A Race to the Top? Staggered Electoral Cycles and Strategic Interactions in Business Taxes

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Abstract

Many theories predict strategic interactions in setting taxes across neighboring jurisdictions, but few papers use quasi-experimental settings for causal empirical evidence. This paper exploits staggered elections, which create variation in neighbor's tax rates that is driven by election-year manipulations rather than reverse causality or spatially correlated omitted variables. Municipalities only mimic the tax rate increases that occur in their neighbor's post-election years, but not the tax rate decreases that occur in election years. Consistent with yardstick competition, tax increases in neighboring municipalities create a window of opportunity for politicians to implement fiscally necessary tax increases in their own municipalities.

Keywords: Fiscal interaction, yardstick competition, tax competition, electoral cycles, fiscal federalism

JEL classification: H20, H71, H77

1. Introduction

The view that tax competition has caused the decline of tax rates on mobile factors in recent decades is prevalent in the academic and public discourse. However, while effective capital tax rates at the country level have declined over the past decades, they are not zero. Moreover, a recent quasi-experimental literature (e.g., Lyttikäinen, 2012; Baskaran, 2014; Isen, 2014; Agrawal, 2015; Baskaran, 2019; Eugster and Parchet, 2019; Parchet, 2019) has found little evidence that tax rates in a jurisdiction positively influence tax rates in neighboring jurisdictions (positively sloped reaction functions). This, however, is a necessary condition for a race to the bottom in tax rates. Moreover, positively sloped reaction functions are not a sufficient condition for a race to the bottom caused by tax competition: even if reaction functions were positively sloped, it would not be clear that this pattern is explained by tax competition, as implications of different theoretical models are observationally equivalent (Bordignon et al., 2003). For example, positively sloped reaction functions are, *a priori*, also consistent with yardstick competition. Recently, Chirinko and Wilson (2017) have argued that common shocks rather than competitive responses may cause interactions in tax rates. The setting studied in this paper could be a prime example for the importance of common shocks on the evolution of the business tax rate: in the context of rising costs for German local governments, both business tax rates and municipal debt strongly increased in the past decades (Figure A1). Thus, common cost shocks might explain a race to the top in business tax rates.

This paper adds to the small, recent quasi-experimental literature by estimating causal interactions in business tax rates and moreover, identifying the main mechanism underlying an interaction in business tax rates. Existing studies have rather focused on identification and less on the explicit reason for tax mimicking – either because no significant tax mimicking was found in the first place or because of the observational equivalence of different explanations. While I find positively sloped reaction functions, the pattern of the empirical results casts doubts on the interpretation that these positively sloped reaction functions are associated with tax competition. Rather, I find evidence for – what I call – asymmetric yardstick competition, where, in the context of rising costs for local governments, politicians react to positive tax rate changes in neighboring municipalities by increasing tax rates themselves, but leave tax rates unchanged when neighbors decrease their tax rates.

For causal identification, I exploit staggered election dates across municipalities, which imply that neighboring municipalities are, in general, at different points in their electoral cycle.[2] Therefore, neighboring municipalities implement, in line with electoral cycle theories (e.g. Rogoff, 1990; Rogoff and Sibert, 1988; Shi and Svensson, 2006) tax rate decreases in election years and tax rate increases in post-election years (Foremny and Riedel, 2014); due to electoral considerations that have caused these changes, they are arguably exogenous to the home municipality's tax rate.

Using election year dummies as instrumental variables for the tax rates of neighboring municipalities, I find positive interactions in business tax rates. Heterogeneity analyses, however, show that municipalities only react to the tax rate increases implemented by their neighbors in their post-election years. Thus, tax rate increases in post-election years of neighboring municipalities create a window of opportunity for politicians in the home municipality to also implement tax rate increases. This effect is more pronounced if the home municipality has a higher pressure on the public budget as measured by the amount of outstanding public debt. In such a case, where reaction functions are positively sloped only for tax increases, but flat otherwise, a race to the top in tax rates, as suggested by Figure A1, can occur.

2 Staggered election dates have been used in the recent literature to identify electoral cycles (e.g., Garmann, 2017; 2018; Repetto, 2018), as a determinant of different nomination schemes of public officials (Ade, 2014; Garmann, 2015; Hessami, 2018) or as a determinant of concurrent elections (Bracco and Revelli, 2018; Garmann, 2016; Leininger et al., 2018). I am only aware of one paper (Ferraresi, 2018) that uses staggered elections to induce variation in neighbor's policy. Specifically, Ferraresi (2018) uses staggered elections to induce variation in neighbor's spending, and finds positive interaction effects in spending. He does not examine tax rates.

2. Sources of interaction

Interactions in tax policy can be theoretically explained via four main models.

First, tax competition models predict that jurisdictions set tax rates strategically to attract tax bases from neighboring jurisdictions. A key condition is that these tax bases are mobile and react to changes in taxes. While tax competition is often considered to imply a race to the bottom in tax rates (and thus positively sloped reaction functions), depending on the underlying assumptions of a tax competition model, reaction functions may well be flat or negatively sloped. Specifically, the sign of the reaction function depends on three main ingredients of the tax competition model. First, it depends on the variable over which governments optimize. Negative reaction functions can occur if governments do not optimize over tax rates, but rather over expenditure levels and let tax rates adjust residually (Wildasin, 1988; 1991). Second, the sign of the reaction function depends on the payoff function: if private goods are valued more than public goods (Brueckner and Saavedra, 2001; Vrijburg and de Mooij, 2016), if the income elasticity of public goods is smaller than the income elasticity of private goods (Chirinko and Wilson, 2017), or if public goods and private consumptions are complements (Mintz and Tulkens, 1986), reaction functions may be flat or even negative. Third, Parchet (2019) has recently shown that the modelling of production can also influence the slope of the reaction function; specifically, economics of scale in the production of the public good may lead to negatively sloped reaction functions.

A second source of interactions are benefit spillovers, which arise if residents or firms of a jurisdiction can benefit from public services provided by neighboring jurisdictions. Then, public good provision by one jurisdiction creates a positive externality for other jurisdictions, which incentivizes them to be spend inefficiently low amounts on public goods (Case et al., 1993). Thus, municipalities would decrease their expenditures and tax rates as a response to an increasing level of public good provision by their neighbors. Under the existence of benefit spillovers, reaction functions are negatively sloped. However, public good provision of a municipality may also impose a negative externality on its neighbors; for example, higher police spending in a municipality may incentivize criminals to shift their activities to neighboring municipalities, forcing these neighboring municipalities to also increase police spending. In such a case, positively sloped reaction functions are possible.

A third source of interdependencies is yardstick competition (Besley and Case, 1995). Yardstick competition can arise if the true costs of providing public services are unknown to voters, but fiscal policy is common knowledge. Then, voters may reason that the costs of providing public services in neighboring municipalities are likely correlated with the costs in their jurisdiction, and fiscal policy of the neighboring municipalities may therefore provide a yardstick with which the performance of the own government can be compared. If neighboring municipalities decrease taxes and expenditures, then there is an incentive for the jurisdiction's politicians to do so themselves. Likewise, if neighboring municipalities increase taxes and expenditures, there is more leeway for local politicians to use public funds for their own benefit, and they will implement increases themselves. Thus, in general, under yardstick competition reaction functions are positively sloped (see, however, a potential exception mentioned by Bordignon et al., 2004).

Finally, fiscal policies could be interdependent because of social learning (Becker and Davies, 2017). This might occur if policy-makers have limited information about potential policy effects, and therefore try to learn from neighboring jurisdictions with potentially similar characteristics. Under social learning, policies converge and fiscal variables are strategic complements.

Distinguishing between these different sources of interactions is important, because they have very different welfare implications. While tax competition and benefit spillovers lead to inefficiently high or low levels of public good provision, yardstick competition is considered to be able to alleviate agency problems between politicians and citizens, and might thus increase welfare by constraining politicians' rent-seeking. Social learning might deliver additional information that allows for more appropriate policy setting and can thus increase welfare.

3. Institutional Setting and Staggered Elections as Quasi-Experiment

I study a panel dataset of all 426 municipalities of the German state of Hesse in the period 1993-2017. Hesse has staggered election dates for the local executive that I will exploit for identification. The period 1993-2017 is characterized by rising expenditures for local governments (Figure A1 in the appendix). These increasing expenditures have mainly been caused by a strong increase in social spending: for example, since 1996, there is a legal claim for the provision of kindergarten capacities for children in the age of three to six, which has been extended later to children below the age of three. Moreover, after the labor market reforms in the 2000s, municipalities are responsible for accommodation of the long-term unemployed, and for additional social security benefits for the elderly and those in specific need due to disabilities (*bedarfsorientierte Grundsicherung im Alter und bei Erwerbsminderung*). Finally, municipalities are responsible for accommodation of asylum seekers, which has recently led to increased costs in the course of the refugee crisis. While state and federal government have, as a compensation, increased transfer payments to local governments, these transfers are in general lump sum payments and thus not directly related to local government's rising expenditures. In sum, the increased expenditures of the municipalities were therefore not completely compensated by these transfers (Deutsche Bundesbank, 2000; 2007; 2016).

There are two profit taxes, for which the tax base consists of operating profits. First, the corporate tax is levied by the federal government. Second, the local business tax is set at the municipality level. Precisely, municipalities do not set the tax rates for the local business tax, but tax multipliers that are multiplied by the so-called *Steuermesszahl* to yield the tax rate. As the *Steuermesszahl* is, however, set at the federal level (as is the tax base for the local business tax) and is therefore fixed across municipalities, the tax multiplier effectively determines the tax rate. In the sample employed in the empirical analysis, tax multipliers for the business tax range from 200% to 515%. The local business tax is the most important German tax on business profits in terms of revenues. Moreover, it is the most important source of financing at the disposal of municipalities, as it generates approximately 75% of municipal tax revenue (Fuest et al., 2018).

