Erasmus University Rotterdam

Erasmus School of Economics

Bachelor Thesis Economics and Taxation (Fiscale Economie)

Property Tax Capitalization in the Netherlands

Marie-Pauline G.A.F. Visser

Student ID: 444317

Abstract

This paper estimates the capitalization of local property taxes into local property values. I exploit the Dutch institutional setting with 2,243 tax changes available for identification over the sample period 2014-2020. House prices are modelled as the present value of future implicit rent values minus the property tax liability. The capitalization effect is estimated using a fixed effects model with municipal and time fixed effects and controls for public expenditures and local business cycles. The results suggest property taxes have a significant negative relationship with house prices. The hypothesis of zero capitalization can be rejected regardless of the model specification, while the hypothesis of full capitalization cannot be rejected as long as the chosen discount rate is sufficiently high. Under the baseline model specification, the 95% confidence interval of [-2.1, -0.07] shows the actual degree of capitalization may fall anywhere between partial and over-capitalization. Additional analyses, show no significant heterogeneous effects related to elasticities of supply. Finally, a series of event studies shows no evidence of dynamic effects of large tax changes when the event window comprises two periods before and after the event and multiple events in the same event window are excluded. Estimated capitalization effects need to be interpreted with caution as these may be subject to reversed causality.

Supervised by

dr. A.A.F. Gerritsen[1]

Second Assessor

dr. S.V. Kapoor

July 17, 2022

[1] My thanks go to Professor Gerritsen for his guidance, advice, and feedback

Index

1. Introduction

House prices in the Netherlands have risen continuously since 2013 and record high prices are an important topic in the political debate. In the fourth quarter of 2021 house prices increased as much as 19% compared to a year earlier, which placed the Netherlands amongst the top five countries in the EU with the largest house price increase (CBS, 2022). Notwithstanding the nationwide upward trend in house prices, there is substantial heterogeneity between regions. While the prices in Amsterdam increased by 78.8% between the second quarter of 2013 and the fourth quarter of 2019, prices in the rural province Zeeland increased only by 21.5% over the same period (Langenberg & Jonkers, 2022).

Previous literature shows there exists a negative relationship between property taxes and house values. When a higher property tax payment leads to a lower house value, the tax is said to be capitalized. The affordability of housing is seen as an important policy topic. To improve affordability, politicians are prone to look at tax or subsidy measures. Employing property tax reliefs as a measure to improve housing affordability would only be effective if the tax is not fully capitalized into house prices. Therefore, this paper studies the effect of the Dutch local property tax on average house prices in the Netherlands and aims to answer the following research question:

To what extent is the Dutch local property tax capitalized into local house prices?

To answer this question, I exploit variation in tax rates within 420 Dutch municipalities over the period 2014-2020. In the Netherlands, municipalities are free to adjust property tax rates yearly. This creates a unique setting with a large number of tax changes; the sample period contains 2,243 tax changes. Consistent with earlier literature, I model house prices as the present value of future implicit rent values minus the property tax liability. To estimate the capitalization effect, I employ a fixed effects model with municipal and time fixed effects and controls for local business cycles. The estimates of the baseline model are as follows: all else equal, a 1% increase in the property tax rate is associated with a 1.1% decrease in house prices. The hypothesis of zero capitalization can be rejected with at least 10% significance. This result is robust to the inclusion/exclusion of controls for local business cycles, the chosen discount rate, the geographical clustering of standard errors, and the inclusion of time fixed effects at different geographical levels. The hypothesis of full capitalization can be rejected as long as the chosen discount rate is sufficiently high. Under the baseline model specification, the 95% confidence interval of [-2.1, -0.07] shows the actual degree of capitalization may fall anywhere between partial and over-capitalization. The evidence of capitalization effects is in line with previous capitalization studies. However, the estimates need to be interpreted with caution as they may be subject to reversed causation.

A typical feature of the Dutch housing market is that the price elasticity of housing supply is low (Caldera & Johansson, 2013). Therefore, standard economic theory suggests the incidence of property taxes is on housing supply. Using data provided by Öztürk et al. (2018), I interact property tax rates with the level of supply constraints, measured by the ratio of developed land to total available developable land. The results show no significant heterogeneous effects and therefore are inconsistent with the theory on tax incidence. Moreover, I execute a series of event studies analysing dynamic effects of large property tax changes. The results show no evidence of dynamic effects of large tax changes when the event window comprises two periods before and after the event and observations with multiple events in the same event window are excluded. The absence of heterogeneous and dynamic effects casts further doubt on the results of the two way fixed effects regressions.

This paper contributes to earlier literature on the capitalization and incidence of property taxes and informs literature on determinants of Dutch (local) housing market dynamics. Previous empirical literature on property tax capitalization has focused on the United States. In the Netherlands, property taxes comprise a much smaller part of local public budgets. Additionally, education is the biggest expense category of local governments in the U.S., while educational expenses are negligible for Dutch municipalities. Because of the differences with the U.S. setting and the similarities with German setting, this paper relates most closely to the study of welfare effects of property taxation in Germany by Löffler and Siegloch (2021). Nevertheless, the large number of yearly tax changes is unique to the Dutch setting.

This paper is structured as follows. In section 2, I discuss the theoretical framework related to property tax capitalization and incidence and present empirical results from previous studies. Section 3 describes the data and empirical strategy. Section 4 reports the results of the main analysis, sensitivity analyses and analyses of heterogeneous and dynamic effects. These results are discussed in section 5. Section 6 concludes.

2. Theoretical Framework

2.1 Capitalization model

Property tax capitalization occurs when, all else equal, an increase in the property tax rate leads to a decrease in property value. Full capitalization occurs when, after accounting for other factors that may affect the price, differences in house prices are exactly equal to the present value of expected tax liabilities (Oates, 1969). Richardson & Thalheimer (1981) formulated the question of property tax capitalization in a simple way: "assuming two houses are equal in all aspects except for the property tax rate, to what extent is the market value of the house with the higher tax rate reduced relative to its counterpart?"

All studies of capitalization are based on the principle that the value of a house, like the value of any asset, equals the present value of the after-tax flow of services from owning it (Yinger et al., 1988). Depending on their outside options, house owners decide either to live in the house they own, or to rent out this house. The market rental rate represents the opportunity cost of renting versus buying a home. Therefore, market rental prices serve as an approximation of (implicit) rental benefits for both rental properties and owner occupied properties. The market value (P_0) of a house equals the present value of the rental flow minus the present value of the property tax flow (equation 1) where π_t is the market rental rate, $\tau_t P_t$ is the tax liability and r_t is the discount rate.

$$P_0 = \sum_{t=0}^{N} \frac{(\pi_t - \tau_t P_t)}{(1+r_t)^t} \tag{1}$$

Housing has a long expected lifetime, so it is reasonable to assume that N is large. Assuming N goes to infinity and π_t, r_t, and τ_t are time-invariant, we can write $P_t = P_0$ for all *t:*

$$P = \frac{(\pi - \tau P)}{r} \tag{2}$$

Which can be rearranged as:

$$P = \frac{\pi}{\tau + r} \tag{3}$$

Taking the natural logarithm of (3) leads to:

$$\ln P = \ln \pi - \ln(\tau + r) \tag{4}$$

Implicit rental benefits are mostly unobservable. By not controlling for market rental prices, the capitalization estimate will include both the effect of higher taxes and the effect via a change in π. In case of full capitalization π does not move with τ. With no capitalization π moves one to one with τ. Looking at equation 2, a €1 increase in $\frac{\tau P}{r}$ leads to a €1 decrease in P under the assumption that π is unaffected. The actual degree of capitalization depends on how π varies with τP and is reflected by the relative elasticities of supply and demand of rental properties. This mechanism relates to the incidence of the tax and is explained in detail in the next section.

2.2 Property tax incidence

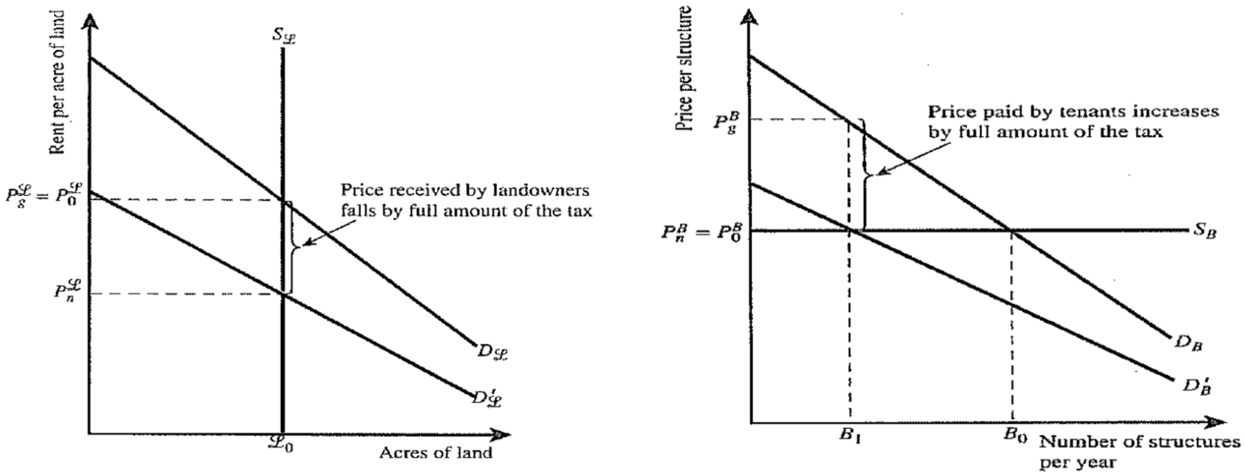
The capitalization and the incidence of property taxes are inherently related. The statutory incidence of a tax, indicates who is legally obliged to pay the tax. The economic incidence of a tax indicates who bears the tax burden. The Dutch local property tax is levied on property owners of land and structures. This means the statutory incidence is on the owner of a property. The owner of a property, however, does not necessarily bear the burden of the tax. Property owners may shift part of the tax burden to tenants by increasing rental prices. If this is the case, the increase in rental benefits, (partly) offsets the cost of a higher future tax burden.

When full capitalization occurs a tax increase is reflected fully in property values. This implies that house owners are not able to offset the cost of the higher tax burden with higher rental benefits. Under partial capitalization or in the extreme case of no capitalization a house owner is able to pass through part of or even the full tax burden to tenants. The degree to which house owners are able to shift the tax burden to tenants depends on the relative elasticities of supply and demand of housing. The more elastic housing supply is relative to demand, the more the tax burden can be shifted from owners to tenants. Two extreme cases are illustrated below.

Consider the case in which the supply of land is fixed and a tax on land is introduced. Supply is perfectly inelastic, which means land owners bear the entire burden of the tax. This mechanism is illustrated by Rosen and Gayer (2008) in panel A of Figure 1, where $S_{\mathcal{L}}$ is the supply of land, $D_{\mathcal{L}}$ is the pre-tax demand curve, and $D'_{\mathcal{L}}$ the after-tax demand curve.

Figure 1: *Property tax incidence*

Panel A: Perfectly inelastic supply Panel B: Perfectly elastic supply



Source: Rosen & Gayer (2008)

The intersection between $S_{\mathcal{L}}$ and $D_{\mathcal{L}}$, gives the pre-tax equilibrium price of land ($P_0^{\mathcal{L}}$). The intersection between $S_{\mathcal{L}}$ and $D'_{\mathcal{L}}$, gives the rent received by landowners after the tax ($P_n^{\mathcal{L}}$). After the tax, the price paid by users of the land is unchanged ($P_0^{\mathcal{L}} = P_g^{\mathcal{L}}$), while the rent received by landowners falls by the full amount of the tax ($P_0^{\mathcal{L}} - P_n^{\mathcal{L}}$). In this case, the property tax is not shifted to users of land and the tax is fully capitalized into land values.

Additionally, consider the case in which capital is perfectly mobile and a tax on structures is introduced. The supply of structures is perfectly elastic, which means not the suppliers of the structures, but tenants bear the full burden of the tax. The mechanism is illustrated by Rosen and Gayer (2008) in panel B of Figure 1, where S_B is the supply curve, D_B is the pre-tax demand curve, and D'_B the after-tax demand curve. At the pre-tax equilibrium price (P_0^B), the quantity exchanged is B_0. After the tax; the equilibrium price received by suppliers of structures is unchanged ($P_n^B = P_0^B$) and the quantity exchanged is B_1. The new price the users of structures pay (P_g^B) exceeds the original price exactly by the amount of the tax. In other words; the tax is fully passed through to tenants and there is no capitalization into property values.

