

# Property Tax Capitalization: Evidence from a Reform in Portugal\*

*PRELIMINARY AND INCOMPLETE: PLEASE DO NOT QUOTE*

Catarina Alvarez<sup>†1</sup> and João Pereira dos Santos<sup>‡1</sup>

<sup>1</sup>Nova School of Business and Economics

December, 2019

## Abstract

Tax capitalization theory predicts that a lower property tax payment leads to higher housing prices. The intuition is that if a property tax reduction is viewed by the buyers as a decrease in the cost of living, they will be willing to pay higher prices. However, empirical evidence is scarce as one needs to consider crucial endogeneity concerns when testing this hypothesis. We tackle these issues by exploiting a quasi-natural experiment: an unexpected reduction in the upper bound of the Portuguese property tax rate for urban real estate in 2008. We rely on a rich dataset on all mainland municipalities that includes demographic, economic, and political variables for 2005–2011 to implement a difference-in-differences approach. We find that municipalities forced to decrease the property tax rate experimented a 5 to 7% increment in mean real estate values *vis-à-vis* the comparison group. These findings support the hypothesis of property tax capitalization and hold both in a sample including all mainland municipalities and in a restricted version with more homogenous municipalities.

---

\*The authors are most grateful to Susana Peralta and José Tavares for invaluable support and guidance. We thank Teemu Lyytikäinen, Clemens Fuest, Andreas Peichl, Sebastian Seigloch, Henry Overman, Gilles Duranton, Miguel Ferreira, Dirk Foremny, Gabriel Ahlfeldt, Momi Daham, Irem Demirci, Sofia Franco, Pedro Portugal, Ana Venâncio, Thierry Madiès and seminar participants at Nova SBE, ifo Institute Macro Seminar, 13<sup>th</sup> Meeting of the Urban Economics Association, 74<sup>th</sup> Annual Congress of the International Institute of Public Finance, 12<sup>th</sup> Annual Meeting of the Portuguese Economic Journal, 8<sup>th</sup> European Meeting of the Urban Economics Association, 2018 ZEW Public Finance Conference, 2018 LSE SERC Annual Conference, IEB/ UEA Summer School, 15<sup>th</sup> PEARL Workshop, Swiss Distance Learning University and Zermatt for their insightful comments. We thank DGEEC and technical assistance by Pedro Freitas and Luís Catela Nunes who generously shared the data on national exams we use in the paper. All errors are our own. Catarina Alvarez gratefully acknowledges financial support by FCT – Fundação para a Ciência e Tecnologia - SFRH/BD/131804/2017. João Pereira dos Santos gratefully acknowledges financial support by FCT – Fundação para a Ciência e Tecnologia - PD/BD/128121/2016.

<sup>†</sup>Nova School of Business and Economics, Campus de Carcavelos, Rua da Holanda, n.1, 2775-405 Carcavelos (Portugal).  
Email: [Catarina.alvarez.2016@novasbe.pt](mailto:Catarina.alvarez.2016@novasbe.pt)

<sup>‡</sup>Nova School of Business and Economics, Campus de Carcavelos, Rua da Holanda, n.1, 2775-405 Carcavelos (Portugal).  
Email: [joao.santos@novasbe.pt](mailto:joao.santos@novasbe.pt)

# 1 Introduction

The question of whether property taxes are capitalized into real estate market values is an old topic in public economics.<sup>1</sup> The answer has scholar but also practical relevance since property taxes affect both individual and government budgets and may, therefore, have real efficiency and equity consequences.<sup>2</sup> Nevertheless, a clear answer to this question has been proven challenging, mainly due to serious empirical concerns.

From a theoretical point of view, the property tax capitalization is based on the assumption that both house characteristics and factors affecting the cost of living determine the net present value a prospective buyer is willing to pay for a house [Oates (1969) and Yinger (1982)]. Therefore, if a property tax reduction is viewed by the buyers as a decrease in the cost of living, they will be willing to pay higher prices. Taking the supply of land and housing as fixed, lower present and future tax payments are then expected to inflate the market value of real estate.

Empirically, there are two main identification issues that must be taken into account. First, the level of public goods is positively correlated with the property tax level, and both independently affect the house prices. For this reason, it is difficult to isolate the effect of the tax separately. Second, there is a likely simultaneity bias between the property tax rate and house prices when local governments set their own tax rate: areas with a high house price level, all else equal, are able to set a lower tax rate to collect a certain amount of tax revenues.

In this paper, we tackle these issues by exploiting a quasi-natural experiment: an unexpected reduction in the upper bound of the Portuguese property tax rate for urban real estate announced by the Prime-Minister on July 2, 2008. This reform allows to divide mainland municipalities in treated (those that were forced to decrease their tax rate) and comparison municipalities. Under the assumption of parallel trends in housing transaction developments of the two groups, the difference-in-differences setting estimates the causal effect of this reform on real estate values. In Portugal, the property tax is set by the municipality, on a tax

---

<sup>1</sup>For a survey on the broader implications of house price capitalization see Hilber (2015).

<sup>2</sup>For example, recent micro contributions show that changes in house prices may have implications for aggregate consumption (Mian et al., 2013).

range defined by the central government. The value of the tax base (i.e., the fiscal value of property) is also decided centrally. We take advantage of a comprehensive dataset based on a single country where all local governments operate under the same institutional framework. This dataset includes socioeconomic, fiscal, and political variables on all mainland Portuguese municipalities from 2005 to 2011.

We find that affected municipalities observed an increase in the mean transaction value of urban real estate *vis-à-vis* the comparison group. Thus, supporting the hypothesis of property tax capitalization. In other words, our findings suggest that buyers take into consideration the lower costs of owning the house which are reflected in a market price increase given the net present value of the tax reduction. Throughout the paper, we contrast these findings in a sample including all mainland municipalities and in a restricted version with more homogenous municipalities. Our conclusions are robust to several specification checks and falsification tests that are performed to dismiss selection bias concerns and alternative mechanisms. In particular, we observe no significant impact on local expenditures, suggesting that there were not differences in public good provision due to the reform allowing us to also rule out the other critical identification issue mentioned above. Furthermore, the property tax law creates an interesting natural counterfactual since rural real estate is subject to a different tax regime that suffered no changes. Unsurprisingly, we find no effects of the reform on this particular outcome. Furthermore, we observe no significant impact on local expenditures, suggesting that there were not potential differences in public good provision due to the reform allowing us to also rule out the other critical identification issue mentioned above.