Besides own tax revenues, municipalities finance themselves mainly through public debt, intergovernmental grants, and parts of the federal income and value added tax revenue; the tax rates of the income and of the value added tax are, however, set at the federal level and do not vary across municipalities. Municipalities fulfill both mandatory tasks (such as basic administration and social welfare) that have been assigned to them by higher-tier governments as well as voluntary tasks (such as culture and sports). Taken together, municipalities are, among other things, responsible for administration, public order (but not police), culture, public transport, infrastructure, and sports.

In Hesse, there are two political institutions to be elected: First, the municipal council as the local legislature, which has the ultimate decision power in all economic affairs that concern the municipality, such as setting the business tax multiplier, is elected at a uniform date across Hesse's municipalities. Because these elections occur at the same date across all municipalities, it is not possible to use these elections as an instrument for neighbor's policies, as it is not possible to differentiate these elections from time effects common to all municipalities. Second, voters elect the head of the public administration (*Bürgermeister*), which implements the decisions of the council. Contrary to the council elections, the *Bürgermeister* elections take place at staggered dates across municipalities.

These staggered dates are caused by historical reasons that are unlikely to be related to recent tax policies. In the beginning of the 1990s, the nomination scheme of the *Bürgermeister* was changed from appointment by the council to election by voters. The date of the first election depended on whether the contract of the last appointed *Bürgermeister* ended, which differed across municipalities for two reasons (Garmann, 2016, Hessami, 2018). First, Hesse's municipalities were formed in the 1970s from municipal mergers that took several years to complete. The state government had planned the merger process county by county, and municipalities from some counties were merged earlier than those from other counties. Some municipalities were therefore able to appoint their first *Bürgermeister* earlier than others. This affected the dates of all subsequent appointments. Second, not all appointed *Bürgermeister* served their full term, which also affected the starting dates of all subsequent terms. Thus, first elections were held during a quasi-random phase-in period 1993-1998. Elections are still on different dates today.

Because the merger process was implemented county by county, there could be some coincidence in the occurrence of elections for neighboring municipalities. Indeed, I have found such a coincidence. An election in municipality j in year t increases the probability that municipality i also holds an election in that year by approximately five percentage points. This effect is statistically significant, but numerically very small. I deal with this issue by always including (pre-/post-) election dummies for municipality i as control variables.

If there is an electoral cycle in tax rates around staggered *Bürgermeister* elections[3], in a given year some municipalities will – due to elections – change their tax rates, while their neighbors will not have any electoral incentives to do so. Thus, any change in the tax rates of

3 As tax rates are set by the council, the electoral cycle in tax rates before *Bürgermeister* elections is created by the council. A follow-up question is what incentives the council should have to implement tax cuts before executive elections. There are two plausible explanations. First, voters are uninformed about the distribution of responsibilities at the local level, and use fiscal variables to assess the competence of the local executive even though these variables are not fully determined by it. In such a case, one would expect that political budget cycles are more pronounced if *Bürgermeister* and council have the same affilication ("unified government"). Second, voters may be fully informed that the political budget cycle is generated by the council, but tend to reward the candidate of those parties that have implemented tax cuts in the council at the *Bürgermeister* elections. Garmann (2018) finds that political budget cycles are more pronounced under unified governments.

neighbors should be a reaction to the electoral cycle, but not caused by reverse causality or omitted correlated local shocks.

Reverse causality would exist if election dates in municipality j depended on tax rates in municipality i. I consider this unlikely, as municipalities seem to have closely followed the staggered election dates that have been determined by the switch in the nomination scheme. In particular, if municipalities have followed this schedule closely, one would expect that the pattern in the distribution of elections over the 1993-1998 period shows up in subsequent six-year cycles, i.e., the distribution of elections over the years 1999-2004 would be very similar to the distribution over the years 1993-1998, and so on. As can be seen from Figure A2, which plots the number of elections per year, this is the case (Garmann, 2019). In general, it would be possible for municipalities to deviate from the staggered six-year cycle by holding early elections. However, early elections are only possible if the incumbent steps down or if she is removed from office. If the *Bürgermeister* steps down before her term ends, she will lose all pension entitlements if not re-elected in the upcoming election. This represents a significant monetary loss. Moreover, the hurdles for removing the *Bürgermeister* from office are extremely high; therefore, it has only occurred only in a handful cases so far.

A further concern for the identification strategy could be spatially correlated omitted variables, i.e., variables that simultaneously affect election dates in j and tax rates in i and are not captured by the included fixed effects and control variables. As every municipality has to hold elections from time to time, municipalities with elections in a specific year likely do not differ in socio-economic characteristics from those without elections in a specific year. There might of course be spatially correlated economic shocks – such as a recession – that affect local tax rates. However, to invalidate the empirical approach, these local shocks must also influence election dates in j. It is unlikely that this happens in the present setting. Besides the difficulty of scheduling early elections as explained above, the *Bürgermeister* has very limited influence on policy, and especially it is unlikely that the *Bürgermeister* has significant influence on economic policy. It is even doubtful that any local institution has so much influence on policy that it could influence the occurrence of recessions. Therefore, it is very unlikely that citizens attribute the economic situation to the *Bürgermeister* or even force her to step down because of a recession, and spatially correlated shocks are therefore unlikely to influence election dates. Moreover, I always control for the electoral cycle of municipality i (although the results are insensitive to excluding these control variables). If spatially correlated shocks lead to early elections, then this should also occur in municipality i. Controlling for election indicators of municipality i thus also captures the (unlikely) occurrence of early elections due to spatially correlated omitted variables.

4. Estimation approach

In the baseline approach, which I call border-pair specification, I create a sample consisting of all pairs of bordering municipalities. In other words, I define neighboring municipalities as those that share a border. I thus assume implicitly that reactions only occur between direct neighbors. This yields 1,138 pairs that enter the sample twice, i.e., each municipality of a pair is used on the left- and on the right-hand side of the estimation equation. Summary statistics for this sample are in Table A1. Table A1 furthermore shows the average change of the business tax rate in the year before an election, in the election year, and in the year after an election. The average change in the post-election year is larger than in the election year and in the pre-election year. The simple OLS estimation equation in the border-pair specification reads

$$\Delta\text{Business Tax Rate}_{it} = \alpha_{ij} + \lambda_t + \beta\Delta\text{Business Tax Rate}_{jt} + u_{ijt}, i \neq j \qquad (1)$$

where α_{ij} captures a pair fixed effect, i.e., a fixed effect for each of the 2276 ij - neighbor pairs, λ_t denotes a year-fixed effect, u_{ijt} is an error term, and Δ denotes the first-difference operator.[4] By including pair-fixed effects, I am differencing out all permanent factors at the pair level that affect the relationship between tax rates in i and j.[5] For example, topographical features could affect the relationship between the tax rates of a pair and do not vary over time. However, identifying the parameter of interest, β, i.e., the causal effect of tax rates in municipality j on tax rates in municipality i, is likely still impossible with this simple OLS specification: for example, if tax rates in both municipalities are subject to the same local, time-varying shock, then the observation that tax rates in municipality i change at the same time as tax rates in j might not be a direct reaction to tax rates in j but rather a similar response to the local shock. Similarly, if tax mimicking exists, tax policies of neighboring municipalities will be, by design, simultaneously determined. I will therefore report estimation results from (1) only as a benchmark for more sophisticated approaches.

What is needed for identification of tax mimicking is a shock to policy in municipality j that is likely exogenous to tax rates in municipality i. For reasons outlined above, the electoral cycle in municipality j likely affects tax rates in j and, because of the staggered timing of elections, is likely exogenous to tax rates in municipality i. Therefore, I employ dummy variables indicating the election year, the year before an election (pre-election year) and the year after an election (post-election year) in municipality j as instrumental variables for tax rates of j. Formally, the IV estimation approach reads

$$\begin{aligned}
&\Delta\text{Business Tax Rate}_{jt} = \mu_{ij} + \eta_t + \delta_1\text{Pre-Election}_{jt} + \delta_2\text{Election}_{\text{jt}} \\
&+\delta_3\text{Post-Election}_{\text{jt}} + \gamma_1\text{Pre-Election}_{\text{it}} + \gamma_2\text{Election}_{\text{it}} + \gamma_3\text{Post-Election}_{\text{it}} + \varepsilon_{ijt} \\
&\Delta\text{Business Tax Rate}_{it} = \alpha_{ij} + \lambda_t + \beta\Delta\text{Business Tax Rate}_{jt} \\
&+\rho_1\text{Pre-Election}_{\text{it}} + \rho_2\text{Election}_{\text{it}} + \rho_3\text{Post-Election}_{\text{it}} + u_{ijt}
\end{aligned} \qquad (2).$$

4 Both Figure A1 as well as panel unit root tests strongly suggest that business tax rates are non-stationary. Therefore, I employ first differences. An alternative would be to use growth rates. Doing so does not change the results.

