CESifo WORKING PAPERS

6647
2017

September 2017

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CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
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Editors: Clemens Fuest, Oliver Falck, Jasmin Gröschl
www.cesifo-group.org/wp

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CESifo *Working Paper No. 6647*
Category 1: Public Finance

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Abstract

I exploit an exogenous reform introducing a local business tax in Portugal to study tax mimicking among jurisdictions. The identification strategy relies on a quasi-experimental difference-in-differences methodology and heterogeneity in treatment intensity. Results show evidence of significant short-run tax mimicking that decreases over time. I study possible generating processes underlying the strategic interaction among municipalities and find significant evidence of electoral concerns. These electoral concerns are not met with electoral consequences at the local elections, which may be behind the diffusion of local business taxation in the long run.

JEL-Codes: D720, H710, H770.

Keywords: tax mimicking, yardstick competition, local reform.

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I thank Thushyanthan Baskaran, Ernesto Dal Bó, Albert Solé-Ollé, and seminar participants at the CESifo Area Conference on Public Sector Economics (Munich, 2017), the ZEW Research Seminar and Conference on Public Finance (Mannheim, 2017), the IEB Workshop on Political Economy and Fiscal Federalism (Barcelona, 2017), and the IIPF (Tokyo, 2017).

1 Introduction

The theoretical literature on fiscal interdependencies among local governments models different interaction processes leading to a variety of outcomes concerning the level of taxation and the provision of public goods.[1] How the choices of one jurisdiction influence others depends, inter alia, on specific assumptions on the nature of the central planner, whether tax rates are determined as residuals or choice variables, and the degree of factor mobility between jurisdictions. The aim of this article is to identify strategic interactions in tax setting and to explore the underlying mechanisms.

Studying local fiscal interactions entails two main difficulties. First, the causal identification of treatment effects. Recent literature has shown the need to rely on quasi-experimental methodologies that capture sources of exogenous variation for causal identification. Second, spatial patterns in policy decisions can be generated by a multitude of processes that are not exclusive to intergovernmental interactions (Manski, 2000). Furthermore, even the lack of significant evidence on inter-jurisdictional dependence in tax setting does not mean interactions do not exist, instead, it is necessary to pinpoint the mechanisms at play within the different contexts for identification.

Large branches of the theoretical literature focus on the competition over tax bases and votes – see, e.g., the seminal contributions by Bradford and Oates (1971) on tax competition, and Salmon (1987) on the fiscal competition over votes, referred to as yardstick competition (Shleifer, 1985) – as well as on the positive or negative spillover effects of fiscal decisions (Case et al., 1993). Tax competition may lead to a race to the bottom (Wilson, 1986; Zodrow and Mieszkowski, 1986) or, alternatively, to the top (Baldwin and Krugman, 2004); yardstick competition predicts a correlation between tax setting and electoral outcomes (Besley and Case, 1995); and spillover effects can generate positive or negative interdependencies in tax rates across jurisdictions (Wildasin, 1988). In addition, models

[1] For a review of the theoretical literature see, e.g., Wilson and Wildasin (2004).

of expenditure competition (Wilson and Gordon, 2003) and, more recently, social learning in tax setting (Becker and Davies, 2014), also feature in the theoretical literature, usually predicting strategic complementarity in tax setting.

Following the theoretical developments, a large body of empirical studies focuses on the identification of strategic interactions among local governments. For a long time, the default methodological approach to the study of fiscal interdependencies among jurisdictions was the Spatial Lag (SL) framework, often combined with instrumental variables based on the characteristics of neighboring jurisdictions.[2] Gibbons and Overman (2012), however, argue that this methodology does not lend itself to causal interpretation. Indeed, three recent studies, by Lyytikainen (2012), Baskaran (2014), and Isen (2014), show that the SL regression framework consistently overestimates fiscal interactions among local governments. Moreover, all three studies fail to identify fiscal interactions within quasi-experimental frameworks, bringing into question the existing body of evidence on strategic tax setting.

Despite the important methodological contribution, these studies should not be taken as evidence against the existence of strategic interactions among local governments. All three studies rely on jurisdictions that suffer treatment only indirectly and do not investigate whether counteracting effects coming from different mechanisms lead to the insignificant results. In fact, two recent quasi-experimental studies identify strategic tax interaction along state borders in the US Agrawal (2015) and the Röstigraben in Switzerland Eugster and Parchet (2014) resuming the study of fiscal interdependencies among jurisdictions.

To address the methodological concerns in this study I exploit an exogenous reform of the local finances in Portugal in 2007 that introduced a local business tax. Inference rests on a quasi-experimental difference-in-differences (diff-in-diff) methodology and heterogeneity in treatment intensity proxied by the exogenous geographic location of municipalities which

[2] For a review of the spatial empirical literature see, e.g., Brueckner (2003).

determines their exposure to treatment. The empirical analysis is divided into a two-step procedure. The first step deals with the identification of tax mimicking, while the second provides insights into the generating processes to identify potential underlying mechanisms.

I find evidence of tax mimicking in the short run, that dissipates over time as more jurisdictions start to rely on local business taxation. The second step of the analysis fails to provide significant evidence on tax competition, expenditure competition, negative spillover effects or policy diffusion by social learning. I identify, however, electoral concerns in tax setting. These electoral concerns are not met with electoral consequences at the 2009 and 2013 local elections. A possible explanation for the results is the existence of short-term yardstick competition that, in the absence of retrospective voting, led to a diffusion in local business taxation. In this context, elections are a source of information for politicians to verify the acceptance of their pursued policies.

This article contributes primarily to the literature on interdependencies in tax setting among local governments by causally identifying tax mimicking in local business taxation. To my knowledge, it is the first study that provides significant causal inference on yardstick competition, though it depicts this regulatory mechanism as a short-term out-of-equilibrium phenomenon in the context of local politics. In addition, this paper contributes to the literature on the impact of information on yardstick competition. In line with the spatial studies by Revelli (2006) and Bivand and Szymanski (1997), this article argues that information mitigates yardstick competition. The main difference of this study is its reliance on a quasi-experimental methodology that lends itself to causal interpretation. Finally, it provides a contribution to the literature on voting behavior to the extent that it fails to uncover retrospective voting in local politics.

2 Empirical Setting

2.1 Institutional Background

Continental Portugal is divided into 278 municipalities that constitute the first subnational level of government.[3] Municipalities are responsible for several aspects of the local public administration, including the supply of public goods and local development. For the purpose, municipalities have their own finances, assets, and personnel.

There are two institutional organs at the municipal level: the executive council, and the legislative assembly. Local elections for both organs take place simultaneously countrywide every four years within a proportional representation system of closed lists with seats being distributed according to the D'Hondt method. Councils are generally composed of up to 14 councilmen, depending on the municipal population size, and headed by a mayor, who is the first name on the list gathering the highest amount of votes. Despite the proportional seat allocation rule, the winning list holds the executive power, while councilmen of other lists are often assigned non-executive roles. Executive decisions are usually summarily approved by the municipal assembly; mayors thus enjoy plenty of policy discretion during their mandates.

Vertical transfers from the central government and European institutions constitute the main source of municipal revenue – on average, over 50% of the budget. Revenues from local taxes and user fees, however, are growing in importance with the introduction of reforms pushing for revenue decentralization and an increase in local tax autonomy.[4] One

[3] For increased comparability I focus on the continental region, leaving out the 30 municipalities in the autonomous regions of Azores and Madeira.

[4] The degree of tax autonomy is still low; municipalities cannot create new taxes, and tax bases are always defined by the central government. Municipalities usually choose the applicable tax rate, but always within a centrally defined range.

such reform, Law no. 2/2007, in effect since January 1, 2007, increases local tax autonomy by introducing, inter alia, a local business tax.[5, 6]

The local business tax substituted a preexisting municipal business surcharge (*Derrama*).[7] The latter was an extraordinary source of capital revenue only available to municipalities involved in financial stability pacts or in need of an increase in fiscal capacity for specific investments. The surcharge was bound to a maximum of 10% of the national corporate tax (*Imposto sobre o Rendimento de Pessoas Colectivas, IRC*) and should be communicated to the Tax and Customs Authority (*Autoridade Tributária e Aduaneira*) by October 31 of the year prior to tax revenue collection. During the period under analysis, the IRC amounted to 25% of taxable profit excluding deductions. Thus, the surcharge corresponded to a maximum effective tax of 2.5%.