Several recent empirical studies have focused on the impact of taxation on the housing market, namely the effect of transaction taxes [Dachis et al. (2012), Besley et al. (2014), Kopczuk and Munroe (2015), Hilber and Lyytikäinen (2017), Slemrod et al. (2017), Best and Kleven (2018)] and income taxes (Basten et al., 2017). Nevertheless, the empirical literature on property taxation is still scarce due to the endogeneity concerns previously mentioned. Most early work on property taxation supports a full or partial capitalization in house prices [for a survey, see Ross and Yinger (1999), Sirmans et al. (2008) or Hilber et al. (2011)]. These studies are mainly focused in a local government context with cross-sectional variation

in property tax rates. However, despite the fact that both identification issues previously described have been known and discussed since the seminal paper by Oates (1969), few studies have successfully dealt with them. For instance, Cushing (1984) and Palmon and Smith (1998) solve the simultaneity and measurement problems by estimating capitalization in a context where the property tax rate is unrelated to local public services and find support for full capitalization. However, both analysis are based on limited datasets.

More recently, Elinder and Persson (2017) focus in a national property tax reform in Sweden, thus unrelated to local public goods, providing good identification of a large property tax cut on house prices. In this setting, the authors find no evidence that the tax reduction led to increases in house prices with the exception of a small segment of the market containing properties with very high tax values. Bradley (2017) exploits temporary idiosyncratic differences accruing to new homebuyers in the Michigan property tax system to study the degree to which these initial tax obligations are capitalized. The idea is to assess whether households recognize intertemporal discontinuities in property tax bases and obligations. The author finds that homebuyers overcompensate sellers of homes with relatively low tax obligations, as if such rules would persist indefinitely. Furthermore, Oliviero and Scognamiglio (2019) exploit an Italian tax reform in 2012, instrumenting the variation in the level of the property tax rates by the exogenous staggered timing of local elections, and they show that a one standard deviation increase in municipal-level property tax intensity leads to a 2.7% reduction of municipal property values in the year of the reform.

The remainder of this chapter is structured as follows. In the next section we describe some institutional details, namely regarding the Portuguese local finance and the property tax reform of 2008. Section 2.3 presents the data used and explains the identification strategy followed. In sections 2.4 and 2.5 we discuss the main results and several robustness checks, respectively. Section 2.6 concludes.

## 2 Institutional Background

### 2.1 Portuguese Local Finance

Portuguese local governments have control over their spending – subject to common laws and regulations – but they do not score very high in terms of local revenue autonomy (OECD, 1999). The reason for the reduced local autonomy is twofold: high reliance on transfers from the central government and reduced freedom to set local tax rates (on centrally set tax bases). Local revenue sources consist essentially of local taxes, transfers from the central and regional governments, and transfers from the EU. The main local taxes are an indirect tax on the transfer of real estate (*Imposto Municipal sobre as Transmissões Onerosas de Imóveis*, IMT), the local property tax (*Imposto Municipal sobre Imóveis*, IMI), a variable tax share of the central government personal income tax (*Imposto sobre o Rendimento de pessoas Singulares*, IRS), and a municipal surcharge on corporate income tax (*Derrama*). Transfers from central government and EU and the revenues of the property tax (IMI) present the highest shares, both significantly greater than the share of any other local source of revenue.

The current Portuguese property tax, IMI, was introduced in 2003 as a result of a general reform of the Portuguese tax system. Local authorities have some discretionary power over the property tax and every year they set tax rate within an interval common to every municipality. Local property tax rates are set within a range which is defined by the central government, as displayed in Table 1. This table displays the lower and upper bound of property tax rates for both urban and rural real estate, before and after the reform.

Table 1: **Property Tax Rates: Minimum and Maximum Values**

Year	Urban		Rural
	Min	Max	
2003-2007	0.20%	0.50%	0.80%
2008-2011	0.20%	0.40%	0.80%

*Source:* Portuguese tax authority.

In order to better understand the consequences of the reform, it is important to discern the differences between the legal definitions of urban and rural real estate in Portugal. According to the Portuguese property tax code (CIMI<sup>3</sup>) the characterization of the real estate depends on its use. It is defined as urban all residential, commercial or industrial real estate and any land already approved for construction. In contrast, it is considered a rural real estate when it does not meet the criteria of an urban real estate, namely those without any construction in place or approved, waters, plantations or when used for agricultural purposes.

## 2.2 Property Tax Reform of 2008

On July 2, 2008, the Portuguese Prime Minister unexpectedly announced a reform forcing the property tax rate upper bound to decrease from 0.5% to 0.4% for urban real estate, as we can observe above in Table 1. The politician promised that this would benefit *hundreds of thousands of real estate owners*.<sup>4</sup> This surprise announcement caused an immediate reaction by the President of the Mayor's Association (*Associação Nacional de Municípios*) who accused the central government of *easing the taxpayers' fiscal burden at the expense of someone else's money*. This representative forecasted the impact at 12.5% of total revenues.<sup>5</sup> All this significantly amplified the media attention devoted to the reform.

The reform allows us to divide the municipalities in mainland Portugal in two groups.<sup>6</sup> One group includes the 94 municipalities setting a tax rate higher than 0.4% before the reform, i.e. those that were forced to decrease it. This is our treatment group and all other municipalities compose the comparison group. In appendix B.1, figure 5 (a) depicts the spatial distribution of these two groups. For robustness, we contrast all estimation results using the full sample of mainland municipalities with a more restricted version where we focus on municipalities with a more homogeneous choice of property tax rates in 2007, the year before the shock. In order to keep intervals of similar length, we remove municipalities

---

<sup>3</sup>*Código do Imposto Municipal sobre Imóveis* approved by Decree-Law no. 287/2003, of 12 November.

<sup>4</sup>Headline in *Expresso*, an important Portuguese newspaper.

<sup>5</sup>See the news articles in *Público* newspaper and in *TVI* website.

<sup>6</sup>Peralta and Pereira dos Santos (2019) use a similar empirical strategy to study the impact of a tax revenue cut on the mayoral decision of seeking re-election.

who choose a tax rate lower or equal to 0.3 in 2007. This means that we compare treated municipalities choosing a tax rate on the following interval  $(0.4; 0.5]$  with control municipalities in the the interval  $(0.3; 0.4]$ . This restricted sample is shown in figure 5 (b). The 65 removed municipalities are mainly clustered in more remote areas close to the border with Spain.

Furthermore, it is also important to notice above in Table 1 that this reform did not alter the tax rate applied to rural real estate with no buildings, that was kept at 0.8%. This allow us to build a counter-factual analysis, supporting the main analysis if no effects of the reform are found this type of properties.

## 3 Data and Identification Strategy

### 3.1 Data

This analysis relies on an extensive dataset at Portuguese municipal level over the period 2005–2011. The outcome variable is a logarithmic transformation of the mean value of urban real estate transactions per municipality (in real terms) obtained from Statistics Portugal (*INE*).<sup>7</sup> As a counter-factual exercise, we also look at the the mean value of rural real estate transactions of land with no buildings per municipality (in real terms).

For robustness, the analysis includes also a number of control variables, namely socio–demographic, economic and political characteristics, local public finance variables and electoral results. Table 2 presents the descriptive statistics of the two outcome variables and all control variables.