5 Results do not change if pair-fixed effects are excluded.

The identification assumption is that the instruments employed in the first stage, i.e., the (pre-/post-) election indicators of municipality j, are exogenous to tax rates in municipality i. In all specifications, I control for the electoral cycle of in municipality i, because elections in i could influence tax rates in i as suggested by electoral cycle theories, and the occurrence of elections in i is, because of spatial correlation in the election timing as described above, potentially correlated with elections in j. I cluster standard errors at the pair level.[6]

In a more general specification, I define neighborhood to be based on distance. Thus, I allow for interaction effects in tax rates also for those municipalities that do not share a border. Specifically, I create a sample consisting of all possible municipality-pairs (i.e. 426*426-426 pairs) and calculate the distance d_{ij} between all municipality-pairs based on Gauß-Krüger coordinates. I then estimate the following specification, which allows using observations from municipalities within a pre-specified distance bandwidth $\left(\underline{d}, \overline{d}\right]$:

$$\Delta \text{Business Tax Rate}_{\text{it}} = \alpha_{ij} + \lambda_t + \beta \Delta \text{Business Tax Rate}_{\text{jt}} + u_{ijt}, \underline{d} < d_{ij} <= \overline{d} \qquad (3)$$

An implementation of (3) in an IV context is, of course, straightforward. Importantly, the spatial reach of strategic interactions in policy has only been addressed by few studies. Eugster and Parchet (2019) as well as Parchet (2019) estimate the spatial reach of tax interactions to be of roughly 15-20 kilometres. Agrawal (2015) finds interactions within 30 minutes driving time, which fits well with a spatial reach of 15-20 kilometers. Janeba and Osterloh (2013) infer from a survey with which municipalities German public officials perceive the most intense tax competition, but study city sizes (in three different size categories) instead of distances. If weighting matrices are used, these require an *a priori* chosen distance within which fiscal policy interactions can occur; because of limited degrees of freedom, these weighting matrices cannot

6 I allow standard errors of pair ij to be correlated with standard errors of pair ji . Thus, there are 1138 clusters in the pair dimension, and my clustering approach allows for unrestricted serial correlation over time within each pair. Restricting serial correlation in the standard errors such that errors of pair ij are not allowed to be correlated with errors of pair ji does not change the results.

be estimated from the data. (3) has the advantage that it allows, by estimating effects for different distance bandwidths, gauging the spatial reach of tax interactions.

It is straightforward to include time-varying control variables, both for municipality i and j, in (1)-(3). I include the population size, the age structure of the population (proportion of people aged below 15 and above 65), the population density, the share of citizens with German citizenship, the share of females, the vote shares of the four most important political parties (the center-right CDU, the center-left SPD, the Greens, and the liberal FDP) at the last state election, and dummy variables for election years of the county administrator (*Landrat*).[7]

An important point when interpreting the estimates is whether they identify the unconditional effect of tax rate changes in municipality j on tax rate changes in municipality i. First, electoral cycles in municipality j may lead to changes in other policies of municipality j that, via the budget constraint, are in turn reflected in j's tax rates. In such a case, the estimates on tax interactions should be interpreted as the interaction effects of tax rates conditional on endogenous changes in other policies (Moretti and Wilson, 2017). Second, electoral cycles in j might, besides its tax rates, influence other policies of municipality j that directly spillover to municipality i and affect i's tax rates directly. Such an effect (if significant), however, would still be informative, because it would clearly indicate that policy interactions exist, although they might not necessarily occur directly in tax rates.

5. Results

5.1 Evidence for tax mimicking

7 Additionally, in non-reported specifications, I include the party affiliation of the *Bürgermeister* as an additional control variable (for which there is missing information in some municipalities in the period 1993-1998 before the switch in the nomination scheme), distinguishing between *Bürgermeister* from the center-left SPD, *Bürgermeister* from the center-right CDU, and those with other affiliations. Furthermore, I include real per capita GDP, which is available at the county level and has missing values for 1994. The results do not change when these controls are included.

This section estimates the effect of changes in business tax rates in municipality j on business tax rates in municipality i. First, to provide a benchmark, Table 1 shows results from the simple OLS estimation approach (1), i.e., I regress changes in tax rates of municipality i on changes in tax rates of municipality j, pair- and year-fixed effects. I find a highly significant, positive, but numerically very small relationship. The point estimates suggest that a one-point increase in the tax multiplier of municipality j is associated with an increase in the tax multiplier of municipality i of roughly 0.05 points. This estimate is virtually unchanged if control variables are included. However, it is questionable whether these OLS estimates provide causal effects.

Table 2 shows the results of the IV procedure (2). In part (a), it shows the first-stage estimates, i.e., the effect of elections in municipality j on its tax rates. Thus, part (a) investigates whether there is an electoral cycle in business tax rates. Specifically, part (a) shows that there is no effect of pre-election years on tax rates changes, that election years have a highly significant, negative effect on business tax rate changes, and that post-election years have a significantly positive effect. Interestingly, the magnitude of the latter two effects is approximately the same, and the null hypothesis that the sum of these two countervailing effects is zero cannot be rejected (p-value: 0.159). In sum, these effects transfer into a first stage F statistic that is larger than 20; the electoral cycle instruments are therefore highly relevant.

Given an average tax multiplier in the sample of 335, the electoral cycle is, although statistically significant, numerically quite small, as the first-stage results suggest that the effect of election years on the business tax multiplier changes is less than one unit. However, I see no reasons why it would be impossible to identify strategic reactions from relatively small changes. Rather, if yardstick competition or social learning are the underlying mechanisms and neighboring municipalities react even to small changes, this makes the argument stronger, as obviously even small changes incentivize local governments to reset their policy. Moreover, if tax competition is the underlying mechanism, then – under the assumption that the marginal utility of the public good is decreasing – one would even expect stronger effects for small tax changes.[8]

Part (b) shows the reduced form relationship between elections in municipality j and tax rates in municipality i. Interestingly, there is only a significant effect of post-election years in j on tax rates in municipality i; neither a pre-election nor an election year in j have a significant effect on i's business tax rates. Specifically, post-election years in municipality j have a positive effect on business tax changes in municipality i. This already suggests that there is some important heterogeneity in the results that will be further investigated below. The positive effect of post-election years in j on tax rate changes in i is slightly smaller than the positive effect on tax changes in j. In other words, it can already be seen that, while there is a positively sloped reaction function at least for positive effects on tax rate changes, its slope must be smaller than 1.

Finally, part (c) shows IV estimates. In this unconditional specification (in the sense that the above-mentioned potential heterogeneity in reactions is neglected here), tax multipliers in i react significantly positively to tax multipliers in j and thus tax rates appear to be strategic complements. Numerically, the results suggest that the slope of the reaction function takes a value of roughly 0.3 and is therefore considerably larger than in the simple OLS specification.

To visualize the heterogeneity suggested by the reduced form results, I first plot the point estimates for the election indicators of municipality j in the first stage (panel (a) of Figure A3) and those from the reduced form results (panel (b) of Figure A3) alongside 95% confidence intervals. The figure shows in panel (a) that election years in municipality j have a significantly

8 Specifically, an increase in the neighbor's tax rate increases the tax base in i and therefore the marginal revenue of increasing tax rates. However, at the same time, the marginal utility of the public good decreases. It could then be optimal to react to large tax rate changes of neighboring municipalities, which strongly increase the tax base, with small tax rate changes in order to not let the marginal utility of the public good decrease too strongly (Parchet, 2019).

negative effect on its own tax rate change and that post-election years have a significantly positive effect of approximately the same magnitude. Panel (b) shows that only post-election years of municipality j have a significant effect on tax rate changes in municipality i. The figure thus suggests that the positive effect of post-election years drives the positive reaction function. To investigate this further, I show estimates of the reaction functions using only (i) the variation from election years (i.e., using only the election year dummy as instrument) and (ii) the variation from post-election years (i.e., using only the post-election year dummy as instrument). Column 1 of Table 3 shows that, when only the variation from the election year is used, the reaction function is insignificant and smaller than in Table 2; when, however, only the variation from the post-election year is used, the reaction function is larger than 0.6 and statistically significant (Column 2).

The positive effect of post-election years in j on tax rate changes in i could be either caused by an increasing likelihood of tax rate increases in post-election years or a decreasing likelihood of tax rate decreases in post-election years. In general, positive reaction functions imply that municipality i increases tax rates if municipality j increases tax rates or that municipality i decreases tax rates if municipality j decreases tax rates. To examine whether tax rate increases or tax rate decreases drive the positive interaction effects, I use the variation from the post-election year in municipality j to estimate the effect of tax rate increases (decreases) in j on tax rate increases (decreases) in i. Specifically, I use dummy variables as outcomes taking the value one for a tax rate increase (decrease), and zero otherwise. As shown (Table A2), post-election years in j increase the likelihood of tax rate increases in j (first stage in panel (a)) and lead to tax rate increases in i (second stage in panel (b)). This effect is large (above 0.5) and statistically significant at the 10% level. At the same time, post-election years in j lower the probability of tax rate decreases in j, but this lower probability of tax rate decreases in post-election years of municipality j does not lead to a lower probability of tax rate decreases in i. If I use the variation in tax rate increases and decreases in j that stems from election years (Table A3), there is no significant interaction between increases (decreases) in j and increases (decreases) in i. To summarize, the positive interaction effect in post-election years appears to stem from the mimicking of tax rate increases.

Figure A4 shows the results of specification (3). Specifically, I show coefficient estimates alongside 95% confidence intervals for distance bandwidths of 10 kilometers (using bandwidths of 15 or 20 kilometers gives similar results). There is a positive interaction effect between municipalities within a distance of less than 10 kilometers when the full electoral cycle is used as instrument. However, if only election year dummies are used as instruments, the interaction in business taxes is virtually zero even for closely located municipalities. When only post-election year dummies are used as instruments, the interaction effect is significant and much larger than before. Thus, the baseline results can also be found in a specification that is based on distance rather than direct neighborhood. For distances above 10 kilometers, the interaction effects are essentially zero. In very few cases, interactions effects are significant if distances are larger than 60 kilometers. However, these significant effects are small in magnitude, and the first stage F statistic is very large, which is likely driven by a very large number of observations, i.e., many municipalities lie in these distance intervals. In particular, many observations lie in the intervals $(50,60]$ and $(60,70]$. Therefore, the standard errors are very small, which causes the numerically small effects to be very precisely estimated.