In contrast, the local business tax has a universal character; it is unconditionally available to all municipalities. In addition, it is independent of the IRC; the tax base is defined by law as the taxable business income apportioned to the municipality prior to deductions. A single-factor formula apportions income to municipalities according to the payroll ratio of the municipality against the total payroll. The increase in the tax base led to a decrease in the range within which the rate can be set. The municipal council can choose a local business tax within the interval 0–1,5% and must communicate the decision to the Tax and Customs Authority by December 31 of the year prior to tax revenue collection, otherwise the zero default sets in.

There are two main differences between the municipal surcharge and the local business tax. First and foremost, the local business tax is, contrary to the surcharge with its conditional character, a new source of municipal financing. Revenues from the local busi-

[5] Law no. 2/2007 in Diário da República 10, Series I, January 15, 2007.

[6] Law no. 2/2007 reformed other aspects of local financing. Two of the most critical changes include the introduction of a local income tax and adjustments to the allocation formula of vertical transfers between municipalities. I control for both aspects in the empirical analysis.

[7] The municipal business surcharge ceased to exist in 2007 with the introduction of the local business tax.

ness tax accrue to the current revenues account; current revenues finance the day-to-day business of the municipality. Revenues from the surcharge were instead confined to capital revenues for investment decisions. This change was predicted to increase the number of municipalities relying on capital taxation. Second, the local business tax is independent of the IRC; it applies to a different tax base. In fact, companies may not have to pay IRC if deductions eliminate all taxable business profits, but they may still be liable to pay the local business tax. The rationale is the following: the local business tax should compensate municipalities for their investment in the local infrastructure necessary to support business activities. This investment takes place independently of business profits.

2.2 To Tax or Not to Tax

There are various reasons for why municipalities may start, or abstain from, levying the local business tax. Taxation is a heated topic in Portugal with property and business taxation at the forefront of the discussion at the local level. Thus, levying the local business tax may entail electoral concerns which can lead to yardstick competition in local business taxation. Even though the local property tax is the most important fiscal instrument at the local level, the local business tax is often contested for two main reasons. First, it usually disproportionately affects small and medium-sized companies which comprise about 60% of the business landscape in Portugal. These companies are often under financial distress – and were especially so during the years of the financial crisis – and exert influence on a reduction in the local tax burden. Second, the local business tax is apportioned according to the payroll ratio, which is connected to another critical issue in Portugal: the level of employment. Even though small companies are unlikely to move production or storage facilities on the basis of local business taxation, companies with more than one production or storage facility may easily optimize their overall payroll ratio to avoid taxation.

The last consideration relates to the theory of tax competition. Tax competition may provide further reasons for or against local business taxation. If certain municipalities benefit from agglomeration effects, they may raise the tax without losing the tax base. Conversely, municipalities may choose not to levy the tax to attract more companies. The maximum tax differential among municipalities, however, is limited to 1.5%. This is a relatively small percentage when compared to the national business tax which amounts to 25% of taxable profits. In addition, by law, mayors decide on the local business tax rate annually, meaning that any differentials may be short-lived. Also, tax competition was purposefully preempted by the choice of apportionment formula for local capital taxation in 1993, when the central government opted for the payroll ratio over the location of the headquarters as the determining factor.[8]

If municipalities compete over expenditure instead, mayors may decide for local business taxation to increase the current account budget and provide more or better municipal services. The revenues from the local business tax, however, do not constitute a major part of the available budget – on average, 2%. Thus, any potential increase in expenditure may be too small to motivate local business taxation or to result in significant changes in expenditure policy.

Spillover effects have limited applicability within the context under study. Benefit spillovers from municipalities levying the local business tax and investing in municipal services are limited as most of these are confined to residents. This would, nonetheless, create a negative interdependence in tax setting, meaning that municipalities would be unlikely to raise the local business tax. Negative spillovers, in contrast, could lead to municipalities raising the local business tax. However, extrapolation from the data on the pre-reform period suggests no negative spillovers as the number and identity of the municipalities relying on the business surcharge remains stable over the years.

[8] Decree-Law no.37/1993 in Diário da República 37, Series I, February 13, 1993.

Finally, social learning could be one of the main reasons behind local business taxation; mayors are dealing with a new fiscal instrument with which they have no prior experience and little knowledge of the consequences. Local business taxation may thus result from both observation and experimentation by municipalities.

2.3 Data

This study relies on a data set comprising electoral and financial outcomes at the Portuguese municipal level for the period 2005–2013. This sample period encompasses three pre- and six post-reform years.[9] The longer post-reform period allows me to study the short- and long-run treatment effects. Moreover, the sample period includes three local elections taking place in 2005, 2009, and 2013; I use electoral outcomes to study electorally motivated interdependence in tax setting.

The main (outcome) variables in this study are the probability of levying the local business tax, the local business tax rate, and local business tax revenues. I measure the probability of raising the local business tax with a dummy that is equal to one for every municipality-year observation in which the tax rate is positive and zero otherwise. The local business tax rate corresponds to the rate chosen by the municipality in each year, and tax revenues refer to the log revenue from the local business tax rate accruing to each municipality. Data on local tax rates and local finances are available online at the General Directorate for Local Authority (*Direcção Geral das Autarquias Locais*) and the Tax and Customs Authority websites for the entire sample period. I deflate tax revenues and other flow variables used as controls, to the year 2015 using the national consumer price index from the World Economic Outlook Database of the International Monetary Fund. I use

[9] The year 2007 belongs to the pre-reform period since local business tax revenue collection occurs for the first time in 2008.

per capita values that I obtain relying on resident population per municipality from the Portuguese National Statistics Institute (INE).

Local election outcomes are also available online at the National Electoral Commission (*Comissão Nacional de Eleições*) and the General Directorate for Internal Affairs (*Direcção Geral da Administração Interna*) websites. Data report the number of votes and seats allocated to each of the parties running for the local elections in each municipality. I construct two main political variables: the probability of reelection and the victory margin. The first is a dummy equal to one if the incumbent party is reelected and zero otherwise. The second is the difference in the vote share between the winner and runner-up for each municipality and election.

For robustness, I rely on a number of control variables. The vector of control variables includes measures of municipal revenues, population size, and economic activity, as well as political dummies indicating whether there is a majority in the municipal council and if the mayor is left-leaning. Municipal revenue variables include revenues from other taxes and fees at the discretion of the local council as well as transfers from the central government. The political variables are defined based on the local electoral results for the municipal council. Municipal population size coincides with the resident population per municipality series from INE. Finally, the measure for municipal economic activity is proxied by night light output over the years under study for each municipality.

3 Identification Strategy

The introduction of the local business tax in Portugal provides an optimal setting to study local tax interactions. Contrary to other quasi-experimental studies, the reform has a direct impact on the municipalities under study. Yet a number of features must be taken into account. First, the fact that even though every municipality abides by the same rules, the

preexistence of a municipal business surcharge available only to some jurisdictions creates two distinct groups: one that has experience with local business taxation – even if in the form of a surcharge – and the other that the reform endowed with a local business tax instrument for the first time in 2007.

In all likelihood, the introduction of the local business tax did not have the same impact on the two sets of municipalities; the two groups are not comparable. In fact, the majority of municipalities collecting the surcharge in the pre-reform period transitioned into the new regime by simply substituting the surcharge with the tax – 149 of 161 municipalities. Figure (1) depicts the average trend in the tax rate for the two groups. In the pre-reform period, the municipalities relying on the surcharge chose, on average, a rate close to the upper bound of 10%, which translates into an effective tax of 2.5%. From 2008 onwards this set of municipalities, on average, substituted the surcharge with the local business tax at a rate close to the new maximum of 1.5%. The second group of municipalities, on the other hand, did not collect the surcharge and, in the aftermath of the reform, has an average tax rate that is only slightly above zero.

Due to selection concerns and to ensure comparability, I drop the municipalities that collected the municipal business surcharge from the analysis. This reduces the number of municipalities under study from 278 to 117 but allows for causal interpretation of the coefficient estimates under weaker identifying assumptions. Figure (2) shows the municipalities included in the sample. The excluded municipalities nonetheless play a crucial role in the identification of tax mimicking.

Since the reform entered into effect at the same time across the country, the identification strategy explores heterogeneity in treatment intensity to establish the control and treatment groups. For the purpose of this study, I rely on the importance that is placed on the similarity between municipalities for the existence of strategic interactions and the usual focus on geographic proximity. Thus, the exogenous geographic location

of a municipality in relation to an excluded jurisdiction determines the intensity of treatment; municipalities located relatively close suffer a higher treatment intensity in relation to distant municipalities.

