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Abstract
This paper studies the impact of municipal non-residential property taxation on firms' performance using a panel data of Italian manufacturing firms in 2001-2010. In the spirit of Duranton et al. (2011), we use a pairwise spatial difference instrumental variable estimator which allows to tackle the endogeneity of local taxation. As well as providing robust inference to arbitrary cross-sectional dependence and serial correlation, our empirical strategy also improves on existing work by exploiting the exogenous variation in local taxes generated by the political alignment of each jurisdiction with the central government. We find that non-residential property taxation exert a negative impact on firms' employment, capital and sales to such an extent as to significantly affect total factor productivity.

JEL classification: H22, H71, R38

Keywords: Local taxation, endogeneity, spatial differencing, generalized method of moments, two-way clustering.

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1 Introduction

The empirical literature on the effect of local taxation on resource allocation is extensive.[1] However, while the conceptual logic of local tax competition is clear and supported by a large body of theoretical literature, the estimation of the impact of taxation is fraught with uncertainty. In particular, Duranton et al. (2011) highlights three main issues. Firstly, bias may arise from the fact that many of the site's characteristics leading to the firm's choice on where to localize the plant are unobservable by the analyst but they are likely to be correlated with firm's characteristics and local taxation. Second, firm's unobserved heterogeneity is likely to provide another source of bias. Third, a reverse causality bias may arise due to the likely correlation between firm decisions and many aspects of the tax system.

By exploiting firm level panel data in which firms are geo-localized through postcodes, they propose a pairwise spatially differenced instrumental variable estimator (PSD-IV) that allows to solve the aforementioned issues by ruling out both time invariant firm-specific and time varying site-specific unobserved heterogeneity.[2] They also show that, by conditioning out local characteristics, spatial differencing is the key to make plausible the exclusion restriction associated with their identification strategy. By instrumenting the tax rate through the share of local politicians affiliated with the three main political parties, they find a high and negative elasticity of employment to the tax rate (i.e., a growth slow-down effect) and no evidence of selection due to non-residential property tax.[3]

In this paper, we revisit the estimation of the effect of local non-residential property taxation on firm performance exploiting a geo-coded panel of italian firms for the period 2001-2010, making the following contributions. First, even thought labor can be considered a good indicator of firm growth from a short run perspective, oversizing firms may be inefficient and are likely to face a productivity slow-down in the medium run (Guiso and Rustichini, 2010). Furthermore, firms may improve their performances without increasing their labor force but just re-organizing or innovating their production process. In order to provide a more complete picture of the effect of local taxation on firms' behavior, we consider a wider range of firms' outcomes by looking not only at labor but also at capital, sales and total factor productivity

[1] For a comprehensive review, see Bartik (1991) and for more recent references on empirical local public finance see Revelli (2015).

[2] We define as PSD-IV the estimator obtained by applying IV after a within-group (over firms) and a spatial difference (between paired firms) transformations. See Section 4 for more details.

[3] Local taxation represents a cost that can be reduced by moving production facilities to a new location characterized by a lower tax rate. However, if a firm choose to relocate, then it will face the cost of moving its assets to the new location. Clearly, if relocation costs are higher than local taxation costs regardless of the location, a firm will linger in its original location suffering what Duranton et al. (2011) define as slow-down effect; while if it relocates this will cause the so-called "selection" effect. Indeed, movers are likely to be the most efficient firms and will tend to relocate in low tax rate jurisdictions. We provide a simple test of selection-into-treatment finding no significant evidence of selection effect in our sample (see Appendix A.1) for details.

(TFP).[4]

Second, we provide a novel identification strategy for the impact of local taxation based on the political alignment between municipal governments and the central one.[5] This strategy has been recently used by Bracco et al. (2015) to trace the effect of grants on local taxes and expenditures. They find that italian municipalities receive 47% more grants in presence of political alignment, and that about 60% of these extra grants are used to reduce local taxes. This evidence, together with the descriptive analysis presented in Section 4.2.1, strongly suggest that political alignment is indeed correlated with local non-residential property tax. Noteworthy, right-wing aligned jurisdictions show lower tax rates than non-aligned ones. Moreover, since political alignment is unlikely to be correlated with firm level outcomes, we argue that it generates an exogenous variation in local tax rates that can be used to identify a local average treatment effect (i.e., LATE, Angrist and Imbens, 1994).

Third, even though Duranton et al. (2011) provide an expression for the asymptotic variance of the PSD-IV estimator that explicitly controls for the particular cross-sectional dependence of the errors induced by the spatial difference transformation, the authors correctly acknowledge that their inference is not robust to other forms of heteroskedasticity, serial correlation and cross-sectional dependence. Since we find strong evidence of heteroskedasticity and serial correlation in the data, we base our inference on efficient generalized method of moments estimates and two-way clustered standard errors (Cameron and Miller, 2015).

Fourth, since agglomeration forces are likely to generate rents that local governors can exploit by raising taxation (Baldwin and Krugman, 2004), we split our sample according to the degree of urbanization in order to provide a test for the presence of agglomeration rents.[6] Finally, we exploit geo-coded information taking into account both administrative boundaries and accurate firms' geographical coordinates (latitude and longitude), thus reducing the bias due to a suboptimal firms' georeferentiation.[7]

The main empirical findings are as follows. We find that the semi-elasticities of the considered outcomes to local taxation are always negative and significant, specifically about -0.3 for capital, -0.11 for employment, -0.14 for TFP and -0.21 for sales. These results are robust to a number of different specifications and instrumenting strategies.

The remainder of this paper is organized as follows. The next section is devoted to describe the italian institutional setting, while Section 3 presents our data with summary statistics. We reference on our empirical strategy in Section 4 while Section 5 discusses our empirical findings. Finally, Section 6 offers some concluding remarks.

[4] See Section 4.1 for some theoretical arguments that could clarify our line of reasoning.

[5] From now on, we refer to the political alignment of municipal governments with the central government just as political alignment.

[6] We find a rather strong evidence that denser italian jurisdictions set higher tax rates. See Section 3 for details.

[7] We provide a discussion of these issues in Section 3.

2 Institutional Framework

Italy is organized as a three-tier system of sub-national governments, whose layers are respectively regions, provinces and municipalities. The 8.100 municipalities are the smallest administrative units (on average each of them lay on area of 14 square miles) with delegated fiscal power. This makes them an ideal environment to test for spatial interactions and informational externalities which, intuitively, are more likely to arise the smaller is the scale of the units under observation. Municipalities are multi purpose governments, their major functions being registry, waste disposal, urban planning, illumination, road maintenance, local transports, social aid, child care, primary schooling, and assistance to the elderly. Expenditures are financed by grants (roughly by 45 per cent), own taxes (30 per cent) and other own revenues (such as tariffs, fees and penalties). Municipal taxes are manifold, but the major one is undoubtedly the local property tax (*Imposta Comunale sugli Immobili*, ICI), which accounts for more than half of total tax revenues; other important funding sources are the taxes on solid waste management (which works fairly as a tariff rather than a tax, covering a given share of waste).

Our analysis focuses on local property taxation, which represent the main leverage of the municipal fiscal power. This tax has a rate spanning from 0.4 to 0.7 percentage points of the tax base (which is a function of property's cadastral values and squared meters) and is differentiated between residential and business properties. We focus on non-residential property tax concerning business properties. In this case, cadastral values are set taking into account also bolted heavy machineries (e.g., blast-furnaces, hydraulic presses, etc.) making this tax a classical tax on capital.

The ability to set business property tax rates was probably the only expression of municipal fiscal powers in the period under observation since alternative forms of taxing powers, namely the ability to manoeuvre the surcharge on national personal income tax (IRPEF), were hampered by national legislation. Finally, since residential ICI has de facto been abolished in 2008 and proposals for a future revision of municipal financing structure point to business properties as the principal source for local taxation, our analysis assumes even more relevance. At regional level another important tax is levied, the business tax (IRAP), a proportional tax on value added mainly financing the italian national health system. It is worth noting that while this tax was the same at national level until 2008, some regions (Abruzzo, Campania, Lazio, Molise and Sicilia) increased the tax rate in order to adjust their fiscal budget starting from 2009.

3 Data and summary statistics

In order to implement the methodology described in Section 4, we build our data set merging information from several sources. The resulting sample is an unbalanced panel containing balance sheet information for a sample of italian manufacturing firms for the period 2001-2010 as well as information on firms' geographical coordinates (latitude and longitude). It is worth noting that such refined data helps to reduce the bias due to inaccurate firm geo-referentiation, as is the case when firms are referenced using zoning system centroids (e.g., zip-codes, postcodes, etc.).

The negative effect of the zoning system on statistical results is known as Modifiable Unit Area Problem (MAUP). The bias induced by scale and shape effects is minimized if the units are: identical (shape, size and neighboring structure) and spatially independent (Arbia, 1989). Two very difficult prerequisites to meet in practice.[8] Figure 1 reports a simple example with four jurisdictions (e.g., municipalities, regions, etc.) and five firms: in panel (a) each firm is paired across jurisdictions by their Euclidean distance using accurate geographical coordinates; in panel (b) each jurisdiction is divided into smaller areas (postcodes) and firm location is determined using the area's centroid. Even assuming a fairly homogenous size distribution, it is evident how the zoning scheme affects the distance between firms. If the objective is to pair firms based on their distance, this will clearly affect the pairing process and, as a consequence, the final estimation sample. Actually, postcode areas are highly heterogeneous in both size and borders shape implying that a postcode-based pairing process may be seriously affected by the MAUP.

We focus our analysis to the smallest administrative italian entity, the municipality, passing over any compound treatment irrelevancy (Keele and Titiunik, 2014). For each municipality and year, we have information on the population and on the non-residential property tax imposed by the local authority. We further merge information on the results of local and national elections, which we exploit to build our identification strategy (see Section 4.2.1).

3.1 Firm level data

Firm level data are obtained from the AIDA dataset, provided by Bureau Van Dijk[9]. We use the AIDA Top version which contains information on companies with a turnover above 1,5 millions of euro. This implies that small and medium enterprises are likely to be under-represented with respect to bigger firms. For example, in 2010 manufacturing firms with less

[8] Briant et al. (2010) show that the size distribution of geographical units affects statistical results, especially when the dependent variable is not aggregated at the same level. However, their results using french employment areas also indicate that the bias induced by the MAUP is of second order with respect to model miss-specification. On the other hand, Menon (2012) exploit MAUP to evaluate the significance of economic concentration in US travel regions using randomly generated spatial units as control.