I have subjected these results to several robustness checks that can be found in an online appendix.

5.2 Mechanism

I argue that my results are consistent with a form of yardstick competition, in which tax hikes in post-election years of neighboring municipalities create a window of opportunity for politicians of the home municipality to increase taxes themselves without suffering political costs. Specifically, tax rate increases may have political costs (voters dislike high taxes and perceive them as a sign of bad public policies), but voters may be more willing to accept tax rate increases if these are considered necessary. Absent any precise knowledge of voters of when tax increases are necessary, politicians may refer to tax increases of neighboring municipalities to justify tax increases in the home municipality.

This suggests that, in contrast to the traditional yardstick competition theory, voters do not use neighboring municipalities as yardsticks on their own, but rather that politicians use neighboring municipalities as a yardstick whenever they need to signal that tax increases are necessary. Of course, politicians would not mention that the tax increases in the post-election year of the neighboring municipality follow tax decreases of a similar magnitude in the election year.[9] Surely, this mechanism necessitates that voters do not see through that neighboring municipalities decreased their tax rates before they implemented tax rate increases. Moreover, voters do not understand that the tax rate changes implemented in neighboring municipalities derive from electoral motivations rather than fiscal necessity. Thus, voters are either irrational and myopic or have limited information. While these may be strong assumptions, these conditions are essentially those that are necessary to generate electoral cycles in the first place: the seminal model by Nordhaus (1975) assumes irrational voters, while subsequent models assume information asymmetries between voters and politicians (e.g., Rogoff and Sibert, 1988; Rogoff, 1990; Shi and Svensson, 2006). In other words, if there were electoral cycles, then we would regard these assumptions to be plausible.

A testable implication of the yardstick competition theory is that tax increases should be implemented when it is politically least costly to do so. Therefore, reactions of municipality

9 An alternative interpretation is that voters use neighboring municipalities as yardsticks on their own, but are more concerned about tax rate increases than tax rates decreases. This would be consistent with loss aversion of voters (Alesina and Pascarelli, 2017; Lockwood and Rockey, 2018). In such a case, political costs associated with tax rate increases are larger than political benefits from tax rate decreases as voters are more likely to vote to avoid an unfavorable policy than to support a favorable policy.

i to tax increases of municipality j should be smaller shortly before elections in municipality i. With staggered elections, this can be tested empirically as it is possible to have a (pre-) election year in municipality i in the same year as a post-election year (and thus a tax increase) in municipality j. I estimate the following reduced form approach:

$$\begin{aligned}\Delta \text{Business Tax Rate}_{it} &= \mu_{ij} + \eta_t + \delta_1 \text{Post-Election}_{jt} + \delta_2 \text{Post-Election}_{jt} \text{BeforeElection}_{it} \\ &+ \gamma_1 \text{Pre-Election}_{it} + \gamma_2 \text{Election}_{it} + \gamma_3 \text{Post-Election}_{it} + \varepsilon_{ijt}\end{aligned} \tag{4}$$

where $\text{BeforeElection}_{it}$ is a dummy variable with a value of one if municipality i is in a pre-election or election year, and we would, in the case of yardstick competition, expect δ_2 to be significantly negative. Similarly, the equation

$$\begin{aligned}\Delta \text{Business Tax Rate}_{it} &= \mu_{ij} + \eta_t + \beta_1 \Delta \text{Business Tax Rate}_{jt} \\ &+ \beta_2 \Delta \text{Business Tax Rate}_{jt} \text{BeforeElection}_{it} \\ &+ \gamma_1 \text{Pre-Election}_{it} + \gamma_2 \text{Election}_{it} + \gamma_3 \text{Post-Election}_{it} + \varepsilon_{ijt}\end{aligned} \tag{5}$$

can be estimated via an IV approach, in which $\Delta \text{Business Tax Rate}_{jt}$ and $\Delta \text{Business Tax Rate}_{jt} \text{BeforeElection}_{it}$ are instrumented by $\text{Post-Election}_{jt}$ and $\text{Post-Election}_{jt} \text{BeforeElection}_{it}$.[10] Again, we would expect that $\beta_2 < 0$. Panel (a) of Table A7 shows that municipality i only reacts to post-election years in municipality j if i is not in a pre-election or election year. Correspondingly, panel (b) shows that the reaction of municipality i to tax rate changes in j is significantly positive in i's non-election years and close to zero and insignificant in pre-election and election years of municipality i. This suggests that, consistent with yardstick competition, reactions occur when it is politically least costly.

A difference of the present setting to the theoretical mechanism in the yardstick competition literature is that politicians might not increase taxes in order to accumulate political

10 Because of the interaction term, which is instrumented by multiplying the instrument with an exogenous variable, the first stage F statistic is quite low (roughly three). When the instruments are weak, IV estimates can be biased. However, this is not the case for the parameters of the reduced form in (4), which are proportional to the causal effects of interest and, as they is estimated with OLS, unbiased (Angrist and Pischke, 2009).

rents, but because this is necessary in an era of rising expenditures. In practice, the local budget laws in Germany are relatively restrictive in that credits have to be repaid by own revenues; in contrast to the state or federal level, it is not possible to delay repayment of debt by follow-up financing on the capital market for an unlimited amount of time. These requirements have led municipal debt to increase at a much slower rate than state or federal debt (Deutsche Bundesbank, 2000; 2007).[11] Politicians at the local level may thus implement the necessary tax rate changes to repay debt when it appears politically least costly, which is when also neighboring municipalities increase taxes. If this interpretation is plausible, we expect that the positive reaction is stronger when there is more pressure on the municipal budget, i.e., municipal debt is higher.[12] I divide the sample into high and low debt observations, i.e., those observations with above and below median real per capita debt, and investigate the reaction to tax rate changes of neighboring municipalities that are identified by post-election years only. Table A8 shows that those municipalities with above median debt show a twice as large reaction to the tax rate increases of their neighbors. Moreover, only the reaction of high debt municipalities is statistically significant.[13]

As discussed, further mechanisms that explain interactions in tax rates are tax competition, benefit spillovers, and social learning. If tax competition were the underlying mechanism, it would be very questionable why municipalities do not mimic the tax rate decreases of neighboring municipalities in election years, as tax rate decreases should – in the

11 Relatively strong increases in municipal tax rates, which have been observed especially in Hesse, have arguably occurred because municipalities had to repay debt (Deutsche Bundesbank, 2016).

12 In a recent contribution, Janeba and Todtenhaupt (2018) show that a jurisdiction with a high level of initial debt can find it optimal to decrease taxes if public borrowing is restricted (as is the case at the German municipal level). Their argument is that a high level of public debt leads to decreasing investment in public infrastructure, which makes the jurisdiction a less attractive location for firms. To compensate this, the jurisdiction lowers taxes to re-attract some of these firms. This argument rests on the assumption that firm location reacts to tax rates. When firm location reacts to tax rates (and infrastructure investment), it may be possible to generate more revenues to repay debt by lowering tax rates. If firm location does not react to tax rates, as is the case here (Table A9), more revenues can be generated by increasing tax rates.

13 Interestingly, the electoral cycle in i (results not shown, but available upon request) is much less pronounced in high debt than in low debt municipalities, and election year decreases in tax rates are insignificant in high debt municipalities.

tax competition case – drive out the tax base from the home municipality to neighboring municipalities. Thus, for the tax competition mechanism to be plausible, it is necessary that tax bases react to tax rate changes (Buettner, 2003) and that, in the present case, tax bases therefore react to elections. To investigate whether this is the case, I build a panel with municipality-year dimension and regress the logarithms of the business tax base[14] on municipality-fixed effects, year-fixed effects and the three election indicators:

$$\begin{aligned} &\text{Log}(\text{Business Tax Base}_{it}) = \alpha_i + \lambda_t + \beta_1 \text{Pre-Election}_{it} \\ &+\beta_2 \text{Election}_{it} + \beta_3 \text{Post-Election}_{it} + u_{it} \end{aligned} \quad (6)$$

Table A9 shows that all election indicators are insignificant. Therefore, as tax bases do not react, it is unlikely that tax competition explains the (conditionally) positive reaction function.

If benefit spillovers were the underlying mechanism, we would also expect that municipalities react to both positive and negative tax rate changes. Moreover, as explained, benefit spillovers in general imply negative rather than positive reaction functions. Positive reaction functions are possible, but require negative externalities (in such a case, it is of course not really appropriate to speak about benefits that spillover, but for simplicity, I use this term here also for negative externalities). At the local level, it is not straightforward to provide an example of a public good that delivers negative externalities for neighboring municipalities, especially because in Germany, police spending is a state responsibility. As a final caveat against benefit spillovers as the mechanism, benefit spillovers imply that public goods offered in a specific municipality (financed by higher taxes) need not be offered in neighboring municipalities, thus allowing them to decrease expenditures on public goods. In the case of negative externalities, the neighboring municipalities would have to provide more of the public good and thus increase expenditures. Thus, we expect that tax increases in a municipality affect

14 Following Buettner (2003), the tax base is calculated by dividing business tax revenues by the business tax multiplier. For 27 observations, the business tax base was negative (see Buettner, 2003, for an explanation). By taking logarithms, these observations automatically disappear from the sample (Buettner, 2003 has also used logarithms).

expenditures in neighboring municipalities if the effects of public good provision spill over. Using the approach in (2) and expenditures of municipality i as outcome in Table A10, I do not find that tax increases in j influence expenditures in i; rather, the point estimate is virtually zero and statistically insignificant.

Finally, positive reaction functions could be caused by social learning. However, again we would then expect that both positive and negative tax rate changes lead to a reaction. Furthermore, social learning is relevant if policy choices of jurisdictions represent private information about unobserved conditions that help to determine appropriate tax rates. This is obviously not the case for tax rate changes because of elections. These tax rate changes occur because of the desire to be reelected, but not because neighboring municipalities have gained new information about how to improve policy setting.