Following Janeba and Osterloh (2013), who show how small jurisdictions interact with their neighbors, I rely on neighborliness; the municipalities under study are divided into treatment and control on the basis of whether they neighbor an excluded jurisdiction. For robustness purposes and to better grasp the dynamics in tax mimicking, I rely on three different measures of neighborliness. First, I define it along contiguous borders; treated municipalities share a common border with an excluded municipality. Second, in addition to sharing a common border, treated municipalities must lie within the same district as the neighboring excluded municipality.[10] This condition restricts the set of treated municipalities and increases comparability. Third, the treatment status depends on the kilometer distance between a municipality and its closest excluded neighbor. I use three critical distances: 10km, 20km, and 30km. In practice, a municipality is treated if its centroid lies within one of the three fixed distances of the centroid of the closest excluded municipality.

The identification strategy implicitly assumes that neighborliness is random. If that is the case, I can assign any significant difference that emerges between neighbors and non-neighbors to the exogenous difference in treatment intensity and interpret the results causally. Neighboring municipalities, however, could differ in their propensity to levy the local business tax. For example, they could suffer from negative economic spillovers coming from the municipalities collecting the surcharge. Yet, that these municipalities never had the need to collect the surcharge in the pre-reform period provides casual evidence of their indifference to negative spillover effects. Alternatively, they could be benefiting from

[10] An alternative but similar definition is to classify as neighbors only municipalities that in addition to a common border, belong to the same economic region as the excluded neighboring municipality. Both definitions lead to similar results.

positive spillover effects due to capital investments undertaken by the excluded municipalities. However, municipal investment can usually be targeted toward municipal residents. Finally, and more generally, there is no obvious reason for the timing of the reform to coincide with a common shock to the municipalities classified as neighbors; the reform was voted and exogenously imposed by the central government.

To support and test the identifying assumptions as well as guarantee a causal interpretation of the estimates, I rely on the outcomes from means comparison and balance checks in the spirit of Pei et al. (2016), that can be thought of as placebo tests, for an extensive list of municipal observable characteristics ranging from socio-economic to political variables.[11] Results show no significant differences between treatment and control for all variables under study.

Finally, to positively reduce concerns regarding omitted variable bias, I estimate regression models that always include municipality fixed effects. These control for invariant municipality-specific characteristics and should placate any further concerns with the exogeneity of neighborliness. In addition, I control for time effects through three different approaches: year fixed effects, district trends, and district-year fixed effects. The three methods control for time-specific shocks common to all municipalities, region-specific trends in the outcome variables, and the latter provide the most conservative estimation by controlling for unobservable district characteristics that vary over time.

4 Tax Mimicking

4.1 Empirical methodology

Within a diff-in-diff design, I exploit the heterogeneity in treatment intensity motivated by the exogenous geographic proximity to municipalities that collected the business surcharge

[11] See Tables A.1 and A.2 in the Appendix.

in the pre-reform period so as to identify interactions in tax setting among local governments. I rely on heterogeneous treatment intensity due to the lack of a clearly defined treatment and control group as a result of the reform. This approach allows me to not only causally identify strategic tax interactions but also to disentangle the effects of the introduction of the local business tax from other aspects of the 2007 reform.

In the first step of the empirical analysis, I study the existence of tax mimicking in local business taxation. In other words, whether neighbor and non-neighbor municipalities use the local business tax differently. Within a diff-in-diff methodology, the difference in the change in the outcome variable between treatment and control groups from the pre- to the post-reform period identifies the average treatment effect. Formally, let N_i be a dummy indicating treatment, equal to one for all neighboring municipalities and zero otherwise. Law no. 2/2007, regulating local business taxation entered into force retroactively on January 1, 2007, i.e., $t_0 = 2007$, but only implied local business tax revenue collection in 2008. Thus, let d_t be a time dummy indicating the post-reform period, that switches to one in 2008, such that $d_t = 1[t > t_0]$. Inference on strategic tax setting is causally identified within the following general diff-in-diff regression model:

$$Y_{it} = \gamma_i + \gamma_t + \delta(N_i \cdot d_t) + Z'_{it}\beta + \gamma_s \cdot t + \gamma_{st} + \epsilon_{it} \tag{1}$$

where Y_{it} refers to each of the outcome variables, $E_{it} = (N_i \cdot d_t)$ indicates treatment, i.e., the neighbor municipalities in the post-reform period, and Z'_{it} is the vector of control variables described in the previous section. The model includes municipality fixed effects, γ_i, and different versions of the model rely on year fixed effects, γ_t, district trends, $\gamma_s \cdot t$, or district-year fixed effects, γ_{st}. The parameter δ measures the average treatment effect identifying strategic tax setting. A positive and significant δ is evidence of tax mimicking.

In addition, I distinguish short- from long-run treatment effects by estimating the regression model using two different samples; one that restricts observations to the years 2005-2010, and another that relies on the entire sample period extending the analysis to 2013. To complement the study of average treatment effects, the following extension of the diff-in-diff regression model identifies the pattern of lagged effects by estimating yearly average treatment effects.

$$Y_{it} = \gamma_i + \gamma_t + \sum_{\tau=1}^{m} \delta_{-\tau} E_{i,t-\tau} + Z'_{it}\beta + \gamma_s \cdot t + \gamma_{st} + \epsilon_{it} \tag{2}$$

The sum in Equation (2) allows for m lags or post-treatment effects. The number of lags is $m = 6$, one for each of the six years of the 2008-2013 post-reform period. The remaining variables are defined as before.

4.2 Descriptive evidence and regression results

Figure (3) shows the development of the average business tax rate for treatment and control municipalities based on the within-district definition of neighborliness. This is the preferred definition of neighborliness for two reasons: one is contextual and the other empirical. The first reason is that it captures historical ties between jurisdictions. The second reason relates to the fact that it provides a better balance between treatment and control units in relation to the other measures – 80 to 37 jurisdictions.[12]

Neighboring municipalities, i.e., the ones more exposed to local business taxation, appear to be more likely to levy the tax after 2007. Control municipalities follow, to a smaller extent, in 2009. However, by 2012 both treatment and control municipalities charge similar average local business tax rates. To the extent that means are informative, there appears

[12] The within-economic region definition of neighborliness provides the same balance between treatment and control while the contiguous neighbors definition classifies 97 out of 117 municipalities as treated. The fixed distance definition of neighborliness defines 13, 70, and 97 out of 117 municipalities as treated for the 10km, 20km, and 30km fixed distances.

to be short-run tax mimicking in local business taxation in Portugal that dissipates with the increase in local business taxation by non-neighbors. The following regression results show whether it translates into significant causal evidence of strategic tax interactions.

Table 1 collects the regression results from estimating the most conservative version of Equation (1) – which includes municipality and district-year fixed effects, as well as the vector of control variables – relying on the contiguous and within-district definitions of neighborliness for the three outcome variables under study.[13] Models (I), (III), and (V) refer to the short-run analysis with sample period between 2005–10, while models (II), (IV), and (VI) rely on the full sample, from 2005–13, to assess the long-run treatment effects. For the within-district definition of neighborliness, all models include two mutually exclusive treatment dummies. The first identifies contiguous neighbors that lie within the same district as the neighboring excluded municipality, and the second indicates all municipalities that previously qualified as neighbors but no longer do, i.e., municipalities that, despite sharing a common border with an excluded municipality, belong to a different district.

Regression results provide evidence of tax mimicking in local business taxation. The magnitude and significance of the coefficient estimates are similar for the short- and long-run analysis for the contiguous neighbors classification. However, these results appear to be driven by same district neighbors in the short-, and different district neighbors in the long run. In the short run within-district neighbors are 14% more likely to levy the local business tax; the tax rate is, on average, significantly higher, by close to 0.2 percentage points; and they raise around 28% more revenues from local business taxation. In the long run, however, the magnitude of each of these effects drops for within-district neighbors

[13] All regression results in the paper are estimated within the most conservative specification of the regression models. Results are robust to different specifications, relying on year fixed effects and district trends; these are available upon request.

while it increases for different district neighbors, suggesting a spatial diffusion in local business taxation over time.