The socio–demographic variables comprise measures of the municipal population, the share of the working population with a tertiary degree, and the share of immigrants employed in the municipality. Population density is taken from *INE* and, to account for different age-structures, we also include the municipal dependency ratio. The average educational level and the share of immigrants are

---

<sup>7</sup>The mean value of real estate transactions is computed as the value of real estate transactions divided by the number of transactions in each municipality, whose data series are both obtained from Statistics Portugal. These values are then deflated to the year 2015 by the national consumer price index from the Statistics Database of the European Commission (*Eurostat*).

Table 2: **Descriptive Statistics**

Variable	Obs.	Mean	Std. Dev.	Min	Max
Mean Transaction Value (log):					
Urban	1946	4.089	.577	2.328	6.592
Rural	1946	2.776	1.404	-1.161	7.853
<i>Demographic</i>					
Population Density	1946	.312	.847	.005	7.3797
Dependency Ratio	1946	.590	.120	.382	1.088
Education Level	1946	.068	.032	.015	.302
Immigrant Share	1946	.083	.054	.003	.337
<i>Economic</i>					
Unemployment Rate	1946	6.615	2.345	1.483	16.933
Electricity Consumption	1946	8.211	.486	7.277	11.106
Touristic Area (%)	1946	.003	.012	0	.105
<i>Public Goods</i>					
Average Exam Score	1946	2.705	.270	1.891	4
Crime Rate	1946	30.915	13.294	6.9	161.4
Hospital Dummy	1946	.311	.463	0	1
Court Dummy	1946	.740	.438	0	1
Official Clinics (>1)	1946	.104	.306	0	1
Highway Access Dummy	1946	.548	.498	0	1
<i>Political</i>					
Left-wing Seats Share	1946	.552	.253	0	1
Aligned Mayor	1946	.387	.487	0	1
Majority Local Gov.	1946	.900	.300	0	1

computed from *Quadros de Pessoal*.<sup>8</sup>

As for the economic characteristics, we include two proxies for municipal income level – the consumption of electricity per capita (in logs), taken from *INE*, and the unemployment rate. The latter is measured by the ratio of resident population

<sup>8</sup>The effect of immigration on house prices was analyzed for the US (Saiz, 2007), Spain (Gonzalez and Ortega, 2013), Italy (Accetturo et al., 2014), and the UK (Sá, 2015).

aged between 15 and 65 years old who is enrolled as unemployed in the Portuguese Institute of Employment and Professional Training (*IEFP*). In addition, we add the share of the touristic area as a proxy for touristic amenities in the municipality. This series is taken from *INE*.

Furthermore, we consider several variables on local public finance and on the provision of public goods.<sup>9</sup> This issue is particularly important since a number of neighborhood features tend to be difficult to observe by the econometrician. In this regard, we include the average national exam score (mean of the 9th-year exams of mathematics and Portuguese) in each municipality to control for the quality of public schools.<sup>10</sup> Other control variables related to public good provision included are: the municipal crime rate; a dummy for the existence of a court of first instance in the municipality; a dummy for the existence of a hospital in the municipality; and a dummy indicating whether there is one or more than one official clinic in the municipality. We consider also transportation infrastructure provision by adding a binary variable which takes the value one if there is at least one highway crossing a given municipality.<sup>11</sup> All of these variables are taken from *INE*.

Finally, the set of municipality's political characteristics and electoral results is constructed based on data obtained from the General Directorate for Internal Affairs' (*Direcção-Geral da Administração Interna, DGAI*) website. This analysis uses information on the local elections of 2001, 2005 and 2009. The data is provided at the party level per municipality, consisting on the number of votes and seats allocated to each party. We include the share of seats in the municipal council occupied by left-wing members. For the winning party in each municipality two dummy variables are constructed: one indicating whether it is from the same party as the central government and other indicating whether the party obtained a majority of seats in the municipal council.

---

<sup>9</sup>See Gibbons and Machin (2008) for a survey on the effects of school quality, better transport, and lower crime rates on house prices.

<sup>10</sup>There is an extensive literature showing that better schools are capitalized into house prices (see, *inter alia*, Downes and Zabel (2002), Fack and Grenet (2010) Ries and Somerville (2010), Dhar and Ross (2012), Gibbons et al. (2013), and La (2015)). Moreover, Figlio and Lucas (2004) point out that this capitalization effect is greater the more available is the information on school quality. Nunes et al. (2015) analyze the effects of the publication of school rankings in Portugal.

<sup>11</sup>Audretsch et al. (2017) highlight the importance of highways for regional development in Portugal.

## 3.2 Empirical Model

We aim to test whether property taxes are capitalized into real estate market values. For this, we take advantage of a quasi-natural experiment in Portugal: an unexpected property tax reform in Portugal. The reform took place in 2008 and forced a decrease of the property tax rate upper bound on urban real estate from 0.5% to 0.4%.

The nature of the property tax reform establishes pre- and post-treatment periods that allow for a quasi-experimental difference-in-differences (DD) approach.<sup>12</sup> Our unit of observation is the municipality. Let  $D_i$  be the dummy indicating treatment, equal to one for all municipalities in the treatment group and zero for all municipalities in the control group. That is, this dummy takes the value one if municipality  $i$  was setting a tax rate higher than 0.4% in 2007 and zero otherwise. Throughout the analysis we consider two samples that vary solely in the control municipalities included. The *Full Sample* includes all 278 mainland Portuguese municipalities and the control group includes all those not in the treatment one. In contrast, in the *Restricted Sample* the control municipalities are the ones setting a tax rate in 2007 within the interval (0.3%; 0.4%]. By excluding municipalities setting a lower tax rate allow us a more robust analysis, as we are comparing groups with more similar fiscal preferences.

The reform was announced in 2008 and this is when the post-treatment period starts. Accordingly, let  $d_t$  be a time dummy that switches to one in the year of the treatment assignment, i.e.  $d_t = 1[t \geq 2008]$ .

Inference on the average treatment effect of the reform is based on the following general DD regression model:

$$y_{it} = \beta T_{it} + X'_{it} \delta + \gamma_i + \lambda_t + \epsilon_{it} \quad (1)$$

---

<sup>12</sup>Our identification strategy is inspired by the empirical analysis of Lyytikäinen (2012) and Baskaran (2014), who rely on similar centrally legislated changes in local tax ranges. Lyytikäinen (2012) uses a change in minimum tax rates set by the Finnish central government for property taxes to identify local tax competition; and Baskaran (2014) uses a difference-in-differences approach by comparing two German states, of which North Rhine-Westphalia faced an increase in business and property tax rates. In Portugal, Peralta and Pereira dos Santos (2019) use the reform at study and a similar empirical mechanism to measure the impact of a tax revenue cut on the mayoral decision of seeking re-election.

where  $y_{it}$  is any of the outcome variables under study,  $T_{it} = (D_i \cdot d_t)$  and  $\beta_1$  captures the effect of interest, and  $X_{it}$  is a vector of control variables, including the socio-economic and political characteristics previously described. Finally,  $\gamma_i$  are the municipality fixed effects (to control for time-invariant factors) and  $\lambda_t$  the time-period fixed effects.

