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 over 1 million properties offered for sale and over 2.4 million properties offered for rent between 2008 and 2018. Empirically, we exploit the German institutional setting with over 4,000 municipal tax changes using an event study design. The estimates suggest that a 1 percentage point business tax increase reduces commercial real estate prices (rents) by 3 percent (2 percent) after 5 years on average. 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 (15-24%), while workers (7-20%) and residential landowners (4-25%) are likely to bear less burden than prior research suggests with firm owners bearing around 44-57%.

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

JEL Classification: H22, H25, H71.

1 Introduction

The incidence of corporate taxation is of great interest to policy makers as it directly affects the progressivity of the tax system and therefore has important distributional implications. Although macroeconomists typically distinguish between three major input factors of production – *capital*, *labor*, and *land* (Auerbach and Kotlikoff, 1998) – the literature fails to disentangle what effects corporate taxation has beyond wages and firm profits. Only recently, the corporate tax incidence literature has started to take into account local labor markets, worker mobility, and private housing, to establish that landlords can be affected by corporate taxes (Suárez Serrato and Zidar, 2016). Still, neither theoretical nor empirical work has been conducted on the effect of corporate taxes on *commercial* properties.

We address this gap and provide the pathway to a broader assessment of the welfare effects of corporate taxation. In particular, we examine how corporate taxes causally affect sales prices and rents of commercial properties. To this end, 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 over 11,000 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 micro data from *F+B*, a real estate consulting firm. Our baseline sample includes roughly 1 million commercial properties offered for sale and 2.4 million commercial properties offered for rent between 2008 and 2018. Compared to previous work on real estate prices, our property dataset is unique in two ways. First, while the vast majority of real estate research does not distinguish between commercial and private properties, we have access to a dataset that specifically covers only *commercial* real estate. Second, unlike most previous studies, we do not rely on one single (type of) data source or advertising platform. Instead, the *F+B* data includes ads from over 140 different sources and covers various leading online platforms and newspapers. Therefore, our data is more likely to be representative of the commercial real estate market as a whole and functions as a proxy of the cost of land as a production factor.

We apply a series of non-parametric event studies 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 relevant (semi-)elasticities that determine how LBT hikes affect the value of 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 and rents. 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 five 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 (2022) 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 find that firm owners bear the largest part of the burden with an estimate of 54 percent in our preferred specification. Around 20 percent of incidence falls on commercial landowners, while residential landowners bear between roughly 13 percent in our preferred specification. Finally, roughly 14 percent of the incidence falls on workers. According to these estimates landowners bear a larger burden than the previous literature suggests, while workers bear a smaller share.

The rest 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 commercial 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 taxes is the local business tax (LBT, *Gewerbesteuer*) which is partly determined locally by municipalities. While 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. This corresponds to 65% of total profit tax revenues from corporate firms, making the LBT a good measure of overall corporate taxation (Statistisches Bundesamt (Destatis), 2021).

The LBT is assessed on the operating profits of firms which 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 hold establishments 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 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 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 decided upon 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]

To use tax increases arising from municipality-specific scaling factors as identifying variation has two advantages (see also the discussion in Fuest et al., 2018; Link et al., 2022). First, we can treat over 11,000 German municipalities 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, especially compared to studies that use 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 federal level. Thus, the variation in tax rates that we exploit empirically does not depend on (current) firm choices. As also explained in Fuest et al. (2018), this especially advantageous compared to previous studies as it has been shown that tax rate changes often happen at the same time as changes in the

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

[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 2009 onwards, 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.

tax base (Kawano and Slemrod, 2012).

Besides 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

Our municipality level dataset closely corresponds to the one in Fuest et al. (2018). The information was retrieved from administrative data from the German Federal States' Statistical Offices (*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 over our sample period (2008-2018).[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 451 municipalities that were subject to merger reforms as well as 525 municipalities that experienced a tax cut during the sample period such that the resulting panel covers 10,113 German municipalities between 2008 and 2018.[4]

Figure A.1 in the Appendix presents the spatial variation of the LBT rates across municipalities during the observation period. Panel A.1a reveals that tax rates are 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 35 percent. Panel A.1b reveals that municipalities in North-West Germany change their taxes sightly more frequently than those in the Southern part. While 2,521 municipalities did not change their LBT rate during the sample period, 201 did so five or more times. On average, per year around 10 percent of municipalities adjust their LBT rates. Figure A.3 in the Appendix shows the size of LBT changes and LPT changes. We see that LBT cuts, which we drop in the analysis, are small and happen rarely. Most LBT increases are smaller than 2 percentage points; the average LBT increase (indicated by the red, vertical line) is at 0.85 percentage points. In 2018, the mean local business tax rate was at 12.71%, the median was at 12.78%. In comparison, the LPT exhibits much smaller changes and the absolute level of the LPT is also much smaller. Most LPT changes are between 0.1 and 0.5 percentage points with an average of 0.1 percentage points.

[3]Unfortunately, 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.

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

2.3 Property Data

To measure the effect of local business taxes on commercial property prices, we use a large and unique micro-dataset on the German real estate market. The data is provided by the real estate consultancy firm *F+B* and includes information from real estate advertisements covering all sorts of buildings and facilities offered for rent or for sale. The unique feature of the data is that it covers both private and commercial reals estate. The data was collected using web-scraping techniques from roughly 140 different sources, covering real estate online portals such as *ImmobilienScout24*, advertisements in (trans-)regional newspapers, as well as real estate agencies. The dataset includes the complete list of sources in which the property was advertised and was thoroughly cleaned to make sure that properties that were listed in more than one source at the same time only appear once in the final dataset. Moreover, for every property, we observe the first and the last day a property was listed as well as a proxy for the actual selling price of the property.[5]

To the best of our knowledge, this micro-dataset is the most comprehensive data source on the German real estate market. While administrative records are only published at a more aggregated level (and micro data is not available for research), many previous studies typically rely on property data from only one real estate website. Moreover, we are among the first to have comprehensive information on *commercial* property prices, which allows us to asses both, the residential real estate channel that the literature explored before (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 information on real estate prices from January 2008 until December 2018 which leaves us with a sample of roughly 2.4 million properties that were offered for rent and 1.1 million properties that were listed for sale all over Germany. We aggregate the value of all properties offered for sale in each year to calculate an approximation for annual transaction volume. Appendix Figure A.5 shows the the estimates for transaction volume for each year between 2008 and 2018. The volume varies between 26 billion (in 2009) and 65 billion (in 2018). This is quite close to total transaction volume estimated by Burkert et al. (2019). They estimate an average volume of 35 billion between 2004 and 2018 which is very close to our estimate. For 2018 they estimate a transaction volume of 61 billion. This shows that our sample likely covers a very large part of the German commercial property market.