In Table 2, I collect the results for the alternative definition of neighborliness based on kilometer distance. Coefficient estimates rely on neighborliness within critical distances of 10km, 20km, and 30km as follows. A municipality is classified as a neighbor if its centroid lies within the radius of the specific critical distance from the centroid of the closest excluded municipality. To introduce a dynamic time dimension into this approach, the sample period is increasing in the critical distance. Model (I), defining treated municipalities as the ones found within 10km of an excluded municipality, relies on a sample restricted to only one post-reform year, encompassing the years 2005–2008. Model (II), where the critical distance is set to 20km, uses a sample encompassing two years of the post-reform period, relying on the period 2005–2009. Finally, models (III) and (IV) that rely on a critical distance of 30km, use the short- and long-run sample periods. The hypothesis is that the closer the municipality is to its excluded neighbor, the more exposed it is to local business taxation. I assume that the degree of exposure encompasses larger distances over time as more municipalities become familiar with the local business tax through either observation or experience. As before, the table provides the coefficient estimates for the most conservative version of Equation (1).

Coefficient estimates provide additional significant evidence of tax mimicking, reinforcing the previous results. The spatial and time dimensions of the distance regressions also suggest a diffusion in local business taxation from the closest to farthest neighboring municipalities over time. Coefficient estimates for the most restrictive definition of neighbor are of a higher magnitude and are more significant than for any of the other alternative definitions. Neighbors under the 10km critical distance definition are 27% more likely to levy the local business tax in the year after the introduction of the reform. Moreover, the tax rate is on average 0.4 percentage points higher and revenues are also significantly

higher. For the 20km and 30km distant neighbors, the magnitude and significance of the results are in line or slightly below the results obtained using the previous definitions.

Finally, Figure 4 shows the yearly coefficient estimates of the treatment effect obtained by estimating Equation (2) using the contiguous definition of neighborliness. The plots show the development of each of the three outcome variables for the entire sample period under study. Coefficient estimates are always significant until 2011; for the years 2012 and 2013 there is no longer a significant difference between neighbors and non-neighbors for any of the outcome variables. These results are, in all likelihood, due to the increase in local business taxation by control municipalities in the long run.

5 Underlying Mechanisms

5.1 Yardstick Competition

In their seminal work on political yardstick competition, Besley and Case (1995) formalize the Salmon (1987) model of horizontal competition by drawing the parallel between the franchised monopoly and regulator in Shleifer (1985) and the incumbent and electorate in local politics. Different industries, including the historical example of the activity-based reimbursement of hospitals, transport services such as railways in Japan or buses in Norway, or water utilities in Great Britain, apply yardstick competition as a regulatory mechanism. In local politics, however, evidence on yardstick competition is scarce.

The theory rests on performance benchmarking by the less informed party as a means to mitigate informational asymmetries, which in politics translates into retrospective voting (Salmon, 1987). Yet, the existing evidence on retrospective voting is inconclusive. In fact, the wide range of responsibilities that accrue to local governments is usually seen as an obstacle to voting retrospectively (Oliver and Ha, 2007; Berry and Howell, 2007).

Here I explore the link between strategic tax setting and electoral outcomes to test whether yardstick competition is the generating process behind local business taxation. Two questions guide the empirical analysis: first, who levies the local business tax?; and second, what is the impact of the tax on the incumbent's reelection probability and popularity?

In order to answer the first question, the inference is based on an extended version of the diff-in-diff model in Equation (1) including a second dummy variable indicating unpopular incumbents and its interaction with the main treatment variable $E_{i,t}$. I classify incumbents as unpopular on the basis of their margin of victory in the previous election and estimate the results for two degrees of unpopularity. To this end, I use two dummy variables, the first indicating mayors that won the previous election with a victory margin of up to 5%, and the second extending the cutoff to 10%.

Formally, let $U_{i,t}$ be the dummy indicating unpopularity, equal to one for all unpopular mayors and zero otherwise. Underlying this dummy variable is the victory margin in the preceding election, $v_{i,t-1}$, with treatment depending on the value of the latter as follows, $U_{i,t} = 1[v_{i,t-1} \in [0, h]]$, and $h = \{5, 10\}$. I estimate the average heterogeneous treatment effects within the following regression framework:

$$Y_{it} = \gamma_i + \delta E_{i,t} + U_{i,t}(\alpha + \rho E_{i,t}) + Z'_{it}\beta + \gamma_{st} + \epsilon_{it} \tag{3}$$

with all other variables defined as before. Table 3 provides the coefficient estimates. Results suggest that it is the popular mayors that engage in tax mimicking. Heterogeneous treatment effects for unpopular mayors are, in contrast to the average effect identified in the previous section, significantly negative.

To answer the second question, I restrict the sample to the local election years, 2005, 2009, and 2013. The outcome variables under study are the probability of reelection –

a dummy variable indicating whether an incumbent was reelected – and the incumbent's margin of victory.

I test the impact of local business taxation on the probability of reelection within the general diff-in-diff regression model in Equation (1), using the 2009 local elections as the short run and extending the analysis to the 2013 elections for the long-run results. Coefficient estimates, in panel A of Table 4, show no evidence of a consistent or significant impact of the tax variables on the probability of reelection for any of the elections.

I obtain an estimate of the impact of the three tax variables on the incumbent's popularity within a version of the extended diff-in-diff regression model in Equation (3) where the dependent variable is the incumbent's margin of victory and the two treatments are defined as follows. The first measures in turns each of the three different tax variables, while the second is a dummy variable indicating reelection. The heterogeneous treatment effects measure the extent to which the margin of victory of an incumbent is affected by tax policy conditional on reelection.[14] As before, regression results test for the short- and long-run treatment effects. Results in panel B of Table 4 show no significant impact of the local business tax on an incumbent's popularity. Heterogeneous effects are not significant for any of the tax variables or elections.

Overall, results suggest that local business taxation entails electoral concerns but no electoral consequences. In particular, popular mayors rely significantly more on the local business tax relative to unpopular mayors. The 2009 and 2013 local elections, however, show no significant impact of local business taxation on the probability of reelection. While this result could be driven by the fact that only popular mayors levy the tax, the impact of local business taxation on the margin of victory is also insignificant. The 2009 election results can nonetheless be explained by the fact that mostly treated municipalities relied on the local business tax, and were thus in line with the policies practiced in the neighboring

[14] I condition on reelection to track whether the local business tax has an impact on the margin of victory of the same party that applied the tax policy in the first place.

(excluded) jurisdictions. By the time of the 2013 elections, however, both treated and control municipalities relied on local business taxation, suggesting that voters did not punish mayors that did not conform to the benchmark set by neighboring jurisdictions; there is no evidence of retrospective voting based on local business taxation.

5.2 Tax Competition

Agrawal (2015) and Eugster and Parchet (2014) are thus far, to my knowledge, the only two quasi-experimental studies causally identifying tax competition. Both find that jurisdictions are constrained in their choice of tax policies by providing evidence of strategic tax interactions along state borders in the US and the Röstigraben in Switzerland. Their results are in line with the Bradford and Oates (1971) definition of tax competition, predicting that jurisdictions will produce too low a level of public goods due to a collapse of the tax rates.

This race to the bottom rationale, however, does not match the present evidence. In contrast, regression results point rather to a race to the top, usually the result of agglomeration economies (Baldwin and Krugman, 2004).[15] However, as shown in the means and balance tests, there appear to be no differences between treatment and control municipalities in terms of the overall economy suggesting that the agglomeration hypothesis is unlikely to explain the previous results. Thus, in the presence of tax competition, the increase in the local business tax by neighboring jurisdictions should drive out the mobile factors of production.

To test this hypothesis, I regress the following outcome variables on each of the tax variables under study; the municipal business volume, and the number of companies in each municipality. I collect the results for the short- and long-run for each of the outcome

[15] This section focuses on the literature on strategic complementarity in tax rates to conform to the previously identified treatment effects. However, a vast literature discusses tax rates as strategic substitutes. In fact, Parchet (2014) shows that tax rates are more likely strategic substitutes rather than complements.

variables in subfigures (a), (b), (c), and (d) of Figure 5.[16] Estimates are obtained within a fixed effects model that includes municipality and district-year fixed effects, as well as the vector of control variables previously described. The graphs show that none of the tax variables have an impact on the municipal business volume or the number of firms located in a municipality significantly different from zero. These results fail to confirm the existence of tax competition in the present setting.

5.3 Expenditure Competition

An alternative explanation for the regression results may be that local governments optimize over expenditures rather than tax rates (Koethenbuerger, 2011). Expenditure competition can lead to tax mimicking with increasing tax rates (Baicker, 2005). This hypothesis implies an increase in the expenditure level of municipalities levying the local business tax in the aftermath of the reform relatively to the pre-reform period and to municipalities not levying the tax.