[9] http://www.bvdep.com.

than 50 employees account for 28% of total turnover in our data, against the 32% reported in the aggregate national statistics. The complete dataset contains the exact (geo-referenced) location along with balance sheet information of over 275 thousand italian companies, in this version financial accounts of subsidiaries are not consolidated in the corporate one. Firms are classified according to the main sector of activity (NACE rev 2), we focus our attention on the manufacturing sector, namely to firms ranging from 2-digit NACE10 to NACE33. After discarding observation with missing data we end up with an unbalanced panel of 21603 manufacturing firms for the period 2001-2010.[10] Looking at the sectorial composition, our data seems to give a fairly good representation of italian's aggregate production structure, the share of employment, turnover and value added by industry (2-digit) correlates above 90% with the shares computed using manufacturing firms population from the italian statistical institute (ISTAT). Moreover, if we look at the sector, province and year shares the distribution of firms in our data correlates at 82% with the official statistics on the overall number of active firms.[11]

Since we only observe firms, not plants, and that our main variable of interest is the local tax rate, multi-plant enterprisers are a cause of concern. Noteworthy the phenomenon of multi-plant firms in Italy is relatively modest, in 2010, according to ISTAT more than 90% of manufacturing firms have only one production plant. Another potential issue is related to firms relocation, that we do not observe directly in the data. Tracking movement of firms across the italian territory is not straight forward, we can only infer the order of magnitude of the phenomenon from the Chambers of Commerce, that maintain the register of all active enterprisers by Province (NUTS3). In 2010, the first year for which the information is available, the Chambers of Commerce recorded around 411 thousand new economic entities, but among them only 213 thousand were "true" new born firms, the remaining registrations updates were due to merge and acquisitions, changes in the juridical status and relocations.[12]Considering that the stock of active firms in 2010 was over 6 million enterprises, such changes, and among them relocation, involved around 3.2% of the total.[13]

An advantage of our dataset is that we can test the effect of the local taxation on employment, but we can also evaluate the effect on firms sales and TFP, extending previous results to other aspect of firm's behavior. We consider a "value added" TFP using the Levinsohn and

[10]We also drop observation with implausible negative values for sales and value added, and keep only firms that stay in the dataset for at least three years.

[11]Those data are provided by the National Chamber of Commerce, *Unioncamere*.

[12]Mayer et al. (2015) study the impact of a French enterprise zone program on establishment location decisions and on labor market outcomes. They show that conditional on locating in a municipality that hosts a ZFU, the policy has a positive and sizeable impact on the probability to locate in the ZFU part rather than in the non-ZFU part of municipalities. However, the impact is highly heterogeneous across zones. Most importantly and in line with our assumptions they show that this positive effect is entirely due to within-municipality diversion effects. They do not find any relocation effect across municipality.

[13]Those figures refers to the whole economy, we do not have detailed information on the manufacturing sector.

Petrin (2003) semi-parametric approach, controlling for unobservable shocks through intermediary and energy inputs. Since book values for value added, fixed assets and intermediary inputs are in nominal terms we use OECD industry specific deflators. Looking at the TFP is particularly interesting given the fact that the non-residential property tax considered here applies to production building and, as such, it can be seen as a tax on fixed capital (see next section for a detailed description of the property tax).

Table 1 reports the descriptive statistics for the main variable used in the estimation, where the sample includes only firms that have at least one neighbor in a different municipality within a 3 km range. Couples are defined within the same 2-digit Nace rev 2 manufacturing sector[14]. Sampled firms spread across 2429 different municipalities and 331 Local labor Systems (out of 750). The average employment size is 60 workers, while the median is around one third the mean value. Similarly, the sales distribution appears to be right-skewed.

3.2 Local taxation

Data on the local property tax are from the italian Minister of Economics and Finances. The property tax rate is defined independently by each municipality and represents one of the main source of financing for local administrations: in 2010 around 45% of total tax revenues was represented by property tax revenues. The effective tax burden results from the application of the effective rate to the property rent. The rent is proportional to the size of the building and the land appraisal, the latter is based on census areas, providing that in each municipality there may be several census areas.[15] This may be a cause of concern for our identification strategy. Interestingly, the first introduction of land appraisal in Italy dates back to 1939.[16] Since their introduction, land values have been revised only once in 1990. Given that our period of interest is 2001-2010 and that we consider only firms within a range of less than 3 km, land values should be completely captured by firms fixed-effects.

Figure 2 reports the frequency distribution of tax rates set by italian municipalities in year 2001 and 2010. The imposed rate spread from 0.4 to 0.7 percent, with a relatively higher frequency around the values from 0.5 to 0.6 percent. Over time, the data shows a generalized convergence of the effective tax rate to the highest level. The main implication of such shift is a significant increase in the local tax burden faced by the firms. Figure 3 reports the distribution of the property tax rates differential across neighboring municipalities. Municipalities A and B are considered as neighbors if there are at least two firms, the first located in A and the second located in B, belonging to the same sector within a 3 km range. As can be seen, there is a substantial heterogeneity with a relatively moderate fraction of municipalities which set

[14]In a spatial difference setup this restriction in pair definition is equivalent to include sector specific dummies in a usual panel framework.

[15]Note that census areas do not spread across multiple municipalities.

[16]Law number 1249, 11th August 1939.

the same property tax rate (about 17%). Figure 4 reports the geographical distribution of the property tax rate, interestingly the map does not show a clear cut pattern. It is worth noting that, since local administrators know exactly the tax base, the property tax is also the most straightforward leverage to be used to deal with budget deficits, so tax rates tend to adjust at a relatively high frequency over time. Furthermore, as we show in Figure 5, denser italian jurisdictions set higher tax rates, supporting the agglomeration rents hypothesis suggested by Baldwin and Krugman (2004). We can also notice that, even if the average tax rate increases over the study period, its conditional distribution on municipal population density remains almost unchanged.

4 Empirical Strategy

4.1 The effect of non-residential property tax on firm performance: expected results

Following previous literature, we assume that relocation expenses exceed the cost of taxation[17]. Within this scenario, we expect non-residential property tax to cause a growth slow-down not only in employment but also across other dimensions.

Let us assume that there are two factors of production, capital and labour. Since it increases input prices, an increase in non-residential property tax should directly and negatively affect capital. Providing that there is imperfect substitutability between the two factors, a credible assumption in the manufacturing sector, the effect of such taxes on employment should be negative but lower than the one on capital. From a general equilibrium perspective, with two sectors, say manufacturing and services, a prediction of this effect is way more difficult. In this case, an increase in the non-residential property tax rate is likely to determine an increase in the cost of capital for the more capital intensive sector. This will, in turn, lead to an increase of the capital intensive products' prices, thus decreasing their relative demand and inducing a negative effect on sales. Firms operating in the capital intensive sector will then try to exploit labour rather than capital, hence the effect of non-residential property tax on employment will depend on the degree of substitutability between the two inputs, on the relative demand for labour of the two sectors and on the availability of this latter input in the local labour market. As a result, the effect of non-residential property tax on employment is ambiguous and an empirical test is needed. This uncertainty sharpens if we assume that the labour input is differently supplied in different local labour markets, e.g. the labour input is more available and heterogenous within denser local labour markets. In this setting, im-

[17] We cannot directly control for relocation effect due to data limitations; however, relocation is a relatively rare event for italian manufacturing firms and does not appear to jeopardize our empirical strategy. We investigate the effect of firm location in Appendix A.1 by exploring the share of new born firms by municipality.

perfect inputs substitutability could induce an unbalanced and inefficient re-organization of production, thus making particularly appealing a test on the effect of local taxation on TFP. If the effect of property tax on employment is positive, then re-organization of the production structure is feasible for the capital intensive sector and TFP should not be influenced at all. On the other hand, if the effect is negative, we expect TFP to be negatively affected by an increase in taxation.

4.2 Econometric strategy

We start by considering the following model

$$y_{it} = \beta_1 r_{at} + \beta_2 \mathrm{a}_{it} + \beta_3 \mathrm{a}_{it}^2 + \alpha_i + \delta_a + \psi_{zt} + \theta_{zt} + \epsilon_{it}, \tag{1}$$

where y_{it} is the log outcome of firm i at time t, r_{at} is the tax rate in municipality a, a_{it} and a_{it}^2 represent a second order polynomial of the firm age, α_i is a firm fixed-effect which captures the impact of unobservable time-invariant firm characteristics, δ_a is a municipality fixed-effect capturing the impact of unobservable time-invariant municipality characteristics,[18] ψ_{zt} is a source of time-varying heterogeneity for location z, defined at a finer spatial scale than a, that is local specific but does not vary continuously across space or, in other words, does not spill over across the jurisdictions, θ_{zt} is a time-varying effect for location z which, on the other hand, is assumed to vary continuously across space. Finally, ϵ_{it} is the standard idiosyncratic error. The main parameter of interest in this model is β_1 which captures the (net) effect of the municipal tax rate.

One approach to estimating $\boldsymbol{\beta} = (\beta_1, \beta_2, \beta_3)$ is to rule out α_i and δ_a through a within-firm transformation to get

$$\tilde{y}_{it} = \beta_1 \tilde{r}_{at} + \beta_2 \tilde{\mathrm{a}}_{it} + \beta_3 \tilde{\mathrm{a}}_{it}^2 + \tilde{\psi}_{zt} + \tilde{\theta}_{zt} + \tilde{\epsilon}_{it}, \tag{2}$$

where $\tilde{y}_{it} = y_{it} - \bar{y}_i$ with $\bar{y}_i = \frac{1}{T_i}\sum_{t=1}^{T_i} y_{it}$. Model (2) will give consistent estimates only if $\mathrm{cov}[(\tilde{r}_{at}, \tilde{\mathrm{a}}_{it}, \tilde{\mathrm{a}}_{it}^2), \tilde{\psi}_{zt} + \tilde{\theta}_{zt} + \tilde{\epsilon}_{it}] = 0$, a condition that is unlikely to hold since the local effects are likely to be correlated across neighboring sites, implying that $\tilde{r}_{at}$ is likely to be correlated with both $\tilde{\psi}_{zt}$ and $\tilde{\theta}_{zt}$. The standard way to deal with this correlation is to find a suitable instrumenting strategy for the municipal tax rate. However, instruments for $\tilde{r}_{at}$ are also likely to be correlated with unobserved time-varying local effects ($\tilde{\psi}_{zt}$ and $\tilde{\theta}_{zt}$) violating the orthogonality condition.