In addition, the pattern of lagged and forward effects is also of interest as it often provides further and more insightful information on the dynamics of the treatment effects. Therefore, average annual treatment effects are assessed through the following extension of the baseline regression model (event study design):

$$y_{it} = \sum_{j=-m}^q \beta_j T_{it+j} + X'_{it} \delta + \gamma_i + \lambda_t + \epsilon_{it} \quad (2)$$

where the sum allows for  $m$  "leads" or pre-treatment effects and for  $q$  "lags" or post-treatment effects. The remaining variables are defined as before. This design has two main advantages. First, we can test the exogeneity of the shock by evaluating pre-trends. Second, this approach enables to evaluate the short and medium run impact of the tax reform on the real estate prices.

Finally, for the purpose of studying the intensity of treatment effects the regression model in Equation 1 is extended to encompass interaction with the imposed decrease of the tax rate. The treatment intensity effects are obtained within the following regression framework:

$$y_{it} = \beta(I_i \cdot T_{it}) + X'_{it} \delta + \gamma_i + \lambda_t + \epsilon_{it} \quad (3)$$

where  $I_i$  is the imposed decrease in the tax rate due to the reform, i.e. the difference between the tax rate set in 2007 and the new tax rate upper bound for the treated municipalities and zero otherwise.

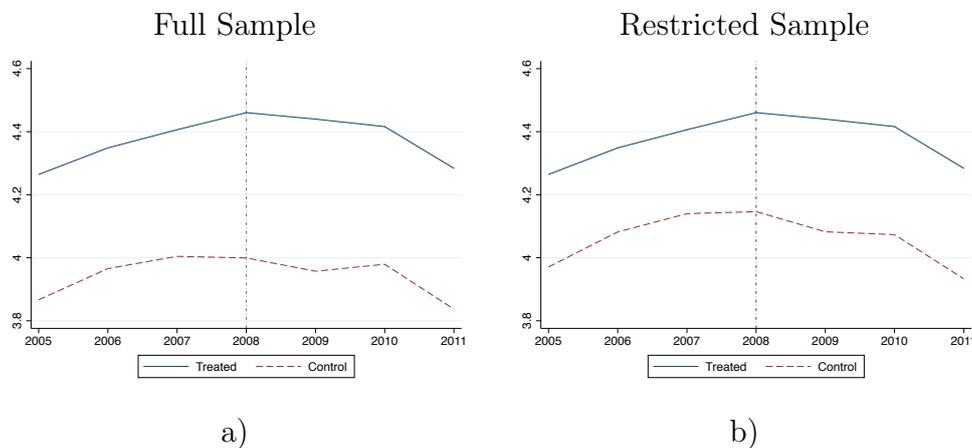
## 4 Empirical Evidence

### 4.1 The Common Trends Assumption

Internal validity of a DD estimation relies on the standard “parallel trends” condition (Meyer, 1995). For it to hold, the trend in each of the dependent variables under study must be the same for all municipalities in the absence of treatment. This assumption can be tested through different procedures. One common approach is to compare the evolution of the different outcome variables in treated and control municipalities during period of analysis (Angrist and Pischke, 2009). Figure 1 provides mean plots for mean value of real estate transaction of urban dwellings (in logs and real terms) for both the full and the restricted sample.

In fact, until announcement of the reform, the graph provide substantive evidence of identical trends, in the two samples, between treatment and comparison municipalities. After the year of the assignment of treatment, in 2008, the two groups start to present subtle differential trends. Hence, this figure illustrates that there are no preexisting trends capable of undermining the empirical design.

Figure 1: **Evolution of Urban Real Estate Transactions**



Notes: Evolution of mean values for treatment and control groups over the period 2005–2011.

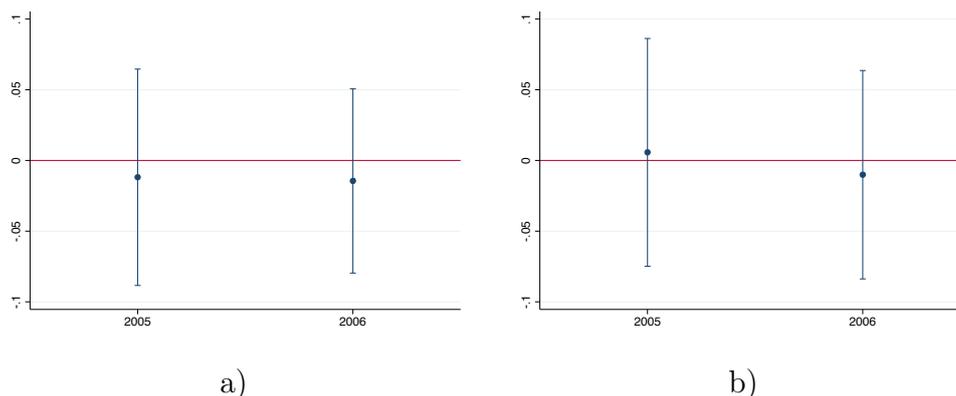
A second approach consists in regressing the outcome variable on yearly dummies indicating the treatment group (Moser and Voena, 2012). This can be as-

sessed by the model in Equation 2 but just focusing in the pre-treatment period as follows:

$$y_{it} = \sum_{j=-m}^0 \beta_j T_{it+j} + X'_{it} \delta + \gamma_i + \lambda_t + \epsilon_{it} \quad (4)$$

where the sum allows for  $m$  "leads" or pre-treatment effects. The remaining variables are defined as before. The omitted category is 2007, the year before the announcement of the reform. Figure 2 presents the results, where the coefficient estimates measure how outcome variables differ between treatment and control municipalities before the reform. As suggested by the previous test, on average, treatment and control real estate markets did not differ significantly in the pre-treatment period in both samples.

Figure 2: **Common Trends Study**



Notes: Coefficient estimates for the a model given by Equation 4 over the period 2005–2007. Caped lines indicate 95% confidence intervals.

In summary, the tests support the assumption of common trends in the property tax outcomes under study for treatment and control municipalities. Thus, DD coefficient estimates can be assumed to capture the causal effect of treatment.

## 4.2 Post-Reform Effects

We first estimate Equation 1 considering the mean value of urban real estate transactions as the dependent variable and vectors of control variables are gradually

included: in column (1) no control variables are included in the model; in column (2) we consider population density, dependency ratio, education level and immigrants share; in column (3) we include also the unemployment rate, consumption of electricity per capita (log) and share of touristic area; in column (4) we then consider the average exam score (mean of mathematics and Portuguese), crime rate, dummy for court, dummy for hospital, dummy for more than one official clinic and dummy for highway access; finally, the political control variables are included in column (5), namely the share of left-wing seats, mayor aligned with central government and majority in the local government. All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level. The results are presented in Table 3 and are very similar in all specifications and for both the full and the restricted samples.