To test this hypothesis I follow the same procedure as in the previous section but use as the outcome variable the log of per capita municipal current expenditures. I collect the results for the short and long run in subfigures (e) and (f) of Figure 5. As before, the graphs show no significant impact of levying the tax, the tax rate, or the revenues from the local business tax on the outcome variable under study. I thus fail to identify any evidence of expenditure competition in the present setting.

5.4 Negative Spillovers

Spillover effects are another source of strategic interdependence identified in the literature (Galletta, 2017); these can be positive or negative. Positive spillovers occur if a municipality can benefit from services provided by other municipalities. This applies to the full

[16] I provide the corresponding coefficient estimates in Table A.4 in the Appendix.

range of services provided by municipalities as long as these services are not restricted to residents. Positive spillover effects may create a negative dependence in tax rates as a higher expenditure incurred by one municipality reduces the spending need of another. Benefit spillovers are thus an unlikely candidate for generating tax mimicking at the local level.

Negative spillovers, on the other hand, can translate into a positive dependence in tax rates as municipalities need to counteract these externalities. For example, if a municipality invests in fighting crime by increasing the policing budget, criminals may move to neighboring jurisdictions thus creating the need for increased policing also in these. Or, if a municipality provides above average services, others may need to increase tax rates in order to compensate for the flight of residents to that municipality.

In this section, I study whether tax mimicking is motivated by negative spillover effects by excluding municipalities that are relatively more exposed to excluded municipalities. Arguably, in the presence of negative spillover effects the most affected jurisdictions, i.e., the ones with the strongest treatment intensity, should be those whose most neighbors used to levy the municipal business surcharge.

I re-estimate the most conservative version of Equation (1) for two different samples using the contiguous definition of neighborliness. In the first sample, I exclude all jurisdictions that are surrounded by excluded municipalities, i.e., whose every neighbor is excluded. The second sample follows the same logic but drops all municipalities whose neighbors are over 50% excluded municipalities. Regression results in panels A and B of Table 5 are in line with the baseline results, indicating that the treatment effects are not driven by specific municipalities that are overexposed to excluded jurisdictions. To this extent, I find no evidence that tax mimicking is a result of negative spillover effects.

5.5 Social Learning

Becker and Davies (2014) recently suggested a new explanation for tax mimicking based on social learning. Their learning-based model of tax policy diffusion relies on incomplete information, which can be overcome through the observation of policy choices by other agents. In contrast to yardstick competition, social learning should not entail electoral concerns.

Given the novelty of the local business tax for the municipalities included in the sample, mayors may have followed the policies of more experienced neighbors, thus leading to tax mimicking. Identifying social learning, however, is an empirical challenge. To my knowledge, only Glick (2012) provides anecdotal evidence based on interviews with colleges and university attorneys on the effects and determinants of policy knowledge diffusion through social learning.

In this section, I test the social learning hypothesis by assuming that policy diffusion is based on geographical location. Arguably, a closer geographical location to excluded municipalities translates to higher exposure to, and thus understanding of, the new tax instrument (Berry and Baybeck, 2005; Shipan and Volden, 2006). To test for social learning, in addition to identifying the neighboring municipalities as before, I also identify the new neighbors that emerge every time a municipality in the sample decides to levy the local business tax. Thus, every year, there is a new set of neighbors that according to the social learning literature should be more likely to impose a local business tax thereafter relative to the remaining non-neighboring municipalities.

Formally, I test for social learning by estimating the following extension of the baseline diff-in-diff regression model:

$$Y_{it} = \gamma_i + \gamma_t + \delta E_{i,t} + \sum_{\tau=2009}^{2013} \delta_\tau \, L_{i,\tau} + Z'_{it}\beta + \gamma_s \cdot t + \gamma_{st} + \epsilon_{it} \tag{4}$$

where the sum refers to dummy variables that correspond to each year from 2009 to 2013 and identify the new neighboring municipalities that are exposed to local business taxation. All remaining variables are defined as before.

I obtain the coefficient estimates in Table 6 within the most conservative specification of Equation (4). Models (I), (III), and (V), provide the short-run results, while models (II), (IV), and (VI), extend the analysis to 2013. Coefficient estimates do not support the social learning hypothesis as defined in this section. There is no evidence of a pattern in policy diffusion among the new yearly sets of neighbors.

6 Discussion

The introduction of a business tax at the Portuguese local level in 2007 provides the ideal laboratory for the study of strategic fiscal interactions among municipalities. I rely on a comprehensive database on electoral and fiscal outcomes for 117 municipalities over the period 2005–2013 and an identification strategy that rests on an exogenous reform, a quasi-experimental diff-in-diff methodology, and heterogeneity in treatment intensity to causally identify tax mimicking and its generating process.

The link between tax mimicking and electoral outcomes, followed by a long-run change in behavior consistent with the information provided by the local elections, points to yardstick competition in local business taxation. The business tax is a visible topic in local electoral campaigns despite its small economic size. It is possible that inexperienced mayors rely on both benchmarking and ensure room for damage control in terms of the vote margin. Voters, however, do not appear to judge the quality of the mayor based on business taxation. This information, made clear during the local elections, may have led to the general diffusion of local business taxation among jurisdictions. Apart from electoral concerns, I find no evidence of other generating processes of tax mimicking.

Local business taxation does not have a significant impact on municipal business volume or the number of firms located in a municipality. These results may be due to the small magnitude of the local business tax rate; when compared to the national corporate tax rate set at 25% of the taxable profit, the impact of the local tax on corporate decisions is arguably low as any tax differential between municipalities gives them little room to poach for each other's tax base. Also, municipalities can change the tax rate annually. Hence, any existing tax differentials may be short-lived, which reduces the incentives for companies to relocate. Moreover, the formula apportionment is based on the payroll ratio, which complicates the relocation process. Finally, small companies, that are more likely to be vulnerable to local business taxation, usually benefit from a reduced tax rate.

I also do not find significant evidence of expenditure competition nor negative spillover effects. These results may be explained by the magnitude and narrowness of the local business tax revenues. The absence of expenditure competition may simply be due to the fact that the revenues raised through the local business tax are of too small a magnitude to produce significant effects or motivate competition. The absence of spillover effects is probably due to the small magnitude of the revenues together with the fact that current expenditures are targeted and are unlikely to produce significant externalities.

Even though I do not find evidence of social learning as hereby defined, the setting under study provides a context of observation and experimentation by mayors. I do not claim that this is not undergoing, however, I discard this option on the basis that pure social learning should not entail electoral concerns that are clearly present.

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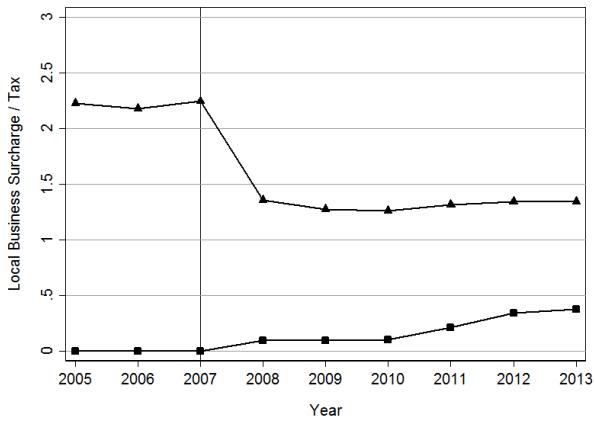


Figure 1: Municipal Surcharge and Local Business Tax. This figure provides a plot depicting the average trend in the municipal business surcharge and local business tax rate for two distinct groups of municipalities. The two groups are divided on the basis of whether they collected the surcharge in the pre-reform period.