The distributional effect of property taxation crucially depends on its incidence and thus on the degree of capitalization. Policy makers may intend to finance transfers to disadvantaged households with an increase in property taxes. However, when the incidence falls on tenants rather than property owners, the tax can cause significant redistribution and have unintended regressive effects (Hilber, 2011).

Full capitalization further implies that current house owners bear the entire burden of expected tax liabilities, whereas partial capitalization suggest that some of the burden is passed on to future owners (Borge & Rattso, 2014). This is explained by Yinger et al. (1988): without capitalization, taxes affect the stream of tax payments but not the market price of a house, so a certain household can escape higher taxes by selling its house and moving to another location. With capitalization on the other hand, tax changes are immediately reflected in house prices and the house owner has no escape: either he stays in his house and pays the higher stream of taxes or he sells his house and suffers the capital loss caused by the increase in the stream tax of payments. If poor information or high search costs lead to incomplete capitalization, a house owner can escape some of the burden of higher future tax payments (Yinger et al., 1988).

2.3 Empirical literature and capitalization estimates[2]

The notion of property tax capitalization was first formally tested by Oates (1969). Following Oates an extensive strand of literature has explored the nature of property tax capitalization. The earlier studies are summarized by Yinger et al. (1988). These studies can be roughly divided into three categories based on the empirical strategy employed. The first category is based on aggregate data on jurisdictional level (e.g. Oates (1969, 1973), Heinberg & Oates (1970), Mcdougall (1976), Rosen & Fullerton (1977) and Dusansky et al. (1981)). The second is based on cross-section micro data on individual housing transactions (e.g. Church (1974) and Richardson & Thalheimer (1981)). The third is based on micro data describing changes in a tax system (e.g. Smith (1970) and Rosen (1982)). The majority of studies falls under the first two categories and explores cross-sectional variation in property tax rates.

Almost all of the pre-1980 studies find a significant negative relationship between property taxes and house values, with the majority opinion that capitalization is somewhere between 50% and 100% (Yinger et al., 1988). As follows from the capitalization model, these estimates depend on the assumed discount rate. Most studies use discount rates of 5% or 6%. Yinger et al. (1988) argue these overstate the real discount rate and thereby overestimate the degree of capitalization. They re-calculate the capitalization estimates of previous studies with a discount

[2] Unless specifically states otherwise, all studies in this section are based in the U.S.

rate of 3% and an infinite time horizon and conclude the degree of capitalization is between 15% and 65%.

Yinger et al. (1988) argue that the capitalization model presented in section 2.1 may not apply to intra-jurisdictional property tax capitalization in three cases and present evidence that the degree of capitalization is far below 100%. First, there may be imperfect information about property taxes of a specific house relative to other houses. Second, buyers avoiding search costs may lead to less than complete capitalization. Third, even if households have perfect information and no search costs, they may not expect current property tax differences to persist indefinitely. Similarly, King (1977) points out the degree of capitalization depends on the visibility and certainty of the tax as perceived by the purchaser of property. Search costs and imperfect information are unlikely to differ over time within a municipality and are therefore not relevant for this study. In the Netherlands there are many yearly tax changes and it is assumed that changes are perceived to be permanent. This does not mean that taxes will not change again in future years, but that current changes in tax rates are not informative for future changes in tax rates. In case there is some level of mean reversion in tax rates, capitalization is expected to be below 100% even if supply is perfectly inelastic.

An issue for the estimation of capitalization effects is omitted variable bias. Property taxes are used to finance public services. While an increase in the tax rate is expected to decrease house prices, the corresponding increase in public services may increase (implicit) rental benefits and thereby increase house prices. Therefore, the capitalization effect may be underestimated and the capitalization coefficient is biased towards zero. Oates (1969, 1973) recognized that local (property) taxation must be seen in the context of financing local services and recognized the difficulty in developing a reliable measure for output of public services. In the United States education is by far the largest single item on local public budgets (Oates, 1969; Urban institute, n.d.). This motivated Oates (1969) to proxy public service output with educational expenditures per pupil. To correct for the potential downward bias in capitalization estimates by this incomplete measure of public services, Oates (1973) re-estimated his earlier paper by adding municipal spending per capita on non-education services. His earlier study estimates two-thirds capitalization, while the subsequent study estimates full capitalization.

Variations in public spending may be caused by other variables and may not reflect the quality of public services provided. Therefore, some studies have tried to employ output measures of public services rather than input measures. Rosen and Fullerton (1977) employ 4^{th} grade test scores as an output measure for public services and estimate 88% capitalization. Mcdougall (1979) explores the effect of specific local public services rather than a single measure for all local services. He finds that education (measured by test scores), police services (measured by crime rates for personal and property crimes), and park and recreation services (measured by an index of the number of sub functions performed in the parks and recreation program for each city) all have a significant positive impact on property values. He estimates full capitalization. Palmon & Smith (1998) try to solve the potential downward bias in capitalization estimates by using a data set that contains little variation in service provision and in which differences in property tax rates are largely determined historically. They find support for full capitalization and conclude that housing market participants rationally discount the value of properties subject to higher taxes. Gallagher et al. (2013) focus on a segment of the housing market that likely places little-to-no value on school quality: small homes. They show that the small homes they investigate are unlikely to have school-age children and they thereby sidestep the problem of omitted or misspecified measures of school quality. They estimate that local property taxes are nearly fully capitalized into the prices of small homes.

A major threat to identification covered by previous literature is reversed causality. Oates (1969) recognized that the negative association between tax rates and home values may stem from a dependence of tax rates on property values rather than the reverse. For a given level of public spending, the property tax rate needed to finance the public expenditure program depends on the average house value in a municipality. Even in the absence of capitalization, the negative effect of house prices on tax rates yields a capitalization estimate that would suggest higher taxes lead to a lower house price. Therefore, reversed causality biases capitalization estimates towards 100%. Chinloy (1978) analyses capitalization in Canada and accounts for simultaneity by estimating a two staged least squares model using a series of instruments. He assumes property tax rates are determined by the tax characteristics of a household (head): pre-tax income, age, sex, marital status, number in household, and number of children. His estimates depend crucially on the use of actual as opposed to effective tax rates. Using actual interest rates, he estimates 50% capitalization. When effective tax rates are used, the hypothesis of zero capitalization cannot be rejected. He concludes the upward bias (in absolute terms) in capitalization when using actual tax rates may be peculiar to Canadian legislation. Borge and Rattsø (2014) exploit a specific feature of the Norwegian setting. In Norway, property taxes currently are optional for local governments, but used to be compulsory for towns and not for municipalities. They argue this historic difference may influence later property tax decisions without affecting the housing market several decades later. Additionally, they use characteristics of the local political system such as party fragmentation, ideology and female representation in the local council as instruments and argue these affect taxation without having a direct effect on the housing market. Their results indicate full capitalization.

This paper is most closely related to the study of Löffler and Siegloch (2021), which analyses the welfare effects of property taxation by evaluating the response of house prices, various types of income, and public services to changes in property taxes. Over the identification period (2007-2020) more than 10% of the 8,481 West German municipalities change their local property tax rate, leading to a large amount of tax changes available for identification. Löffler and Siegloch implement a series of event studies to exploit the within-municipality variation in tax rates over time and extract exogenous variation in the tax variable by exploiting a specific feature of the German fiscal equalisation scheme. This scheme provides the main source of municipal income and allocates resources based on fiscal needs relative to fiscal capacities. The fiscal capacity is determined by multiplying a municipalities' property tax base with a standard rate. An increase in the standard rate incentivizes municipalities to increase the local tax rate. The higher the new standard rate relative to a municipalities' old tax rate, the stronger the incentive for a subsequent local tax increase. To exploit these dynamics, Löffler and Siegloch (2021) use an instrument that interacts a dummy variable indicating an increase in a states' standard rate with a measure that captures the relative difference between the new standard tax rate and the old local tax rate.

Löffler and Siegloch (2021) find that gross rents increase moderately in the short run implying that part of the tax burden is on the landlord. In the medium run, starting three years after a tax hike, gross rents further increase to a level implying a full pass-through on gross rents. This suggests there is no capitalization of property taxes into house prices in the long run. Additionally, they find the pass-through of property taxes on rents is higher in municipalities that have a lower share of developed land, a lower share of physically undevelopable land, smaller population levels, or a higher share of private (rather than public) housing. Consistent with the theory on tax incidence these finding suggest the pass-through is higher when housing supply is relatively elastic. Finally, Löffler and Siegloch show that utility losses due to a tax increase are larger for households at the bottom of the income distribution and increase inequality in consumption. Therefore, they conclude that the property tax is regressive.

3. Data and Empirical Strategy

3.1 Sample Selection

This study consists of a panel analysis of all municipalities in the Netherlands between 2014 and 2020, with the aim of determining, other things being equal, the relationship between local property values and local property taxes. The dependent variable of interest is average sales prices of houses sold in a certain year in a certain municipality. The main explanatory variable is the local property tax rate.

During the sample period various municipal mergers occurred (Table A.1). In total 420 municipalities are observed, 81 of which are observed only in part of the 8-year period.[3] Moreover, 10 municipalities are amended during the sample period.[4] Although these municipalities are amended at some point in time, the data regards pre- and post-merger municipalities as the same municipality throughout the sample period. This leads to a total of 2,664 observations. Table A.2 summarizes the descriptive statistics of all variables.

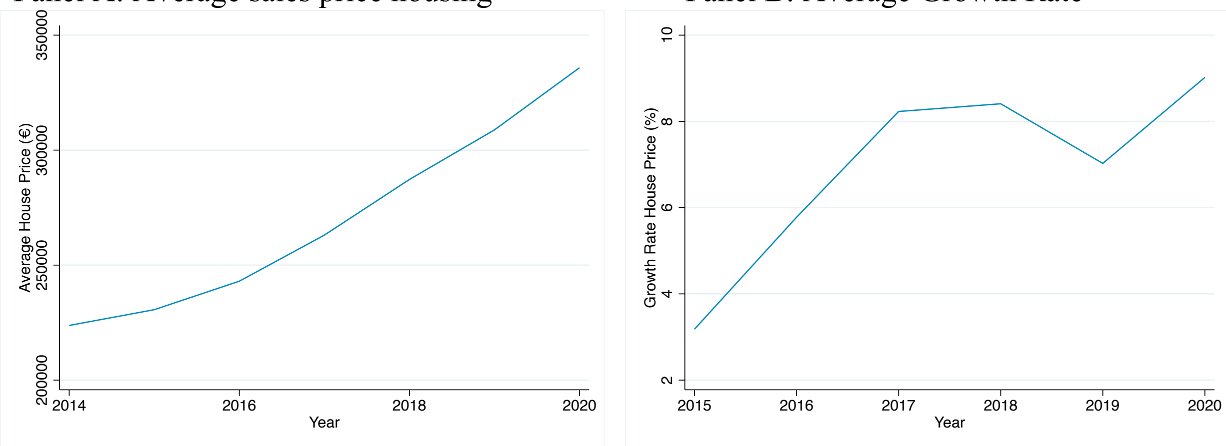
3.2 Dependent Variable

The dependent variable is the average sales price of all housing transactions that are registered by the Kadaster during a specific year. This data is published by the Dutch Central Bureau for Statistics (CBS).

Figure 2

House price trends between 2014-2020

Panel A: Average sales price housing Panel B: Average Growth Rate

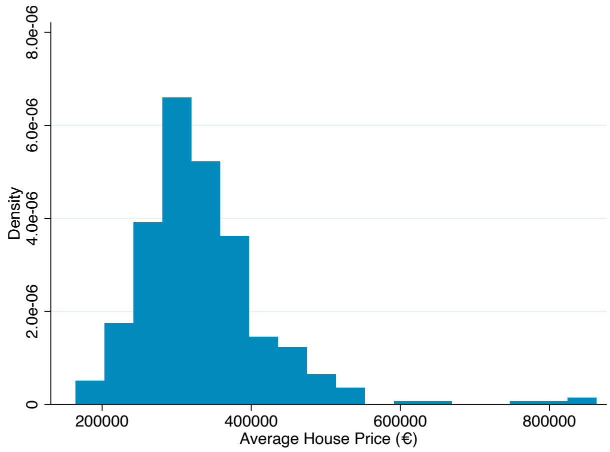


[3] These are reported in columns 2 and 3 of Table A.1
[4] These are reported in column 4 of Table A.1

Figure 2 shows the average house price trend in the Netherlands between 2014 and 2020. At the start of the sample period the average house price in the Netherlands was approximately €224,000. At the end of the sample period this was approximately €336,000. The first half of the period shows a stable increase in the growth rate of house prices. Between 2017 and 2019 the growth rate declines slightly, after which it seems to return its pre-2017 trend. Figure 3 shows there is substantial variation in average house prices per municipality. In 2020, Pekela reported the lowest average house prices of €164,123, while Bloemendaal reported the highest average house price of €863,297.