Table 3: **Property Tax Reform Effects on Urban Real Estate Transactions**

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Full Sample</i>					
$T_{it}$	0.063 (0.02)***	0.060 (0.02)***	0.064 (0.02)***	0.060 (0.02)***	0.062 (0.02)***
Obs.	1946	1946	1946	1946	1946
<i>Panel B: Restricted Sample</i>					
$T_{it}$	0.066 (0.02)***	0.064 (0.02)***	0.068 (0.02)***	0.062 (0.02)***	0.065 (0.02)***
Obs.	1491	1491	1491	1491	1491
Controls:					
– <i>Demographic</i>		✓	✓	✓	✓
– <i>Economic</i>			✓	✓	✓
– <i>Public Goods</i>				✓	✓
– <i>Political</i>					✓

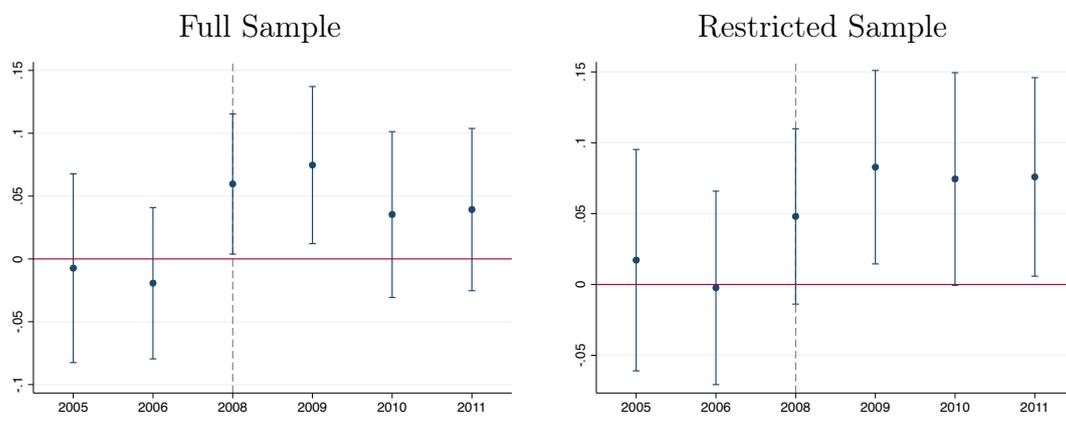
*Note:* Results are obtained from the estimation of Equation 1. The dependent variable is the logarithm of the mean value of total real estate transactions (in real terms). All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. \*, \*\* and \*\*\* denote 10%, 5% and 1% significance, respectively.

We find a positive and significant effect of the reform on the mean value of total real estate transactions. Point estimates marginally decrease as more controls are

included, and in the more conservative model, we obtain an effect of around 6%, which suggests that property tax capitalization is at place. The results are robust to a plethora of checks, which we explore in more detail in Section 2.5.

The annual average treatment effects in Figure 3 in turn, obtained from estimating Equation 2, are in line with the previous assessment and provide additional information as to the timing of the buyers response to property tax reform. In fact, this parametric event study suggests that the bulk of the effect is concentrated in the first year after the announcement. Additionally, as time goes by, mayors are able to better cope with the consequences of the reform by re-adapting their policies to the new environment.

Figure 3: **Yearly Effects of the Reform on Urban Real Estate Transactions**



*Note:* This figure provides plots depicting the coefficient estimates for yearly dummy variables indicating the treated group over the period, 2005–2011. Coefficients are obtained estimated the a model given by Eq. (2), including a vector of control variables and controlling for municipality and year fixed effects. Caped lines indicate 95% confidence intervals.

Finally, we proceed to the estimation of the treatment intensity effects by estimating Equation 3. Table 4 presents the results for both the overall and the restricted sample. We find that municipalities who experienced a higher decrease in the tax rate also experienced a higher increase in their real estate mean transaction value.

Table 4: **Intensity Effects on Urban Real Estate Transactions**

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Full Sample</i>					
$I_{it} \times T_{it}$	0.650 (0.24)***	0.619 (0.24)**	0.670 (0.24)***	0.615 (0.23)***	0.628 (0.23)***
Obs.	1946	1946	1946	1946	1946
<i>Panel B: Restricted Sample</i>					
$I_{it} \times T_{it}$	0.665 (0.25)***	0.652 (0.25)***	0.694 (0.24)***	0.628 (0.24)***	0.653 (0.24)***
Obs.	1491	1491	1491	1491	1491
Controls:					
– <i>Demographic</i>		✓	✓	✓	✓
– <i>Economic</i>			✓	✓	✓
– <i>Public Goods</i>				✓	✓
– <i>Political</i>					✓

*Note:* Results are obtained from the estimation of Equation (3). The dependent variable is the logarithm of the mean value of total real estate transactions (in real terms). All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. \*, \*\* and \*\*\* denote 10%, 5% and 1% significance, respectively.

## 5 Robustness

The results are robust to a plethora of checks. Firstly, we replicate the baseline estimation in Table 3 but applying different clustering levels for the standard errors - at the NUTS III and regional level. Appendix B.2 presents the results that are basically identical to those presented before. Secondly, we take into account some heterogeneity in the dynamics of urban real estate transactions that might affect the magnitude of the reform effect by including population and regional trends. Moreover, we address a crucial problem with DD estimations related to issues of selection into treatment. For that, we replicate the estimations by excluding some critical groups of municipalities, we run the standard placebo tests and a counter-factual analysis of rural real estate transactions. Finally, we focus on the identification concern related to the local public goods provision.

## 5.1 Population and Regional Trends

In order to account for heterogeneous dynamics of urban real estate transactions, such as the municipality size or regional shocks, we include in the baseline regression: i) pre-treatment municipal population quartile dummies interacted with year dummies, ii) a regional time trend, and iii) region dummies interacted with time dummies. Region dummies comprise the 18 regional Portuguese districts. The results, reported in Table 5, are once again extremely similar to our baseline ones.

Table 5: **Robustness: Including Population and Regional Trends**

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Full Sample</i>					
$T_{it}$	0.065 (0.02)***	0.065 (0.02)***	0.044 (0.02)**	0.055 (0.02)**	0.053 (0.02)**
Obs.	1946	1946	1946	1946	1946
<i>Panel B: Restricted Sample</i>					
$T_{it}$	0.074 (0.02)***	0.068 (0.02)***	0.049 (0.02)**	0.062 (0.02)**	0.062 (0.02)***
Obs.	1491	1491	1491	1491	1491
Pop Quartile	✓			✓	✓
Region Trend		✓		✓	✓
Region x Year			✓	✓	✓
Controls					✓

*Note:* Results are obtained from the estimation of Equation (1). The dependent variable is the logarithm of the mean value of urban real estate transactions (in real terms). All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. \*, \*\* and \*\*\* denote 10%, 5% and 1% significance, respectively.