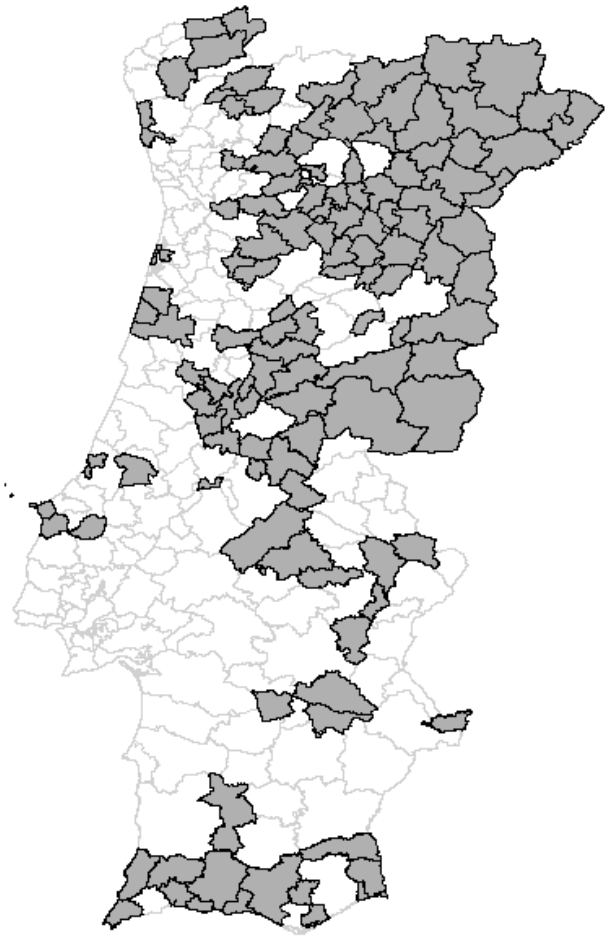


Figure 2: Municipalities included in the sample. This figure provides a map depicting the 117 municipalities that did not rely on the municipal business surcharge; these constitute the sample under study.

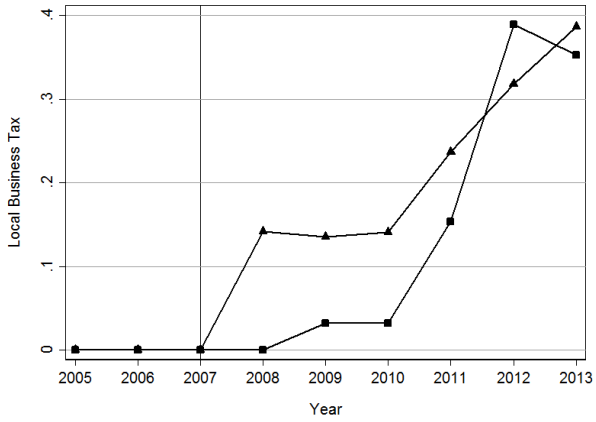
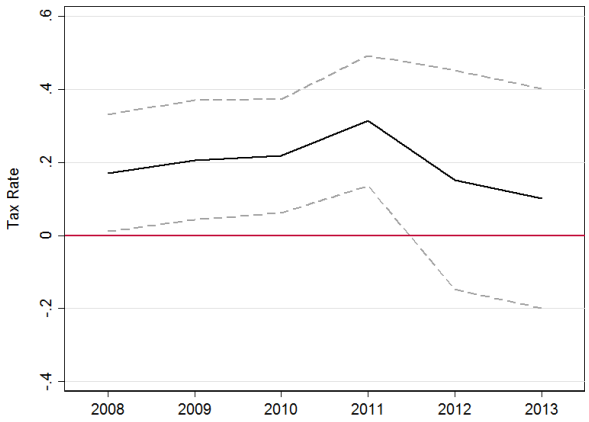
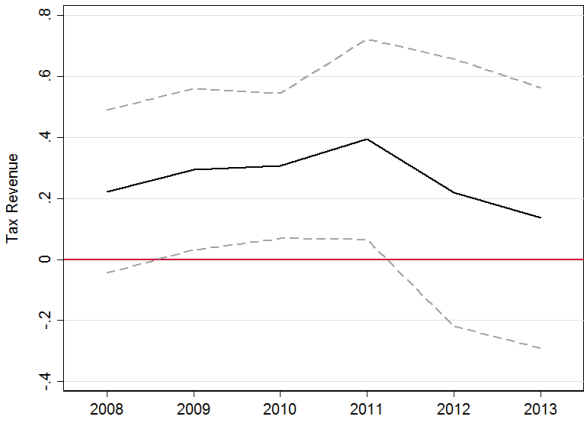


Figure 3: Local Business Tax. This figure provides a plot depicting the average trend in the local business tax rate in treated (triangle) and control (square) municipalities.

(a) Prob. Levy Tax

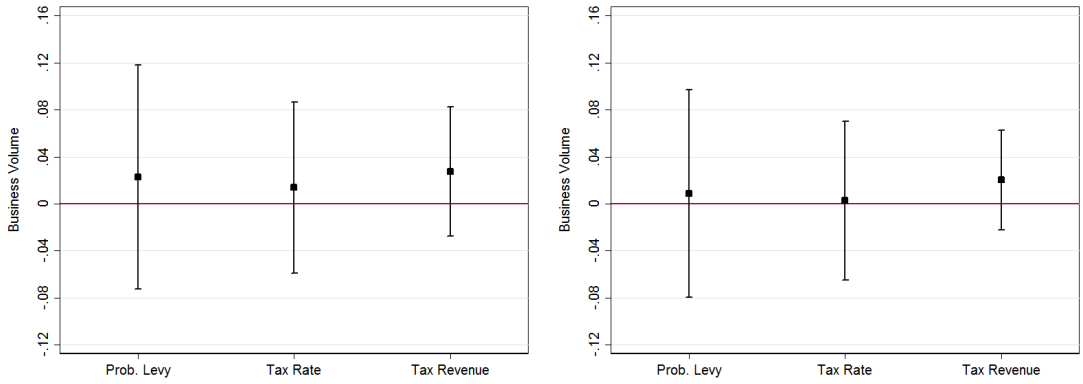


(b) Local Business Tax Rate



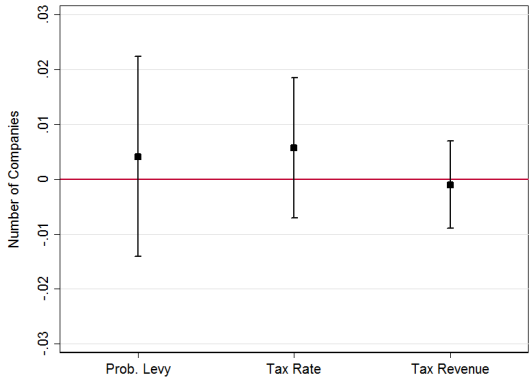
(c) Local Business Tax Revenue

Figure 4: Yearly Treatment Effects. This figure provides plots depicting the yearly average treatment effects for the three outcome variables: (a) the probability of levying the local business tax, (b) the local business tax rate, (c) the log of per capita local business tax revenue. Coefficient estimates are obtained within the most conservative version of Equation (2) that includes municipality and district-year fixed effects, and the vector of control variables. The dashed lines correspond to 95% confidence intervals.

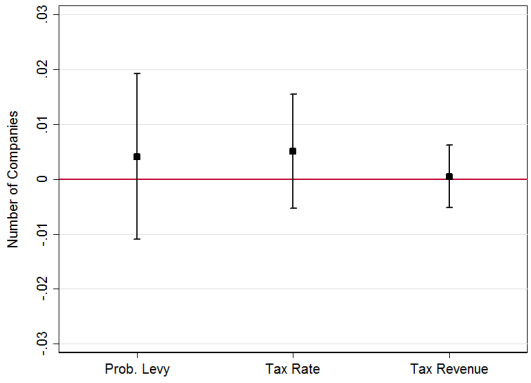


(a) Business Volume – Short-run

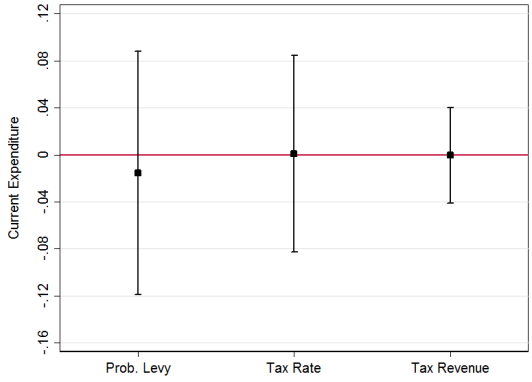
(b) Business Volume – Long-run



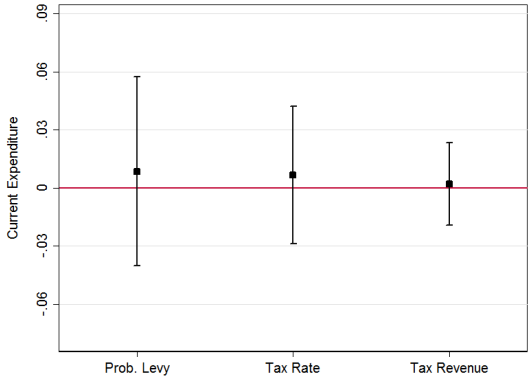
(c) No. of Firms – Short-run



(d) No. of Firms – Long-run



(e) Current Expenditure – Short-run



(f) Current Expenditure – Long-run

Figure 5: Tax and Expenditure Competition. This figure provides plots depicting the short- and long-run impact of levying the local business tax, the local business tax rate, and the local business tax revenues, on the municipal business volume (subfigures (a) and (b)), the number of firms located in a municipality (subfigures (c) and (d)), and the log of per capita of municipal current expenditure (subfigures (e) and (f)). Coefficients are obtained from the estimation of a fixed effects models regressing the outcome variables on each of the tax variables under study. All specifications include municipality and district-year fixed effects, and the vector of control variables. The capped lines correspond to 95% confidence intervals.