Figure 3

Average house price per municipality in 2020



The dependent variable does not control for characteristics of specific houses that are sold in a year. Therefore, some variation in house prices may be caused by the type of houses sold, rather than differences in property values. A measure that does control for individual housing characteristics is the house price index from the CBS. However, this measure is not available on a municipal level. Figure A.1 shows that, on a national level, the trends in average house prices and the house price index are very similar.

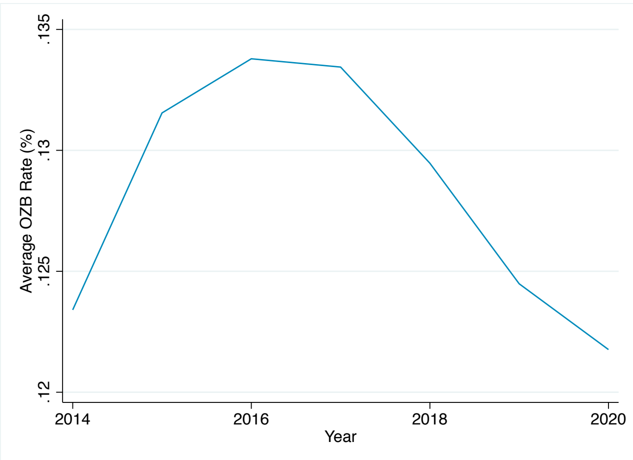
3.2 Main explanatory variable

The Dutch local property tax is paid yearly by the owners of residential and non-residential properties to Dutch municipalities. Until 2014, limitations existed in the maximum (national) property tax rate increase. Since 2014, these restrictions have been lifted and municipalities can freely adjust tax rates. A municipality can choose different tax rates for residential and non-residential properties. To determine the property tax liability, the assessed value of a property (WOZ value) is multiplied by the appropriate tax rate. WOZ values are assessed yearly by each municipality, but the WOZ value of a property in any year is the assessed value on January 1st of the preceding year. Additionally, it is important to note that the legal norms regarding property assessment, the definition of the tax base and exempted property types are determined by the national government. In case municipalities could change tax revenues by adjusting the tax base as well as the tax rate, this would complicate identification of the effect of changes in the tax rate.

The Dutch local property tax only accounts for a small portion of municipal income as the majority of income is derived from the national municipal fund (Gemeentefonds) or other non-tax resources. To illustrate this: in 2020 the total municipal income in the Netherlands was 6.3 billion euros, 51% of which came from the national municipal fund while only 7% came from property tax proceeds (Ministerie van Financiën, n.d.). Most own resources of municipalities are earmarked to finance only certain expenditures. The property tax comprises 75% of not-earmarked own revenues and is therefore the most important controllable source of revenue.

Figure 4

Average OZB rates 2014-2020

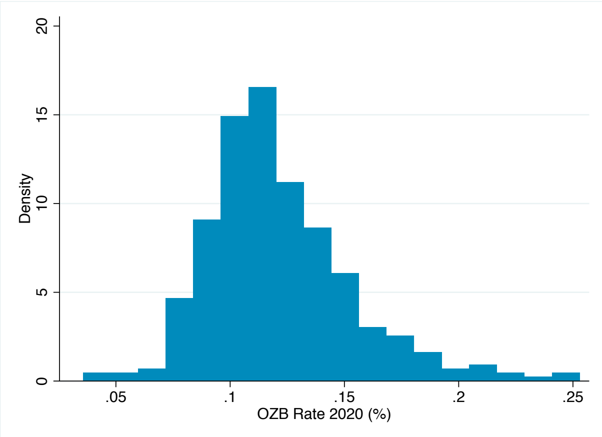


This study focusses on the property tax on residential properties only and will refer to this as the OZB. Data on yearly OZB rates is provided by COELO. In the sample period, the average OZB rate is 0.128% (Figure 4). Over the sample period each municipality changed the OZB rate each year. This results in 2,243 tax changes available for identification. The average yearly change in the OZB rate per municipality is 0.00016 percentage points (or 0.12%). The biggest decrease is 0.014 percentage points (or 22.0%) and the biggest increase is 0.052 percentage points (or 48.1%). There is substantial variation in tax rates and tax changes between municipalities (Figure 5). In 2020, OZB rates ranged from 0.0356% to 0.2529% of the WOZ value. The average municipal OZB payment ranged from €120 to €816. The average change in OZB rate per municipality in 2020 as compared to the preceding year ranged from -0.019 percentage point (or -18.18%) to 0.050 percentage point (or 35.72%).

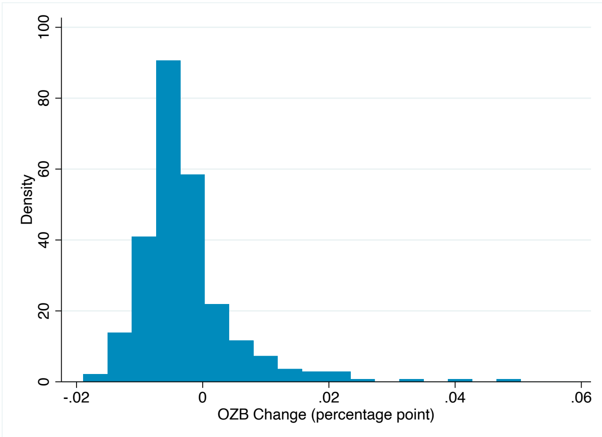
Figure 5

OZB rate and change per municipality in 2020

Panel A: OZB rate



Panel B: OZB change (relative to 2019)



It important to note that in five instances, the OZB rate was changed, but the total tax payment did not change.[5] In these cases the change in OZB rate exactly offsets the change in WOZ values, which suggests that higher WOZ values caused lower OZB rates and that reversed causality is a particular issue in these municipalities. Moreover, in the presentation of their 2022 budget, the municipality of Amsterdam stated specifically that the OZB rate would be defined at a later point in time because the rate would depend on the development of property values (Gemeente Amsterdam, 2021). This is a clear example of reversed causality, which in turn is a threat to identification.

A fixed effects regression of OZB changes on the three of its lags shows a significant negative relationship between the OZB rate in a year and the two preceding years (Table A.3). This suggest there exists some mean reversion in OZB rates. As explained in section 2.3 this means capitalization is expected to be below 100% even if supply is perfectly inelastic.

[5] Amsterdam 2016; Dronten 2018; Emmen 2018; Lingewaal 2017; Nuenen, Gerwen en Nederwetten 2016

3.3 Baseline model

Following previous capitalization studies, I model the present value of a house as the present value of (implicit) rental benefits minus the tax rate (equation 4). To exploit within municipality variation, I estimate a fixed effects model. To estimate the capitalization effect, the explanatory variable should be uncorrelated with the error term. In other words, all factors that could influence property tax rates and housing prices at the same time should be controlled for. While time invariant omitted variables are captured by municipal fixed effects, it is important to control for time varying omitted variables.

Previous studies have highlighted the importance of controlling for public service provision when estimating capitalization effects. Although in the Netherlands property tax revenues comprise a small part of municipal income, these are the main source of revenue that is not earmarked and which the municipality itself controls. Therefore, the property tax rate may be important for changes in local public service provision. Because of the similarity between Dutch and German local tax systems, I will follow Löffler and Siegloch (2021) and proxy public service provision with per capita municipal expenditures. This data is provided by the Dutch central government through the platform Findo. A disadvantage is that this data is not reported for municipalities that are merged into other municipalities at any point during the identification period. Therefore, the data is missing for the municipalities specified in column 2 of Table A.1 (Appendix). Previous studies criticize the use of an input variable (expenditures) with an output variable (services) and argue for the use of educational output variables. These studies are mainly based the United States, where education is by far the largest single item on local public budgets (Oates, 1969). In 2019, 40% of direct local government spending in the US went to elementary and secondary education (Urban Institute, n.d.). In the Netherlands this was only 3.7% (Findo, n.d.). Therefore, it is unlikely that educational output provides a good measure of public service output in the Netherlands.

Löffler and Siegloch (2021) argue that the main source of confounding variables at the local level are local business cycles. Local municipality specific business cycles drive both municipal property tax rates and local housing market outcomes. To account for this, I include municipal unemployment rates, average municipal disposable household income, municipal population growth rates, and the total number of company establishments per municipality in the estimation. Data used to control for local business cycles is provided by the CBS and descriptive statistics are summarized in Table A.2. Unemployment rates are measured by dividing the number of people receiving short term unemployment benefits (through the Werkloosheid wet) by the population. Disposable income refers to income households can use for consumptions purposes. It consists of primary income, benefits, and other income transfers minus taxes and social contributions. Population growth rates are measured by comparing the total number of inhabitants from year to year and include migration and natural growth (births minus deaths). The number of company establishments excludes government, education and health care sectors due to unreliability in those estimates. In line with Öztürk et al. (2018), I purposely do not control for the municipal stock of housing as this is directly related to the elasticity of supply and is used to estimate heterogeneous effects.

Time varying omitted variables on a national level, such as CPI, construction costs, and mortgage rates, will be captured by time fixed effects. The Netherlands is divided in 40 Corop-regions that consist of multiple adjacent municipalities. To account for local shocks I will include the Corop-region*year interaction effects. The baseline specification includes municipal clustered standard errors which are robust to heteroscedasticity and autocorrelation within municipalities. The baseline specification is as follows[6]:

$$\begin{aligned}\ln(Housingprice_{m,t}) = {} & \beta_1 \ln\left(\tau_{m,t} + r\right) + \beta_2 \ln(Public\ Expenditures_{m,t}) \\ & + \beta_3 \ln\,(Household\ Income_{m,t}) + \beta_4 Unemployment_{m,t} \\ & + \beta_5 \ln\,(Companies_{m,t}) + \beta_6 Population\ Growth_{m,t} + \alpha_m + \psi_{c,t} + \gamma_t \\ & + \varepsilon_{m,t}\end{aligned} \tag{5}$$

Municipal, Corop-region and year fixed effects are denoted by α_m, $\psi_{c,t}$ and γ_t respectively. The error term is denoted by $\varepsilon_{m,t}$. For a given discount rate, the degree of capitalization depends on the coefficient β_1. I will test the following two hypotheses:

$$H_1: \beta_1 = 0 \ v.s.\ \beta_1 \neq 0$$
$$H_2: \beta_1 = -1 \ v.s.\ \beta_1 \neq -1$$

Hypothesis 1 represents the extreme case of no capitalization. As derived in the theoretical framework, this implies changes in property taxes are completely passed through to tenants.

[6] Subscript c is suppressed for all variables other than $\psi_{c,t}$ as c is fixed for any value of m.

Hypothesis 2 represents the case of full capitalization, where property owners bear the full burden of taxation. The estimated degree of capitalization depends on the discount rate. Following Yinger et al (1988) the baseline model will include a 3% discount rate. A lower discount rate would lead to smaller capitalization estimates. As explained earlier, public services are expected to have a positive impact on house prices. I expect a positive relationship between household income and house prices, as an increase in income enables households to buy a more expensive house. I expect a negative relationship between unemployment rates and house prices, as unemployment reduces the number of people who can afford a house. The number of company establishments affects the attractiveness of a municipality and is expected to have a positive effect on house prices. The larger the number of people living in a region, the higher the demand for housing is. Therefore, population growth should have a positive relationship with house prices.

