowner supplies real estate at cost $r_c = G^{-1}\left(G_c^S; B_c^G\right)$. For tractability, we assume $G\left(r_c^G; B_c^G\right) \equiv \left(B_c^G r_c^G\right)^{\eta_c^G}$, where the local commercial real estate supply elasticity $\eta_c^G > 0$ governs the strength of the price response to changes in demand and productivity. The commercial real estate market clearing condition, $G_c^S = G_c^D$, determines the rents r_c^G in location c and is given in log-form by the following expression:

$$-\eta_c^G \ln B_c^G = (1+\eta_c^G)\ln r_c^G - \ln N_c - \ln w_c \tag{C.6}$$

C.5 Equilibrium

We characterize the incidence of corporate taxes on wages, rents, and profits and relate these effects to the welfare of workers, landowners, and firms. We focus on the welfare of local residents as the policies we study are determined by policymakers with the objective of maximizing local welfare.

Assuming full labor force participation, i.e., $L_c^S = N_c$, spatial equilibrium c depends on market clearing in factor markets, housing markets, and output markets, and can be expressed in terms of the expressions for log labor supply (Equation C.1), the log of housing market clearing condition (Equation C.2), the log of the clearing condition for commercial real estate (Equation C.6) and log labor demand (Equation C.4) as follows:

$$\begin{bmatrix} -\frac{\bar{A}_c}{\sigma^W} \\ -\eta_c^H B_c^H \\ -\left(\ln\kappa_2 - \frac{\ln\left(1-\tau_c^b\right)}{(\varepsilon^{PD}+1)\sigma^F} - \ln\bar{\pi} + \left(-\left(\epsilon^{PD}+1\right) + \left(\frac{1}{\sigma^F}\right)\right)\bar{B}_c - \frac{\ln\tilde{\mu}_{ic}}{(\varepsilon^{PD}+1)\sigma^F} + z_c\right) \\ -\eta_c^G B_c^G \end{bmatrix}$$
$$= \begin{bmatrix} -1 & \frac{1}{\sigma^W} & -\frac{\alpha}{\sigma^W} & 0 \\ -1 & -1 & 1+\eta_c^H & 0 \\ -1 & \varepsilon^{LD} & 0 & \varepsilon^{LC} \\ -1 & -1 & 0 & 1+\eta_c^G \end{bmatrix} \times \begin{bmatrix} \ln N_c \\ \ln w_c \\ \ln r_c^H \\ \ln r_c^G \end{bmatrix}$$

where

$$\varepsilon^{LC} = \eta\left(\varepsilon^{PD} + 1 - \frac{1}{\sigma^F}\right) = \underbrace{\eta}_{Substitution} + \underbrace{\eta\varepsilon^{PD}}_{Scale} - \underbrace{\frac{\eta}{\sigma^F}}_{Firm-Location}$$

which is the elasticity of labor demand to changes in commercial property rents.

We then use Cramers rule to obtain the derivatives with respect to $\ln\left(1-\tau_c^b\right)$ for $\ln N_c$, $\ln w_c$, $\ln r_c^H$ and $\ln r_c^G$.

C.6 Incidence

Let $\dot{w}_c = \frac{d \ln w_c}{d \ln\left(1-\tau_c^b\right)}$ and define $\dot{r}_c^H$, $\dot{r}_c^G$ and $\dot{\pi}_c$ analogously. The effects of a local corporate tax cut on local wages, rents, and after-tax profits are given by the following expressions:

$$\dot{w}_c = \frac{\left(\frac{\partial \ln L_c^D}{\partial \ln\left(1-\tau_c^b\right)}\right)}{\varepsilon^{LS} - \varepsilon^{LD} + \frac{(1+\varepsilon^{LS})\varepsilon^{LC}}{1+\eta_c^G}} = \frac{\frac{(\mu-1)}{\sigma^F}}{\left(\frac{1+\eta_c^H-\alpha}{\sigma^W(1+\eta_c^H)+\alpha}\right) - \gamma\left(\varepsilon^{PD} - \frac{1}{\sigma^F}\right) + 1 + \frac{1+\eta_c^H}{1+\eta_c^G}\frac{(1+\sigma^W)\varepsilon^{LC}}{\sigma^W(1+\eta_c^G)+\alpha}} =$$

$$\dot{w}_c = \frac{\frac{(\mu-1)}{\sigma^F}}{\varepsilon^{LS} - \varepsilon^{LD} + \frac{(1+\varepsilon^{LS})\varepsilon^{LC}}{1+\eta_c^G}}, \text{ and}$$

$$\dot{r}_c^H = \left(\frac{1+\varepsilon^{LS}}{1+\eta_c^H}\right)\dot{w}_c$$

$$\dot{r}_c^G = \left(\frac{1+\varepsilon^{LS}}{1+\eta^G}\right)\dot{w}_c$$

$$\dot{\pi}_c = 1 \underbrace{-\delta\left(\varepsilon^{PD}+1\right)}_{\text{Reducing capital wedge}} + \underbrace{\gamma\left(\varepsilon^{PD}+1\right)\dot{w}_c}_{\text{Higher labor costs}} + \underbrace{\eta\left(\varepsilon^{PD}+1\right)\dot{r}_c^G}_{\text{Higher rental costs}},$$

where $\dot{\pi}_c$ is the percentage change in after-tax profits, δ is the output elasticity of capital, ε^{PD} is the product demand elasticity, γ is the output elasticity of labor, and $\dot{w}_c$ is the percentage change in wages following a corporate tax cut.