6. Conclusion

The empirical identification of interactions in policy across neighboring jurisdictions is prone to endogeneity concerns, and those studies that have addressed endogeneity through quasi-experimental designs come to diverse results. Moreover, existing studies have rather focused on identification and less on the explicit reason for tax mimicking – either because no significant tax mimicking was found in the first place or because different mechanisms are observationally equivalent. This paper uses staggered elections to instrument for variation in neighbor's policies, and finds that, on average, tax reaction functions are positively sloped. However, positively sloped reaction functions are only a necessary, but not a sufficient condition for a downward pressure on tax rates due to tax competition, as the present analysis demonstrates. Reaction functions are positively sloped because local governments mimic only positive tax rate changes, which leads to a race to the top. Instead of tax competition, the mechanism is rather consistent with yardstick competition, where tax increases of neighboring municipalities create a window of opportunity for home politicians to increase tax rates themselves without incurring political costs. This mechanism, meanwhile, rather seems to explain the *timing* of tax rate changes than the upward spiral in tax rates in the first place. This upward spiral can rather be explained by aggregate shocks such as a rising pressure on the local governments' budget.

There are important welfare implications of the results in this study. The typical form of yardstick competition in the literature is characterized as welfare increasing as comparisons with neighboring jurisdictions may constrain politicians' rent-seeking. The results of this paper, however, indicate that rather than because of rent seeking, politicians increase tax rates because of fiscal need, and the form of yardstick form identified herein suggests that the question is not *whether*, but *when* tax rates are increased. Thus, yardstick competition might not be welfare-increasing in the present setting. Conversely, that tax rate changes are timed for political reasons might even introduce a welfare-decreasing distortion if it is optimal to engage in tax smoothing (Barro, 1979).

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Table 1: Results from naïve OLS estimation procedure

VARIABLES	(1) ΔBusiness Tax_i	(2) ΔBusiness Tax_i
ΔBusiness Tax_j	0.056***	0.052***
	(0.011)	(0.011)
Controls	NO	YES
Observations	56,900	56,900
R-squared	0.101	0.106

The table shows the results of (1). Standard errors clustered at the pair level in parentheses (1138 clusters). ***Significant at the 1 percent level.

Table 2: Results from the electoral cycle approach

	(1)	(2)
(a) First stage results		
	ΔBusiness Tax$_j$	ΔBusiness Tax$_j$
Pre-Election$_j$	0.019	0.018
	(0.112)	(0.112)
Election$_j$	-0.661***	-0.662***
	(0.104)	(0.104)
Post-Election$_j$	0.409***	0.407***
	(0.126)	(0.126)
Observations	56,900	56,900
Kleibergen-Paap first stage F statistic	20.86	20.67
(b) Reduced form results		
	ΔBusiness Tax$_i$	ΔBusiness Tax$_i$
Pre-Election$_j$	0.039	0.035
	(0.111)	(0.111)
Election$_j$	-0.082	-0.073
	(0.110)	(0.110)
Post-Election$_j$	0.334***	0.332**
	(0.127)	(0.128)
Observations	56,900	56,900
R-squared	0.100	0.104
(c) Second stage results		
	ΔBusiness Tax$_i$	ΔBusiness Tax$_i$
ΔBusiness Tax$_j$	0.336**	0.325**
	(0.140)	(0.140)
Observations	56,900	56,900
Controls	NO	YES

The table shows the results (2) in which (pre-/post) election year dummies were used as instruments. Standard errors clustered at the pair level in parentheses (1138 clusters). **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 3: Using only variation from the election-year or post-election year

	(1) Only election year as IV	(2) Only post-election year as IV
(a) First stage results		
	ΔBusiness Tax $_j$	ΔBusiness Tax $_j$
Election $_j$	-0.748***	
	(0.101)	
Post-Election $_j$		0.544***
		(0.123)
Observations	56,900	56,900
Kleibergen-Paap first stage F statistic	56.23	19.67
(b) Second stage results		
	ΔBusiness Tax $_i$	ΔBusiness Tax $_i$
ΔBusiness Tax $_j$	0.197	0.626**
	(0.145)	(0.258)
Observations	56,900	56,900
Controls	YES	YES

The table shows the results of (2) in which only the election year dummy (column 1) or the post-election year dummy (column 2) is used as instrument. Standard errors clustered at the pair level in parentheses (1138 clusters). **Significant at the 5 percent level, ***Significant at the 1 percent level.

Appendix

1. Robustness checks
2. Additional tables
3. Figures

1. Robustness checks

To rule out that the results are spurious, I perform two placebo estimations. First, I assume that only municipalities whose name has the same first letter (Edmark and Ågren, 2008) are neighbors and can influence each other. Finding a significant interdependence effect with such a neighborhood definition would cast doubt on the baseline estimates. However, as shown (Table A4), the placebo effects are insignificant and close to zero. As a second placebo test, I randomize neighborhood status by assuming that 2% of all possible $i-j$-pairs are neighbors (i.e., on average, each municipality has 8.5 neighbors) and employing a random number generator for 0-1 neighborhood status. The resulting placebo estimates are again close to zero and insignificant (column 2 of Table A4).

The baseline analysis has used statistically significant, but numerically quite small tax rate changes as identifying variation. Do the interaction effects change if larger tax rate changes are used as identifying variation? To investigate this, I use that tax rate changes are more pronounced under unified governments (footnote 2 in the manuscript). As instruments, I use interaction terms of a unified government indicator (with the value one if the party with the absolute majority in the municipal council also holds the *Bürgermeister* office) with the electoral cycle variables in municipality j.[15] As unified governments in municipality j do not occur randomly, I include the non-interacted unified government indicator as a control variable. Table A5 shows the results when only the variation from the election year (column 1) and only the variation from the post-election year (column 2) is used in the first stage. In both cases, the first stage results show that the tax rate changes induced by the electoral cycle under unified governments are larger than in the baseline specification; the effect of post-election years on tax rate changes under unified governments is even four times as large as in the baseline

[15] The unified government indicator has missing data for the period before the switch in the nomination scheme, as the party affiliation of the *Bürgermeister* was not known before the switch (see also footnote 8 in the manuscript).

specification. Nonetheless, the results from the baseline specification carry over to the case of larger tax rate changes: there is no interaction effect when only election years are used as identifying variation, and there is a significant interaction effect when post-election years are used as identifying variation.

A caveat with specifications (1)-(3) is that tax rate changes in the neighboring municipality are only identified by elections in this municipality. In other words, a municipality can only cause a reaction in its neighbor's tax rate if it experiences an election. However, there may be indirect effects in the sense that an election in municipality j may influence tax rates in municipality k, and because municipality k borders municipality i, it influences also i's tax rate. Unless k also experiences an election in the specific year, the baseline modelling approach does not capture this indirect effect. If tax rates are strategic complements (as is the case at least for tax increases), the estimated effect will therefore be too small. This issue can be addressed with a specification in which tax rates of municipality i are regressed on a spatially weighted average of tax rates of its neighboring municipalities. As instruments, one can then use a spatially weighted average of the (pre-/post-) election dummy. In this case, the instruments can in principle affect the tax rates of all neighboring municipalities. Specifically, I estimate

$$\begin{aligned} &\sum_j w_{ij}\Delta\text{Business Tax Rate}_{\text{jt}} = \mu_i + \theta_t + \rho_1\text{Pre-Election}_{\text{it}} + \rho_2\text{Election}_{\text{it}} \\ &+\rho_3\text{Post-Election}_{\text{it}} + \gamma_1\sum_j w_{ij}\text{Pre-Election}_{\text{jt}} \\ &+\gamma_2\sum_j w_{ij}\text{Election}_{\text{jt}} + \gamma_3\sum_j w_{ij}\text{Post-Election}_{\text{jt}} + v_{it} \end{aligned} \tag{A1}$$

$$\begin{aligned} &\Delta\text{Business Tax Rate}_{it} = \alpha_i + \lambda_t + \beta\sum_j w_{ij}\Delta\text{Business Tax Rate}_{\text{jt}} \\ &+\delta_1\text{Pre-Election}_{\text{it}} + \delta_2\text{Election}_{\text{it}} + \delta_3\text{Post-Election}_{\text{it}} + u_{it} \end{aligned}$$

where α_i is a municipality-fixed effect and $\mathrm{w_{ij}}$ is an element of one of the four weight matrices W that I use. First, I consider only direct neighbors and give every neighbor of municipality i the same weight, i.e., the weight of each neighbor is $1/\mathrm{n_i}$, where $\mathrm{n_i}$ is the number of neighbors of municipality i ("$\mathrm{W^{direct}}$"). Second, I use again the same weights for all neighbors, but define the neighborhood of municipality i to consist of all municipalities within a distance of 10 kilometers ("W^{10km}"). Third, I weight all neighbors within 10 kilometers by inverse distance; thus, municipalities that are closer to each other are assumed to react more strongly ("$W^{distance}$"). Fourth, I give positive weights only to direct neighbors, but allow these weights to vary with population size; thus, larger municipalities are allowed to have a larger influence on their neighbors' tax rates ("W^{pop}").[16] Table A6 shows that the results are similar to the baseline results. The reaction function that is induced by post-election years in neighboring municipalities is, for every weight matrix, much larger than the one that is induced by election years. However, the reaction functions are not always significant (but point estimates are, as supposed, often much larger than in the baseline specification), as standard errors are quite large.

[16] All weight matrices are row standardized.