3.4 Sensitivity analyses

In the baseline model, the ten municipalities that change over time due to municipal mergers are treated as the same municipality pre- and post-merger. To test whether the estimates are sensitive to the treatment of these municipalities, I will re-estimate equation 5 regarding merged municipalities as different municipalities pre- and post-merger. In other words, these municipalities will be given a different post-merger identity. Furthermore, the model will be re-estimated excluding the five municipalities that show particular issues with reversed causality as shown in section 3.2.[7]

To assess the relevance of confounders at the local level and the robustness of the results, I will run an alternative specification controlling for local shocks at a higher geographical level, namely provincial level as opposed to Corop-region level. Additionally, I will check the sensitivity of the estimates to the inclusion/exclusion of local business cycle controls and alternatively include the number of jobs instead of number of company establishments. Moreover, I will estimate the effect of excluding the public expenditures variable. Following Löffler and Siegloch (2021), I will alternatively cluster standard errors at the Corop-region level instead of municipality level and check the sensitivity of the estimates.

[7] Amsterdam, Dronten, Emmen, Lingewaal and Nuenen, Gerwen en Nederwetten

Previous literature adopts various estimates of the discount rate. The majority of the early literature assumes a discount rate of 5% and a time horizon of 40 years. Yinger et al. (1988) argue for the use of a 3% discount rate and infinite time horizon and re-estimate all earlier literature with this discount rate. The use of a discount rate of 3% facilitates comparison of the results to earlier research (as estimated by Yinger et al.). Following Gallagher et al. (2013), the results will be re-estimated using lower discount rates of 2% and 1% which seem more appropriate with prevailing low interest rates (as compared to the 1980s).

3.5 Supply constraints and heterogeneous effects

As derived in the theoretical framework, the degree of capitalization depends on relative elasticities of supply and demand. A typical feature of the Dutch housing market is that the price elasticity of housing supply is low, which is partly related to the relatively high population density (Caldera & Johansson, 2013). In the literature a low supply elasticity is often linked to physical supply constraints related to geography (Saiz, 2010) or regulatory supply constraints related to a rigid planning system (Hilber & Vermeulen, 2016). For the Netherlands both physical and regulatory supply constraints are relevant. In various, mostly urban, areas new construction is restricted because a considerable share of land is already developed (physical constraints) (Öztürk et al., 2018). Moreover, new housing construction is further hindered by a restrictive planning system (Rouwendal & Vermeulen, 2007).

To test for heterogeneous effects of the Dutch local property tax on house prices driven by the elasticity of supply, I will use the data on supply side constraints from Öztürk et al. (2018). They base their index for the extent of supply side constraints in a given municipality on the methodology developed by Hilber and Vermeulen (2016) and relate the amount of already developed land to the total available developable land. Based on this variable they divide the sample into three equally sized groups of municipalities: municipalities with low, medium, and high supply constraints. The data is limited to the sample of municipalities present in 2018. Due to a lack of data on the rigidity of the planning system, this measure does not distinguish between physical and regulatory supply constraints. However, in practice both types of constraints are highly correlated (Saiz, 2010). I will interact the tax variable with two dummies indicating medium and high supply constraints. Because the supply constraints variable is constant, the standalone effect is captured by the municipal fixed effects.

3.6 Dynamic effects

Following the methodology of Fuest, Peichl and Siegloch (2018), I will perform an event study to estimate dynamic treatment effects of tax changes.[8] I will estimate the effect of large tax increases and large tax decreases. This alternative approach is taken because house prices might not respond to small tax changes and because municipalities experience less large changes within the sample period. A large tax change is defined as any change greater or equal to the 75th percentile of tax increases/decreases respectively. A large increase is bigger than 0.0085 percentage points (or 6.52%) and a large decrease is bigger (in absolute terms) than -0.0062 percentage points (or -5.04%). Equation 4 is re-estimated as follows[9]:

$$\begin{aligned}\ln(Housingprice_{m,t}) = \sum_{j=-3}^{3} \gamma_j\, D_{m,t}^{j} + \beta_2 \ln(Public\ Expenditures_{m,t}) \\ + \beta_3 \ln\,(Household\ Income_{m,t}) + \beta_4 Unemployment_{m,t} \\ + \beta_5 \ln\,(Companies_{m,t}) + \beta_6 Population\ Growth_{m,t} + \alpha_m + \psi_{c,t} + \varepsilon_{m,t}\end{aligned} \tag{6}$$

The independent variables of interest are a set of dummies $D_{m,t}^{j}$ indicating an event happening -j periods away. The event window consists of three periods before and after the tax change. Municipal, Corop-region and time fixed effects are denoted by α_m, $\psi_{c,t}$ and γ_t respectively. The error term is denoted by $\varepsilon_{m,t}$. For large tax increases (decreases), observations that have a large decrease (increase) during the observation period are excluded. This is done because the effects of a large increase and decrease in the same event window, may offset each other.

Ideally, municipalities do not experience multiple large tax increases or decreases within the same event window. When they do, it is not possible to discern the effect of one change to the effect of another. Large decreases tend to be followed by more large decreases within three years (Table 1). Following Fuest, Peichl and Siegloch (2018), I will check the sensitivity of the estimates to the exclusion of observations that contain multiple large increases or multiple large decrease in the same event window. As an additional sensitivity check, I will re-estimate the model with an event window of two periods before and after the tax change.

[8] Fuest, Peichl and Siegloch (2018) use this methodology to analyse the impact of local business taxes on wages
[9] Subscript c is suppressed for all variables other than $\psi_{c,t}$ as c is fixed for any value of m.

Table 1

Number of large tax changes in the same event window

	Large increase	Large decrease
Changes	Occurrences	Occurrences
0	176	161
1	121	89
2	36	54
3	4	29
4	0	3
5	0	1
6	0	0

4. Results

4.1 Baseline model

The results of the baseline model are reported in column A of Table 2. The point estimate of the OZB rate coefficient of -1.07 can be interpreted as follows: all else equal, a 1% increase in the OZB rate is associated with a 1.07% decrease in house prices. This coefficient is different from 0 at a 5% significance level. The hypothesis of no capitalization (H_1: $\beta_1 = 0$) can be rejected with 96.3% certainty. The hypothesis of full capitalization (H_2: $\beta_1 = -1$) cannot be rejected.[10] The 95% confidence interval of the coefficient [-2.07, -0.066] shows that although there is a negative relationship between OZB rates and house prices, the magnitude of the effect is uncertain. The degree of capitalization falls anywhere between partial and over capitalization.

The coefficients of the control variables are not significantly different from zero, meaning these variables have no significant relationship with house prices. The within R^2 shows that 94% of variation in house prices within municipalities is explained by the model.

4.2 Sensitivity analyses

Column B of Table 2 shows the results of the analysis in which municipalities are treated as different municipalities pre- and post-merger. The magnitude and significance of the coefficient of OZB rates decreases slightly. The coefficient is different from 0 at a 10% significance level. The hypothesis of no capitalization can be rejected with 90.2% certainty. The hypothesis of full capitalization cannot be rejected.[11] Although there is a significant negative relationship between OZB rates and house prices, the 95% confidence interval [-1.73, 0.15] shows the magnitude of the capitalization effect is uncertain. It is noteworthy that the effect of the number of company establishment is highly significant in this specification.

Column C of Table 2 shows the results of the analysis that excludes the five municipalities that showed specific indications of reversed causality. The changes in magnitude and significance of the coefficient of OZB rates are negligible.

[10] P-value 0.90
[11] P-value 0.67

Table 2

Main results

Outcome: ln(House Price)						
	(A)	(B)	(C)	(D)	(E)	(F)
Ln(OZB rate)	-1.07**	-0.79*	-1.06**	-1.11**	-0.73**	-0.39**
	(0.51)	(0.48)	(0.52)	(0.50)	(0.35)	(0.19)
Ln(Expenditures)	0.0040	0.0055	0.0046	0.000041	0.0040	0.0040
	(0.087)	(0.0085)	(0.0087)	(0.0082)	(0.0087)	(0.0087)
Ln(Household Income)	0.099	0.054	0.093	0.14*	0.099	0.099
	(0.087)	(0.080)	(0.089)	(0.085)	(0.087)	(0.087)
Unemployment	-0.016	-0.019	-0.015	-0.0095	-0.016	-0.016
	(0.011)	(0.012)	(0.011)	(0.010)	(0.011)	(0.011)
Population growth	-0.00046	-0.0035	-0.00046	-0.00056	-0.00046	-0.00046
	(0.00038)	(0.00040)	(0.00039)	(0.00039)	(0.00038)	(0.00038)
Ln(Companies)	0.16	0.50***	0.15	0.23*	0.16	0.16
	(0.12)	(0.096)	(0.12)	(0.13)	(0.12)	(0.12)
Discount rate	3%	3%	3%	3%	2%	1%
Corop*year	Yes	Yes	Yes	No	Yes	Yes
Province*year	No	No	No	Yes	No	No
Different post-merger identity	No	Yes	No	No	No	No
Excluded 5 municipalities with reversed causality issues	No	No	Yes	No	No	No
Observations	2,405	2,405	2,377	2,405	2,405	2,405
R^2						
Within	0.94	0.95	0.94	0.94	0.94	0.94
Between	0.0032	0.0011	0.0014	0.0038	0.0032	0.0033
Overall	0.17	0.036	0.17	0.12	0.17	0.17
H_1 ($\beta_1 = 0$) Rejected	Yes	Yes	Yes	Yes	Yes	Yes
H_2 ($\beta_1 = -1$) Rejected	No	No	No	No	No	Yes

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; ***p<0.01, **p<0.05, *p<0.1

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects. Sample period is 2014-2020.

Column D of Table 2 shows the results of the analysis including Province*year interaction effect instead of Corop*year interaction effects. The changes in the magnitude and significance of the coefficient of OZB rates are minor. The hypothesis of no capitalization can be rejected with 97.3% certainty. The hypothesis of full capitalization cannot be rejected.[12] The 95% confidence interval [-2.09, -0.13] again shows the magnitude of the capitalization effect is uncertain. Notably, the coefficients for disposable household income and number of company establishments are different from 0 at a 10% significance level, both variables have a positive relationship with house prices.

Column E of Table 2 shows the results of the analysis adopting a 2% discount rate. As predicted in the theoretical framework and model description, the coefficient of OZB rates decreases in magnitude. However, the coefficient remains significantly different from 0. The coefficient of -0.73 can be interpreted as follows: all else equal, a 1% increase in OZB rates is associated with a 0.73% decrease in house prices. The hypothesis of no capitalization can be rejected with 96.3% certainty. The hypothesis of full capitalization cannot be rejected.[13] The 95% confidence interval [-1.41, -0.045] again shows the magnitude of the capitalization effect is uncertain.

Column F of Table 2 shows the results of the analysis adopting a 3% discount rate. Again, the coefficient of OZB rates decreases in magnitude, but remains significantly different from 0. The coefficient of -0.39 can be interpreted as follows: all else equal, a 1% increase in OZB rates is associated with a 0.39% decrease in house prices. The hypothesis of no capitalization can be rejected with 96.4% certainty. Contrary to the other analysis, the hypothesis of full capitalization can be rejected with 99.9% certainty.[14] The 95% confidence interval [-0.75, -0.025] shows evidence of partial capitalization.

Tables A.4, A.5, A.6, and A.7 show the sensitivity of the estimates of columns A, D, E and F of Table 2 respectively to the inclusion/exclusion of local business cycle controls. All tables show that excluding controls for local business cycles, in particular population growth and the number of company establishments, increases the estimated coefficients of OZB rates.[15] Without local business controls, all OZB rate coefficients remain different from 0 at a 5% significance level. Conclusions regarding hypotheses are unchanged. The hypothesis of zero

[12] P-value 0.83
[13] P-value 0.44
[14] P-value 0.44
[15] Estimated coefficients of OZB rates of columns A, D, E, F of Table 2 without population growth and company estbalishments are: -1.27, -1.44, -0.87, -0.47 respectively. These results are reported in column B of tables A.4, A.5, A.6 and A.7

capitalization is rejected with at least 95% certainty in all specifications. The hypothesis of full capitalization is not rejected for the specifications in tables A.4, A.5 and A.6.[16] The hypothesis of full capitalization is rejected for all specifications in table A.7.[17] The estimated coefficient of the number of jobs is insignificant in all specifications; adding this variable does not change OZB coefficients and does not add explanatory power. This may be caused by an overlap between the number of jobs and population growth or unemployment rates. The estimated coefficient of public expenditures is highly insignificant in statistical and economical terms across all model specifications. However, this variable is an important control variable as including it decreases the magnitude and significance of the capitalization effect. Contrary to expectations, this means that omitting public services biases capitalization estimates upwards (in absolute terms). OZB proceeds are used to finance local public expenditures, so these variables have a positive relationship. The upward bias (in absolute terms) implies there is a negative relationship between public service expenditures and house prices.