## 5.2 Selection Bias

A potential problem with DD estimations that should be taken into account relates to issues of selection bias. A key identification assumption is that municipalities are exogenously assigned to treatment and control group. Solely when this holds, one can causally identify the effect of the reform as the differential change in the

outcome variables from pre- to post-treatment period between in treated and non-treated municipalities. If it is not the case and there may be intrinsic differences between the two groups, causality is then undermined.<sup>13</sup>

One can test for selection bias in a number of ways. One possibility is to exclude more urban municipalities. This has the advantage of providing a more homogeneous sample, as urban municipalities may differ from the rest of the country in a number of ways. Therefore, by solely focusing on less urban areas, we mitigate the risk of mistakenly reflecting confounders by rulling out possible outliers. We test this possibility restricting the full sample in four ways: i) excluding the metropolitan areas of Lisbon and Oporto, ii) excluding all coastal municipalities; iii) excluding the 18 district capitals<sup>14</sup>; and iv) restricting to municipalities applying even more similar property tax rates in 2007. This latter test is very demanding as it leaves us with a particularly small sample. Table 6 shows the results where Equation 1 is reestimated excluding these several groups of municipalities. In columns (7) and (8), the *Conservative Sample* only considers municipalities setting property tax rates between [0.4% and 0.5%) in 2007. From the treatment group, we remove municipalities setting the tax rate at the previous maximum whereas from the control group we only consider the municipalities setting a tax rate equal to 0.4%. All other municipalities are excluded from the estimation. Even though statistical significance decreases, point estimates are consistent with previous results.

---

<sup>13</sup>For a discussion on the importance of choosing careful comparison groups to evaluate place-based policies see Neumark and Simpson (2015).

<sup>14</sup>Municipalities were grouped into 18 districts in 1835, replacing previous clerical dioceses. The 1976 Constitution abolished districts as an official local administrative unit. Still, they cluster, to a certain extent, similar municipalities.

Table 6: **Selection Bias Tests I: Restricted Samples**

	No Metropolitan		No Coastal		No Capitals		Conservative	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$T_{it}$	0.072 (0.02)***	0.073 (0.02)***	0.061 (0.02)**	0.055 (0.02)**	0.059 (0.02)**	0.055 (0.02)**	0.049 (0.03)*	0.053 (0.03)*
Obs.	1708	1708	1694	1694	1820	1820	924	924
Controls		✓		✓		✓		✓

*Note:* Results are obtained from the estimation of Equation (1), without and with the vector of control variables. The dependent variable is the logarithm of the mean value of urban real estate transactions (in real terms). The control variables included are: population density; dependency ratio; education level; immigrant share; unemployment rate; consumption of electricity per capita (log); share of touristic area; average exam score (mean of mathematics and Portuguese); crime rate; dummy for court; dummy for hospital; dummy for more than one official clinic; dummy for highway access; share of left-wing seats; mayor aligned with central government; and majority in the local government. All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. \*, \*\* and \*\*\* denote 10%, 5% and 1% significance, respectively.

The most common approach in the context of a DD framework however, is to devise placebo tests. These tests usually consist on reestimating the baseline results relying on a placebo treatment setting in at a fake treatment year. Here, the sample is restricted to the pre-treatment period, i.e. 2005-2007, and 2006 is considered as the treatment year. As Columns (1) and (2) Table 7 show, estimates are close to zero and always insignificant.

As a further analysis, we focus also on the effect on the rural real estate. The property tax law provides a counterfactual since land with no buildings is subject to a different tax regime that suffered no changes in 2008. Columns (3) and (4) of Table 7 present the results. Unsurprisingly, we find that there is no effect on the rural buildings. This provides additional support for the validity of our results as the reform did not change the tax rate applied to this type of buildings.

Table 7: Selection Bias Tests II: Placebo and Rural Transactions

	Placebo		Rural	
	(1)	(2)	(3)	(4)
<i>Panel A: Full Sample</i>				
$T_{it}$	-0.005 (0.03)	0.004 (0.03)	-0.104 (0.06)*	-0.041 (0.07)
Obs.	834	834	1946	1946
<i>Panel B: Restricted Sample</i>				
$T_{it}$	-0.005 (0.03)	0.004 (0.03)	-0.025 (0.07)	0.024 (0.07)
Obs.	834	834	1491	1491
Controls:		✓		✓

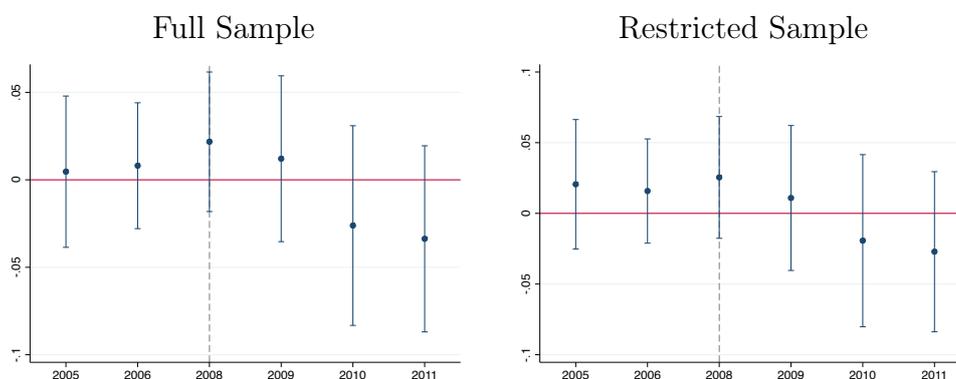
*Note:* Results are obtained from the estimation of Equation (1). In columns (1) and (2) the dependent variable is the logarithm of the mean value of urban real estate transactions (in real terms). In columns (3) and (4) the dependent variable is the logarithm of the mean value of rural real estate transactions (in real terms). The control variables included are: population density; dependency ratio; education level; immigrant share; unemployment rate; consumption of electricity per capita (log); share of touristic area; average exam score (mean of mathematics and Portuguese); crime rate; dummy for court; dummy for hospital; dummy for more than one official clinic; dummy for highway access; share of left-wing seats; mayor aligned with central government; and majority in the local government. All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. \*, \*\* and \*\*\* denote 10%, 5% and 1% significance, respectively.

### 5.3 Local Public Good Provision

A critical identification concern of property tax capitalization is related with the local public good provision and so it must be also taken into account in our identification strategy. The main intuition is that if the reaction is substantially different in treated and comparison municipalities, this risks the introduction of biases to our estimates. In theory, one should note that if the property tax cut was translated into a lower supply of public goods in the comparison group, the average real estate prices should decrease – and not increase in these regions. Therefore, our estimates would represent a lower bound of the true effect of the reform. Even though we cannot totally dismiss these concerns related to unobservables, we present some

suggestive evidence that these factors are not extremely important in this setting. If we run the event study design 2 with the log of total municipal expenditures net of interest payments as outcome variable, we do not observe statistically significant results, as shown by Figure 4.<sup>15</sup> This means that affected mayors were able to mix their tax choices, at least in the short run, to keep expenditures constant. In addition, controlling for this variable in our baseline specification leaves the point estimates basically unchallenged.<sup>17</sup>

Figure 4: **Total Expenditures (log)**



Notes: Coefficient estimates for the a model given by Eq. (2) over the period 2005–2011. Caped lines indicate 95% confidence intervals.