An alternative to the instrumental variable approach is the spatial differentiation à la Duranton et al. (2011), that is taking for each time t the difference between each reference

[18] It is worth noting that in the case in which firms do not change their location, the firm-fixed effects also control for unobserved time-invariant at local level.

firm and any neighboring firm in the sample located at a distance less than d from the reference one. Applying this transformation to model (2) gives

$$\Delta^d \tilde{y}_{it} = \gamma \Delta^d \tilde{r}_{at} + \beta_2 \Delta^d \tilde{\mathrm{a}}_{it} + \beta_3 \Delta^d \tilde{\mathrm{a}}^2_{it} + \Delta^d \tilde{\psi}_{zt} + \Delta^d \tilde{\theta}_{zt} + \Delta^d \tilde{\epsilon}_{it}, \tag{3}$$

with Δ^d being the spatial difference operator. It is worth noting that, differently from Duranton et al. (2011), we only pair firms belonging to different municipalities and distant less then d. This means that here we are exploiting only neighbouring firms located across municipalities to identify the effects of taxation.[19]

Consistent estimates of $\boldsymbol{\beta}$ in model (3) can be obtained only if $\mathrm{cov}[(\Delta^d \tilde{r}_{at}, \Delta^d \tilde{\mathrm{a}}_{it}, \Delta^d \tilde{\mathrm{a}}^2_{it}), \Delta^d \tilde{\psi}_{zt} + \Delta^d \tilde{\theta}_{zt} + \Delta^d \tilde{\epsilon}_{it}] = 0$, a condition that is likely to hold only if the spatial differentiation is performed using a (arbitrary small) "optimal" distance d^* and both $\tilde{\psi}_{zt}$ and $\tilde{\theta}_{zt}$ vary continuously across space. Since ψ_{zt} is not smoothed over space by assumption, $\Delta^{d^*} \tilde{\psi}_{zt} \neq 0$ and the parameters of interest will not be properly estimated by applying least squares to model (3).

It is worth to emphasize that the spatial difference transformation aims to remove any source of smooth-over-space "local" spillovers affecting firms performance. Thus, it does not mirror the practical estimation of the treatment effect in a regression discontinuity design (RDD).[20] Furthermore, RDD assumptions are likely to be violated in our setting. Indeed, not only the treatment variable is endogenous, but also the Stable Unit of Treatment Value Assumption (SUTVA) is likely to be violated due to the presence of (unobserved) local spillovers that cross the boundaries (i.e. θ_{zt}) affecting the performance of both treated and control groups.

For example, among other duties, italian municipalities spend their non-residential property tax revenues in urban planning. Suppose that one of this programs provides better conditions for local businesses and improved logistics of goods and raw materials in an area that is close to the border (e.g., quality roads or new infrastructures that reduce transport costs). In this case, it is very likely that firms located near but beyond the border will benefit too. Thus, even thought the treatment was exogenously assigned, the presence of these local spillovers creates the conditions for a violation of the SUTVA in a regression discontinuity design at the border (i.e., the spatial regression discontinuity design). This kind of local spillovers might also naturally arise from fiscal competition among neighboring jurisdictions. For example, a jurisdiction might want to create better conditions for local businesses by lowering the non-residential tax rate. In order to keep up with the competition, neighboring jurisdictions might reduce their own tax rate too creating, in turn, better conditions for their

[19] Notice that including also neighbouring firms located in the same municipality does not improve the identification of the effect of taxation, the variable of interest in this study, while clearly helps in improving the precision of the estimates of other exogenous firm-level covariates included in the model (here, a_{it} and a^2_{it}).

[20] See Imbens and Lemieux (2008) and Baum-Snow and Ferreira (2015) for a comprehensive reviews of RDD and RDD applications in urban economics, respectively.

local businesses.

These are examples of spatially smooth time-varying unobserved factors that can be easily ruled out if the aforementioned spatial difference transformation is performed at the optimal distance d^*. However, it is also possible to have other factors which may not spill over across jurisdictions (i.e. ψ_{zt}) but are correlated with firm performances. An example could be the (unobserved) quality of a locally provided public good that, due to institutional constraints, affects only firms located in the specific jurisdiction in which the public good is effectively provided, i.e. the efficiency of public kindergartens that release female labour supply in a local labour market (Cascio, 2009). This kind of endogeneity cannot be easily removed through data transformation, even if the spatial difference is performed at the optimal distance. Furthermore, since in practice one needs enough observations to estimate the model and the optimal distance d^* is unknown, spatial differentiation is likely to be applied at a non-optimal distance level, implying that this strategy alone will be able to reduce but not eliminate all the endogeneity of the municipal tax rate coming from θ_{zt}. This explain why we propose to exploit both spatial differentiation and instrumental variables techniques to enhance the proper identification of the β_1 parameter.

More formally, a consistent IV estimator of $\boldsymbol{\beta}$ can be expressed in matrix notation as

$$\hat{\boldsymbol{\beta}}_{IV} = (\Delta^d \tilde{\boldsymbol{X}} \boldsymbol{P}_Z \Delta^d \tilde{\boldsymbol{X}})^{-1} (\Delta^d \tilde{\boldsymbol{X}} \boldsymbol{P}_Z \Delta^d \tilde{\boldsymbol{y}}), \tag{4}$$

with $\boldsymbol{P}_Z = \Delta^d \tilde{\boldsymbol{Z}} (\Delta^d \tilde{\boldsymbol{Z}}', \Delta^d \tilde{\boldsymbol{Z}})^{-1} \Delta^d \tilde{\boldsymbol{Z}}'$ and where $\Delta^d \tilde{\boldsymbol{X}} = (\Delta^d \tilde{r}_{at}, \Delta^d \tilde{\text{a}}_{it}, \Delta^d \tilde{\text{a}}_{it}^2)$ and $\Delta^d \tilde{\boldsymbol{Z}}$ are the design and instruments matrices after within-firm projection and spatial differentiation, respectively. As noted before, spatial differentiation induces a specific type of cross-sectional dependence. In this case, the asymptotic variance of (4) can then be estimated by means of the following sandwich type estimator

$$\widehat{V}(\hat{\boldsymbol{\beta}}_{IV}) = \hat{\sigma}^2 \boldsymbol{A} \boldsymbol{B} \boldsymbol{A} \tag{5}$$

where $\boldsymbol{A} = (\Delta^d \tilde{\boldsymbol{X}} \boldsymbol{P}_Z \Delta^d \tilde{\boldsymbol{X}})^{-1}$, $\boldsymbol{B} = \Delta^d \tilde{\boldsymbol{X}}' \boldsymbol{P}_Z \Delta^d \Delta^{d\prime} \boldsymbol{P}_Z \Delta^d \tilde{\boldsymbol{X}}$, $\hat{\sigma}^2 = \frac{\Delta^d \tilde{\boldsymbol{\varepsilon}}' \Delta^d \tilde{\boldsymbol{\varepsilon}}}{2N - tr(\boldsymbol{AB})}$, $\Delta^d \tilde{\boldsymbol{\varepsilon}} = \Delta^d \tilde{\boldsymbol{y}} - \Delta^d \tilde{\boldsymbol{X}} \hat{\boldsymbol{\beta}}$, $N = \sum_{p=1}^{P} T_p$, P the number of pairs, T_p the number of years that pair p appears in the data and $tr()$ is the trace operator.[21] However, a key concern of this covariance matrix estimator is that it completely ignores issues arising from serial correlation or any other type of cross-sectional dependence of the errors. We applied the Wooldridge (2001) test for serial correlation before any data transformation, i.e. model (1), and after the within-group/spatial difference transformations, i.e. model (3) with $d = (0.5km, 1km, 1.5km, 2km, 3km)$. In all

[21]Notice that Δ^d, the spatial difference operator, is the (block) matrix that allows to spatially differentiate the within-firm transformed data (i.e., model (2)). For a formal representation of this matrix, see Appendix A, p.1040 of Duranton et al. (2011).

the cases the null hypothesis of no first-order autocorrelation was strongly rejected. Then, following Cameron and Miller (2015), we based our statistical inference on the following two-way cluster-robust covariance matrix estimator

$$\widehat{V}_{2\text{way}}(\hat{\boldsymbol{\beta}}_{IV}) = \widehat{V}_1(\hat{\boldsymbol{\beta}}_{IV}) + \widehat{V}_2(\hat{\boldsymbol{\beta}}_{IV}) - \widehat{V}_{12}(\hat{\boldsymbol{\beta}}_{IV}), \tag{6}$$

where

$$\begin{aligned}
\widehat{V}_1(\hat{\boldsymbol{\beta}}_{IV}) &= \boldsymbol{A}\Delta^d\tilde{\boldsymbol{X}}'\Delta^d\tilde{\boldsymbol{Z}}(\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{Z}})^{-1}\widehat{\boldsymbol{W}}_1(\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{Z}})^{-1}\Delta^d\tilde{\boldsymbol{X}}\boldsymbol{A}, &(7)\\
\widehat{V}_2(\hat{\boldsymbol{\beta}}_{IV}) &= \boldsymbol{A}\Delta^d\tilde{\boldsymbol{X}}'\Delta^d\tilde{\boldsymbol{Z}}(\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{Z}})^{-1}\widehat{\boldsymbol{W}}_2(\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{Z}})^{-1}\Delta^d\tilde{\boldsymbol{X}}\boldsymbol{A}, &(8)\\
\widehat{V}_{12}(\hat{\boldsymbol{\beta}}_{IV}) &= \boldsymbol{A}\Delta^d\tilde{\boldsymbol{X}}'\Delta^d\tilde{\boldsymbol{Z}}(\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{Z}})^{-1}\widehat{\boldsymbol{W}}_{12}(\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{Z}})^{-1}\Delta^d\tilde{\boldsymbol{X}}\boldsymbol{A}, &(9)
\end{aligned}$$

with

$$\begin{aligned}
\widehat{\boldsymbol{W}}_1 &= \left(\sum_{p=1}^{P}\Delta^d\tilde{\boldsymbol{Z}}_p'\Delta^d\tilde{\boldsymbol{\varepsilon}}_p\Delta^d\tilde{\boldsymbol{\varepsilon}}_p'\Delta^d\tilde{\boldsymbol{Z}}_p\right), &(10)\\
\widehat{\boldsymbol{W}}_2 &= \left(\sum_{t=1}^{T}\Delta^d\tilde{\boldsymbol{Z}}_t'\Delta^d\tilde{\boldsymbol{\varepsilon}}_t\Delta^d\tilde{\boldsymbol{\varepsilon}}_t'\Delta^d\tilde{\boldsymbol{Z}}_t\right), &(11)\\
\widehat{\boldsymbol{W}}_{12} &= \left(\sum_{m=1}^{M}\Delta^d\tilde{\boldsymbol{Z}}_m'\Delta^d\tilde{\boldsymbol{\varepsilon}}_m\Delta^d\tilde{\boldsymbol{\varepsilon}}_m'\Delta^d\tilde{\boldsymbol{Z}}_m\right), &(12)
\end{aligned}$$

with P the numbers of clusters at pair level, T the numbers of clusters at time (year) level and M the numbers of clusters at pair-by-time level. Results from a set of preliminary Monte Carlo simulations based on the data generating process in (1) show that, with critical value from the $T(J-1)$ distribution with $J = min(P, T)$, the proposed two-way cluster-robust covariance matrix estimator has the expected rejection rates in presence of multiplicative heteroskedasticity and first-order serial correlation even in the case of few clusters ($J = 10, 20, 30$) with unequal size.[22] By using (6), we are making specific assumptions about the errors, that is observations on the same pair in two different time periods are correlated (serial correlation) as well as observations on two different pairs in the same time period (cross-sectional dependence). In this case, and considering we are in a overidentified model (see Section 4.2.1), the classical IV estimator is less efficient compared to the linear GMM estimator. Hence, our empirical analysis is based on

$$\hat{\boldsymbol{\beta}}_{GMM} = (\Delta^d\tilde{\boldsymbol{X}}\Delta^d\tilde{\boldsymbol{Z}}\widehat{\boldsymbol{W}}_Z\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{X}})^{-1}(\Delta^d\tilde{\boldsymbol{X}}\Delta^d\tilde{\boldsymbol{Z}}\widehat{\boldsymbol{W}}_Z\Delta^d\tilde{\boldsymbol{Z}}'\Delta^d\tilde{\boldsymbol{y}}), \tag{13}$$