Tables A.4 and A.5 further show the estimates of columns A and D of Table 2 respectively are robust to aggregating standard errors at Corop-region level. Confidence intervals increase slightly, but significance levels are unchanged. In all specifications, the null hypothesis of no capitalization is rejected with at least 95% certainty. The hypothesis of full capitalization cannot be rejected in any specification.[18] Table A.4 further shows that unemployment rates have a significant negative relationship with house prices when standard errors are clustered at Corop-region level. However, this relationship is not economically significant.[19] Table A.5 additionally shows population growth has a significant negative relationship with house prices when standard errors are clustered at Corop-region level. Although the negative direction of this coefficient is unexpected, the coefficient is not economically significant.[20]

The within R^2 of all model specifications lies between 0.93 and 0.95. This means all models explain a high share of the variation in house prices within municipalities.

[16] P-values without local business cycle controls are 0.60, 0.40, 0.71 respectively (column A Table A.4, A.5, A.6)

[17] P-value without local business cycle controls is 0.0053 (column A Table A.7)

[18] P-values with local business cycle controls are 0.90, 0.84 in Table A.4, Table A.5 respectively

[19] Coefficient of unemployment rate with local business cycle controls is -0.016, meaning a 1 percentage point increase in unemployment rates is associated with a 0.016% decrease in house prices (Table A.4 column F)

[20] Coefficient of population growth with local business cycle controls is -0.00056, meaning a 1 percentage point increase in the population growth is associated with a 0.00056% decrease in house prices (Table A.5 column F)

4.3 Supply constraints and heterogeneous effects

Table 3 reports the regression outcome of the model including interaction effects between OZB rates and supply constraints. Column A represents the baseline model specification with dummies for middle and low supply constraints. For high supply constrained municipalities, the reference category, the estimated OZB coefficient is different from 0 at a 10% significance level. The point estimate can be interpreted as follows: all else equal, a 1% increase in the OZB rate is associated with a 1.63% decrease in house prices. The hypothesis of zero capitalization can be rejected with 92.3% certainty. The hypothesis of full capitalization cannot be rejected.[21] Although there is a significant negative relationship between OZB rates and house prices, the 95% confidence interval [-3.42, 0.15] shows the magnitude of the capitalization effect is uncertain. For middle and low supply constrained municipalities, estimated coefficients are not significantly different from 0 and the hypothesis of no capitalization cannot be rejected.[22] This may suggest that, consistent with the theory on tax incidence, capitalization effects are driven by the elasticity of supply. However, the results need to be interpreted with caution as the interaction effects show that effects in medium and low supply constrained municipalities are not significantly different from the effect in high supply constrained municipalities.

Column B reports the regression outcome of the model with interaction effects between OZB rates and supply constraints when municipalities are treated as different municipalities pre- and post-merger. Compared to column C of Table 2, standard errors increase because the observations are split in three groups. Although none of the estimated coefficients is statistically significant, the coefficient of high supply constrained municipalities is only marginally insignificant. The hypothesis of full capitalization cannot be rejected for any level of supply constraints.[23] For high supply constrained municipalities, the hypothesis of no capitalization can be rejected with 87.3% certainty. For middle and low supply constrained municipalities, the hypothesis of no capitalization cannot be rejected.[24] Although this may indicate that capitalization effects are driven by supply side constraints, the interaction effects again show capitalization effects in medium and low supply constrained municipalities are not significantly different from the capitalization effect in high supply constrained municipalities.

---

[21] P-value 0.48
[22] P-values 0.63, 0.69 respectively
[23] P-values 0.76, 0.15, 0.88 for high, middle and low constraints respectively
[24] P-values 0.82, 0.21 respectively

Concluding, there is some evidence that supply constraints drive capitalization effects. However, estimated interaction effects have large standard errors and difference between municipalities with different levels of supply constraints are not statistically significant. The absence of significant heterogeneous effects casts doubt on earlier results.

Table 3
Heterogeneous treatment effects by supply side constraints

Outcome: ln(House Price)		
Model	(A)	(B)
Ln(OZB rate)	-1.63*	-1.25
	(0.91)	(0.81)
Ln(OZB rate)*Middle Supply Constraints	1.2	1.11
	(1.02)	(0.93)
Ln(OZB rate)*Low Supply Constraints	0.60	0.35
	(1.08)	(1.02)
Ln(Expenditures)	0.0046	0.0060
	(0.599)	(0.0084)
Ln(Household Income)	0.11	0.060
	(0.227)	(0.079)
Unemployment	-0.016	-0.0193
	(0.161)	(0.012)
Population growth	-0.00046	-0.00035
	(0.233)	(0.00040)
Ln(Companies)	0.15	0.459**
	(0.209)	(0.095)
Different Post-Merger Identity	No	Yes
Observations	2,388	2,388
R^2		
Within	0.94	0.95
Between	0.0002	0.00
Overall	0.0030	0.00011

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; ***p<0.01 **p<0.05, *p<0.1

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects and Corop*year interaction effects. Interest rate 3%. Sample period is 2014-2020.

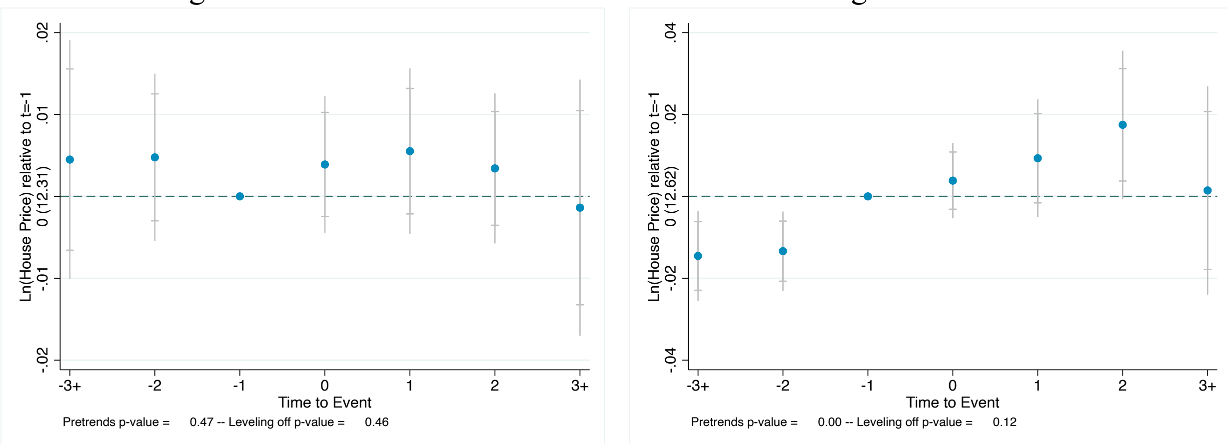
4.4 Dynamic effects

Figure 6 shows the results of the event studies of large tax changes in which municipalities with multiple large increases or decreases are not excluded and the event window is three periods before and after the tax change. Panel A shows that for large increases, none of the estimated coefficients is significantly different from 0. This means there are no significant differences between the period just before the tax increase ($t = -1$) and any other period in the event window. Moreover, there is no significant pre-trend (p-value 0.47) and the trend after the event is not significantly different from the trend before the event (p-value 0.48). Concluding, there are no significant dynamic effects for large tax increases.

Figure 6

Event studies of large tax changes (event window 3)

Panel A: Large tax increases Panel B: Large tax decreases



Notes: This figure illustrates the estimated treatment effect of a large change in the OZB rate on house prices (in logs) relative to the pre-form year. The underlying econometric model is described in equation 6. The specification controls for public expenditures, local business cycles, and year fixed effects (on national and regional levels). Observations that experience a large tax increase and decrease in the same event window are excluded. Vertical bars indicate 95% confidence intervals. Heteroskedasticity- and autocorrelation-consistent standard errors are adjusted for clustering at municipality level. Regression results are reported in Tables A.9 and A.10

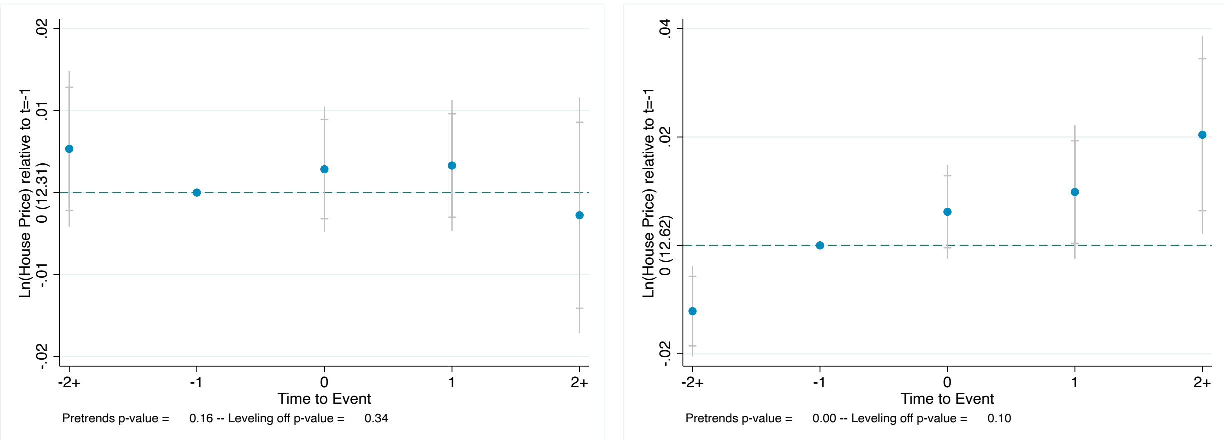
Panel B of Figure 6 shows that periods $t = -3, -2, 1, and\ 2$ are significantly different from $t = -1$. However, the figure shows a significant pre-trend (p-value 0.00) and shows the difference in trends before and after the event is not significant (p-value 0.12). The full sample is the same length as the event window. This means that in case an event occurs in the second half of the sample there is no observation at $t = 3$. The number of observations three periods away from the event is relatively small, which likely causes the wide confidence interval around the estimated coefficient at $t = 3$.

Figure 7 shows the results of the event studies in which municipalities with multiple large increases or decreases are not excluded and the event window consists of two periods before and after the tax change. For large tax increases, there are no significant differences between $(t = -1)$ and any other period. Moreover, there are still no significant pre-trends (p-value 0.12) and the trend before and after the event do not differ significantly (p-value 0.34). Meaning, there is no evidence of dynamic effects for large tax increases. For large decreases, the trend after the event is different from the trend before the event at a 10% significance level. This increased significance is likely caused by eliminating the insignificant coefficient at $t = 3$ and provides evidence of the presence of dynamic effects of large tax decreases.

Figure 7

Event studies of large tax changes (event window 2)

Panel A: Large tax increase Panel B: Large tax decrease



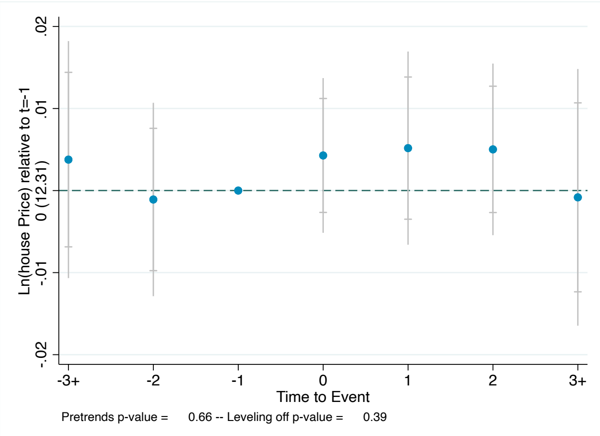
Notes: This figure illustrates the estimated treatment effect of a large change in the OZB rate on house prices (in logs) relative to the pre-form year. The underlying econometric model is described in equation 6. The specification controls for public expenditures, local business cycles, and year fixed effects (on national and regional levels). Observations that experience a large tax increase and decrease in the same event window are excluded. Vertical bars indicate 95% confidence intervals. Heteroskedasticity- and autocorrelation-consistent standard errors are adjusted for clustering at municipality level. Regression results are reported in Tables A.9 and A.10

Figure 8 shows the results of the event studies in which observations with multiple events within the same event window are excluded and the event window consists of three periods before and after the tax change. For large increases, still there are no significant differences between $(t = -1)$ and any other period. Again, this means there is no evidence of dynamic effects for large tax increases. For a large tax decrease, all coefficients are significantly different from 0, so all time periods differ significantly from $(t = -1)$. Although there is a significant pre-trend (p-value 0.02), the difference between trends before and after the event is highly significant (p-value 0.00). This provides evidence for the presence of dynamic effects of large tax decreases. However, this result needs to be interpreted with caution as it is may influenced by the negative coefficient at ($t = 3$).