In summary, these analyses lend credence to the view that our empirical analysis is able to assess the causal impact of the property tax reform on real estate prices.

## 6 Conclusion

In this paper, we attempt to determine if property tax differentials on real estates are capitalized. The scarce empirical evidence has reported mixed results. The aftermath of a reform introducing a lower maximum tax rate for urban properties

<sup>15</sup>The same results hold if we substitute this variable by the log of capital expenditures (in real terms). Both data series can be retrieved from the General Directorate for Local Authority’s (*Direcção-Geral das Autarquias Locais, DGAL*) website.<sup>16</sup>

<sup>17</sup>We decided not to include this control variable in our analysis due to endogeneity concerns. Results adding this covariate are available from the authors upon request.

is the perfect laboratory to study whether there is an effect on housing prices. We take advantage of this reform to establish causality using a rich dataset on the universe of mainland Portuguese municipalities. We believe that this strategy allows for credible inference upon the causal effects of this reform. Since the present paper studies the impact of a nationwide policy, rather than a number of local jurisdictions, we also believe that it presents a higher degree of external validity.

We perform several robustness checks and falsification tests to have a better view regarding possible mechanisms. Our findings suggest that agents rationally incorporate the decreased cost of living in their buying decisions, which are reflected in a market price increase with the net present value of the tax reduction. Our results thus corroborate standard capitalization theory. In our most conservative estimate, we observe an increase close to 5%.

## References

- Accetturo, A., Manaresi, F., Mocetti, S. and Olivieri, E. (2014), ‘Don’t stand so close to me: the urban impact of immigration’, *Regional Science and Urban Economics* **45**, 45–56.
- Angrist, J. and Pischke, J. (2009), *Mostly harmless economics: An empiricist’s companion*, Princeton University Press, Princeton.
- Audretsch, D. B., Dohse, D. and dos Santos, J. P. (2017), Do toll-free highways foster firm formation and employment growth? results from a quasi-natural experiment, Technical report, Kiel Working Paper.
- Baskaran, T. (2014), ‘Identifying local tax mimicking with administrative borders and a policy reform’, *Journal of Public Economics* **118**, 41–51.
- Basten, C., von Ehrlich, M. and Lassmann, A. (2017), ‘Income taxes, sorting and the costs of housing: Evidence from municipal boundaries in switzerland’, *The Economic Journal* **127**(601), 653–687.

- Besley, T., Meads, N. and Surico, P. (2014), ‘The incidence of transaction taxes: Evidence from a stamp duty holiday’, *Journal of Public Economics* **119**, 61 – 70.
- Best, M. C. and Kleven, H. J. (2018), ‘Housing market responses to transaction taxes: Evidence from notches and stimulus in the u.k.’, *The Review of Economic Studies* **85**(1), 157–193.
- Bradley, S. (2017), ‘Inattention to deferred increases in tax bases: How michigan home buyers are paying for assessment limits’, *Review of Economics and Statistics* **99**(1), 53–66.
- Cushing, B. (1984), ‘Capitalization of interjurisdictional fiscal differentials: An alternative approach’, *Journal of Urban Economics* **15**(3), 317–326.
- Dachis, B., Duranton, G. and Turner, M. A. (2012), ‘The effects of land transfer taxes on real estate markets: evidence from a natural experiment in toronto’, *Journal of Economic Geography* **12**(2), 327–354.
- Dhar, P. and Ross, S. L. (2012), ‘School district quality and property values: Examining differences along school district boundaries’, *Journal of Urban Economics* **71**(1), 18–25.
- Downes, T. A. and Zabel, J. E. (2002), ‘The impact of school characteristics on house prices: Chicago 1987–1991’, *Journal of Urban Economics* **52**(1), 1–25.
- Elinder, M. and Persson, L. (2017), ‘House price responses to a national property tax reform’, *Journal of Economic Behavior and Organization* **144**, 18–39.
- Fack, G. and Grenet, J. (2010), ‘When do better schools raise housing prices? evidence from paris public and private schools’, *Journal of public Economics* **94**(1-2), 59–77.
- Figlio, D. N. and Lucas, M. E. (2004), ‘What’s in a grade? school report cards and the housing market’, *American Economic Review* **94**(3), 591–604.

- Gibbons, S. and Machin, S. (2008), ‘Valuing school quality, better transport, and lower crime: evidence from house prices’, *Oxford Review of Economic Policy* **24**(1), 99–119.
- Gibbons, S., Machin, S. and Silva, O. (2013), ‘Valuing school quality using boundary discontinuities’, *Journal of Urban Economics* **75**, 15–28.
- Gonzalez, L. and Ortega, F. (2013), ‘Immigration and housing booms: Evidence from Spain’, *Journal of Regional Science* **53**(1), 37–59.
- Hilber, C. (2015), ‘The economic implications of house price capitalization: a synthesis’, *Real Estate Economics* **45**(2), 301–339.
- Hilber, C. A. and Lyytikäinen, T. (2017), ‘Transfer taxes and household mobility: Distortion on the housing or labor market?’, *Journal of Urban Economics* **101**, 57 – 73.
- Hilber, C. A., Lyytikäinen, T. and Vermeulen, W. (2011), ‘Capitalization of central government grants into local house prices: Panel data evidence from England’, *Regional Science and Urban Economics* **41**(4), 394–406.
- Kopczuk, W. and Munroe, D. (2015), ‘Mansion tax: The effect of transfer taxes on the residential real estate market’, *American Economic Journal: Economic Policy* **7**(2), 214–57.
- La, V. (2015), ‘Capitalization of school quality into housing prices: Evidence from Boston public school district walk zones’, *Economics Letters* **134**, 102–106.
- Lyytikäinen, T. (2012), ‘Tax competition among local governments: Evidence from a property tax reform in Finland’, *Journal of Public Economics* **96**(7), 584–595.
- Meyer, B. D. (1995), ‘Natural and quasi-experiments in economics’, *Journal of Business and Economic Statistics* **13**(2), 151–61.
- Mian, A., Rao, K. and Sufi, A. (2013), ‘Household balance sheets, consumption, and the economic slump’, *The Quarterly Journal of Economics* **128**(4), 1687–1726.