[22] Since they are beyond the objective of this paper, for reasons of space these results are not included here but are available from the authors upon request.

where, for two-way clustered errors, the efficient two-step GMM estimator uses $\widehat{\boldsymbol{W}}_Z = (\widehat{\boldsymbol{W}}_1 + \widehat{\boldsymbol{W}}_2 - \widehat{\boldsymbol{W}}_{12})^{-1}$ in which $\Delta^d \tilde{\boldsymbol{\varepsilon}}_g$ for the generic cluster g is the IV residual, and

$$\widehat{V}_{\text{2way}}(\hat{\boldsymbol{\beta}}_{GMM}) = c(\Delta^d \tilde{\boldsymbol{X}} \Delta^d \tilde{\boldsymbol{Z}} \widehat{\boldsymbol{W}}_Z \Delta^d \tilde{\boldsymbol{Z}}' \Delta^d \tilde{\boldsymbol{X}})^{-1} \tag{14}$$

where $c = \frac{J}{J-1}\frac{N-1}{N-k}$.[23]

4.2.1 Identification strategy

Duranton et al. (2011) identification strategy is based on the municipal political color. The rationale beyond this instrument is that, given their preference for redistribution, left-wing administrators are more likely to set higher tax rates with respect to right-wing ones. However, this is likely to be correlated with unobserved local conditions such as inequality and/or unemployment which, in turn, are correlated with firm-level outcomes. Alternatively, our instrumenting strategy is based on the political alignment of municipal government with the central one, distinguishing whether the alignment is with right-wing or left-wing governments. The relevance of our instrument comes from the fact that, as shown by Bracco et al. (2015), municipalities sharing the same political color with the upper tier of government may exploit up to 43% of extra grants compared to those that are not aligned, and more grants are associated with lower local tax revenues. Stylized facts on non-residential property tax rate differentials between aligned and non aligned jurisdictions reported in Table 2 support this evidence, showing that there is a statistically significant difference between the average tax rate set by aligned and non aligned jurisdictions. Furthermore, Table 2 also suggests the presence of heterogeneity within the aligned jurisdictions, with municipalities aligned with a right-wing central government systematically setting a lower tax rate (see also Figure 6).

As far as the validity of our instrument is concerned, even thought local conditions may affect local voting behavior, the latter is marginal in determining the central government political color. Indeed, non ideological "rational" citizens may exhibit different voting preferences conditional on the type of election, e.g. foreign policy preferences may be important for national elections while public transport and recycling may drive local voting behavior. This rules out the possibility that local unobserved heterogeneity determining θ_{zt} and/or ψ_{zt} could also affect our instrument. Furthermore, political alignment is also unlikely to be correlated with firm level outcomes, thus generating exogenous variation in local taxes that can be used to estimate a LATE. As in Duranton et al. (2011), to reflect the share of the municipal aligned party we weight the political alignment dummies with the share of the municipality's population over the reference electoral district population.[24] The intuition behind such re-weighting

[23] GMM estimates have been obtained using the Baum and Schaffer (2012)'s `ivreg2h` Stata command on appropriately transformed data, that is by applying the efficient two-step linear GMM to model (3).

[24] We use the electoral districts for the election of the National Parliament, districts borders are defined within

is strictly related to the importance of distinguishing between municipalities like Rome or Milan and smaller municipalities characterized by a different socio-economic framework as well as by a completely different tax revenues and public spending profile. This interaction ameliorates the relevance of our instrumenting strategy leaving validity unchanged, as documented by the reported Hansen tests (Section 5). Since we find a clear evidence of heteroskedasticity driven by the exogenous regressors (a_{it} and a_{it}^2), we also use the method described in Lewbel (2012) to supplement our instruments, improving both the identification of the tax-rate effect and the efficiency of the GMM estimator.[25]

Finally, we also test an alternative identification strategy based on mandated compulsory administration. When needed, italian law gives to the central government the power to remove elected local officials and substitute them with external commissioners. This happens especially because of criminal organization infiltration but also when local budget administration is under bailout. A similar instrument is used by Acconcia et al. (2014) to identify the effect of public spending on the growth rate at provincial level (i.e., fiscal multiplier). We believe that this could be considered a suitable instrument also for local non-residential property tax rates, since commissioners usually suspend investment projects and regulates financial flows into local public works. Moreover, under bailout, they raise taxes in order to increase tax revenues.

5 Results

Table 3 reports our benchmark results, obtained by estimating model (3) for the four considered outcomes through the linear GMM estimator in equation (13) using a 1 km distance threshold and the aforementioned instrumenting strategy based on political alignment supplemented by Lewbel (2012)'s instruments. Unless specified, standard errors are estimated by clustering at pair and year level using (14).

We find a statistically significant slow-down effect of local taxes regardless of the considered firm-level outcome, while its magnitude reveals more heterogeneity. Consistently with Duranton et al. (2011), we find a negative effect of taxation on employment: the estimated semi-elasticity is negative and significant, about -0.11.[26] This result largely confirms previous findings, suggesting that an increase in property taxation may be harmful for production. As

regions based on the electoral base the actual boundaries were established in 1993 law n. 227.

[25] See Section 3.2 of Lewbel (2012) (pag. 73) for more details. In order to justify the use heteroskedastic covariance restrictions, we test for the presence of heteroskedaticity by using the LM test proposed by Juhl and Sosa-Escudero (2014, see p.486) after model (2), strongly rejecting the null of homoskedasticity ($\chi^2_2 = 53.51$, p-value$= 0.000$).

[26] Table A.1.6 in Appendix A.1 confirms that, when we replicate the Duranton et al. (2011)'s model specification and instrumenting strategy, our estimated elasticity of employment to the non-residential property tax rate (about -1.4) is fully consistent with their estimate (about -1.1).

mentioned in Section 3.1, the non-residential property tax analyzed here can be considered as a tax on capital, thus we expect a direct effect on firms' capital stock. The second column of Table 3 confirms this expectation: the semi-elasticity of capital is negative and statistically significant (about -0.3) and three times bigger than the one for employment. This sizeable difference together with the fact that the two elasticities have the same sign suggest the presence of imperfect substitutability between the two main factors of production. Even though this is not a formal test of this hypothesis, we argue that the latter is also supported by the negative and statistically significant effect of taxation on TFP (about -0.14, third column of Table 3). The negative effect on sales (about -0.21, fourth column of Table 3) comes full circle, confirming our expectations (see Section 4.1 for details). These results imply that an average increase in non-residential property taxation between two consecutive years, which in our sample is found to be about 0.05 percentage points, induces a contraction in both firms' employment and capital, by about 0.5 workers and 8150 euros, respectively. Finally, the estimated effect of age confirms that, on average, older firms perform better than younger firms, but their premium is diminishing over time.

Table from A.1.1 to A.1.3 provide support for our empirical strategy. A simple look at the first column of Table A.1.2, which reports estimates from the fixed-effects model in equation (2), shows why controlling only for time-invariant firm unobserved heterogeneity is not enough: we obtain a positive and statistically significant semi-elasticity of capital to the tax rate, something that is really hard to believe. We argue that this result is likely to be driven by the endogeneity of local taxation, hence as discussed in Section 4.2, instrumenting the tax rate should solve the problem. Nonetheless, the second column of the same table shows that a fixed-effects IV regression is still not sufficient to obtain meaningful results, even thought the coefficients of the first stage regression (reported in the second column of Table A.1.1) do have the expected sign, supporting the hypothesized first-stage mechanism. We believe that, as pointed out in Section 4.2, the instruments themselves are correlated with unobserved time-varying local effects, thus violating the orthogonality condition: the Hansen tests reported in the bottom panel of the table strongly reject the validity of the over-identifying restrictions. On the other hand, the first stage F-statistic and the Hansen tests reported in Table 3 show that spatial differencing and within-group transformations are able to make our instrumenting strategy meaningful. In particular, even if the spatial transformation seems to dilute the hypothesized first-stage mechanism (third column of Table A.1.1), the first stage F-statistic is largely above the rule of thumb suggested by Stock et al. (2002) and the Hansen J does not reject the over-identifying restrictions, regardeless of the considered outcome.

Another result supporting our empirical strategy and in particular the need for clustering is shown in the last two rows of Table A.1.3. The latter reports the results obtained by estimating model (3) through the IV estimator in (4) and standard errors computed according to Appendix A of Duranton et al. (2011). When errors are clustered but the variance-covariance matrix does not take into account this within-clusters correlation, the Hansen J statistic and the first stage F-statistic are invalid (Hoxby and Paserman, 1998). In our view, the implausible high value of the F-statistic and the zero p-values of the J statistics suggest that this is exactly the case.

Finally, Table 4 shows our test for the Baldwin-Krugman agglomeration rent effect. We expect to find that firms in denser areas suffer less the burden of taxation. A simple test is conduced performing our baseline regression just on firms paired across neighboring Local Labor Systems (LLS) both above the median density.[27] The effect of taxation in a high density environment is generally negative but not statistically significant, suggesting that in high density areas the negative effect of taxation is in some way diluted. It is difficult to identify in which way agglomeration economies alleviate the harmfulness of the non-residential property tax. Indeed, agglomeration benefits may arise in different ways: *i*) from labor pooling or from the access to heterogenous labour markets; *ii*) from the increasing returns to scale in intermediate inputs; *iii*) from the relative ease of communication and exchange of resources and innovative ideas due to the proximity among firms. The fact that none of the estimated semi-elasticities is statistically significant, especially the capital one, suggests that the third channel plays a key role in our scenario. This evidence, consistently with theoretical expectations, suggests that agglomeration externalities in denser areas may help to overcome the penalizing effect of a tax shock without affecting firm productivity. As far as we know, this is the first study in which this kind of empirical test is performed taking simultaneously into account spatial spillovers.