C.7 Local Incidence on Welfare

Effect of a tax cut on the welfare of workers in location c:

$$\frac{d\mathcal{V}^W}{d\ln\left(1-\tau_c^c\right)} = N_c\left(\dot{w}_c - \alpha\dot{r}_c^H\right)$$

This expression assumes that the tax change in location c has no effect on wages and rental costs in other locations.

The effect of a corporate tax cut on firm owners is:

$$\frac{d\mathcal{V}^F}{d\ln\left(1-\tau_c^c\right)} = E_c\dot{\pi}_c$$

Next, consider the effect on the welfare of residential landowners in location c. Landowner welfare in each location is the difference between housing expenditures and the costs associated with supplying that level of housing. This difference can be expressed as follows:

$$\mathcal{V}^{LR}=N_c\alpha w_c-\int_0^{N_c\alpha w_c/r_c}G^{-1}\left(q;Z_c^h\right)dq=\frac{1}{1+\eta_c^H}N_c\alpha w_c,$$

and is proportional to housing expenditures. The effect of a corporate tax cut on the welfare of residential real estate owners is then given by

$$\frac{d\mathcal{V}^{LR}}{d\ln\left(1-\tau_c^c\right)}=\frac{\dot{N}_c+\dot{w}_c}{1+\eta_c^H}=\dot{r}_c^H$$

The total expenditure on commercial real estate is given by:

$$\ln\left(G_c^D\tilde{r}_c^G\right)=\ln N_c+\ln\tilde{w}_c$$

By the same logic as above the effect of a corporate tax change on the welfare of commercial real estate owners is given by:

$$\frac{d\mathcal{V}^{LC}}{d\ln\left(1-\tau_c^c\right)}=\frac{\dot{N}_c+\dot{w}_c}{1+\eta_c^G}=\dot{r}_c^G$$

These equations show how welfare changes for workers, landowners and firm owners are related to reduced form estimates. This is shown in Table 1 in the main text.

C.8 Incidence weighted by Income Share

In the model workers have income wN, residential landowners have income αwN and commercial landowners have income $r^G G$. Capital owners obtain firm profits π and the return to capital $-(\varepsilon^{PD}+1)\pi\delta$.

Assuming all agents spend their income in the product market, total expenditure is given by:

$$(1-\alpha)wN+\alpha wN+r^G G+\pi-\left(\varepsilon^{PD}+1\right)\pi\delta=wN+r^G G+\pi\left(1-\left(\varepsilon^{PD}+1\right)\delta\right)$$

Since total expenditure is $-\varepsilon^{PD}\pi$ we can rewrite profits as:

$$\pi=\frac{wN+r^G G}{-(\varepsilon^{PD}+1)(1-\delta)}$$

We can then express total income as:

$$wN + \alpha wN + r^G G + \frac{wN + r^G G}{-(\varepsilon^{PD}+1)(1-\delta)}(1 - \left(\varepsilon^{PD}+1\right)\delta)$$

Assuming $r^G G = \theta wN$, we can rewrite total income as:

$$wN\left[1 + \alpha + \theta + (1+\theta)\frac{(1 - \left(\varepsilon^{PD}+1\right)\delta)}{-(\varepsilon^{PD}+1)(1-\delta)}\right] = wNI$$

Then we obtain the following income shares:

$$\underbrace{\frac{1}{I}}_{\text{Workers}}, \quad \underbrace{\frac{\alpha}{I}}_{\text{Residential Land}}, \quad \underbrace{\frac{\theta}{I}}_{\text{Commercial Land}}, \quad \underbrace{\frac{(1+\theta)\frac{(1-\left(\varepsilon^{PD}+1\right)\delta)}{-(\varepsilon^{PD}+1)(1-\delta)}}{I}}_{\text{Firm Owners}}$$

The shares depend on α, δ, θ and ε^{PD}. As before we parameterize $\alpha = 0.3$. We take a range of values for $\varepsilon^{PD} = [-2.5, -5]$. We set the output elasticity of capital $\delta = 0.2$, based on cost shares from the statistical offices. Similarly we also set the ratio between commercial real estate and wage expenditures $\theta = 0.24$.

Table C.7: Parameters

Parameter	Values	Source
Housing cost share: α	**0.3**	Statistisches Bundesamt (2025a)
Elasticity of Product Demand: ε^{PD}	[**-2.5**, -5]	Head and Mayer (2014)
Output elasticity of capital: δ	**0.2**	Statistisches Bundesamt (2025b)
Ratio of property costs to labor costs: θ	[0.14, **0.24**]	Statistisches Bundesamt (2025b)