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

[5]This proxy is equal to the offering price on the last day of the listing minus an estimated deduction. The deduction is estimated by *F+B* by matching a subsample of the ads data to actual transaction data.

property was listed. Note that we only observe offered prices, not transaction prices.[6] Besides price information, the dataset covers a wide range of property 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 features. Commercial properties are categorized into five different types: offices, retail, storage, production, or restaurant spaces. Figure A.4 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 contains information on the location of every advertised object, such that we can match every property to its corresponding municipality.

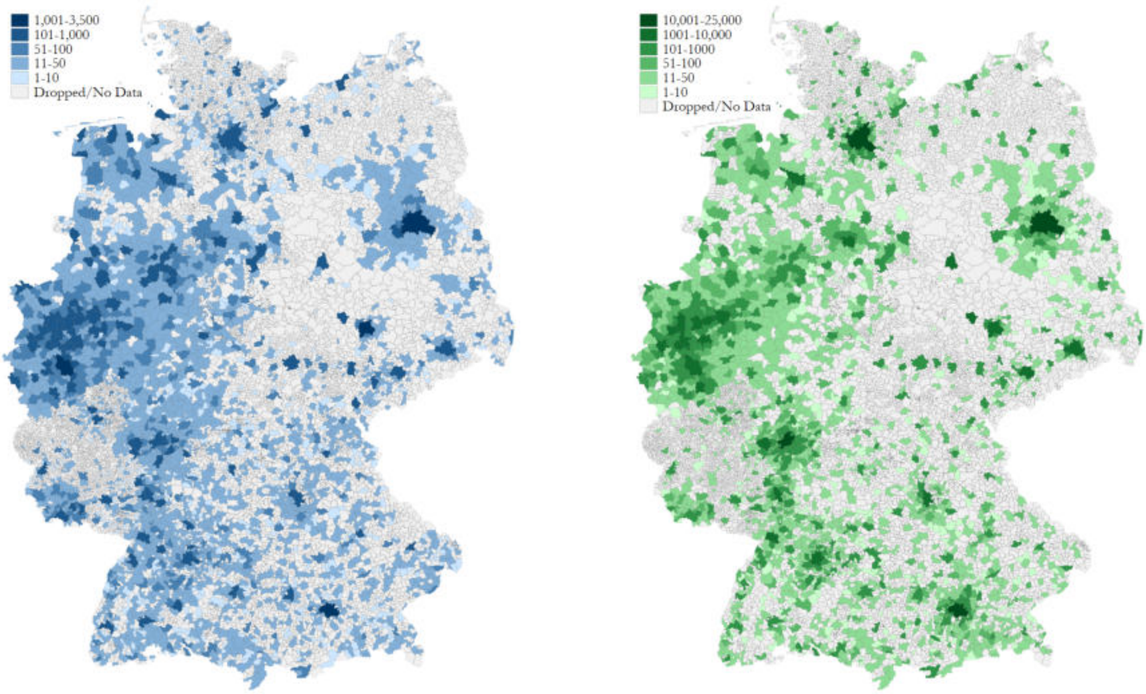
2.4 Estimation Sample

We combine and harmonize the municipality and the property data to construct four annual panel datasets – split by usage type of properties (commercial vs. residential) and by type of offering (for sale vs. for rent) – for all German municipalities with municipality-year observations spanning the years from 2008 to 2018. We use information on local scaling factors starting in 2004 to account for the correct leads in our event study estimation. In our baseline specification, we use the commercial real estate samples and require a minimum number of one ad per municipality-year cell, which leaves us with 6,561 municipalities per year in the sales sample (1,002,914 price observations) and 4,648 municipalities (2,396,532 price observations) in the rental sample. Figure 1 illustrates the spatial variation of the average number of postings per year between 2008 and 2018 in both the commercial sales sample (Panel 1a) and the commercial rents sample (Panel 1b). Not surprisingly, the number of postings is high in the densely populated West Germany and around big agglomeration areas. Tables A.3 and A.4 in the Appendix show the selection steps we follow to generate our baseline commercial estimation samples.

Municipalities that are dropped (as they have non 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 (especially in Saxony-Anhalt and Mecklenburg-Western Pomerania), or are small jurisdictions in the South and West of the country. Interestingly, even though we observe a lower number of sales postings (1 million compared to 2.4 million rental postings), the number of municipalities is higher in the sales sample (by roughly 2,000 mu-

[6] This also implies that properties that did not make it to the market are not included in the data. Nevertheless, while previous studies that use offering prices argue that the final offering price can be regarded as close approximation of the actual sale price or rent (e.g. Löffler and Siegloch, 2021), using the estimated transaction price delivers virtually the same estimated effect.

Figure 1: Average Number of Postings per Year (2008-2018)



(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 6,500 municipalities in the commercial sales sample (Panel 1a) and roughly 4,600 municipalities in the commercial rents sample (Panel 1b) between 2008 and 2018. 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 (2008-2018)), 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.

nicipalities). This highlights that the commercial sales data is more equally distributed, while information on commercial rents is strongly concentrated in and around big agglomeration areas. Tables A.1 and A.2 in the Appendix show descriptive statistics for property, tax, and municipal variables in our estimation samples for sales and rents 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 log 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[7] and state

[7]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.

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 caused by changes in the municipal scaling factor.[8] 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:

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

where $\hat{\beta}_j$ includes the estimates of interest measuring the dynamic causal effect before ($j < 0$) and after ($j \geq 0$) treatment with $\overline{j}$ lags and $\underline{j}$ leads of the treatment variable. The event study indicators in ΔT_{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. Equation 2 also includes a set of time-varying controls in X_{imt}. The main control variables included in all specifications are the scaled leads and lags of municipal property tax rate changes in municipality m at time t. 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 rate, the property tax 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. In some estimations, we also include dynamic measures of district GDP, district unemployment rate, municipal population and the municipal share of income tax revenues in logs and twice lagged, to control for time-varying disturbances that occur shortly before or after a tax change.[9] 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 term, μ_m, whereas θ_{st} captures "state $\times$ year" fixed effects, thus controlling for time-varying trends and shocks at 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 municipality data between 2008 and 2018, we set $\underline{j}$ = 4 and $-\overline{j}$ = 5 allowing

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

[9]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.

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 (2020), 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 2022. 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 composes 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 points in time 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 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 control groups (Schmidheiny and Siegloch, 2020).

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. For this purpose, we implement a DiD model with fixed effects of the following form:

$$ln(p_{imt}) = \gamma ln(1 - \tau_{mt}) + \delta X_{imt} + \mu_m + \theta_{st} + \varepsilon_{mt}, \tag{3}$$

where instead of event indicators, we regress the log net-of-LBT rate, $ln(1 - \tau_{mt})$, in municipality m and year t on the log sales price or rent per sqm of property i in municipality m and year t, $ln(p)_{imt}$. The estimates $\hat{\gamma}$ deliver the wanted measure of the elasticity of sales and rental prices with respect to the business tax rate. The regression specified in Equation 3 includes 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 also 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 systematical shocks on municipality level that influence property prices or tax rates. While the event study setup allows for a visual test of the 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 district unemployment, district GDP and municipal population as outcome variables. 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). 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 do not significantly affect the municipal net firm formation rate. Thus, it is unlikely that tax increases significantly influence commercial land prices in surrounding municipalities, especially as most municipalities are small, as are tax increases.