2. Additional tables

Table A1a. Summary statistics of border-pair sample

VARIABLES	(1) N	(2) mean	(3) sd	(4) min	(5) max
ΔBusiness Tax	56,900	2.66	9.84	-100	110
Pre-Election	56,900	0.17	0.38	0	1
Election	56,900	0.17	0.38	0	1
Post-Election	56,900	0.17	0.37	0	1
Population size	56,900	21,260	67,651	612	736,414
Proportion of old, 65+	56,900	18.52	3.34	8.40	31.80
Proportion of young, 0-15	56,900	15.10	1.99	8.24	22.60
Share of females	56,900	0.51	0.01	0.40	0.55
Population density	56,900	361	445	20	2,966
Share of German citizens	56,900	0.93	0.05	0.66	1.00
Vote share CDU	56,900	0.42	0.09	0.17	0.79
Vote share SPD	56,900	0.35	0.10	0.09	0.68
Vote share FDP	56,900	0.08	0.05	0.01	0.28
Vote share Greens	56,900	0.08	0.04	0.01	0.26
Election of county administrator	56,900	0.17	0.38	0	1

Note: As municipalities within a pair appear both on the left- and on the right-side of the estimation equation, the summary statistics are the same for municipalities i and j and therefore do not need to discriminate between i and j.

Table A1b. Average change of business tax rate

VARIABLES	(1) Pre-election year	(2) Election year	(3) Post-election year
ΔBusiness Tax	2.68	1.99	3.20

Table A2: Using dummy variables for tax increases and decreases; only post-election years used in first stage

	(1) Tax increases	(2) Tax decreases
(a) First stage results		
	Tax Increase$_j$	Tax Decrease$_j$
Post-Election$_j$	0.013***	-0.003***
	(0.003)	(0.001)
Observations	56,900	56,900
Kleibergen-Paap first stage F statistic	13.12	12.64
(b) Second stage results		
	Tax Increase$_i$	Tax Decrease$_i$
Tax Increase$_j$	0.568*	
	(0.318)	
Tax Decrease$_j$		0.146
		(0.322)
Observations	56,900	56,900
Controls	YES	YES

The table shows the results of (2) in which only the post-election year dummy is used as instrument and in which the tax rate variables are dummies indicating either tax rate increases or tax rate decreases. Standard errors clustered at the pair level in parentheses (1138 clusters). *Significant at the 10 percent level, ***Significant at the 1 percent level.

Table A3: Using dummy variables for tax increases and decreases; only election years used in first stage

	(1) Tax increases	(2) Tax decreases
	(a) First stage results	
	Tax Increase_j	Tax Decrease_j
Election_j	-0.024***	0.007***
	(0.003)	(0.001)
Observations	56,900	56,900
Kleibergen-Paap first stage F statistic	60.89	31.61
	(b) Second stage results	
	Tax Increase_i	Tax Decrease_i
Tax Increase_j	-0.146	
	(0.141)	
Tax Decrease_j		-0.092
		(0.156)
Observations	56,900	56,900
Controls	YES	YES

The table shows the results of (2) in which only the election year dummy is used as instrument and in which the tax rate variables are dummies indicating either tax rate increases or tax rate decreases. Standard errors clustered at the pair level in parentheses (1138 clusters). ***Significant at the 1 percent level.

Table A4: Placebo estimates

Neighborhood based on	(1) Alphabet	(2) Randomization
	(a) First stage results	
	ΔBusiness Tax_j	ΔBusiness Tax_j
$Pre\text{-}Election_j$	0.062	-0.162**
	(0.047)	(0.087)
$Election_j$	-0.564***	-0.628***
	(0.046)	(0.083)
$Post\text{-}Election_j$	0.580***	0.369***
	(0.055)	(0.105)
Observations	308,250	89,100
Kleibergen-Paap first stage F statistic	102.19	27.75
	(b) Reduced form results	
	ΔBusiness Tax_i	ΔBusiness Tax_i
$Pre\text{-}Election_j$	0.027	0.098
	(0.048)	(0.093)
$Election_j$	0.060	0.033
	(0.048)	(0.088)
$Post\text{-}Election_j$	-0.007	-0.005
	(0.051)	(0.096)
Observations	308,250	89,100
R-squared	0.112	0.108
	(c) Second stage results	
	ΔBusiness Tax_i	ΔBusiness Tax_i
ΔBusiness Tax_j	-0.057	-0.056
	(0.055)	(0.111)
Observations	308,250	89,100
Controls	YES	YES

The table shows the results of (2) when the neighborhood definition is based on the alphabet (column 1), on randomization of neighbors (column 2). Standard errors clustered at the pair level in parentheses. There are 6165 clusters in column 1 and 1782 clusters in column 2. **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table A5: Using only variation from unified governments in (post-)election years of neighboring municipalities

	(1) Only variation from election year as IV	(2) Only variation from post-election year as IV
	(a) First stage results	
	ΔBusiness Tax $_j$	ΔBusiness Tax $_j$
Election $_j$ * Unified Government $_j$	-0.967***	
	(0.217)	
Post-Election $_j$ * Unified Government $_j$		1.814***
		(0.298)
Observations	54,872	54,872
Kleibergen-Paap first stage F statistic	21.00	34.92
	(b) Second stage results	
	ΔBusiness Tax$_i$	ΔBusiness Tax$_i$
ΔBusiness Tax $_j$	0.024	0.296*
	(0.233)	(0.155)
Observations	54,872	54,872
Controls	YES	YES

The table shows the results of (2) in which the election year dummy (column 1) and the post-election year dummy (column 2) interacted with a unified government indicator were used as instruments. All regressions include, but do not report, an indicator for unified governments in municipality j. Standard errors clustered at the pair level in parentheses (1138 clusters). *Significant at the 10 percent level, ***Significant at the 1 percent level.

Table A6:"Traditional" estimation approach with different weight matrices

	(1) Full electoral cycle as IV	(2) Only election year as IV	(3) Only post-election year as IV
	(a) W^{direct}		
	ΔBusiness Tax_i	ΔBusiness Tax_i	ΔBusiness Tax_i
ΔBusiness Tax_j	0.681*	0.241	0.928*
	(0.394)	(0.509)	(0.493)
Observations	10,625	10,625	10,625
Kleibergen-Paap first stage F statistic	9.08	16.16	15.52
	(b) W^{10km}		
	ΔBusiness Tax_i	ΔBusiness Tax_i	ΔBusiness Tax_i
ΔBusiness Tax_j	0.938**	0.642	1.122**
	(0.417)	(0.531)	(0.507)
Observations	10,550	10,550	10,550
Kleibergen-Paap first stage F statistic	5.98	11.49	11.75
	(c) $W^{distance}$		
	ΔBusiness Tax_i	ΔBusiness Tax_i	ΔBusiness Tax_i
ΔBusiness Tax_j	0.855**	0.590	1.035**
	(0.399)	(0.507)	(0.488)
Observations	10,550	10,550	10,550
Kleibergen-Paap first stage F statistic	6.21	12.50	11.99
	(d) W^{pop}		
	ΔBusiness Tax_i	ΔBusiness Tax_i	ΔBusiness Tax_i
ΔBusiness Tax_j	0.193	0.066	1.058
	(0.302)	(0.358)	(0.743)
Observations	10,625	10,625	10,625
Kleibergen-Paap first stage F statistic	17.48	28.58	9.14

The table shows the results of the IV estimation approach (A1). All regressions include, but do not report, the control variables mentioned in the text. Standard errors clustered at the municipality level in parentheses. There are 425 clusters in columns 1 and 4 and 422 clusters in columns 2 and 3. *Significant at the 10 percent level, **Significant at the 5 percent level.

Table A7: Heterogeneity of *i*'s reaction with regard to *i*'s electoral cycle

	(1)
(a) Reduced form results	
	ΔBusiness Tax_i
$\text{Post-Election}_j \mid \text{BeforeElection}_i = 0$	0.558***
	(0.158)
$\text{Post-Election}_j \text{BeforeElection}_i$	-0.649***
	(0.243)
$\text{Post-Election}_j \mid \text{BeforeElection}_i = 1$	-0.091
	(0.187)
Observations	56,900
R-squared	0.104
(b) Second stage results	
	ΔBusiness Tax_i
$\Delta\text{Business Tax}_j \mid \text{BeforeElection}_i = 0$	0.835***
	(0.286)
$\Delta\text{Business Tax}_j \text{BeforeElection}_i$	-0.822*
	(0.421)
$\Delta\text{Business Tax}_j \mid \text{BeforeElection}_i = 0$	0.013
	(0.407)
Observations	56,900
Kleibergen-Paap first stage F statistic	2.95
Controls	YES

The table shows the results of (4) and (5). Standard errors clustered at the pair level in parentheses (1138 clusters). *Significant at the 10 percent level, ***Significant at the 1 percent level.

Table A8: Reaction to post-election years in municipality j in low and high debt municipalities

	(1) Low debt municipalities	(2) High debt municipalities
	(a) First stage results	
	ΔBusiness Tax $_j$	ΔBusiness Tax $_j$
Post-Election $_j$	0.410***	0.655***
	(0.150)	(0.194)
Observations	28,365	28,373
Kleibergen-Paap first stage F statistic	7.24	11.90
	(b) Second stage results	
	ΔBusiness Tax $_i$	ΔBusiness Tax $_i$
ΔBusiness Tax $_j$	0.404	0.802**
	(0.390)	(0.356)
Observations	28,365	28,373
Controls	YES	YES

The table shows the results of (2) in which only the post-election year dummy is used as instrument and the sample is divided in municipalities with low and high debt. Standard errors clustered at the pair level in parentheses. There are 1054 clusters in column 1 and 1060 clusters in column 2. **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table A9: Effect of elections on business tax base

VARIABLES	(1) Log(Business Tax Base)	(2) Log(Business Tax Base)
Pre-Election$_i$	0.022	0.023
	(0.015)	(0.015)
Election$_i$	-0.004	-0.004
	(0.016)	(0.016)
Post-Election$_i$	0.007	0.006
	(0.014)	(0.014)
Controls	NO	YES
Observations	10,623	10,623
R-squared	0.072	0.078

The table shows the results of (6). Standard errors clustered at the municipality level in parentheses (426 clusters).