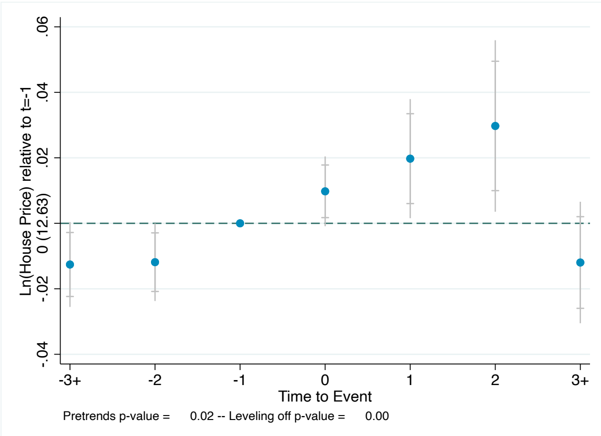
Figure 8

Event studies of large tax changes excluding multiple events in same window (event window 3)

Panel A: Large tax increase



Panel B: Large tax decrease



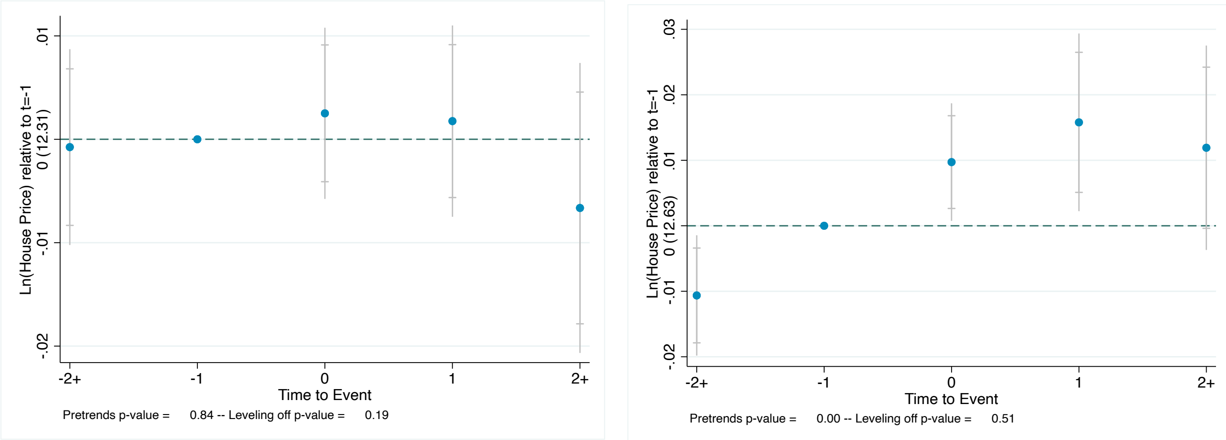
Notes: This figure illustrates the estimated treatment effect of a large change in the OZB rate on house prices (in logs) relative to the pre-form year. The underlying econometric model is described in equation 6. The specification controls for public expenditures, local business cycles, and year fixed effects (on national and regional levels). Observations that experience a large tax increase and decrease, or multiple large increases or decreases in the same event window are excluded. Vertical bars indicate 95% confidence intervals. Heteroskedasticity- and autocorrelation-consistent standard errors are adjusted for clustering at municipality level. Regression results are reported in Tables A.9 and A.10

Figure 9 shows the results of the event studies in which observations with multiple events within the same event window are excluded and the event window consists of two periods before and after the tax change. For large increases, there are still no significant differences between ($t = -1$) and any other period. Again, there is no evidence of dynamic effects for large tax increases. For a large tax decrease, all coefficients are significantly different from 0, so all time periods differ significantly from $t = -1$. However, there is a significant pre-trend and the difference between trends before and after the event is not significant (p-value 0.51). This means there is no significant evidence of dynamic effects of large tax decreases. Previous estimated dynamic effects are likely the result of multiple subsequent large decreases in combination with imprecise estimates caused by the small number of observations three periods away from the event time.

Figure 9

Event studies of large tax changes excluding multiple events in same window (event window 2)

Panel A: Large tax increase | Panel B: Large tax decrease



Notes: This figure illustrates the estimated treatment effect of a large change in the OZB rate on house prices (in logs) relative to the pre-form year. The underlying econometric model is described in equation 6. The specification controls for public expenditures, local business cycles, and year fixed effects (on national and regional levels). Observations that experience a large tax increase and decrease, or multiple large increases or decreases in the same event window are excluded. Vertical bars indicate 95% confidence intervals. Heteroskedasticity- and autocorrelation-consistent standard errors are adjusted for clustering at municipality level. Regression results are reported in Tables A.9 and A.10

None of the specifications shows evidence of dynamic effects of large increases in OZB rates on house prices. Although there is some evidence of dynamic effects of large decreases, this evidence is not robust to a simultaneous (i) change in the event window and (ii) exclusion of municipalities that experience multiple large tax decreases within the same event window. These results cast further doubt on the results of the two way fixed effects model.

5. Discussion

5.1 Policy Implications

The results of the two way fixed effects regressions show that the hypothesis of zero capitalization can be rejected at all employed specifications. The hypothesis of full capitalization can only be rejected when the discount rate is sufficiently high. There is a significant negative relationship between OZB rates and house prices. This result is robust to the inclusion/exclusion of control variables, the chosen discount rate, the geographical clustering of standard errors, and the inclusion of time fixed effects at different geographical levels. The results indicate at least partial capitalization, but the actual degree of capitalization is uncertain as estimated confidence intervals are wide. As derived in the theoretical framework the capitalization hypothesis has important policy implications. Partial capitalization implies that a tax increase is passed through to tenants at least to some degree and that a tax increase is not reflected fully in house prices. The lower the degree of capitalization, the higher the pass through on rents and the more likely property taxes are to have a regressive effect. Moreover, when partial capitalization occurs, current owners are able to pass on part of the burden to future owners. When local governments try to improve housing affordability by easing the tax burden, capitalization effects (partially) offset these measures. The results need to be interpreted with caution due to the endogeneity issues described in the rest of this chapter.

5.2 Simultaneity and reversed causality

The main issue regarding reliability of the estimates is that the estimated capitalization effect may in fact reflect the impact of house prices on OZB rates rather than the reversed. As explained in the theoretical framework, when reversed or simultaneous causality occurs, capitalization estimates are biased upwards (in absolute terms). The results of the heterogeneous and dynamic effects analyses cast doubt the results of the two way fixed effects regressions. The absence of significant heterogeneous effects related to supply elasticities, is inconsistent with the theory on tax incidence. Moreover, the event studies show no significant effects of large tax increases on house prices and show the effect of large decreases is not robust to a simultaneous (i) change in the event window and (ii) exclusion of municipalities that experience multiple large tax decreases within the same event window. Moreover, some municipalities are suspected to use property values to determine OZB rates or even state this in budget presentations. Excluding these municipalities does not impact estimated coefficients, which may indicate that other municipalities also display reversed causality. The results need to be interpreted with caution as capitalization estimates may overstate actual degrees of capitalization.

In case assessed property values (used to determine the tax liability) would not change over time, estimated capitalization effects would not be subject to reversed causality. This is not the case in the Netherlands, but may be used in a future research design in a different setting. Löffler and Siegloch (2021) extract exogenous variation in the tax variable by exploiting a specific feature of the German fiscal equalisation scheme. The German equalisation scheme is similar to the Dutch municipal fund allocation. The allocation of resources from both funds depend on the fiscal need relative to the fiscal capacity of each municipality. The fiscal capacity is determined in both cases by multiplying the property tax base with a standard factor. Löffler and Siegloch instrument relative differences between the new standard factor and the old local tax rate. Future research could investigate whether this set up can be used in the Dutch setting, where standard factors and OZB rates change yearly. This investigation is out of scope for this paper.

5.3 Measurement error and omitted variable bias

Property values are measured by the sales price of houses sold in a certain year. However, houses sold in a specific year do not necessarily give an accurate representation of the stock of houses in a municipality. Öztürk et al. (2018) estimate an annual house price index at municipality level using individual transaction data from the Dutch Association of Real Estate Brokers and Real Estate Experts to control for characteristics of sold houses and solve the measurement error. Unfortunately I do not have access to this data and therefore could not correct for property level characteristics. Although the measurement error in property values could cause increased standard errors, there is no reason to assume it is correlated with OZB rates and it therefore is unlikely to bias estimates.

Public service provision is measured by per capita municipal expenditures. However, as explained in the theoretical framework, this input measure needs to reflect public service output. In fact, there may not exist a perfect measure for the output of public services. The sensitivity analyses show that omitting per capita expenditures leads to an upward bias (in absolute terms) in capitalization estimates. However, due to the positive collinearity between public services and tax rates this upward bias could only occur if public expenditures have a negative relationship with house prices. A negative relationship between public service provision and house prices is unlikely: if two houses are equal in all aspects except public service provision, the house that receives more benefits from public services should have a value that is at least as high as the value of the other house. Therefore, it is likely that public expenditures are not a good measure of public service output. Previous studies have tried to overcome the potential downward bias and have adopted output measures related to education. However, most of these studies focus on the U.S. setting in which property tax rates are the main source of revenue and the largest single item on local public budget is education. Therefore, these measures are not likely to solve the measurement error. Omitted variable bias is less concerning in this paper, because even though omitting public services would bias capitalization estimated towards zero, the hypothesis of zero capitalization is rejected under all model specifications.

5.4 Selection bias

Some municipalities are only observed in part of the sample. In principle, without missing data, this does not raise any concerns. However, the public expenditure data is only defined for municipalities existing in the year 2019. In the year 2019, some new municipalities are formed. For these municipalities data exists pre-2019 as well, because a weighted average of former municipalities is taken for these time periods. However, individual data on these former municipalities is not available, meaning public expenditure data is missing for municipalities that merge and disappear in or before 2019. In this paper municipalities that are amended due to a merger are treated as a new municipality post-merger. An alternative way to treat these municipalities is employed by Öztürk et al. (2018) and Burgers (2017) and uses a weighted average for merged municipalities based on population sizes of pre-merged municipalities for the periods before the merge occurs. Employing this alternative measure would solve the missing data issue in the public expenditures variable and any possible sample selection bias associated with this. Moreover, it would solve the missing data issue for supply side constraints, as this variable is similarly only defined for municipalities existing in a specific identification year (2018). The weighted average approach is out of scope for the current paper, but it can be adopted in future research.

5.5 Event study design

Estimated dynamic effects are sensitive to the length of the event window. The number of observations 3 periods away from an event is relatively small and therefore estimated confidence intervals of these are large. This could be solved by employing a larger sample period. Because restrictions on the level of OZB rates have been lifted since 2014 and data on municipal expenditures and local business control variables is available until 2020, adopting a larger sample period was not possible for this study. Results could be re-estimated in the future with a larger sample period or could investigate whether data before 2014 could be used in the estimation as well.

The large number of tax changes in the Netherlands may influence reliability of the outcome of the event studies. In the German setting exploited by Fuest, Peichl and Siegloch (2021), 19% of the municipalities did not change the tax rate and half of the municipalities changed their tax rate only once or twice over the 20-year sample period. Contrastingly, in the Netherlands 100% of municipalities changed their tax rate each year in the period 2014-2020. The estimates of the event studies are sensitive to the inclusion/exclusion of observation that experience multiple large tax increases or decreases within the same event window. Before the exclusion of these observations, the analysis shows significant dynamic effects of large tax decreases regardless of the chosen event window. When the event window comprises two periods before and after the tax change and observations with multiple large changes are excluded, there are no significant dynamic effects. This could mean that observed dynamic effects are caused by accumulating effects of multiple large tax decreases within the same event window. Alternatively, absence of significant dynamic effects after excluding multiple changes in the same event window could be caused by the large decrease in number of events. Even though multiple large tax changes are excluded, all other observations still experience yearly smaller tax changes within the event window. This may bias estimated dynamic effects.