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.[10] 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 (2022), 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 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, 2022). The control group consists of both not-yet-treated and never-treated municipalities.

3.3 Measuring Tax Incidence

The DiD estimate from Equation 3 measures the elasticity of rent and sales prices 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 (2023). 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. Furthermore, workers and residential landowners can bear part of the burden. 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})

[10] 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.

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 have to infer it from other estimates (Firm entry, wages) 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 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}

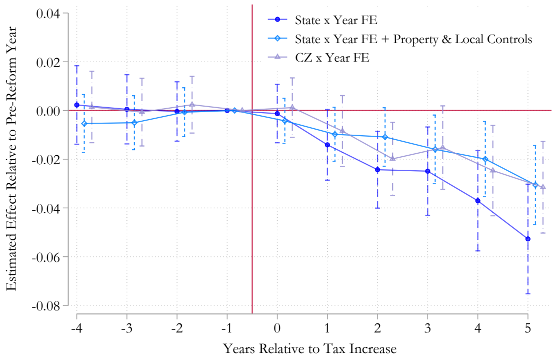
Notes: This table shows how estimable elasticities map into the incidence formulae obtained from a spatial equilibrium model in the style of Suárez Serrato and Zidar (2016). The parameter α refers to the housing expenditure share and has to be calibrated. In Germany housing expenditures make up between 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

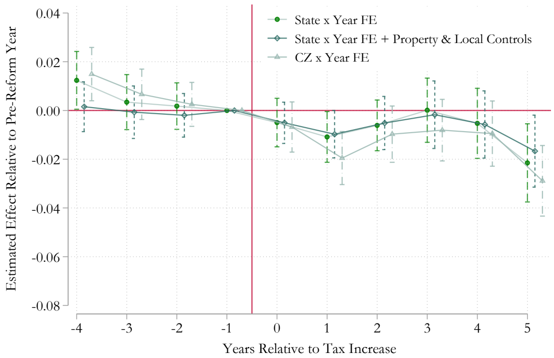
4.1 Estimation Results

Figure 2 presents our baseline estimates separately for the commercial sales prices (Panel 2a) and commercial rents (Panel 2b) as dependent variables together with the corresponding 95% confidence intervals. We show three specifications of Equation 2 which (i) include only “state × year” fixed effects, (ii) include property and municipality controls in addition to “state × year” fixed effects, and (iii) include “commuting zone × year” fixed effects. Panel 2a reveals that following a tax hike, the (offered) sales prices of commercial properties decreases significantly. While the effect is small and not statistically significant in the first year after a tax hike, it increases over time such that after five years the estimate is strongly significant and amounts to -0.03 for the specification that includes the most comprehensive controls. This corresponds to a semi-elasticity of around 3, that is, following a one percentage point tax hike, commercial sales prices decrease by 3 percent after five years relative to the year prior to the tax reform. In all three specifications pre-trends are very flat. This suggest that treatment and

Figure 2: Baseline Effects on Commercial Properties

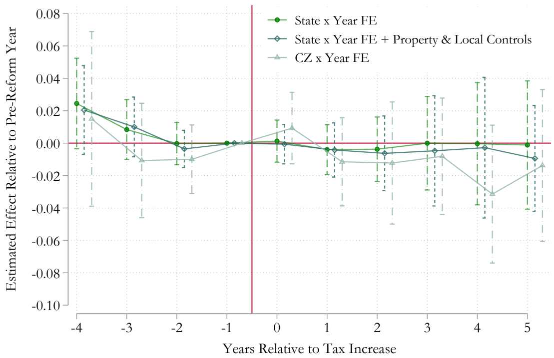


(a) TWFE: Sales Prices



(b) TWFE: Rents

(c) Heterogeneity Robust: Sales Prices



(d) Heterogeneity Robust: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-4, 5]$) 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 6,561 (4,648) municipalities and 4,627 (3,343) 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 (2022). 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.

control municipalities were developing similarly before the tax change providing support for the parallel trends assumption.

The estimates for the effect of tax hikes on rental prices in Panel 2b are slightly less clear. While the estimates for the final period look similar to the sales result (with a semi-elasticity of around -2 after five years), the other estimates are smaller, partly insignificant, and follow a slightly decreasing pre-trend prior to a tax hike. The specification with property and municipality controls in addition to "state $\times$ year" fixed effects displays no pre-trends and still a significantly negative medium run effect. There could be two reasons for a smaller effect on rents. First, rents are deductible from the tax base, 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 sales prices, but it might take a longer time to reach rental markets as firms gradually relocate over multiple years.

In Panels 2c and 2d of Figure 2 we also show the results of using the estimator proposed by De Chaisemartin and d'Haultfoeuille (2022). 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), 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, but they are less precise then the TWFE results. This is not surprising since our setting is quite complex. Still, the magnitude of the estimates remains unchanged, confirming that the results are not caused by heterogeneous treatment effects.

The results for the equivalent specification on private properties are shown in Appendix Figure B.6. In general the effects on private properties are quite small and often insignificant. In particular the effect on rents is very close to zero. The effect on sales prices is more statistically and economically significant, but the specification also suffers from slightly diverging trends in the pre-treatment period.

To estimate price elasticities for sales prices and rents we run the DiD regressions as proposed in Equation 3. Panel A of Table 2 shows the corresponding elasticity estimates for commercial Properties. The sales price elasticity estimate for the naive baseline case (with no controls except for the property tax rate) reveals an elasticity of 1.65 and decreases slightly when adding controls and using "CZ $\times$ year" fixed effects. For the most demanding specification we estimate an elasticity of about 1.1. That is, following a one percent increase in the net-of-tax rate, sales prices of commercial properties increase by 1 percent. The estimated elasticities for rents are similar in magnitude and hover around 1.1. The only exception is the specification with "State $\times$ year" fixed effects and controls. The elasticity is only 0.65 and not statistically significant. Panel B of Table 2 shows the estimated elasticities for private properties. The elasticities are generally smaller than for commercial properties. For sales prices it ranges from 1.7 (only "State $\times$ year" fixed effects) to 0.7 (full controls). For rents the elasticity declines similarly when including more demanding sets of control variables (from 0.9 to 0.2). According to our estimates private 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 2.3 to 3 depending on the exact specification. These estimates are within the range of point estimates obtained by Suárez Serrato and Zidar (2023) for their specifications relying on productivity and intensive margin labor demand to quantify effects on firm profits.