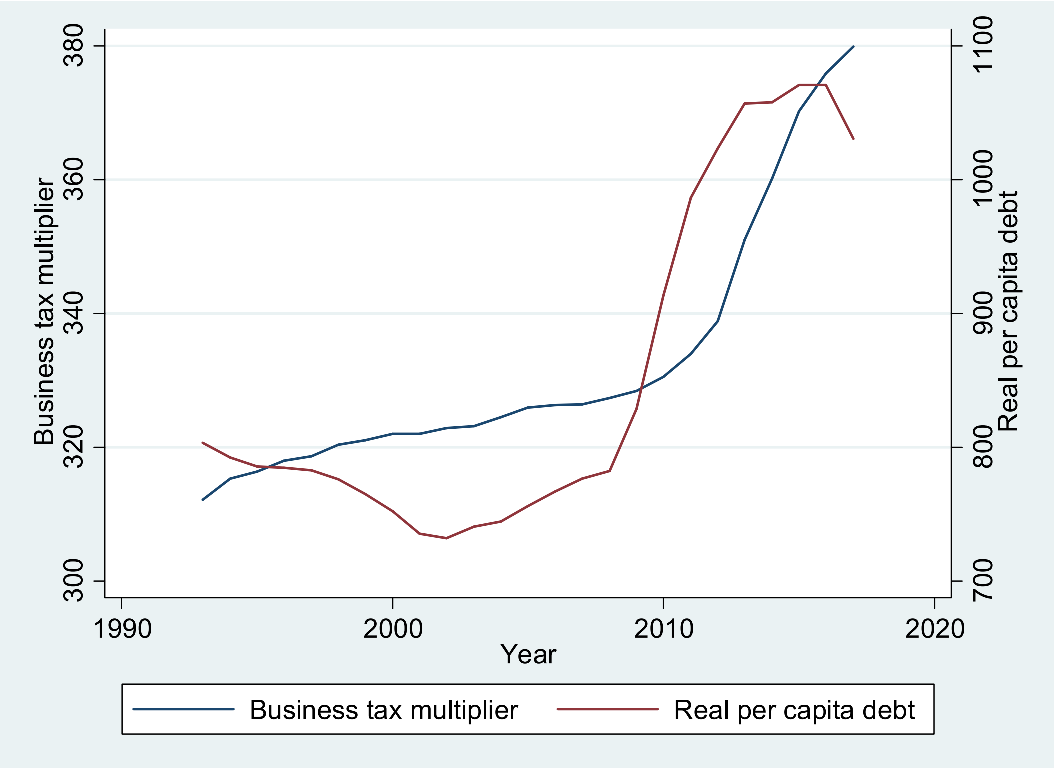
Table A10: Effect of neighbor's tax rates on expenditures

	(1)	(2)
	(a) Second stage results	
	Log(Expenditures$_i$)	Log(Expenditures$_i$)
ΔBusiness Tax $_j$	-0.001	-0.001
	(0.003)	(0.003)
Observations	56,887	56,887
Controls	NO	YES
Kleibergen-Paap first stage F statistic	20.93	20.87

The table shows the results of (2) in which (pre-/post) election year dummies were used as instruments and expenditures of municipality i are used as outcome. For two municipalities, expenditure data are missing in 2008. Therefore, 13 observations are missing compared to Table 2 (one of these municipalities has 5 neighbors and the other one has 8 neighbors). As first stage results are virtually identical to those in Table 2, I only show second stage results. Standard errors clustered at the pair level in parentheses (1138 clusters).

3. Figures

Figure A1: Average business tax multiplier and real per capita debt in Hesse over time



Notes: Evolution of business tax multiplier and real per capita debt looks similar for the universe of all German municipalities.

Figure A2: Number of *Bürgermeister* elections per year

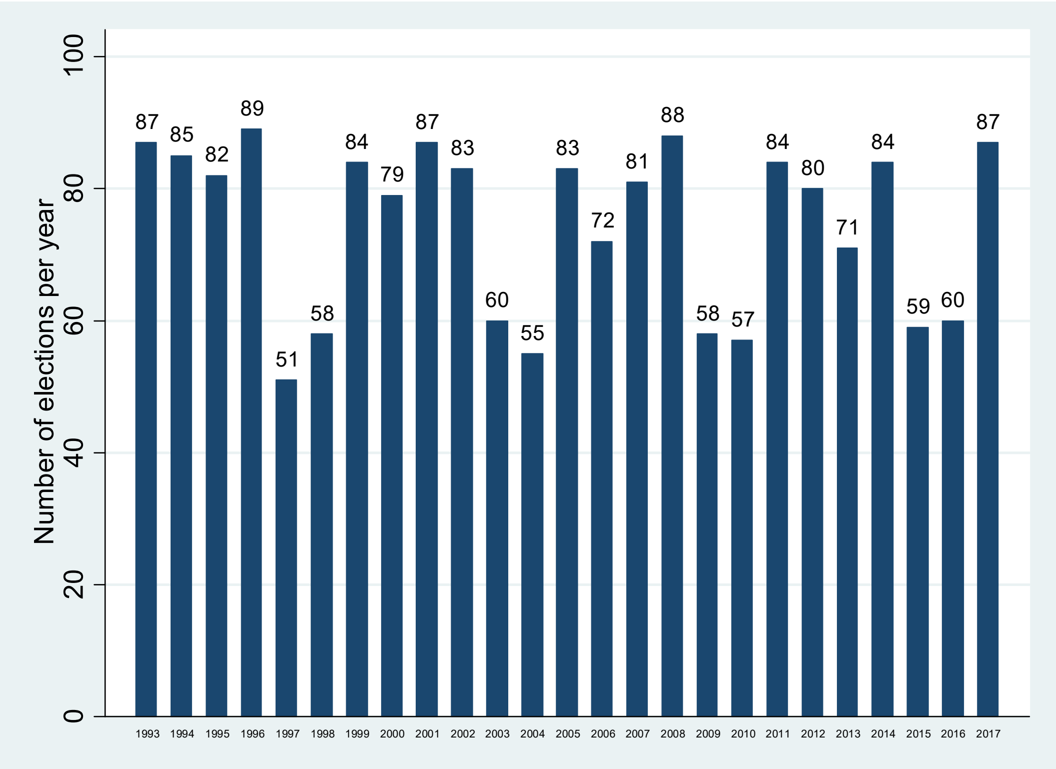
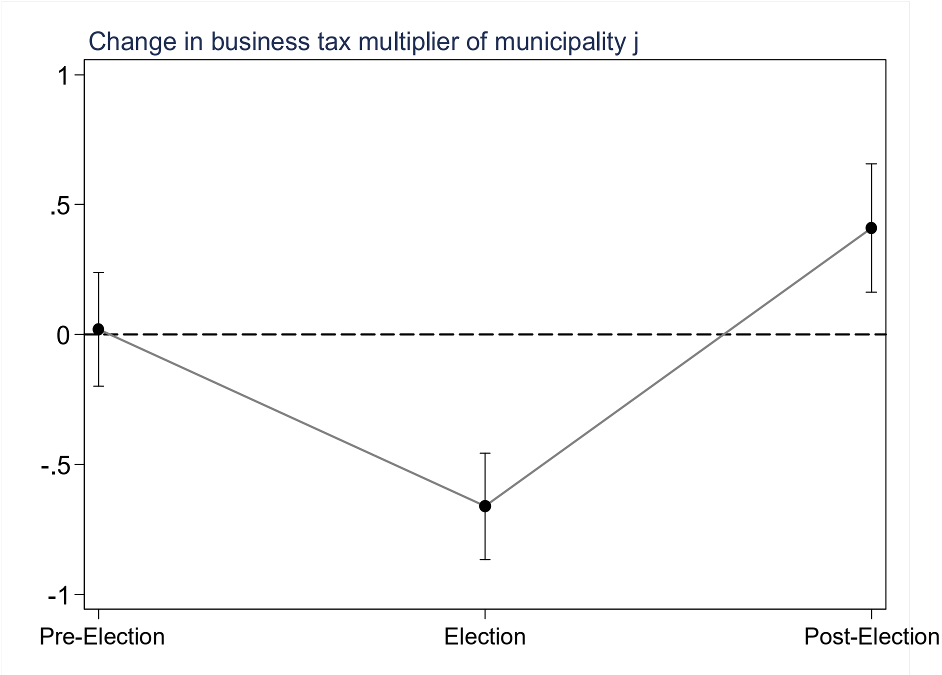
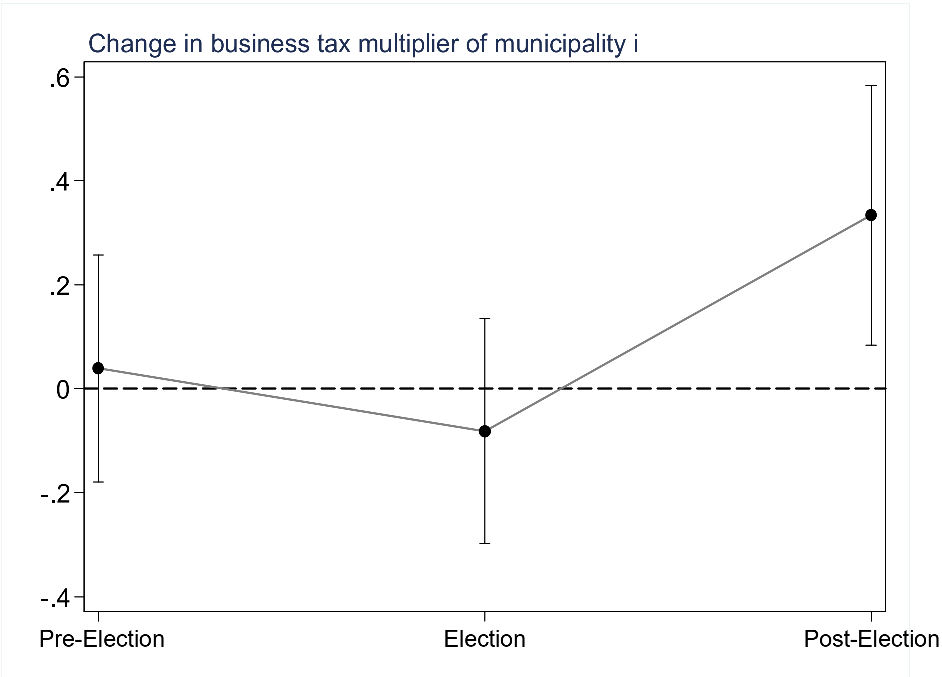