5.1 Robustness checks

Table 5 and 6 report the results obtained using the same estimation strategy of Table 3 but using specific subsamples. In Table 5 we focus on the robustness of our findings with respect to the firm size by excluding large firms according to a criteria that drastically reduces the likelihood to find multi-plant firms in the selected sample.[28] This is a very important robustness check given that balance sheet data usually do not allow to identify this kind of firms. Even if our data are not an exception, we believe that the peculiarities of the italian manufacturing sector make this test plausible. In fact, as noticed in Section 3, the incidence of multi-plant firms is relatively modest (roughly 9.5%). Moreover, enterprises having multiple production facilities are by far and large concentrated among the big ones: on average 87%

[27]As robustness check we replicated our test focusing on high density neighbor municipalities finding similar results. The latter are available from the authors upon request.

[28]In particular, We exclude all the firms with a number of workers two standard deviation above the mean.

of firms with more than 500 workers have multiple production plants. Estimation results fully confirm the empirical evidence reported in the previous section. In Table 6, we check the robustness of our results excluding all the firms located in some italian regions (Abruzzo, Campania, Lazio, Molise e Sicilia) which levied in 2008 a different (greater) tax rate for the italian business tax (IRAP).[29] Even in this case, estimation results fully support our findings. A further "geographical" robustness test is performed using the sub-sample of firms located in the northern regions. Given the north-south gradient and the fact the most of the manufacturing firms are located in the north of the country, this check appears to be compulsory for the italian case. Estimation results in Table 7 confirm that our findings are not completely driven by the geographical distribution of firms.

5.2 Sensitivity Analysis

In this section we present three interesting sensitivity analyses that allow us to argue about the validity of our findings and our identification strategy. Firstly, Table 8 reports the estimates obtained by estimating model (3) pairing only firms in different municipalities but belonging to the same sector and production quintile, the latter identified over the sectorial sales distribution by year. Interestingly, despite the huge drop in the sample size due to the more stringent pairing process, previous findings are largely confirmed, suggesting a stronger (negative) effect of taxation on capital relatively to employment, TFP is no longer significant but still is negative, sales remains negatively affected by taxation.

Secondly, in Table 9 we summarize the results obtained by estimating model (3) for different distance thresholds, different estimators - the IV in equation (4) and the linear GMM in equation (13)) - with and without augmenting the instruments set using Lewbel (2012)'s instruments. Table 9 shows a clear cut pattern in which almost all the estimates are negative and strongly significant and coefficients tend to decrease with the distance threshold.[30] In particular, estimates seem to point towards zero as the pairing distance increases. This evidence suggests that enlarging the threshold distance increases the likelihood to fail in conditioning out unobserved heterogeneity from the model. This also implies that the exclusion restriction

[29] This fiscal intervention was aimed to adjust the regional fiscal budget. Before 2008, the tax rate was the same of the rest of Italy.

[30] It is worth emphasizing that, even thought our preliminary Monte Carlo simulation results show that the two-way clustered variance-covariance matrix estimator used in this paper has the expected rejection rates in presence of multiplicative heteroskedasticity and first-order serial correlation even in the case of few clusters with unequal size, its consistency requires that $min(P, T) \to \infty$. Given that $T = 10$ in our estimation samples, and that these year-level clusters are of unequal size, $\widehat{V}_{2\text{way}}(\hat{\boldsymbol{\beta}}_{GMM})$ may lead to over-rejection (Cameron and Miller, 2015). Table A.1.4 is a copy of Table 9 but reports Quasi-F test statistics computed using the score wild bootstrap proposed by Kline and Santos (2012) for linear GMM with clustered errors. In particular, we impose the null hypothesis of statistical significance on each of the coefficients of interest and applied the bootstrap only with the final optimal-weight matrix $\widehat{\boldsymbol{W}}_Z$. We used the Roodman (2015)'s `boottest` Stata command for practical implementation. As can be seen, our statistical inference is not affected by the few clusters issue.

is more likely to hold for short distances.

It is always difficult to find good instruments and there is always a source of concern. In order to check for the sensitivity of our results to the instrumenting strategy, we investigate two alternatives. The first is based on mandated administrations. The central government in Italy may, under certain specific conditions, appoint an interim town administrator which substitute the one in charge (e.g., the major, the municipal council). Most of the times, this happens when a criminal organization acquires direct or indirect control of legal economic activities, especially public investment and public services; or when local administrators are unable to balance the year budget, generally due to poor public management. This type of instrument is used in Acconcia et al. (2014) to estimate the local fiscal multiplier of italian provinces. Its validity relies on the randomness of the event of being under a mandated administration while its relevance derive from the fact that the commissioner first act consists of suspending financial flow into public works and investments projects. In the case of budget restructuring, the first act of a commissioner is to raise taxes in order to increase tax revenues. Column 1 of Table A.1.5 report estimates with such instrument which are in line with our baseline. Column 2 report estimate with Mandated administration and political alignment together. Also this last estimation largely confirm our previous findings.

6 Conclusions

In this paper, we study the impact of non-residential local property taxation on a wide range of firm-level outcomes. To this aim, we propose to sequentially apply two data transformations, within-group and spatial difference, allowing to rule out unobserved time-invariant firm heterogeneity and unobserved time-varying local effects together with the instrumental variables technique. This approach is used to analyze a panel data set of georeferenced italian manufacturing firms in 2001-2010. Furthermore, we propose a new set of instruments based on the political alignment of each specific jurisdiction with the national government, which in this case serves as strong exclusion restrictions. Our semi-elasticity estimates show that non-residential property taxation has a negative and statistically significant impact on employment, capital, TFP and sales. Back of the envelope calculations, based on the full AIDA sample, suggests that an average increase in local tax induces a negative variation of around 0.5 workers, the same increase induces a reduction in capital around 8150 euro.

The overall analysis of the results seems to suggest that tax is not capitalised into prices, employment decreases due to imperfect substitutability with capital but, since market imperfections prevent an efficient re-organization of the production, productivity is slowed-down and sales reduce. We test for the presence of agglomeration rents a la Baldwin-Krugman finding no effects of taxation on firm performances in denser jurisdictions. The fact that capital is not directly affected seems to suggest that the source of agglomeration at work is not labour pooling but more credibly the relative ease of communication, workers and ideas induced by spatial proximity; as far as we know this is the first credible empirical attempt to provide such test controlling for spatial spillovers. We perform several robustness check in order to rule out typical confounding factors as the presence of multi-plants, the possibility to relocate or the co-existence between property taxes and other business taxes. We also investigate regional differences given that most of the firms are located in the northern regions.

A spatial sensitivity analysis, based on the comparison among estimations performed at different pairing distance, shows the decay of the local taxation effects towards zero, suggesting that spatial differentiation rules effectively out time-varying spatially smooth unobserved heterogeneity at local level. Moreover, we argue that this test can be used as a bare bone argument for the validity of the proposed identification strategy. We ameliorates respect to the previous scarce literature in several aspects. First of all, we look at the effect of non-residential local taxation using a variety of firm-level indicators, including TFP which has been rarely used to asses the effect of local taxation. We hope that our contribution could help to develop further applied analysis that face directly the presence of spatial spillovers, this seems to be crucial if economic literature, in particular local public finance would like to progress on the causal identification and estimation of the effect of local taxes on economic outcomes, on this side we claim that spatial difference methods should be better understood, we hope to have contributed in this direction, however if this methodology is performed correctly can provide LATE estimates that are credible and robust to SUTVA violations.

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Table 1: Descriptive statistics, firm level variables

variable	Obs	# id	Mean	Sd	min	p50	max
Age	138368	21603	22	15	1	20	135
Employment	138368	21603	60	251	2	23	25147
Sales	138368	21603	17170	147905	2	4808	2.08e+07
Capital	138368	21603	3103	19339	1	629	2356447
Density (Municipality)	20616	2429	685	1015	14	368	13348
Density (LLS)	2,940	331	295	394	23	182	3988

Note: all firms with at least one pair in a 3 km range. Sales and Capital are expressed in thousand euro; Density measures the number of residents per squared kilometer.

Table 2: Non-residential property tax differential by alignment status and year

	non aligned	aligned	difference†	central gov. color
2001	5.897	5.622	-0.275***	right-wing
2002	6.093	5.731	-0.362***	right-wing
2003	6.198	5.825	-0.373***	right-wing
2004	6.306	5.871	-0.435***	right-wing
2005	6.407	5.942	-0.465***	right-wing
2006	5.992	6.490	0.498***	left-wing
2007	6.081	6.544	0.463***	left-wing
2008	6.558	6.180	-0.378***	right-wing
2009	6.583	6.199	-0.384***	right-wing
2010	6.582	6.197	-0.385 ***	right-wing

† t-test on the equality of aligned and non aligned non-residential property tax rate means. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table 3: Baseline specification†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
Tax rate	-0.112	***	-0.299	**	-0.143	***	-0.210	***
	(0.038)		(0.137)		(0.049)		(0.058)	
Age (levels)	0.014	***	0.068	***	0.029	***	0.044	***
	(0.003)		(0.008)		(0.004)		(0.004)	
Age^2	-0.050	***	-0.019	**	-0.020	***	-0.031	***
	(0.004)		(0.008)		(0.004)		(0.004)	
Observations	32557		32557		32557		32557	
# of Couples	6340		6340		6340		6340	
# of Firms	5650		5650		5650		5650	
F-test (Alignment + Lewbel)	30.66		30.66		30.66		30.66	
F-test (Lewbel)	10.78		10.78		10.78		10.78	
F-test (Alignment)	32.19		32.19		32.19		32.19	
Hansen-J p-value (Alignment + Lewbel)	0.24		0.41		0.20		0.25	
Hansen-J p-value (Lewbel)	0.98		0.93		0.71		0.53	
Hansen-J p-value (Alignment)	0.31		0.22		0.13		0.56	

† The distance threshold used for spatial differencing is 1km. Standard errors (in parentheses) are clustered at pair and year level. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table 4: Only firms across high density local labour systems†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
Tax rate	-0.029		0.031		-0.087		-0.072	
	(0.080)		(0.130)		(0.058)		(0.090)	
Age (levels)	-0.0001		0.059	***	0.028	***	0.034	***
	(0.003)		(0.008)		(0.003)		(0.005)	
Age^2	-0.038	***	-0.035	***	-0.015	**	-0.028	***
	(0.005)		(0.008)		(0.006)		(0.006)	
Observations	14736		14736		14736		14736	
# of Couples	3062		3062		3062		3062	
# of Firms	2738		2738		2738		2738	
F-test (Alignment + Lewbel)	22.83		22.83		22.83		22.83	
F-test (Lewbel)	7.42		7.42		7.42		7.42	
F-test (Alignment)	24.92		24.92		24.92		24.92	
Hansen-J p-value (Alignment + Lewbel)	0.39		0.75		0.29		0.41	
Hansen-J p-value (Lewbel)	0.81		0.77		0.48		0.73	
Hansen-J p-value (Alignment)	0.24		0.71		0.80		0.42	