Table 2: DiD Estimation

Panel A:	Commercial Properties					
	Ln Sales Price sqm			Ln Rent Price sqm		
Δ Ln Net-of-Tax Rate	1.646***	1.215**	1.095*	1.108*	0.647	1.130**
	(0.475)	(0.467)	(0.446)	(0.434)	(0.394)	(0.391)
Property Controls		✓	✓		✓	✓
Municipality Controls		✓	✓		✓	✓
State x Year FE	✓	✓		✓	✓	
CZ x Year FE			✓			✓
Observations	897,804	890,163	890,160	2,125,364	2,099,526	2,099,522

Panel B:	Private Properties					
	Ln Sales Price sqm			Ln Rent Price sqm		
Δ Ln Net-of-Tax Rate	1.708***	0.877**	0.716***	0.923***	0.451*	0.195
	(0.504)	(0.316)	(0.204)	(0.270)	(0.188)	(0.110)
Property Controls		✓	✓		✓	✓
Municipality Controls		✓	✓		✓	✓
State x Year FE	✓	✓		✓	✓	
CZ x Year FE			✓			✓
Observations	12,988,552	12,905,538	12,905,538	10,762,438	10,638,794	10,638,790

Panel C:	Ln Net Profit		
Δ Ln Net-of-Tax Rate	3.001***	2.329***	3.017***
	(0.776)	(0.803)	(0.708)
Municipality Controls		✓	✓
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 3. 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 private 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 2018. 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.

4.2 Incidence Analysis

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), workers and firm owners. 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 summarized in Table 1. Panel B then shows the share of the incidence borne by each type of agent.[11] The shares for commercial landowners and firm owners are relatively stable through all the specifications while the shares for workers and residential landowners vary more. This is not surprising, as disposable income and rents are tightly linked in model. Firm owners bear the largest part of the burden with estimates between 44 percent and 57 percent. Between 15 and 24 percent of the incidence falls on commercial landowners, while residential landowners bear between 3.6 and 25 percent. Finally, between 7.2 percent and 20 percent of the incidence falls on workers.

Our incidence estimates for firm owners are very similar to the updated estimates reported by Suárez Serrato and Zidar (2023). Our estimates for residential landowners vary from specification to specification, but are generally of similar or slightly smaller magnitudes than their estimates. The main differences come from our estimates for commercial landowners and workers. Since Suárez Serrato and Zidar (2023) do not include commercial landowners, adding them mechanically generates a larger incidence on landowners. Our estimate on workers (which we take from Fuest et al. (2018)) is lower than the estimate by Suárez Serrato and Zidar (2023). This is potentially unsurprising due to institutional differences. The German LBT is set on the municipal level, while Suárez Serrato and Zidar (2023) rely on variation at the state level. Workers are likely more mobile between municipalities (with a median population of about 2,000) than they are between US states. This could explain the smaller incidence falling on workers in our setting.

4.3 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 3. 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. For sales prices we find that

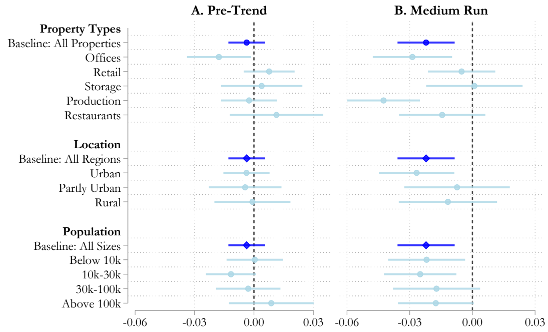
[11] We follow Suárez Serrato and Zidar (2016) and show the unweighted shares of incidence. This basically assumes that there is one representative agent of each type with equal income. Suárez Serrato and Zidar (2023) extend their incidence estimates to include income share weighted estimates, which captures the different sizes of the groups. This mostly lowers the share of residential landowners and slightly increases the share of workers and firm owners.

Table 3: Incidence Estimates

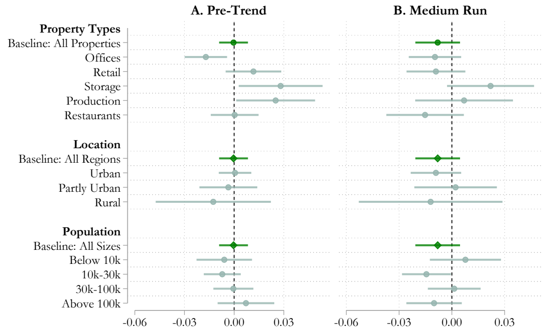
A. Incidence						
Landowners (Residential)	1.708***	0.877**	0.716***	0.923***	0.451*	0.195
	(0.504)	(0.316)	(0.204)	(0.270)	(0.188)	(0.110)
Landowners (Commercial)	1.646***	1.215**	1.095*	1.108*	0.647	1.130**
	(0.475)	(0.467)	(0.446)	(0.434)	(0.394)	(0.391)
Workers	0.490***	0.737***	0.785***	0.723***	0.865***	0.942***
	(0.099)	(0.155)	(0.189)	(0.169)	(0.194)	(0.217)
Firm owners	3.001***	2.329***	3.017***	3.001***	2.329***	3.017***
	(0.776)	(0.803)	(0.708)	(0.776)	(0.803)	(0.708)
B. Share of Incidence						
Landowners (Residential)	24.9%	17%	12.8%	16%	10.5%	3.6%
Landowners (Commercial)	24%	23.6%	19.5%	19.3%	15.1%	21.4%
Workers	7.2%	14.3%	14%	12.6%	20.2%	17.8%
Firm owners	43.8%	45.2%	53.8%	52.1%	54.3%	57.1%
Rent or Sales	Sales	Sales	Sales	Rent	Rent	Rent
Property Controls		✓	✓		✓	✓
Municipality 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 estiamting the elasticities. The first three columns use sales prices and the last three columns use rent prices. 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 2018. 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 3: 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 3a) and rent (Panel 3b) 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 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. Standard errors are robust to clustering at the municipality level.
Source: Own calculation based on data from *F+B* and Statistical State Offices.

the effects are largest for offices and production properties. 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 or restaurants. Other factors (such as taxes) play a larger role in these more location-independent sectors. The heterogeneity for the rent sample is smaller and less clear.

When controlling for differences in 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 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. In the sales sample effects do not vary much between smaller and larger municipalities. The effects are strongest for municipalities with a population of 10,000 to 30,000, but only marginally smaller in the other groups. The effect on rents is strongest in municipalities with a population of 10,000 to 30,000 and a population above 100,000. Estimates for the other groups are close to zero or even positive (for the smallest municipalities).