6. Conclusion

This paper investigates a panel of 420 Dutch municipalities over the sample period 2014-2020 with the aim of estimating the capitalization effect of property taxes into property values. The research question "to what extent is the Dutch local property tax capitalized into local house prices" is answered by performing a series of fixed effects regressions. House prices are modelled as the present value of future implicit rental rent values minus the tax liability. To account for omitted variables, municipal fixed effects, time fixed effects, and controls for public service provision and local business cycles are added to the estimation.

The hypothesis of zero capitalization can be rejected, while the hypothesis of full capitalization cannot be rejected as long as the chosen discount is sufficiently high. These results are robust to various sensitivity analyses. Under the baseline estimation, all else equal, a 1% increase in the property tax rate is associated with a decrease in property values by approximately 1.1%. The wide confidence interval of [-2.1, -0.07] shows the actual degree of capitalization may fall anywhere between partial and over-capitalization. The results need to be interpreted with caution as reversed causality may bias capitalization estimates towards 100%. Additional analyses show no significant heterogeneous effects related to supply elasticities and therefore are inconsistent with theory on tax incidence. Moreover, there is no evidence of dynamic effects of large tax changes when the event window comprises two periods before and after the event and observations with multiple events in the same event window are excluded. The absence of significant heterogeneous and dynamic effects casts doubt on the results of the two way fixed effects regressions.

Future research could solve issues regarding missing data and possible sample selection bias by adopting a weighted average approach after municipal mergers. Moreover, the use of a house price index as opposed to average sales prices, could increase the reliability of the estimated capitalization coefficient. Re-estimating the results over a longer sample period would allow for better estimation of dynamic effects. Most importantly, future research may extract exogenous variation in the property tax variable to establish the direction of causality or exploit a setting in which assessed property values are fixed over time to avoid reversed causation issues. In case property taxes in reality decrease house prices, further research may investigate the use of the property tax as an instrument to control house price fluctuations.

7. References

Borge, L. E., & Rattsø, J. (2014). Capitalization of property taxes in Norway. *Public Finance Review*, *42*(5), 635-661.

Burgers, S. (2017). Determinants of regional house price dynamics: the role of supply constraints and sales activity. *University of Amsterdam Thesis.*

Caldera, A., & Johansson, Å. (2013). The price responsiveness of housing supply in OECD countries. *Journal of Housing Economics*, *22*(3), 231-249.

CBS. (2019, December 1). *Gemeenten heffen in 2019 meer dan 10 miljard euro.* Centraal Bureau Statistiek Nederland. Retrieved July 17, 2022 from: https://www.cbs.nl/nl-nl/nieuws/2019/05/gemeenten-heffen-in-2019-meer-dan-10-miljard-euro

CBS. (2022, April 12). *Dutch house price increase among EU top five.* Central Bureau of Statistics Netherlands. Retrieved June 18, 2022 from: https://www.cbs.nl/en-gb/news/2022/15/dutch-house-price-increase-among-eu-top-five

Chinloy, P. (1978). Effective property taxes and tax capitalization. *The Canadian Journal of Economics/Revue canadienne d'Economique*, *11*(4), 740-750.

Church, A. M. (1974). Capitalization of the effective property tax rate on single family residences. *National Tax Journal*, *27*(1), 113-122.

Dusansky, R., Ingber, M., & Karatjas, N. (1981). The impact of property taxation on housing values and rents. *Journal of Urban Economics*, *10*(2), 240-255.

Findo. (n.d.) *Data Financiën Centrale Overheden*. Rijksoverheid. Retreived May 31, 2022, from: https://findo.nl/jive?cat_open=jaarrekening

Fuest, C., Peichl, A., & Siegloch, S. (2018). Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany. *American Economic Review*, *108 (2),* 393-418.

Gallagher, R. M., Kurban, H., & Persky, J. J. (2013). Small homes, public schools, and property tax capitalization. *Regional Science and Urban Economics*, *43*(2), 422-428.

Gemeente Amsterdam (2021, Sept. 3). *Begroting 2022.* Gemeente Amsterdam. Retrieved on 09/06/2022 from: https://www.amsterdam.nl/bestuur-organisatie/financien/begroting-2022/

Heinberg, J. D., & Oates, W. E. (1970). The incidence of differential property taxes on urban housing: A comment and some further evidence. *National Tax Journal*, *23*(1), 92-98.

Hilber, C. A. (2011). The Economic implications of house price capitalization: A survey of an emerging literature. *Spatial Economics Research Centre Discussion Paper*, (91).

Hilber, C. A., & Vermeulen, W. (2016). The impact of supply constraints on house prices in England. *The Economic Journal*, *126*(591), 358-405.

King, A. T. (1977). Estimating property tax capitalization: A critical comment. *Journal of Political Economy*, *85*(2), 425-431.

Langenberg, H. & Jonkers, W. (2022, March 11). *Achtergrond bij de huizenprijsstijging vanaf 2013.* Centraal Bureau Statistiek Nederland. Retrieved June 18, 2022, from: https://www.cbs.nl/nl-nl/longread/de-nederlandse-economie/2022/achtergrond-bij-de-huizenprijsstijgingen-vanaf-2013

Löffler, M., & Siegloch, S. (2021). Welfare effects of property taxation. *ZEW-Centre for European Economic Research Discussion Paper*, (21-026).

McDougall, G. S. (1976). Local public goods and residential property values: Some insights and extensions. *National Tax Journal*, *29*(4), 436-447.

Meadows, G. R. (1976). Taxes, spending, and property values: A comment and further results. *Journal of Political Economy*, *84*(4, Part 1), 869-880.

Ministerie van Financiën. (n.d.) *Inkomstenbronnen van gemeenten.* Ministerie van Financiën. Retreived May 31, 2022, from https://www.rijksfinancien.nl/memorie-van-toelichting/2021/OWB/B/onderdeel/630958

Oates, W. E. (1969). The effects of property taxes and local public spending on property values: An empirical study of tax capitalization and the Tiebout hypothesis. *Journal of political economy*, *77*(6), 957-971.

Oates, W. E. (1973). The effects of property taxes and local public spending on property values: A reply and yet further results. *Journal of Political Economy*, *81*(4), 1004-1008.

Öztürk, B., van Dijk, D., van Hoenselaar, F., & Burgers, S. (2018). *The relation between supply constraints and house price dynamics in the Netherlands Bank* (Working Paper No. 601). De Nederlandse Bank.

Palmon, O., & Smith, B. A. (1998). New evidence on property tax capitalization. *Journal of Political Economy*, *106*(5), 1099-1111.

Richardson, D. H., & Thalheimer, R. (1981). Measuring the extent of property tax capitalization for single family residences. *Southern Economic Journal*, 674-689.

Rosen, H. S., & Fullerton, D. J. (1977). A note on local tax rates, public benefit levels, and property values. *Journal of Political Economy*, *85*(2), 433-440.

Rosen, H. S., & Gayer, T. (2008). *Public finance*. *Boston, Mass*.

Rosen, K. T. (1982). The impact of Proposition 13 on house prices in Northern California: A test of the interjurisdictional capitalization hypothesis. *Journal of Political Economy*, *90*(1), 191-200.

Saiz, A. (2010). The geographic determinants of housing supply. *The Quarterly Journal of Economics*, *125*(3), 1253-1296.

Smith, R. S. (1970). Property tax capitalization in San Francisco. *National Tax Journal*, *23*(2), 177-193.

Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of political economy*, *64*(5), 416-424.

Urban Institute. (n.d.) *State and Local Expenditures*. The Urban Institute. Retrieved May 31, 2022, from https://www.urban.org/policy-centers/cross-center-initiatives/state-and-local-finance-initiative/state-and-local-backgrounders/state-and-local-expenditures

Vermeulen, W., & Rouwendal, J. (2007). Housing supply and land use regulation in the Netherlands.

Yinger, J., Bloom, H.S., Borsch-Supan, A. & Ladd, H.F. (1988). *Property taxes and house values: The theory and estimation of intrajurisdictional property tax capitalization.* Boston etc.: Academic Press.

Appendix

Table A.1

Municipal Mergers between 2014 and 2020

Date	*Former Municipality (64)*	*New Municipality (17)*	*Amended Municipality (10)*
1/1/15	Graft-De Rijp, Schermer		Alkmaar
	Millingen aan de Rijn, Ubbergen, Groesbeek	Berg en Dal	
	Maasdonk		‘s-Hertogenboss, Oss
	Bergambacht, Nederlek, Ouderkerk, Schoonhoven, Vlist	Krimpenerwaard	
	Bernisse, Spijkenisse	Nissewaard	
1/1/16	Zeevang		Edam-Volendam
	Bussum, Muiden, Naarden	Gooise Meren	
1/1/17	Schijndel, Sint-Oedenrode, Veghel	Meierijstad	
1/1/18	Leeuwarderadeel, Littenseradeel*		Leeuwarden,
	Hoogezand-Sappemeer, Menterwolde , Slochteren	Midden-Groningen	
	Franekeradeel, het Bildt , Menaldumadeel, Littenseradeel*	Waadhoeke	
	Littenseradeel*		Súdwest-Fryslân
		Westerwolde	
	Rijnwaarden		Zevenaar
1/1/19	Dongeradeel, Ferwerderadiel, Kollumerland en Nieuwkruisland	Noardeast-Fryslân	
	Geldermalsen, Lingewaal, Neerijnen	West Betuwe	
	Haren, ten Boer		Groningen
	Bedum, De Marne, Eemsmond, Winsum*	Het Hogeland	
	Grootegast, Leek, Marum, Zuidhorn, Winsum*	Westerkwartier	
	Nuth, Onderbanken, Schinnen	Beekdaelen	
	Aalburg, Werkendam, Woudrichem	Altena	
	Haarlemmerlied, Spaarnwoude		Haarlemmermeer
	Leerdam, Zederik, Vianen	Vijfheerenlanden	
	Binnenmaas, Cromstrijen, Korendijk, Oud-Beijerland, Strijen	Hoekschewaard	
	Giessenlanden, Molenwaard	Molenlanden	
	Noordwijkerhout		Noordwijk

*Municipality was split into multiple new and existing municipalities

Figure A.1

House price trends between 2014 and 2020

Panel A: Average House Price Panel B: House Price Index

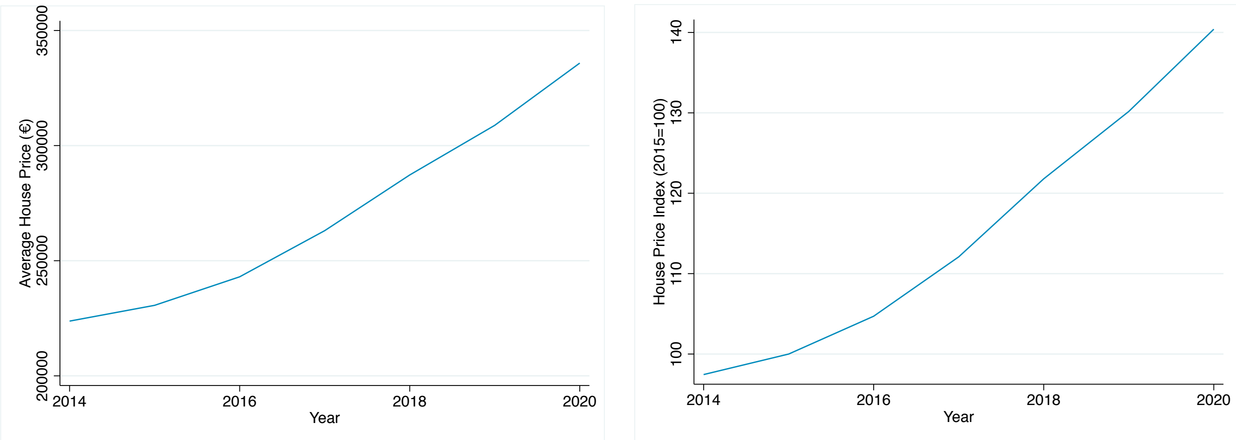


Table A.2

Mean reversion and predictability of OZB rates

Outcome: OZB Rate$_t$	
Variable	
OZB Rate$_{t-1}$	-0.29***
	(0.038)
OZB Rate$_{t-2}$	-0.17***
	(0.045)
OZB Rate$_{t-3}$	0.049
	(0.031)
R^2	
Within	0.073
Between	0.53
Overall	0.069
Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; ***p<0.01	
Note: Model includes municipal fixed effects	