† The distance threshold used for spatial differencing is 1km. Standard errors (in parentheses) are clustered at pair and year level. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table 5: Firms Workers $\leq \mu + 2\sigma$ (≈ 400)†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
Tax rate	-0.069	**	-0.284	**	-0.161	***	-0.195	***
	(0.029)		(0.130)		(0.058)		(0.052)	
Age (levels)	0.011	***	0.068	***	0.031	***	0.042	***
	(0.003)		(0.007)		(0.004)		(0.004)	
Age^2	-0.052	***	-0.018	**	-0.020	***	-0.032	***
	(0.004)		(0.008)		(0.004)		(0.004)	
Observations	31083		31083		31083		31083	
# of Couples	6158		6158		6158		6158	
# of Firms	5537		5537		5537		5537	
F-test (Alignment + Lewbel)	26.32		26.32		26.32		26.32	
F-test (Lewbel)	10.67		10.67		10.67		10.67	
F-test (Alignment)	26.72		26.72		26.72		26.72	
Hansen-J p-value (Alignment + Lewbel)	0.27		0.46		0.19		0.35	
Hansen-J p-value (Lewbel)	0.91		0.79		0.82		0.73	
Hansen-J p-value (Alignment)	0.28		0.25		0.15		0.61	

† The distance threshold used for spatial differencing is 1km. Standard errors (in parentheses) are clustered at pair and year level. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table 6: Excluding regions with different business tax rates (IRAP)†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
Tax rate	-0.117	***	-0.292	**	-0.148	***	-0.220	***
	(0.040)		(0.128)		(0.045)		(0.060)	
Age (levels)	0.012	***	0.068	***	0.029	***	0.043	***
	(0.003)		(0.008)		(0.003)		(0.004)	
Age^2	-0.049	***	-0.020	**	-0.020	***	-0.030	***
	(0.004)		(0.008)		(0.004)		(0.004)	
Observations	31719		31719		31719		31719	
# of Couples	6147		6147		6147		6147	
# of Firms	5390		5390		5390		5390	
F-test (Alignment + Lewbel)	31.88		31.88		31.88		31.88	
F-test (Lewbel)	10.06		10.06		10.06		10.06	
F-test (Alignment)	36.08		36.08		36.08		36.08	
Hansen-J p-value (Alignment + Lewbel)	0.27		0.39		0.20		0.24	
Hansen-J p-value (Lewbel)	0.76		0.76		0.71		0.66	
Hansen-J p-value (Alignment)	0.31		0.21		0.12		0.49	

† The distance threshold used for spatial differencing is 1km. Standard errors (in parentheses) are clustered at pair and year level. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table 7: Only firms located in the northern of Italy†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
Tax rate	-0.151	***	-0.296	**	-0.162	***	-0.244	***
	(0.045)		(0.126)		(0.051)		(0.066)	
Age (levels)	0.011	***	0.069	***	0.029	***	0.043	***
	(0.003)		(0.008)		(0.003)		(0.004)	
Age^2	-0.046	***	-0.021	**	-0.017	***	-0.028	***
	(0.004)		(0.008)		(0.004)		(0.005)	
Observations	29282		29282		29282		29282	
# of Couples	5617		5617		5617		5617	
# of Firms	4889		4889		4889		4889	
F-test (Alignment + Lewbel)	34.95		34.95		34.95		34.95	
F-test (Lewbel)	11.03		11.03		11.03		11.03	
F-test (Alignment)	34.60		34.60		34.60		34.60	
Hansen-J p-value (Alignment + Lewbel)	0.28		0.49		0.17		0.24	
Hansen-J p-value (Lewbel)	0.72		0.78		0.65		0.73	
Hansen-J p-value (Alignment)	0.32		0.28		0.12		0.47	

† The distance threshold used for spatial differencing is 1km. Standard errors (in parentheses) are clustered at pair and year level. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table 8: Firms paired within the same sector and production quintile†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
Tax rate	-0.075	*	-0.528	***	-0.134		-0.251	***
	(0.045)		(0.165)		(0.093)		(0.089)	
Age (levels)	0.008	**	0.086	***	0.039	***	0.048	***
	(0.004)		(0.010)		(0.004)		(0.006)	
Age^2	-0.048	***	-0.038	***	-0.029	***	-0.037	***
	(0.005)		(0.012)		(0.006)		(0.005)	
Observations	7462		7462		7462		7462	
# of Couples	2461		2461		2461		2461	
# of Firms	2952		2952		2952		2952	
F-test (Alignment + Lewbel)	39.23		39.23		39.23		39.23	
F-test (Lewbel)	2.47		2.47		2.47		2.47	
F-test (Alignment)	43.86		43.86		43.86		43.86	
Hansen-J p-value (Alignment + Lewbel)	0.32		0.55		0.26		0.35	
Hansen-J p-value (Lewbel)	0.75		0.24		0.29		0.08	
Hansen-J p-value (Alignment)	0.18		0.30		0.15		0.57	

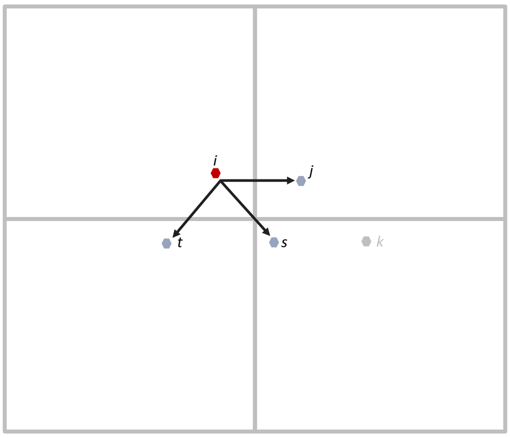
† The distance threshold used for spatial differencing is 1km. Standard errors (in parentheses) are clustered at pair and year level. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table 9: Tax rate effect: summary of results by estimation strategy and different distance thresholds for spatial differencing[†]

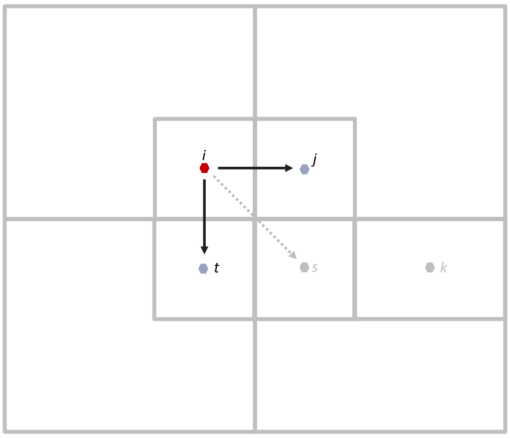
	0.5km		1km		1.5km		2km		3km	
GMM - Intruments: Alignement + Lewbel										
Employment	-0.055	**	-0.112	***	-0.106	***	-0.086	***	-0.079	***
	(0.027)		(0.038)		(0.036)		(0.019)		(0.018)	
Capital	-0.413	*	-0.299	**	-0.203	***	-0.110	**	-0.108	**
	(0.225)		(0.137)		(0.072)		(0.047)		(0.050)	
TFP	-0.148	*	-0.143	***	-0.106	***	-0.075	*	-0.068	**
	(0.076)		(0.049)		(0.032)		(0.039)		(0.029)	
Sales	-0.115		-0.210	***	-0.164	***	-0.117	***	-0.092	***
	(0.072)		(0.058)		(0.039)		(0.030)		(0.027)	
GMM - Intruments: Alignement										
Employment	-0.201	*	-0.290	***	-0.231	***	-0.147	***	-0.098	***
	(0.120)		(0.080)		(0.067)		(0.051)		(0.033)	
Capital	-0.690	**	-0.463	***	-0.268	**	-0.082		-0.132	**
	(0.276)		(0.179)		(0.126)		(0.078)		(0.065)	
TFP	-0.288	**	-0.269	***	-0.196	***	-0.144	***	-0.127	***
	(0.140)		(0.083)		(0.044)		(0.045)		(0.032)	
Sales	-0.268	**	-0.428	***	-0.302	***	-0.212	***	-0.178	***
	(0.118)		(0.082)		(0.062)		(0.051)		(0.040)	
2SLS - Intruments: Alignement + Lewbel										
Employment	-0.069		-0.127	**	-0.115	**	-0.088	**	-0.073	*
	(0.050)		(0.061)		(0.049)		(0.037)		(0.039)	
Capital	-0.512		-0.292	*	-0.190	*	-0.087		-0.087	
	(0.318)		(0.164)		(0.102)		(0.061)		(0.060)	
TFP	-0.144		-0.127		-0.081		-0.063		-0.060	
	(0.093)		(0.086)		(0.056)		(0.059)		(0.060)	
Sales	-0.131		-0.212	**	-0.166	**	-0.133	*	-0.114	
	(0.080)		(0.107)		(0.080)		(0.074)		(0.076)	
2SLS - Intruments: Alignement										
Employment	-0.154		-0.258	***	-0.216	***	-0.138	***	-0.105	***
	(0.123)		(0.087)		(0.070)		(0.051)		(0.034)	
Capital	-0.833	**	-0.497	**	-0.270	**	-0.085		-0.103	
	(0.336)		(0.211)		(0.137)		(0.092)		(0.082)	
TFP	-0.203		-0.217	**	-0.146	**	-0.114		-0.106	
	(0.162)		(0.099)		(0.062)		(0.075)		(0.067)	
Sales	-0.258	**	-0.417	***	-0.315	***	-0.247	***	-0.204	***
	(0.120)		(0.091)		(0.075)		(0.067)		(0.053)	
OLS										
Employment	-0.013		-0.033	**	-0.031	**	-0.027	**	-0.019	**
	(0.023)		(0.010)		(0.011)		(0.010)		(0.007)	
	0.5km		1km		1.5km		2km		3km	
Capital	0.024		0.014		-0.007		-0.002		-0.008	
	(0.041)		(0.025)		(0.018)		(0.017)		(0.012)	
	0.5km		1km		1.5km		2km		3km	
TFP	0.011		-0.007		0.005		0.007		0.011	
	(0.030)		(0.024)		(0.017)		(0.013)		(0.010)	
	0.5km		1km		1.5km		2km		3km	
Sales	0.007		-0.019		-0.010		-0.012		-0.007	
	(0.018)		(0.014)		(0.010)		(0.008)		(0.006)	

[†] Baseline specification. *Age* and Age^2 not reported. Standard errors (in parentheses) are clustered at pair and year level. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Figure 1: Spatial difference: latitude/longitude vs postcode centroids



(a) Latitude/longitude georeferentiation



(b) Postcode centroid georeferentiation

Figure 2: Property tax rates distribution. The plot reports the distribution of the of property tax rate by municipality.