Table 1: Tax Mimicking: Contiguous and Within-District Neighbors

	Pr. Levy Tax		Tax Rate		Tax Revenue	
	I	II	III	IV	V	VI
			Panel A: Contiguous Neighbors			
Neighbors	0.135	0.132	0.196	0.193	0.269	0.262
	(0.053)	(0.060)	(0.078)	(0.086)	(0.124)	(0.137)
	[0.012]	[0.028]	[0.012]	[0.026]	[0.030]	[0.055]
			Panel B: Within-District Neighbors			
District	0.137	0.128	0.201	0.190	0.283	0.274
	(0.057)	(0.063)	(0.083)	(0.091)	(0.133)	(0.145)
	[0.016]	[0.040]	[0.016]	[0.036]	[0.034]	[0.059]
$\neq$ District	0.122	0.150	0.166	0.205	0.193	0.192
	(0.060)	(0.088)	(0.077)	(0.117)	(0.105)	(0.143)
	[0.043]	[0.088]	[0.031]	[0.079]	[0.065]	[0.180]
District x year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	2005-10	2005-13	2005-10	2005-13	2005-10	2005-13
Observations	702	1053	702	1053	702	1053
Municipalities	117	117	117	117	117	117

This table collects difference-in-differences regression results from the estimation of Equation (1) using the contiguous (Panel A) and within-district (Panel B) definitions of neighborliness. The dependent variables are indicated in the first row. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table 2: Tax Mimicking: Fixed Critical Distance Neighbors

	Short-run			Long-run
	I	II	III	IV
			Panel A: Prob. Levy Tax	
Neighbors	0.272	0.112	0.115	0.118
	(0.111)	(0.059)	(0.052)	(0.062)
	[0.014]	[0.059]	[0.029]	[0.058]
			Panel B: Tax Rate	
Neighbors	0.396	0.165	0.167	0.168
	(0.164)	(0.084)	(0.077)	(0.091)
	[0.016]	[0.051]	[0.030]	[0.063]
			Panel C: Tax Revenue	
Neighbors	0.714	0.114	0.180	0.200
	(0.345)	(0.111)	(0.101)	(0.117)
	[0.039]	[0.308]	[0.076]	[0.087]
District x Year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Sample Period	2005-2008	2005-2009	2005-2010	2005-2013
Distance	10km	20km	30km	30km
Observations	468	585	702	1053
Municipalities	117	117	117	117

This table collects difference-in-differences regressions results from the estimation of Equation (1) using the fixed critical kilometer distance definition of neighborliness. The dependent variables are indicated above the coefficient estimates. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table 3: Yardstick Competition: Popular Vs. Unpopular Mayors Heterogeneous Effects

	Pr. Levy Tax		Tax Rate		Tax Revenue	
	I	II	III	IV	V	VI
Neighbors · Margin	-0.154	-0.109	-0.226	-0.152	-0.312	-0.186
	(0.068)	(0.051)	(0.088)	(0.069)	(0.143)	(0.118)
	[0.024]	[0.032]	[0.010]	[0.028]	[0.029]	[0.114]
Neighbors	0.162	0.166	0.238	0.241	0.324	0.323
	(0.062)	(0.061)	(0.090)	(0.089)	(0.141)	(0.130)
	[0.009]	[0.007]	[0.008]	[0.007]	[0.021]	[0.013]
Margin	0.073	0.027	0.113	0.037	0.227	0.117
	(0.064)	(0.047)	(0.085)	(0.064)	(0.156)	(0.102)
	[0.255]	[0.570]	[0.184]	[0.568]	[0.145]	[0.251]
District x year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	2005-13	2005-13	2005-13	2005-13	2005-13	2005-13
Vote Margin	5	10	5	10	5	10
Observations	1053	1053	1053	1053	1053	1053
Municipalities	117	117	117	117	117	117

This table collects difference-in-differences regression results from the estimation of the heterogeneous effects model in Equation (3) using the contiguous definition of neighborliness and identifying mayors that won the previous election with a 5% or 10% victory margin. The dependent variables are indicated in the first row. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table 4: Yardstick Competition: Impact of Taxation on Electoral Outcomes

	Pr. Levy Tax		Tax Rate		Tax Revenue	
	I	II	III	IV	V	VI
			Panel A: Probability of Reelection			
Tax	-0.025	0.137	0.004	0.106	-0.011	0.073
	(0.167)	(0.092)	(0.115)	(0.066)	(0.069)	(0.041)
	[0.879]	[0.136]	[0.975]	[0.108]	[0.876]	[0.075]
			Panel B: Incumbent Margin of Victory			
Tax · Reelection	-0.001	0.020	-0.006	0.014	0.016	0.009
	(0.045)	(0.018)	(0.040)	(0.013)	(0.020)	(0.009)
	[0.987]	[0.249]	[0.873]	[0.271]	[0.426]	[0.322]
Reelection	0.025	0.012	0.025	0.011	0.023	0.012
	(0.013)	(0.011)	(0.013)	(0.011)	(0.012)	(0.011)
	[0.051]	[0.283]	[0.050]	[0.287]	[0.058]	[0.270]
Tax	0.028	-0.004	0.028	-0.001	0.009	-0.002
	(0.031)	(0.011)	(0.031)	(0.008)	(0.013)	(0.006)
	[0.366]	[0.735]	[0.372]	[0.854]	[0.476]	[0.743]
District x Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	2005-09	2005-13	2005-09	2005-13	2005-09	2005-13
Observations	234	351	234	351	234	351
Municipalities	117	117	117	117	117	117

This table collects regressions results from the estimation of fixed effects models regressing the outcome variables (Panel A) probability of reelection and (Panel B) the incumbent's victory margin on each of the tax variables in the first row. Panel B studies the impact of the tax variables conditional on reelection. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table 5: Negative Spillover Effects

	Pr. Levy Tax		Tax Rate		Tax Revenue	
	I	II	III	IV	V	VI
			Panel A: Exclude 100% surrounded municipalities			
Neighbors	0.144	0.147	0.208	0.214	0.286	0.291
	(0.057)	(0.062)	(0.082)	(0.090)	(0.129)	(0.140)
	[0.011]	[0.018]	[0.011]	[0.017]	[0.027]	[0.038]
Observations	654	981	654	981	654	981
Municipalities	109	109	109	109	109	109
			Panel B: Exclude 50% surrounded municipalities			
Neighbors	0.127	0.138	0.184	0.204	0.192	0.212
	(0.058)	(0.065)	(0.085)	(0.095)	(0.106)	(0.124)
	[0.028]	[0.034]	[0.030]	[0.031]	[0.070]	[0.088]
Observations	420	630	420	630	420	630
Municipalities	70	70	70	70	70	70
District x Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	2005-10	2005-13	2005-10	2005-13	2005-10	2005-13

This table collects difference-in-differences regressions results from reestimating Equation (1) using the contiguous definition of neighborliness and samples that exclude municipalities that are (Panel A) 100% or (Panel B) over 50% surrounded by excluded municipalities. The dependent variables are indicated in the first row. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table 6: Social Learning