Table A.3

Descriptive Statistics

Variable	**Obsv.**	**Mean**	**Std.dev.**	**Min.**	**Max.**
OZB rate (in%)	2,664	0.13	0.032	0.036	0.27
Average House Price (in €)	2,664	268,642	82,485.85	119,488	902,214
Expenditures per Capita (in €)	2,411	3,209.99	1106.54	257	18,426
Average Disposable Household Income (in €)	2,658	45,227.46	6,990.65	31,300	109,500
Unemployment Rate (in %)	2,664	1.73	0.55	0.24	4.19
Population Growth (in %)	2,644	4.39	7.60	-33.4	62.6
Jobs	2,664	21,413.44	46,395	400	654900
Companies	2,664	4015.85	7783.94	115	141080
Share of Developed Land (in %)	381	21.59	15.75	2.87	76.89

Table A.4

Results model with main specification and various control variables

Outcome: ln(House Price)						
Model	(A)	(B)	(C)	(D)	(E)	(F)
Ln(OZB rate)	-1.55******	-1.28****	-1.27****	-1.23****	-1.23****	-1.07****
	(0.50)	(0.52)	(0.52)	(0.52)	(0.52)	(0.51)
	(0.54)	(0.56)	(0.57)	(0.59)	(0.59)	(0.54)
Ln(Expenditures)	-	0.0047	0.0034	0.0036	0.036	0.0040
		(0.0094)	(0.0092)	(0.0089)	(0.0088)	(0.087)
		(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
Ln(Household Income)	-	-	0.13	0.13	0.13	0.099
			(0.090)	(0.088)	(0.088)	(0.087)
			(0.10)	(0.098)	(0.098)	(0.092)
Unemployment	-	-	-0.017**	-0.016*	-0.016*	-0.016*
			(0.011)	(0.011)	(0.011)	(0.011)
			(0.0039)	(0.0082)	(0.0082)	(0.0081)
Population growth	-	-	-	-.00047	-0.00047	-0.00046
				(0.00038)	(0.00038)	(0.00038)
				(0.00033)	(0.00033)	(0.00034)
Ln(jobs)	-	-	-	-	0.0049	-
					(0.00080)	
					(0.026)	
Ln(Companies)	-	-	-	-	-	0.16
						(0.12)
						(0.13)
Observations	2.644	2,411	2,405	2,405	2,405	2,405
R^2						
Within	0.94	0.94	0.94	0.94	0.94	0.94
	0.94	0.94	0.94	0.94	0.94	0.94
Between	00.49	0.00	0.12	0.11	0.094	0.0032
	0.10	0.046	0.30	0.21	0.17	0.014
Overall	0.27	0.25	0.34	0.33	0.33	0.17
	0.31	0.31	0.44	0.40	0.38	0.20

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; ***p<0.01 **p<0.05, *p<0.1. Standard errors are alternatively clustered at Corop-region level; ***p<0.01 **p<0.05, *p<0.1.

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects and Corop*year interaction effects. Interest rate 3%. Sample period is 2014-2020.

Table A.5

Sensitivity analysis local shocks at province level

Outcome: ln(House Price)						
Model	(A)	(B)	(C)	(D)	(E)	(F)
Ln(OZB rate)	-1.62******	-1.44*****	-1.44*****	-1.36****	-1.36****	-1.11****
	(0.51)	(0.53)	(0.52)	(0.53)	(0.53)	(0.50)
	(0.56)	(0.60)	(0.61)	(0.62)	(0.62)	(0.52)
Ln(Expenditures)	-	-0.0019	-0.0035	-0.0030	-0.0029	0.000041
		(0.0093)	(0.0090)	(0.0087)	(0.0086)	(0.0082)
		(0.010)	(0.011)	(0.010)	(0.010)	(0.0090)
Ln(Household Income)	-	-	0.19****	0.18****	0.18****	0.14**
			(0.089)	(0.086)	(0.086)	(0.085)
			(0.087)	(0.086)	(0.086)	(0.079)
Unemployment	-	-	-0.0089	-0.073	-0.073	-0.0095
			(0.0097)	(0.0097)	(0.0097)	(0.010)
			(0.0086)	(0.0085)	(0.0084)	(0.0083)
Population growth	-	-	-	-.00059*	-0.00058*	-0.00056*
				(0.00037)	(0.00038)	(0.00039)
				(0.00029)	(0.00029)	(0.00032)
Ln(jobs)	-	-	-	-	0.0094	-
					(0.025)	
					(0.023)	
Ln(Companies)	-	-	-	-	-	0.23**
						(0.13)
						(0.14)
Observations	2,664	2,411	2,405	2,405	2,405	2,405
R^2						
Within	0.93	0.93	0.94	0.94	0.94	0.94
	0.93	0.93	0.94	0.94	0.94	0.94
Between	0.051	0.025	0.27	0.24	0.21	0.0038
	0.089	0.13	0.35	0.21	0.18	0.0026
Overall	0.28	0.26	0.40	0.38	0.37	0.12
	0.30	0.34	0.43	0.38	0.36	0.12

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; ***p<0.01 **p<0.05, *p<0.1. Standard errors are alternatively clustered at Corop-region level; ***p<0.01 **p<0.05, *p<0.1.

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects and Province*year interaction effects. Interest rate 3%. Sample period is 2014-2020.

Table A.6
Sensitivity analysis interest rate 2%

Outcome: ln(House Price)						
Model	(A)	(B)	(C)	(D)	(E)	(F)
Ln(OZB rate)	-1.06***	-0.87**	-0.87**	-0.84**	-0.84**	-0.73**
	(0.34)	(0.36)	(0.35)	(0.36)	(0.36)	(0.35)
Ln(Expenditures)	-	0.0047	0.0034	0.0036	0.0036	0.0040
		(0.0094)	(0.0092)	(0.0089)	(0.0088)	(0.0087)
Ln(Household Income)	-	-	0.13	0.13	0.13	0.099
			(0.090)	(0.088)	(0.088)	(0.087)
Unemployment	-	-	-0.017	-0.016	-0.016	-0.016
			(0.011)	(0.011)	(0.011)	(0.011)
Population growth	-	-	-	-0.00047	-0.00047	-0.00046
				(0.00038)	(0.00038)	(0.00038)
Ln(jobs)	-	-	-	-	0.0049	-
					(0.025)	
Ln(Companies)	-	-	-	-	-	0.16
						(0.12)
Observations	2,664	2,411	2,405	2,405	2,405	2,405
R^2						
Within	0.94	0.94	0.94	0.94	0.94	0.94
Between	0.049	0.0003	0.12	0.11	0.095	0.0032
Overall	0.27	0.25	0.34	0.33	0.33	0.17

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; **p<0.05

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects and Corop*year interaction effects. Interest rate 5%. Sample period is 2014-2020.

Table A.7
Sensitivity analysis interest rate 1%

Outcome: ln(House Price)						
Model	(A)	(B)	(C)	(D)	(E)	(F)
Ln(OZB rate)	-0.57***	-0.47**	-0.47**	-0.45**	-0.45**	-0.39**
	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)
Ln(Expenditures)	-	0.0047	0.0034	0.0036	0.0036	0.0040
		(0.0094)	(0.0092)	(0.0089)	(0.0088)	(0.0087)
Ln(Household Income)	-	-	0.13	0.13	0.13	0.099
			(0.090)	(0.088)	(0.088)	(0.087)
Unemployment	-	-	-0.017	-0.016	-0.016	-0.016
			(0.011)	(0.011)	(0.011)	(0.011)
Population growth	-	-	-	-0.00047	-0.00047	-0.00046
				(0.00038)	(0.00038)	(0.00038)
Ln(jobs)	-	-	-	-	0.0049	-
					(0.025)	
Ln(Companies)	-	-	-	-	-	0.16
						(0.12)
Observations	2,664	2,411	2,405	2,405	2,405	2,405
R^2						
Within	0.94	0.94	0.94	0.94	0.94	0.94
Between	0.050	0.0002	0.13	0.11	0.096	0.0033
Overall	0.27	0.25	0.34	0.33	0.33	0.17

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; **p<0.05

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects and Corop-region*year interaction effects. Interest rate 6.5%. Sample period is 2014-2020.

Table A.9
Event studies of large tax increases

Outcome: ln(House Price)				
Model	(A)	(B)	(C)	(D)
Time to Event (relative to period -1)				
-3	0.0045	0.0052	-	-
	(0.0056)	(0.0050)		
-2	0.0048	0.0017	0.0053	-0.00076
	(0.0056)	(0.0040)	(0.0038)	(0.0039)
0	0.0039	0.0023	0.0028	0.0025
	(0.0032)	(0.0033)	(0.0031)	(0.0034)
1	0.0055	0.0036	0.0033	0.0018
	(0.0039)	(0.0042)	(0.0032)	(0.0038)
2	0.0034	0.0041	-0.0028	-0.0066
	(0.0035)	(0.0037)	(0.0058)	(0.0057)
3	-0.0014	0.0017	-	-
	(0.0060)	(0.0055)		
Ln(Expenditures)	0.0034	0.0051	0.0032	0.0052
	(0.0081)	(0.0081)	(0.0081)	(0.0082)
Ln(Household Income)	0.089	0.083	0.086	0.076
	(0.063)	(0.063)	(0.063)	(0.063)
Unemployment	-0.015**	-0.014**	-0.015**	-0.014**
	(0.0070)	(0.0071)	(0.0070)	(0.071)
Population growth	-0.00049***	-0.00048***	-0.00049***	-0.00048***
	(0.00017)	(0.00017)	(0.00016)	(0.00017)
Ln(Companies)	0.17***	0.17***	0.17***	0.17***
	(0.041)	(0.041)	(0.041)	(0.042)
Multiple Changes Excluded	No	Yes	No	Yes
Observations	2,385	2,365	2,385	2,345
R^2	0.98	0.98	0.98	0.98
Adjusted R^2	0.98	0.98	0.98	0.98

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; ***p<0.01, **p<0.05, *p<0.10

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects and Corop-region*year interaction effects. Sample period is 2014-2020. Observations that experience a tax decrease within the event window are excluded.

Table A.10
Event studies of large tax decreases

Outcome: ln(House Price)				
Model	(A)	(B)	(C)	(D)
Time to Event (relative to period -1)				
-3	-0.015***	-0.013**	-	-
	(0.0043)	(0.0050)		
-2	-0.013***	-0.012***	-0.012***	-0.011***
	(0.0037)	(0.0046)	(0.0033)	(0.0037)
0	0.0038	0.0098**	0.0062*	0.0097***
	(0.0037)	(0.0041)	(0.0034)	(0.0036)
1	0.0093*	0.020***	0.0098	0.016***
	(0.0056)	(0.0070)	(0.0048)	(0.0055)
2	0.017**	0.030***	0.020	0.012*
	(0.0070)	(0.010)	(0.0072)	(0.0063)
3	0.0014	-0.0012*	-	-
	(0.0098)	(0.0071)		
Ln(Expenditures)	0.0047	0.0045	0.0049	0.0042
	(0.0081)	(0.0082)	(0.0081)	(0.0082)
Ln(Household Income)	0.096	0.058	0.083	0.060
	(0.062)	(0.064)	(0.062)	(0.0064)
Unemployment	-0.016**	-0.016**	-0.017**	-0.015**
	(0.0070)	(0.0070)	(0.048)	(0.0071)
Population growth	-0.00051***	-0.00050***	-0.00050***	-0.00051***
	(0.00016)	(0.00017)	(0.00016)	(0.00017)
Ln(Companies)	0.15***	0.14***	0.15***	0.16***
	(0.042)	(0.042)	(0.041)	(0.042)
Multiple Changes Excluded	No	Yes	No	Yes
Observations	2,381	2,297	2,381	2,297
R^2	0.98	0.98	0.98	0.98
Adjusted R^2	0.98	0.98	0.98	0.98

Heteroskedasticity- and autocorrelation-consistent standard errors (in parenthesis) are adjusted for clustering at municipality level; ***p<0.01, **p<0.05, *p<0.10

Notes: Only coefficients of the main variables are reported. All models include municipal and time fixed effects and Corop-region*year interaction effects. Sample period is 2014-2020. Observations that experience a tax increase within the event window are excluded.