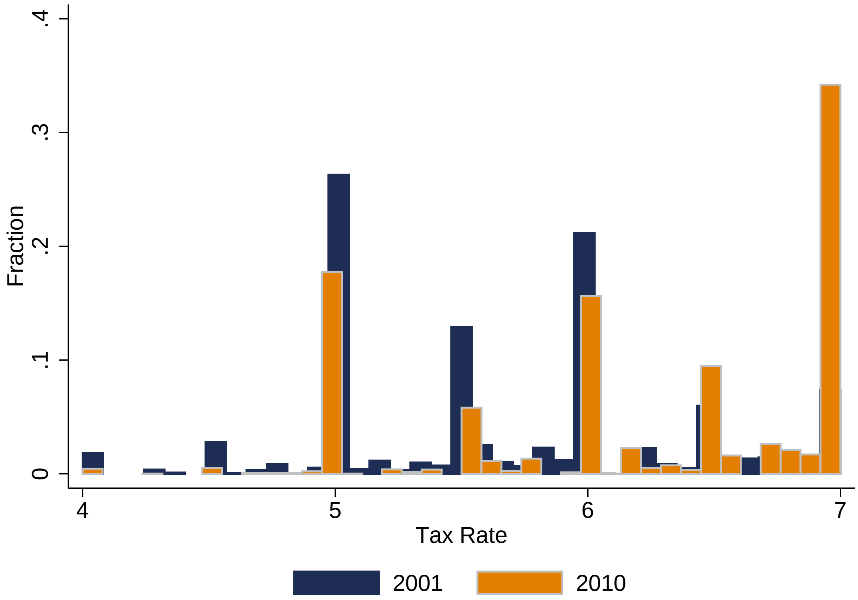


Figure 3: Property tax rates differentials distribution. The plot reports the distribution of tax rate differentials by municipality, in absolute values for the period 2001-2010.

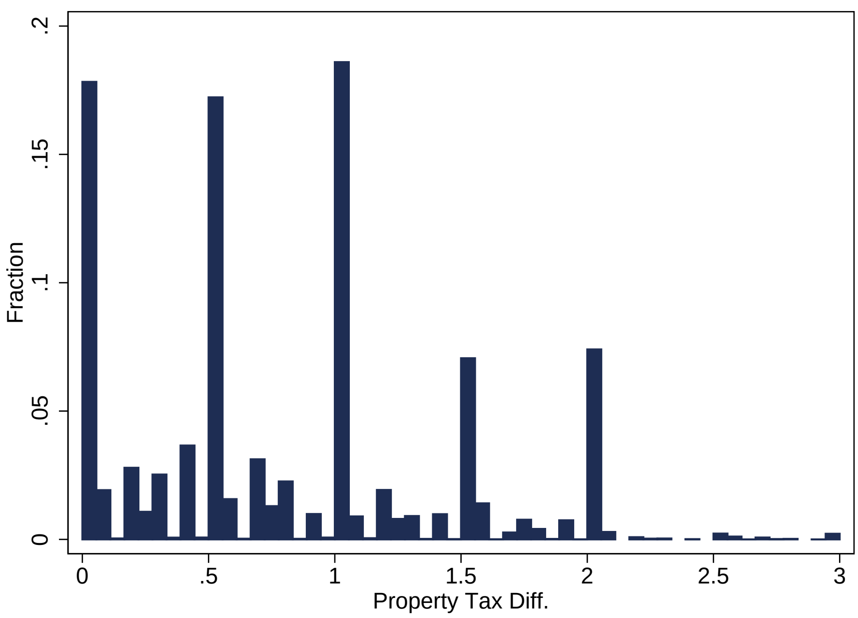


Figure 4: Spatial distribution of the average property tax rate. The map reports the 2001-2010's average property tax rate quintiles by municipality, deeper colors reflect higher values.

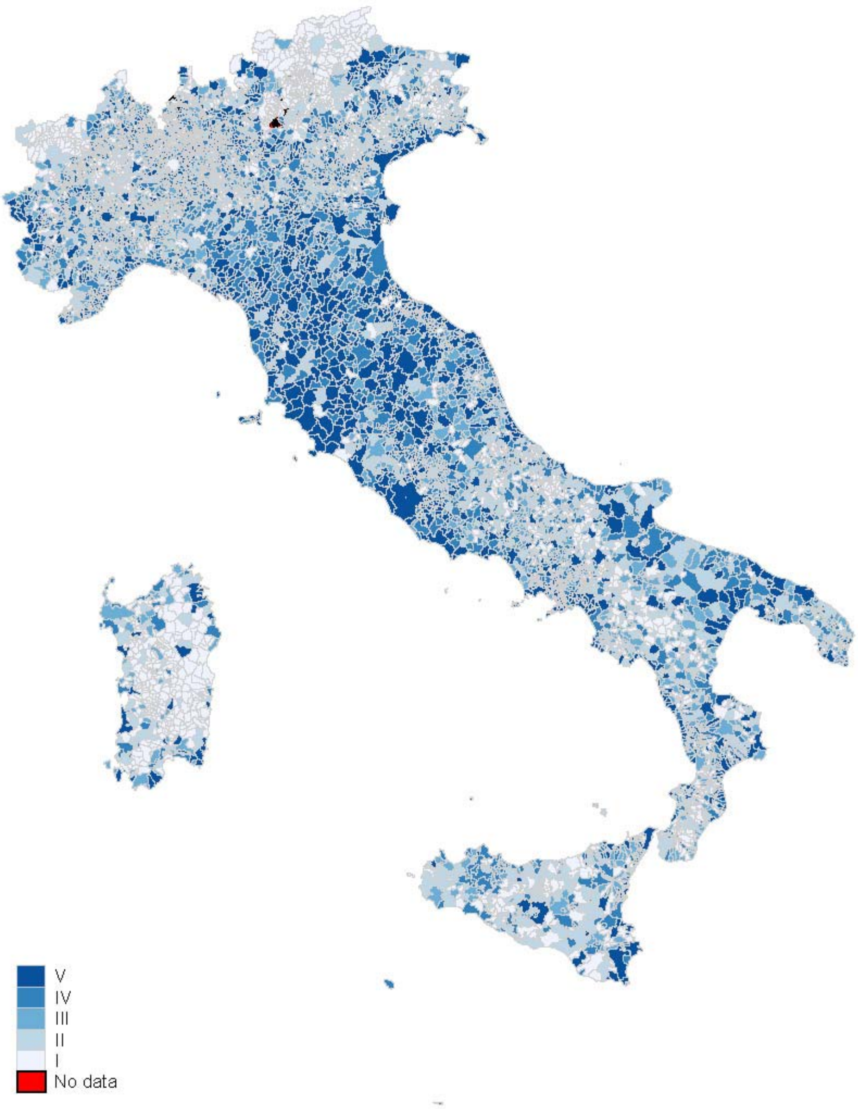


Figure 5: Tax Rate and Municipality Density: Local Polynomial Approximation (selected years).

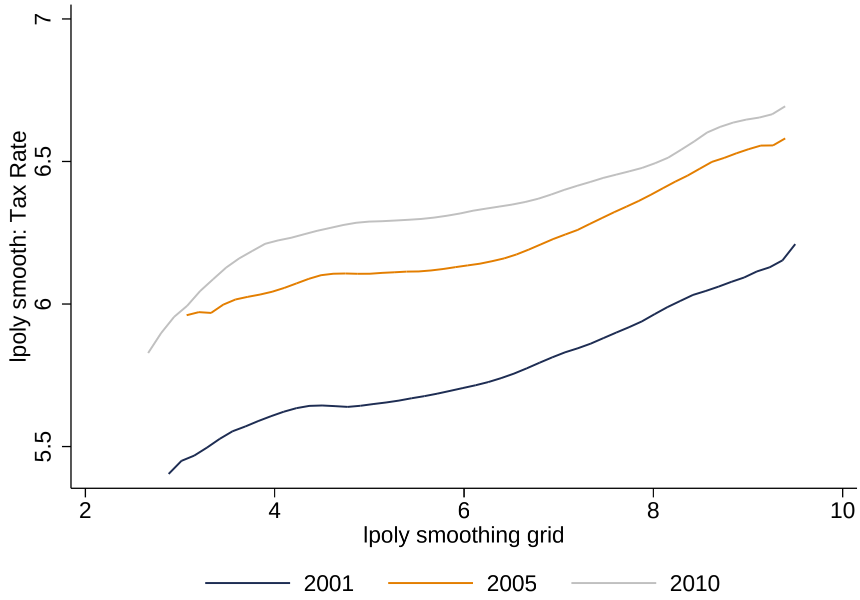
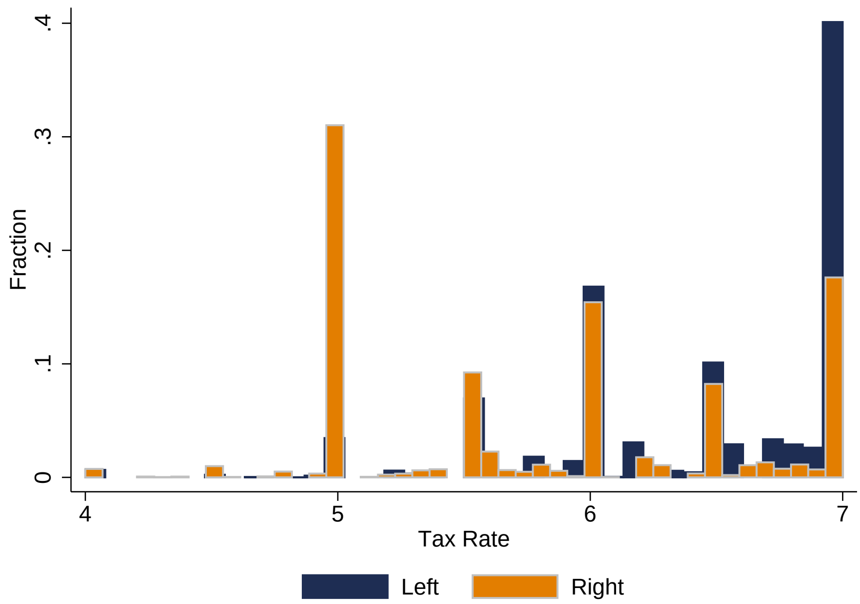


Figure 6: Non-residential property tax rate distribution by alignment to central government and political color (2001-2010).