Figure A3: Illustration of estimation results (Table 2)



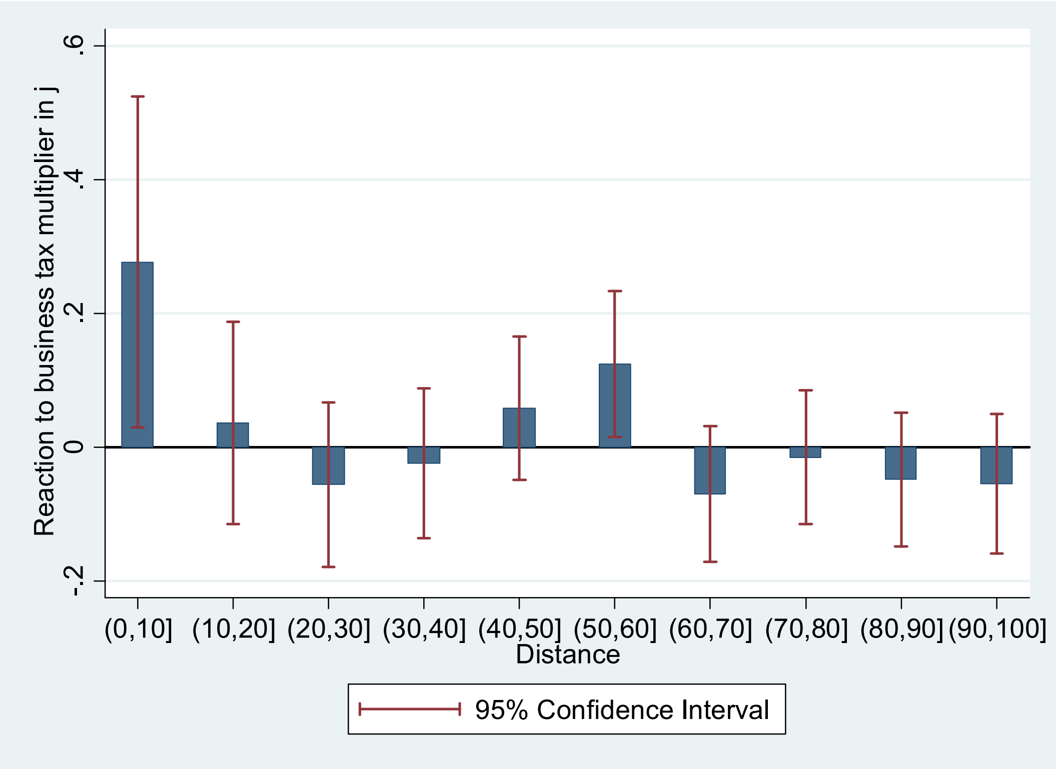
(a) First stage results



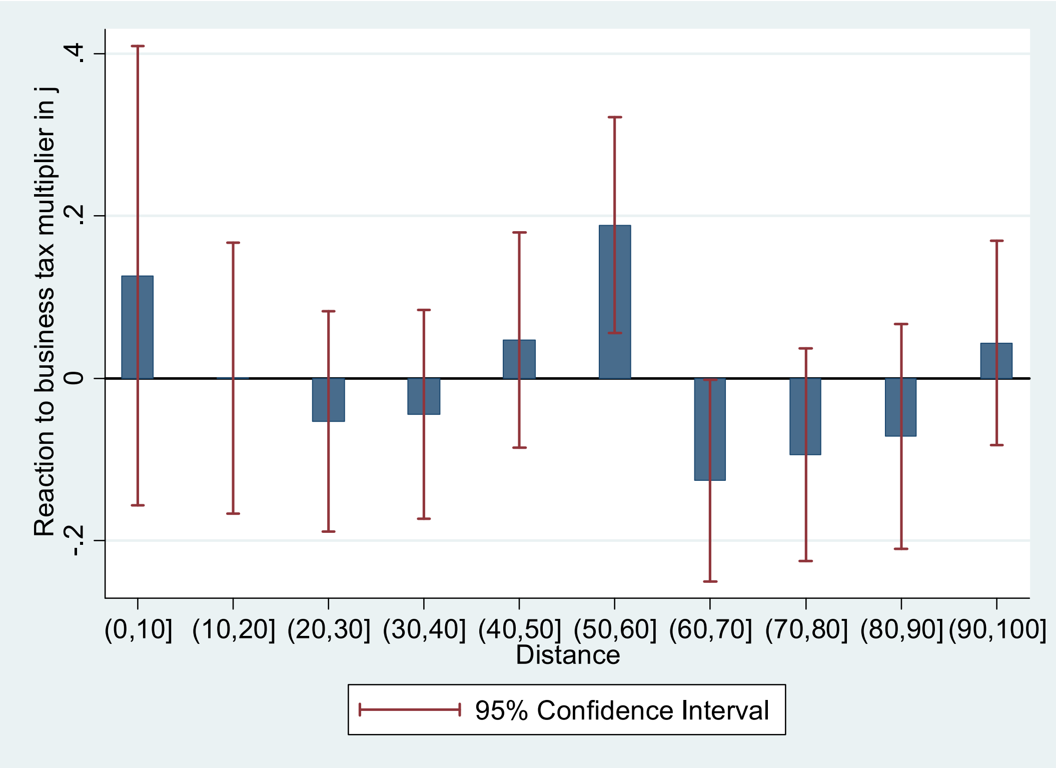
(b) Reduced form results

Notes: The figure plots coefficient estimates from panels (a) and (b) of Table 3 alongside 95% confidence intervals.

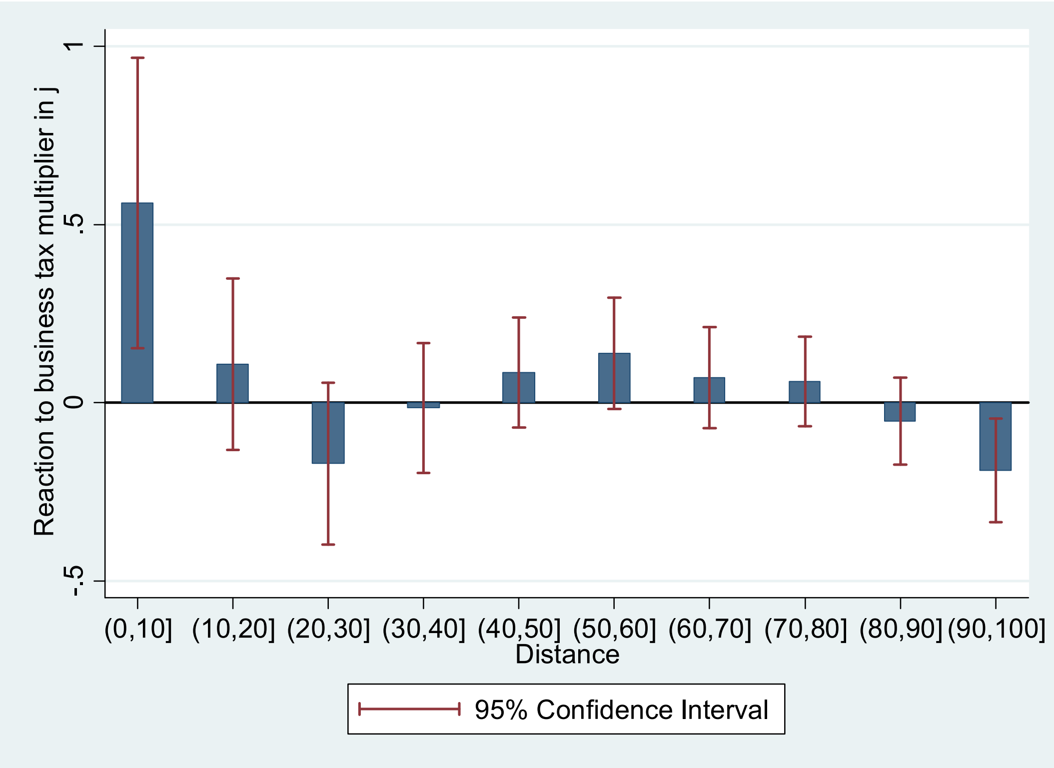
Figure A4: Results of (3) for different distance bandwidths



(a) Full electoral cycle as instrument



(b) Only election year as instrument



(c) Only post-election year as instrument

References

Edmark, K. and H. Ågren (2008). Identifying Strategic Interactions in Swedish Local Income Tax Policies, *Journal of Urban Economics*, 63(3), 849-857.