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 4,600 business tax reforms between 2008 and 2018. 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 leverages information from 140 different sources on over 2.4 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 five 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 (2022) 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. Utilizing the model developed by Suárez Serrato and Zidar (2016) and our estimates, we obtain incidence shares for workers, landowners and firm owner. Firm owners bear the largest part of the burden with estimates between 44 percent and 57 percent. Between 15 and 24 percent of the incidence falls on commercial landowners, while residential landowners bear between 3.6 and 25 percent. Finally, between 7.2 percent and 20 percent of the incidence falls on workers. This shows that it is crucial to account for both commercial and residential landowners in an evaluation of corporate taxation.

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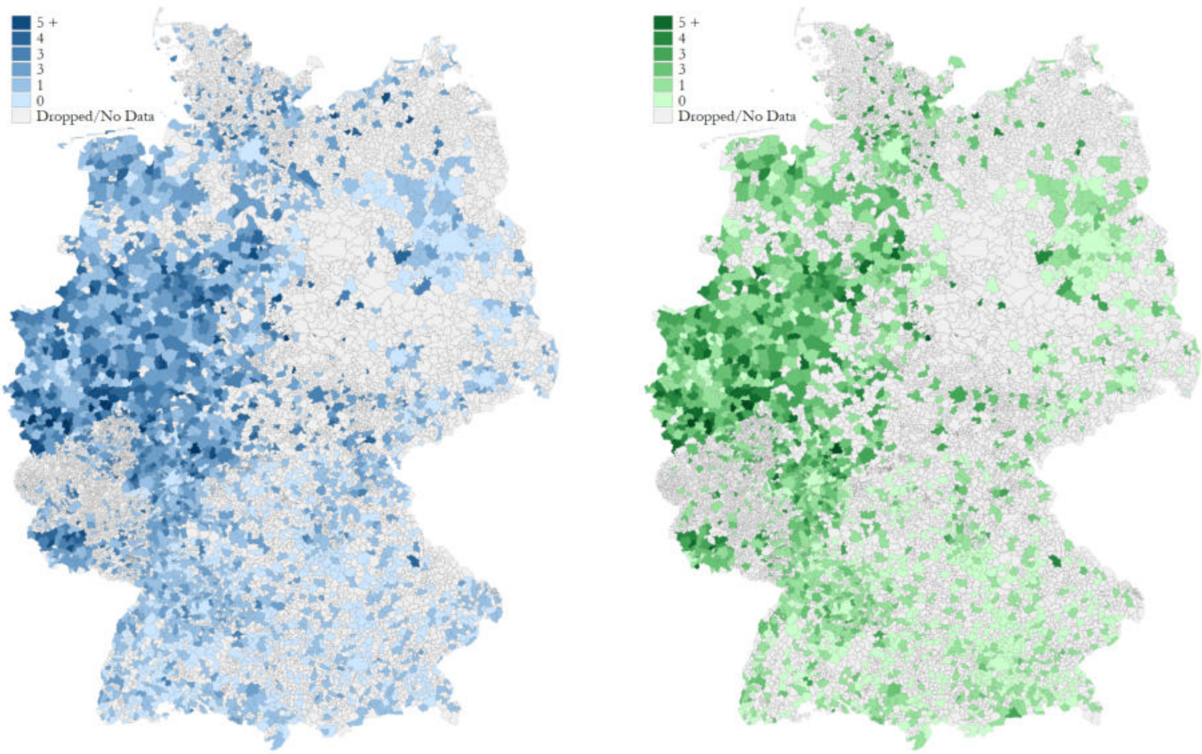
Appendix

A Descriptive Statistics

Figure A.1: Number of LBT Hikes (2008-18)

(a) Commercial Sales Sample

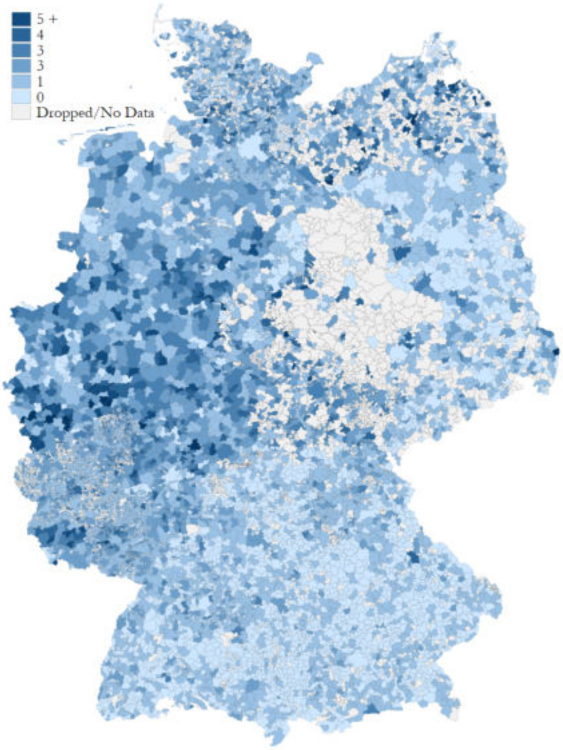
(b) Commercial Rents Sample



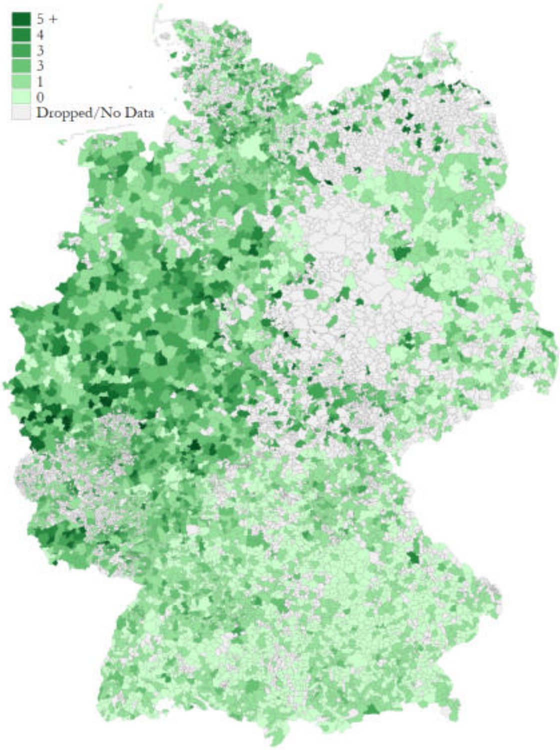
Notes: The figure shows the number of municipal LBT increases in Germany for a sample of 10,113 municipalities. Grey areas indicate municipalities that experienced municipal merger reforms or tax cuts during the sample period (2008-2018) and are consequently dropped from the sample. Panel A.1a shows the average LBT rate (in percent) for each municipality between 2008 and 2018. Panel A.1b plots the number of tax increases (defined as a scaling-factor induced LBT increase) between 2008 and 2018.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.2: Number of LBT Hikes (2008-18)