- Moser, P. and Voena, A. (2012), ‘Compulsory licensing: Evidence from the trading with the enemy act’, *American Economic Review* **102**(1), 396–427.
- Neumark, D. and Simpson, H. (2015), Place-based policies, in ‘Handbook of Regional and Urban Economics’, Vol. 5, Elsevier, pp. 1197–1287.
- Nunes, L. C., Reis, A. B. and Seabra, C. (2015), ‘The publication of school rankings: A step toward increased accountability?’, *Economics of Education Review* **49**, 15–23.
- 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.
- OECD (1999), ‘Taxing powers of state and local government’, *Tax policy studies No 1* p. Paris.
- Oliviero, T. and Scognamiglio, A. (2019), ‘Property tax and property values: Evidence from the 2012 italian tax reform’, *European Economic Review* **118**, 227–251.
- Palmon, O. and Smith, B. A. (1998), ‘New evidence on property tax capitalization’, *Journal of Political Economy* **106**(5), 1099–1111.
- Peralta, S. and Pereira dos Santos, J. (2019), ‘Who seeks reelection: local fiscal restraints and political selection’, *Public Choice* .
- Ries, J. and Somerville, T. (2010), ‘School quality and residential property values: evidence from vancouver rezoning’, *The Review of Economics and Statistics* **92**(4), 928–944.
- Ross, S. and Yinger, J. (1999), Sorting and voting: A review of the literature on urban public finance, in P. C. Cheshire and E. S. Mills, eds, ‘Handbook of Regional and Urban Economics’, Vol. 3 of *Handbook of Regional and Urban Economics*, Elsevier, chapter 47, pp. 2001–2060.
- Sá, F. (2015), ‘Immigration and house prices in the uk’, *The Economic Journal* **125**(587), 1393–1424.

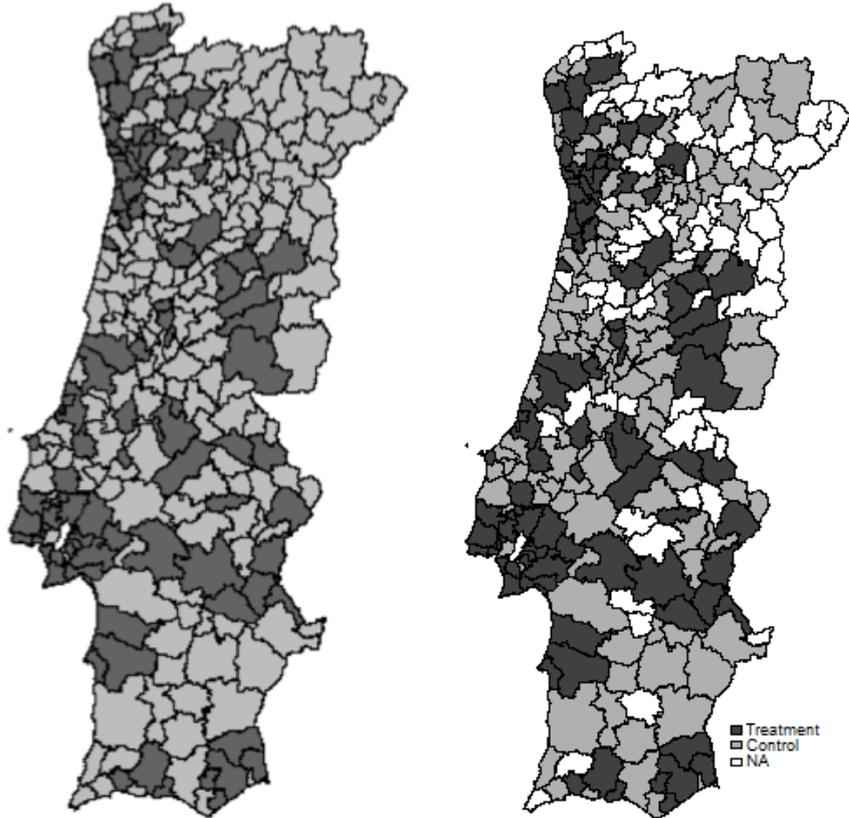
- Saiz, A. (2007), 'Immigration and housing rents in american cities', *Journal of urban Economics* **61**(2), 345–371.
- Sirmans, S., Gatzlaff, D. and Macpherson, D. (2008), 'The history of property tax capitalization in real estate', *Journal of Real Estate Literature* **16**(3), 327–344.
- Slemrod, J., Weber, C. and Shan, H. (2017), 'The behavioral response to housing transfer taxes: Evidence from a notched change in dc policy', *Journal of Urban Economics* **100**, 137–153.
- Yinger, J. (1982), 'Capitalization and the theory of local public finance', *Journal of Political Economy* **90**(5), 917–943.

## Appendix A Treated vs. Control Municipalities

Figure 5: Treated vs. Control Municipalities

(a) Complete Sample

(b) Restricted Sample



Note: Treated municipalities – forced to decrease the property tax rate in 2008 – in darker grey and control in lighter grey.

# Appendix B Robustness I: Different Clustered Standard Errors

## B.1 NUTS III Level Clustering

Table 8: Property Tax Reform Effects on Urban Real Estate Transactions

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Full Sample</i>					
$T_{it}$	0.063 (0.02)***	0.060 (0.02)***	0.064 (0.02)***	0.060 (0.02)***	0.062 (0.02)***
Obs.	1946	1946	1946	1946	1946
<i>Panel B: Restricted Sample</i>					
$T_{it}$	0.066 (0.02)***	0.064 (0.02)***	0.068 (0.02)***	0.062 (0.02)***	0.065 (0.02)***
Obs.	1491	1491	1491	1491	1491
Controls:					
– <i>Demographic</i>		✓	✓	✓	✓
– <i>Economic</i>			✓	✓	✓
– <i>Public Goods</i>				✓	✓
– <i>Political</i>					✓

*Note:* Results are obtained from the estimation of Equation (2.1). The dependent variable is the logarithm of the mean value of urban real estate transactions (in real terms). All estimates include municipality and year fixed effects. Robust standard errors clustered at NUTS III level are presented in parentheses. \*, \*\* and \*\*\* denote 10%, 5% and 1% significance, respectively.

## B.2 Regional Level Clustering

Table 9: Property Tax Reform Effects on Urban Real Estate Transactions

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Full Sample</i>					
$T_{it}$	0.063 (0.02)***	0.060 (0.02)***	0.064 (0.02)***	0.060 (0.02)***	0.062 (0.02)***
Obs.	1946	1946	1946	1946	1946
<i>Panel B: Restricted Sample</i>					
$T_{it}$	0.066 (0.02)***	0.064 (0.02)***	0.068 (0.02)***	0.062 (0.02)***	0.065 (0.02)***
Obs.	1491	1491	1491	1491	1491
Controls:					
– <i>Demographic</i>		✓	✓	✓	✓
– <i>Economic</i>			✓	✓	✓
– <i>Public Goods</i>				✓	✓
– <i>Political</i>					✓

*Note:* Results are obtained from the estimation of Equation (2.1). The dependent variable is the logarithm of the mean value of urban real estate transactions (in real terms). All estimates include municipality and year fixed effects. Robust standard errors clustered at regional level are presented in parentheses. \*, \*\* and \*\*\* denote 10%, 5% and 1% significance, respectively.