A Appendix

A.1 Additional estimation results

Table A.1.1: First Stage regressions†

	Least-squares		Fixed-effects		Fixed-effects + Spatial differencing	
Municipality's share	-0.005	***	0.081	***	0.190	***
	(0.0006)		(0.017)		(0.032)	
Alignment: center-right	-0.063	***	-0.029	***	0.022	
	(0.010)		(0.007)		(0.017)	
Alignment: center-left	0.075	***	0.035	***	-0.004	
	(0.010)		(0.008)		(0.021)	
Municipality's share × Alignment: center-right	-0.006	***	0.002	***	0.002	**
	(0.0009)		(0.0003)		(0.001)	
Municipality's share × Alignment: center-left	0.003	***	-0.001	**	-0.001	
	(0.0005)		(0.0002)		(0.001)	
Age (levels)	-0.016	***	0.021	***	0.053	***
	(0.005)		(0.007)		(0.004)	
Age^2	2.4e-04	**	6.5e-05		-0.001	
	(0.0001)		(0.0001)		(0.003)	
Age (Lewbel)	-0.063	***	-0.051	***	-0.032	
	(0.002)		(0.007)		(0.038)	
Age^2 (Lewbel)	0.001	***	0.0008	***	0.226	*
	(0.0001)		(0.0002)		(0.110)	
Observations	38024		38024		32557	
# of Couples	-		-		6340	
# of Firms	5650		5650		5650	

† Baseline specification. *Age* and Age^2 not reported. Standard errors (in parentheses) are clustered at firm level (column 1 and 2) and at pair and year level (column 3). Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table A.1.2: Results without spatial differencing transformation (1 km sample, 38024 observations, 5650 firms).†

	ln(Emp)	ln(Cap)		TFP(Lev-Pet)	ln(Sales)
Fixed-effects	0.001	0.058	**	-0.004	0.009
	(0.010)	(0.024)		(0.009)	(0.008)
Fixed-effects IV	-0.025	0.093		-0.043	-0.041
	(0.060)	(0.129)		(0.049)	(0.059)
First Stage F-test	105.6				
Hansen J (p-value)	0.000	0.000		0.000	0.000

† Baseline specification. *Age*, Age^2 and sector-year fixed-effects included in all regressions but not reported. Instrumenting strategy based on political alignment. Standard errors in parentheses clustered at firm level. First stage regression for Fixed-effects IV are reported in the second column of Table A.1.1.

Table A.1.3: Baseline specification. Standard Errors (in parentheses) are obtained according to Appendix A of Duranton et al. (2011)†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
ICI	-0.258	***	-0.497	***	-0.217	***	-0.417	***
	(0.045)		(0.095)		(0.052)		(0.047)	
Age (levels)	0.020	***	0.078	***	0.032	***	0.056	***
	(0.003)		(0.006)		(0.003)		(0.003)	
Age^2	-0.047	***	-0.019	***	-0.018	***	-0.030	***
	(0.003)		(0.006)		(0.003)		(0.003)	
Observations	32557		32557		32557		32557	
# of Couples	6340		6340		6340		6340	
# of Firms	5650		5650		5650		5650	
First Stage F-test	230.52		230.52		230.52		230.52	
Hansen J (p-value)	0.00		0.00		0.00		0.00	

† The distance threshold used for spatial differencing is 1km. Instrumenting strategy based on political alignment supplemented by Lewbel (2012)'s instruments. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table A.1.4: Tax rate effect: summary of results using different distance thresholds for spatial differencing (wild bootstrap)[†]

	0.5km		1km		1,5km		2km		3km	
GMM - Intruments: Alignement + Lewbel										
Employment	-0.055		-0.112	***	-0.106	***	-0.086	***	-0.079	***
	(1.09)		(7.37)		(16.32)		(16.11)		(16.48)	
Capital	-0.413	**	-0.299	***	-0.203	***	-0.110	***	-0.108	***
	(4.36)		(9.94)		(9.96)		(8.39)		(11.79)	
TFP	-0.148	**	-0.143	***	-0.106	***	-0.075	***	-0.068	***
	(5.70)		(7.18)		(16.57)		(12.47)		(14.98)	
Sales	-0.115	**	-0.210	***	-0.164	***	-0.117	***	-0.092	***
	(4.06)		(19.32)		(25.88)		(21.08)		(22.64)	
GMM - Intruments: Alignement										
Employment	-0.201	*	-0.290	***	-0.231	***	-0.147	***	-0.098	***
	(3.50)		(16.79)		(38.23)		(31.66)		(23.83)	
Capital	-0.690	**	-0.463	***	-0.268	**	-0.082		-0.132	**
	(7.39)		(12.54)		(11.78)		(1.83)		(6.98)	
TFP	-0.288	***	-0.269	***	-0.196	***	-0.144	***	-0.127	***
	(8.75)		(24.91)		(35.82)		(31.81)		(36.94)	
Sales	-0.268	**	-0.428	***	-0.302	***	-0.212	***	-0.178	***
	(6.28)		(32.78)		(61.39)		(57.49)		(64.08)	
2SLS - Intruments: Alignement + Lewbel										
Employment	-0.069		-0.127	***	-0.115	***	-0.088	***	-0.073	*
	(1.43)		(12.30)		(28.37)		(30.69)		(35.69)	
Capital	-0.512	***	-0.292	***	-0.190	***	-0.087	**	-0.087	
	(12.85)		(13.81)		(15.90)		(5.49)		(9.99)	
TFP	-0.144	***	-0.127	***	-0.081	***	-0.063	***	-0.060	
	(7.04)		(16.49)		(17.71)		(17.82)		(28.11)	
Sales	-0.131	**	-0.212	***	-0.166	**	-0.133	***	-0.114	
	(5.23)		(31.98)		(54.80)		(62.11)		(69.60)	
2SLS - Intruments: Alignement										
Employment	-0.154		-0.258	***	-0.216	***	-0.138	***	-0.105	***
	(2.24)		(15.77)		(36.39)		(27.91)		(27.97)	
Capital	-0.833	***	-0.497	***	-0.270	***	-0.085		-0.103	**
	(10.15)		(13.92)		(11.29)		(1.92)		(5.15)	
TFP	-0.203	**	-0.217	***	-0.146	***	-0.114	***	-0.106	***
	(4.34)		(16.97)		(22.63)		(23.20)		(34.07)	
Sales	-0.258	**	-0.417	***	-0.315	***	-0.247	***	-0.204	***
	(6.04)		(37.16)		(70.99)		(78.80)		(87.72)	

[†] Baseline specification, *Age* and Age^2 not reported. Quasi-$F(1,9)$ test statistics computed using the score wild bootstrap with 10,000 replications and Rademacher weights are in parentheses.

Table A.1.5: Alternative instrumenting strategies†

	Mandating Administration (Alone)	Mandating Administration and Political Alignment
Employment	-0.106**	-0.108***
	(0.051)	(0.037)
Capital	-0.398**	-0.332***
	(0.161)	(0.130)
TFP	-0.107	-0.138**
	(0.079)	(0.048)
Sales	-0.190***	-0.209***
	(0.061)	(0.058)
F-test (All)	11.82	27.37

† Baseline specification, *Age* and Age^2 not reported. In both cases excluded instruments have been augmented using Lewbel (2012)'s method. The distance threshold used for spatial differencing is 1km. Significance levels: * $p < 10\%$; ** $p < 5\%$, *** $p < 1\%$.

Table A.1.6: Replication of Duranton et al. (2011) using our sample. Log-Log specification using 1 km as distance threshold for spatial differencing.†

	ln(Emp)		ln(Cap)		TFP(Lev-Pet)		ln(Sales)	
log(ICI)	-1.411	***	-3.068	***	-1.327	***	-2.362	***
	(0.262)		(0.557)		(0.302)		(0.271)	
Age (levels)	0.019	***	0.078	***	0.033	***	0.054	***
	(0.003)		(0.006)		(0.003)		(0.003)	
Age^2	-0.047	***	-0.018	***	-0.018	***	-0.030	***
	(0.003)		(0.006)		(0.003)		(0.003)	
Observations	32557		32557		32557		32557	
# of Couples	6340		6340		6340		6340	
# of Firms	5650		5650		5650		5650	
First Stage F-test	276.68		276.68		276.68		276.68	
Hansen J (p-value)	0.00		0.00		0.01		0.50	

† Standard Errors in parentheses are obtained according to Appendix A of Duranton et al. (2011): $*\ p < 0.1$, $**\ p < 0.05$, $***\ p < 0.01$. Duranton et al. (2011)'s full set of instruments includes the share of local politicians affiliated with the three main political parties (Conservative, Labour and Liberal Democrat), a set of dummies indicating whether the local authority is controlled by one of the three main parties and a set of interactions giving the share of the three main parties if they control the local authority. The replication has been obtained using two dummies indicating whether the municipality is controlled by one of the two main coalitions (Center-right, Center-left), the share of the municipality's population over the reference electoral district population and two interactions giving the share of the municipality's population over the reference electoral district population if a specific coalition controls the municipality.

A.2 Selection-into-treatment

In order to check if local taxation is correlated with the share of new born firms we set up a simple empirical test for the selection effect. Our empirical strategy is twofold: first, we regress the share of new born firms at the municipality level on local tax rates, controlling for location fixed effects; second using spatial differenced data we test if the probability that a new firm locate in a municipality correlates with the tax differential with neighboring jurisdictions. In the latter approach we basically compare locations' tax rates instead of firms, i.e. the new born firm's location tax rate with the relevant alternatives (neighbors). The dependent variable in the first case would be the share of new firms on the number of existing ones by municipality regressed on the (log) tax rate, given the large amount of zeros in the dependent variable we adopt a poisson estimator. Data are arranged as a standard panel where observations are municipality by year.

In the second case the dataset consist of a series of coupled firms located in different municipalities as in the main analysis. However, we are not interested in comparing firms but locations. Relevant alternative for firm i is defined as the nearest production facility in another municipality. Our dependent variable is equal to 1 if there is a new born firm in one side of the border and an already established firm in the same sector and production quintile in the other side of the border. The only covariate is the tax rate differential between paired locations, a significant coefficient in this case would suggest that a firm location choice may be correlated with tax differentials (selection effect). At time t we define as new born firms those starting business at time $t-1$ according to the information reported in the financial account. We extend the definition at $t-2$ to maximize the estimation sample. In both empirical approaches we use 3 km threshold estimation sample[31].

Results are reported in table **??**. Panel data estimation (Column 1 and 2) does not show any significant correlation between the share of new (or young) business and the tax rate at the municipality level. Moving to a spatial difference approach (Column 3) confirms previous findings shoving no significant correlation between firms location choices and tax rate differentials, when comparing all relevant alternatives as in Column (3). Those results, consistently with previous studies, suggest no evidence of selection effect in our estimation sample.

[31]Results are robust to different thresholds and available upon request.

Table A.2.7: Selection Effect: New Born Firm Share and Local Taxation.†

	CF (QML) (1)	Poisson (2)	Spat-Diff NN (IV) (3)
New Born $\leq$ 2 years			
IV Res	-0.169		
	(1.198)		
ICI	-0.075	-1.204	0.017
	(1.203)	(0.761)	(0.055)
Observations	20616	6847	17534 (5189 couples)
First Stage F-test			10.47
Fixed Effects	Munic & Year	Munic & Year	Munic-Pairs

† Standard Errors in parentheses clustered by Municipality (Column 1 and 2) or by Municipality Pairs (Column 3): $*\ \ p < 0.1$, $**\ \ p < 0.05$, $***\ \ p < 0.01$. In Column (3) tax rates are instrumented using the same set of political variables as in the baseline specification.