(a) Private Sales Sample

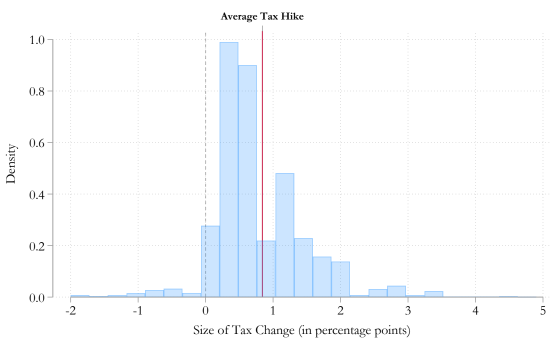


(b) Private Rents Sample

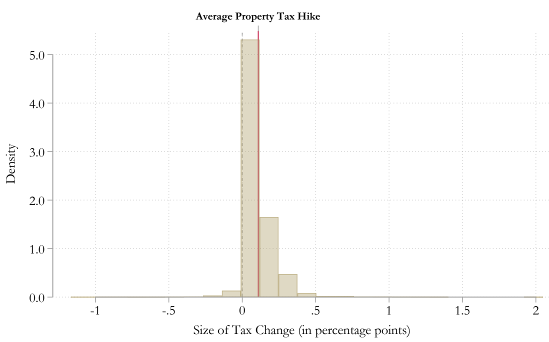


Notes: The figure shows the number of municipal LBT increases in Germany for a sample of 10,113 municipalities. Grey areas indicate municipalities that experienced municipal merger reforms or tax cuts during the sample period (2008-2018) and are consequently dropped from the sample. Panel A.2a shows the average LBT rate (in percent) for each municipality between 2008 and 2018. Panel A.2b plots the number of tax increases (defined as a scaling-factor induced LBT increase) between 2008 and 2018.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.3: Histogram of Tax Increases



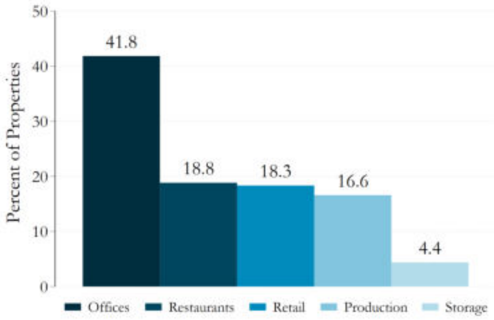
(a) Local Business Tax Changes



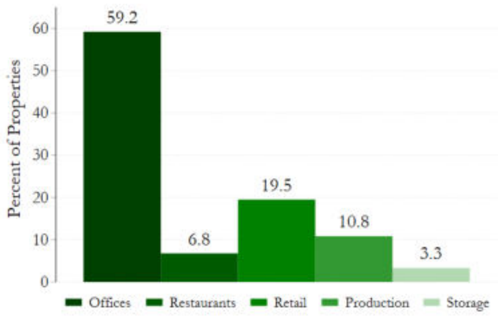
(b) Local Property Tax Changes

Notes: The histogram displays changes in the LBT rate, induced by municipal scaling factor changes between 2008 and 2018 for the 10,638 municipalities that were not subject to merger reforms. The average increase (excluding tax drops) is 0.85 percentage points, the 75th percentile increase is at 1.1 percentage points. The number total tax changes between 2008 and 2018 amounts to 11,924. For illustrative reasons, around 0.1 percent of observations with increases greater than 5 or smaller than -2 percentage points are omitted.
Source: Own calculation based on data from the Statistical State Offices.

Figure A.4: Distribution of Property Types



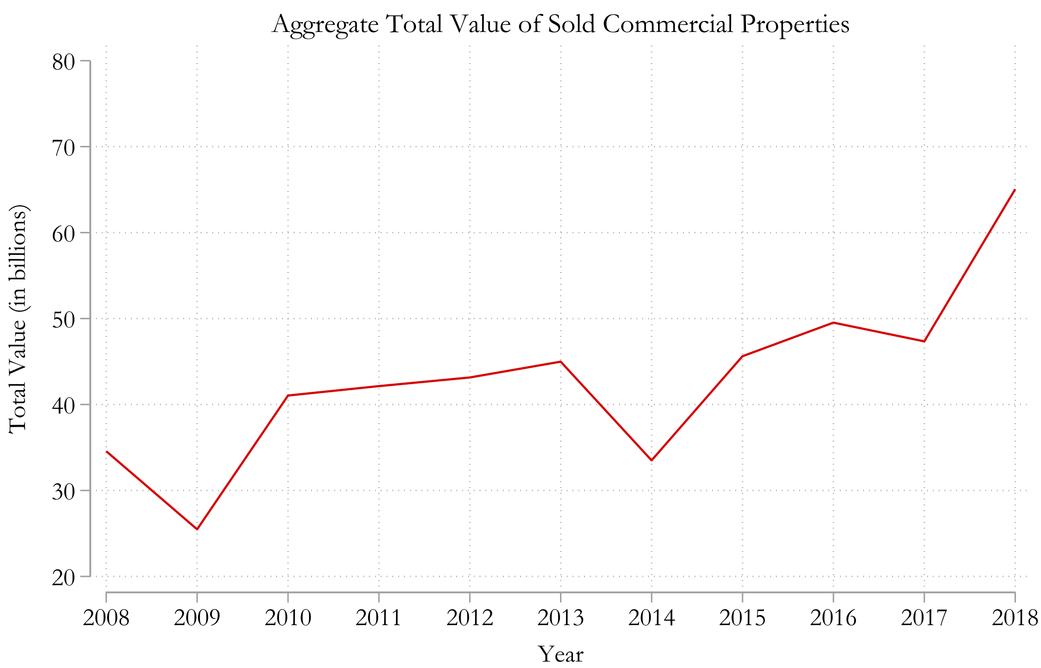
(a) Sales Sample



(b) Rents Sample

Notes: The figure shows the distribution of property types for both the commercial sales sample (Panel A.5) the commercial rents sample (Panel A.4b) between 2008 and 2018. 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 2,446,382. *Source*: Own calculation based on data from *F+B*.