	Pr. Levy Tax		Tax Rate		Tax Revenue	
	I	II	III	IV	V	VI
Neighbors	0.118	0.120	0.175	0.175	0.250	0.253
	(0.050)	(0.054)	(0.073)	(0.079)	(0.127)	(0.134)
	[0.018]	[0.026]	[0.017]	[0.027]	[0.049]	[0.060]
2009	-0.070	-0.111	-0.094	-0.157	-0.058	-0.063
	(0.054)	(0.059)	(0.077)	(0.084)	(0.109)	(0.125)
	[0.193]	[0.060]	[0.222]	[0.062]	[0.596]	[0.613]
2010	-0.116	0.217	-0.123	0.285	-0.240	0.066
	(0.066)	(0.121)	(0.083)	(0.132)	(0.117)	(0.172)
	[0.080]	[0.072]	[0.139]	[0.031]	[0.040]	[0.700]
2011		-0.087		-0.131		-0.168
		(0.096)		(0.142)		(0.278)
		[0.367]		[0.354]		[0.545]
2012		0.006		0.013		0.072
		(0.188)		(0.281)		(0.490)
		[0.974]		[0.964]		[0.883]
2013		-0.099		-0.148		-0.055
		(0.171)		(0.257)		(0.431)
		[0.563]		[0.565]		[0.899]
District x Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	2005-10	2005-13	2005-10	2005-13	2005-10	2005-13
Observations	702	1053	702	1053	702	1053
Municipalities	117	117	117	117	117	117

This table collects difference-in-differences regressions from the estimation of Equation (4) using the contiguous definition of neighborliness. The dependent variables are indicated in the first row. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table A.1: Means Comparison

		Treatment	Control	Difference		
Income Tax Rev.	Mean	2.660	2.693	-0.034	t-Statistic	-0.354
	Std. error	0.046	0.075	0.095	p-Value	0.724
Property Tax Rev.	Mean	1.164	0.999	0.165	t-Statistic	0.422
	Std. error	0.190	0.316	0.392	p-Value	0.674
Municipal Taxes	Mean	3.989	3.779	0.210	t-Statistic	1.470
	Std. error	0.065	0.143	0.143	p-Value	0.144
Total Tax Rev.	Mean	4.738	4.551	0.187	t-Statistic	1.157
	Std. error	0.075	0.153	0.161	p-Value	0.250
Municipal Fees	Mean	2.594	2.621	-0.027	t-Statistic	-0.179
	Std. error	0.069	0.142	0.149	p-Value	0.858
General Transfer	Mean	5.586	5.799	-0.213	t-Statistic	-1.127
	Std. error	0.092	0.145	0.189	p-Value	0.262
Specific Transfer	Mean	2.778	2.782	-0.004	t-Statistic	-0.048
	Std. error	0.046	0.050	0.090	p-Value	0.962
Total Current Rev.	Mean	6.584	6.694	-0.110	t-Statistic	-1.332
	Std. error	0.040	0.064	0.082	p-Value	0.185
No. Companies	Mean	1605.613	1248.333	357.280	t-Statistic	0.924
	Std. error	193.611	263.728	386.869	p-Value	0.358
Sales Volume	Mean	240783.3	153075.8	87707.98	t-Statistic	1.239
	Std. error	35863.39	44150.19	70788.52	p-Value	0.218
Unemployment	Mean	551.946	432.222	119.724	t-Statistic	0.951
	Std. error	63.676	79.932	125.961	p-Value	0.344
Income	Mean	3763.151	3865.538	-102.388	t-Statistic	-0.166
	Std. error	303.208	435.541	618.656	p-Value	0.869
Population	Mean	15798.51	11881.3	3917.209	t-Statistic	1.300
	Std. error	1540.365	1734.384	3012.822	p-Value	0.196
Left Council	Mean	0.341	0.370	-0.030	t-Statistic	-0.282
	Std. error	0.050	0.095	0.105	p-Value	0.778
Vote margin	Mean	0.219	0.188	0.030	t-Statistic	0.901
	Std. error	0.017	0.025	0.034	p-Value	0.370
Council vote share	Mean	0.561	0.571	-0.010	t-Statistic	-0.472
	Std. error	0.010	0.014	0.020	p-Value	0.638
Assembly vote share	Mean	0.564	0.583	-0.019	t-Statistic	-1.019
	Std. error	0.010	0.013	0.019	p-Value	0.311

This table collects a means comparison between treatment and control municipalities for a number of socio-economic and political observable characteristics.

Table A.2: Balance Tests

	Coefficient	Std. error	P-value
Income Tax Rev.	-.038313	.1045735	0.715
Property Tax Rev.	.1636475	.1447154	0.260
Municipal Taxes	-.038313	.1045735	0.715
Total Tax Rev.	.1570757	.1433318	0.275
Municipal Fees	.0611588	.1466561	0.677
General Transfer	-.2213259	.1722291	0.201
Specific Transfer	-.0096699	.0687058	0.888
Total Current Rev.	-.1115116	.0752514	0.141
No. Companies	378.8222	328.8378	0.252
Sales Volume	89569.24	57260.02	0.120
Unemployment	126.7778	102.7999	0.220
Income	-49.64957	532.2059	0.926
Population	4114.559	2337.539	0.081
Left Council	-.0259259	.1064878	0.808
Vote margin	.0280969	.0298757	0.349
Council vote share	-.0098216	.0168391	0.561
Assembly vote share	-.0191169	.0160857	0.237

This table collects balance tests between treatment and control municipalities for a number of socio-economic and political observable characteristics.

Table A.3: Tax Mimicking: Within Economic Region Neighbors

	Pr. Levy Tax		Tax Rate		Tax Revenue	
	I	II	III	IV	V	VI
Region	0.136	0.127	0.197	0.185	0.266	0.247
	(0.054)	(0.061)	(0.079)	(0.088)	(0.125)	(0.136)
	[0.012]	[0.037]	[0.013]	[0.035]	[0.033]	[0.071]
$\neq$ Region	0.123	0.189	0.184	0.286	0.300	0.440
	(0.061)	(0.099)	(0.088)	(0.146)	(0.141)	(0.248)
	[0.043]	[0.055]	[0.038]	[0.050]	[0.033]	[0.076]
District x Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	2005-10	2005-13	2005-10	2005-13	2005-10	2005-13
Observations	702	1053	702	1053	702	1053
Municipalities	117	117	117	117	117	117

This table collects difference-in-differences regression results from the estimation of Equation (1) using the within economic region definition of neighborliness. The dependent variables are indicated in the first row. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table A.4: Tax and Expenditure Competition

	Expenditure		B. Volume		No. companies	
	I	II	III	IV	V	VI
Levy Tax	-0.016	0.009	0.023	0.009	0.004	0.004
	(0.053)	(0.025)	(0.049)	(0.045)	(0.009)	(0.008)
	[0.766]	[0.730]	[0.642]	[0.842]	[0.656]	[0.593]
Tax Rate	0.001	0.007	0.014	0.003	0.006	0.005
	(0.043)	(0.018)	(0.037)	(0.035)	(0.007)	(0.005)
	[0.986]	[0.709]	[0.710]	[0.937]	[0.377]	[0.331]
Tax Revenue	-0.001	0.002	0.028	0.020	-0.001	0.001
	(0.021)	(0.011)	(0.028)	(0.022)	(0.004)	(0.003)
	[0.980]	[0.853]	[0.325]	[0.349]	[0.806]	[0.856]
District x Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample Period	2005-10	2005-13	2005-10	2005-13	2005-10	2005-13
Observations	702	1053	702	936	701	935
Municipalities	117	117	117	117	117	117

This table collects regressions results from fixed effects models regressing the outcome variables in the first row – the log of per capita municipal current expenditures, municipal business volume, and the number of firms located in a municipality – on each of the tax variables under study on the first column. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.

Table A.5: Yearly regressions

	Pr. Levy Tax	Tax Rate	Tax Revenue
2008	0.112	0.171	0.217
	(0.054)	(0.082)	(0.135)
	[0.041]	[0.036]	[0.108]
2009	0.141	0.206	0.291
	(0.057)	(0.083)	(0.134)
	[0.013]	[0.013]	[0.029]
2010	0.156	0.218	0.303
	(0.057)	(0.079)	(0.121)
	[0.006]	[0.006]	[0.013]
2011	0.230	0.314	0.396
	(0.066)	(0.091)	(0.169)
	[0.000]	[0.001]	[0.019]
2012	0.096	0.152	0.228
	(0.107)	(0.153)	(0.225)
	[0.370]	[0.319]	[0.311]
2013	0.058	0.102	0.141
	(0.106)	(0.154)	(0.219)
	[0.586]	[0.507]	[0.520]
District x Year	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	1053	1053	1053
Municipalities	117	117	117

This table collects difference-in-differences yearly regressions results from the estimation of Equation (2) using the contiguous definition of neighborliness. The dependent variables are indicated in the first row. All specifications include municipality fixed effects. Heteroscedasticity robust standard errors clustered at the municipal level in parentheses. P-values in brackets.