Figure A.5: 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.1: Summary Statistics: Sales Price Baseline Sample

	Mean	Std. Dev.	Min	Max	N	Years
Panel A – Property Variables						
Price (in €/m^2)	1,540	1,114	59.50	6,000	842,150	2008-18
First price (in €/m^2)	1,551	1,126	0.01	48,750	842,150	2008-18
Construction year	1962	52	1500	2020	689,788	2008-18
Floor size (in m^2)	583.2	1,648	1	99,329	842,150	2008-18
# Rooms	7.000	6.453	1	99	401,538	2008-18
Basement dummy	0.254	0.435	0	1	842,150	2008-18
Parking spots dummy	0.485	0.500	0	1	842,150	2008-18
Web portal dummy	0.766	0.424	0	1	842,150	2008-18
Panel B – Tax Variables						
LBT rate (in %)	13.74	1.84	7.00	20.30	1,074,272	2008-18
LBT change (in %p)	0.07	0.42	-8.68	7.04	1,074,272	2008-18
LBT hike dummy	0.13	0.34	0	1	1,074,272	2008-18
# total tax changes	2.19	1.83	0	10	1,074,272	2008-18
Property tax rate (in %)	1.50	0.43	0.70	3.68	1,074,272	2008-18
Panel C – Economic Indicators						
Muni. Population	230,252	654,986	26	3,613,495	935,486	2008-17
Muni. Revenue (in thousand €)	172,505	366,331	34.93	1,988,511	396,393	2008-14
Muni. Expenditure. (in thousand €)	171,778	354,865	29.96	1,828,094	370,797	2008-14
Dist. GDP per capita (in €)	35,227	13,647	14,065	184,312	1,067,095	2008-18
Dist. Unemployment rate (in %)	7.06	3.25	1.40	21.24	1,066,782	2008-18

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 (2008-2018) are excluded. The sample covers 9,556 municipalities and 8,094 tax increases (see Table A.3 for more context on the number of municipalities). *Source:* Statistical State Offices and *F+B* .

Table A.2: Summary Statistics: Rental Price Baseline Sample

	Mean	Std. Dev.	Min	Max	N	Years
Panel A – Property Variables						
Price (in €/m^2)	9.69	6.80	1	66.67	2,446,382	2008-18
First price(in €/m^2)	9.48	5.83	1.43	40	2,446,382	2008-18
Construction year	1973	44	1500	2018	1,340,624	2008-18
Floor size (in m^2)	511.3	1.10	13	10,000	2,446,382	2008-18
# Rooms	3.23	2.02	1	15	893,259	2008-18
Basement dummy	0.16	0.37	0	1	2,446,382	2008-18
Parking spots dummy	0.37	0.48	0	1	2,446,382	2008-18
Web portal dummy	0.81	0.39	0	1	2,446,382	2008-18
Panel B – Tax Variables						
LBT rate (in %)	14.73	1.835	7.00	20.30	2,446,382	2008-18
LBT change (in %p)	0.04	0.36	-8.68	5.25	2,446,382	2008-18
LBT hike dummy	0.08	0.27	0	1	2,446,382	2008-18
# total tax changes	1.59	1.65	0	10	2,446,382	2008-18
Property tax rate (in %)	1.73	0.49	0.70	3.68	2,446,382	2008-18
Panel C – Economic Indicators						
Muni. Population	719,886	1,030,095	37	3,613,495	2,309,905	2008-17
Muni. Revenue (in thousand €)	412,000	543,084	59.49	1,988,511	755,312	2008-14
Muni. Expenditure. (in thousand €)	405,582	521,857	47.01	1,828,094	709,940	2008-14
Dist. GDP per capita (in €)	46,343	20,366	14,065	184,312	2,343,232	2008-18
Dist. Unemployment rate (in %)	8.23	3.49	1.40	21.24	2,343,232	2008-18

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 (2008-2018) are excluded. The sample covers 8,099 municipalities and 5,852 tax increases (see Table A.4 for more context on the number of municipalities). *Source:* Statistical State Offices and *F+B* .

Table A.3: Sample Selection - Sales Data

	# Municipalities	# Tax Hikes	# Properties
Municipality Data (2008–18)	11,085	13,859	-
Dropped mergers	10,638	12,640	-
No tax drops	10,113	11,924	-
Merge with Property Data	9,556	8,094	1,074,272
>5 Ads per year	6,561	4,627	1,002,914
Max. 1 Tax Hike	4,218	1,214	598,775

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

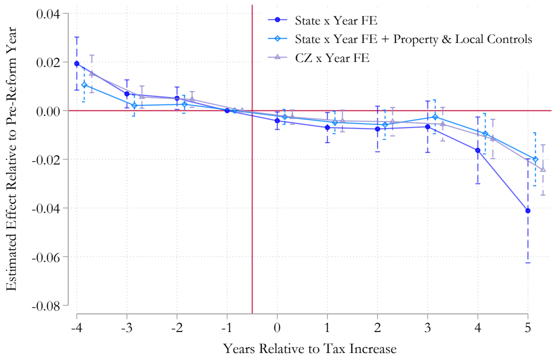
Table A.4: Sample Selection - Rental Data

	# Municipalities	# Tax Hikes	# Properties
Municipality Data (2008–18)	11,085	13,859	-
Dropped mergers	10,638	12,640	-
No tax drops	10,113	11,924	-
Merge with Property Data	8,099	5,852	2,446,382
>5 Ads per year	4,648	3,343	2,396,532
Max. 1 Tax Hike	2,980	862	1,804,260

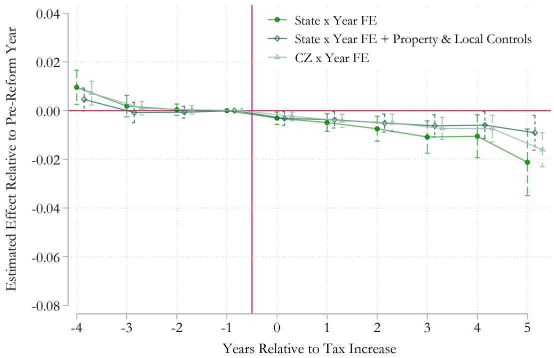
Notes: The table shows the number of municipalities, tax hikes, and rental price observations per sample selection step for the rental property price samples used in the analysis.
Source: Statistical State Offices.

B Additional Results

Figure B.6: Baseline Effects on Private Property Prices

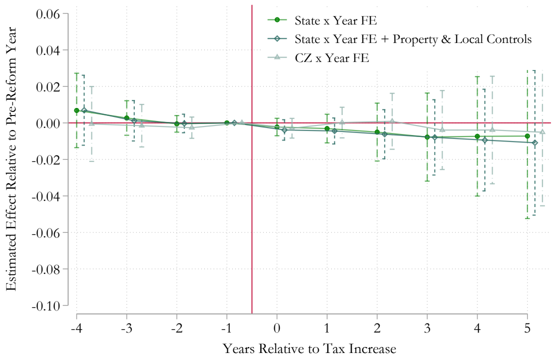


(a) TWFE: Sales Prices



(b) TWFE: Rents

(c) Heterogeneity Robust: Sales Prices

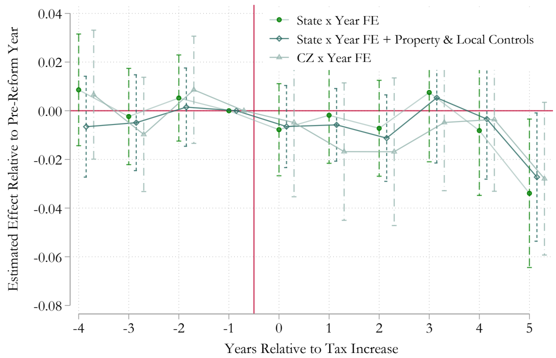


(d) Heterogeneity Robust: Rents

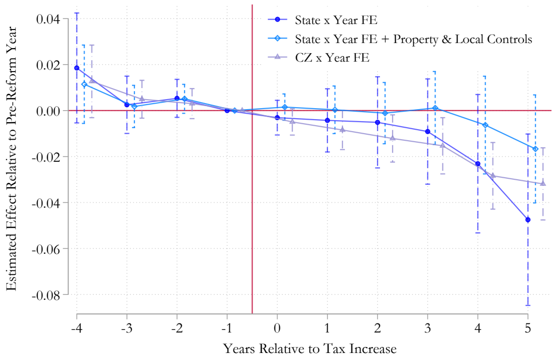
Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-4, 5]$) and associated 95% confidence intervals of the event study model from Equation 2. The dependent variables are the log sales price per sqm (Panel B.7a) and the log rental price per sqm (Panel B.7b). Treatment variables 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 municipal property tax rate as control. They also include the controls described in the figure. Standard errors are clustered at the municipal level. Panels B.7c and B.7d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2022). 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.

Figure B.7: Baseline Effects: Only One Tax Hike in Event Window

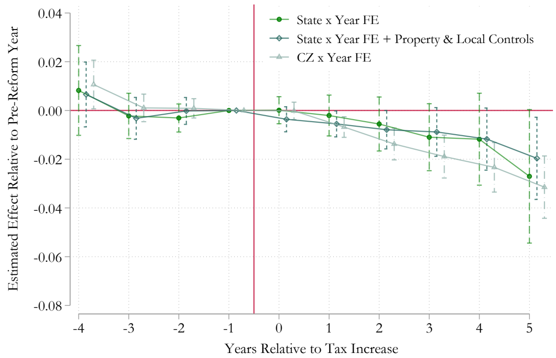
(a) Commercial: Sales Prices



(b) Commercial: Rents



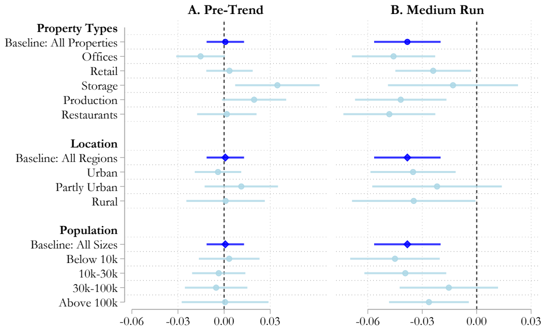
(c) Private: Sales Prices



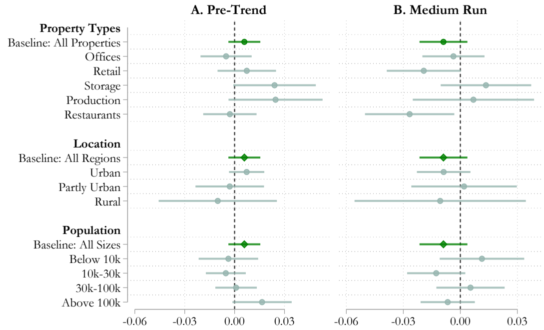
(d) Commercial: Rents

Notes: This graph plots the event study estimates ($\hat{\beta}_j, j \in [-4, 5]$) 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 2a) and the log rental price per sqm (Panel 2b). 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 (2008-18). All regressions include municipal and "state $\times$ year" fixed effects and 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 B.7c and B.7d show the implementation of the estimator proposed by De Chaisemartin and d'Haultfoeuille (2022). 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.8: Heterogeneity Analysis: Commercial Real Estate



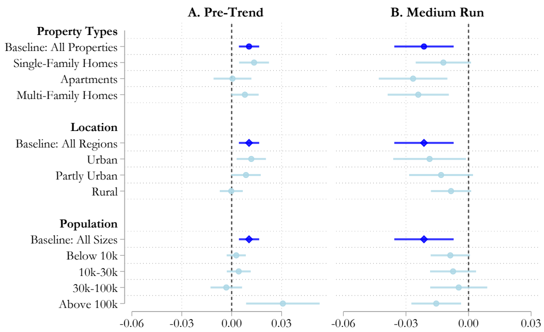
(a) Sales Sample



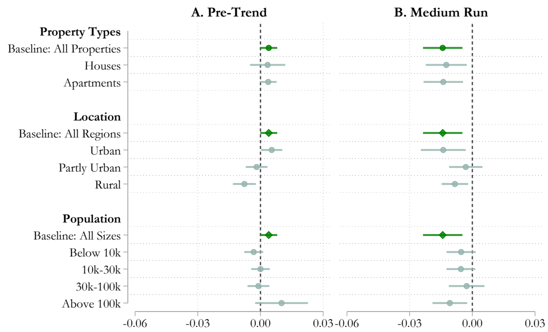
(b) Rental Sample

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 3a) and rental (Panel 3b) 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. For the rental sample, balancing weights are applied. 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.9: Heterogeneity Analysis: Private Real Estate



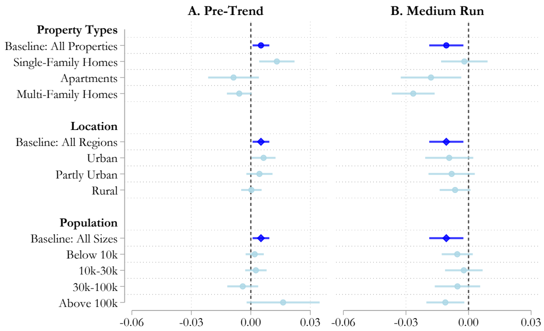
(a) Sales Sample



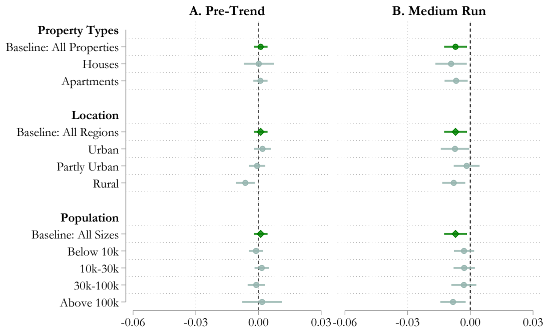
(b) Rental Sample

Notes: This Figure presents the results for different subsamples of observations according to property and municipal level variables in the private samples. Estimates depict the estimated treatment effect of a one percentage point increase in the LBT rate on the offered sales (Panel 3a) and rental (Panel 3b) 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.10: Heterogeneity Analysis: Private 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 private samples. Estimates depict the estimated treatment effect of a one percentage point increase in the LBT rate on the offered sales (Panel 3a) and rental (Panel 3b) 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.