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Candid Lame Ducks

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Abstract

This study relies on a recent reform introducing gubernatorial term limits at the Portuguese local level to explore how an exogenous variation in eligibility for re-election affects local fiscal policy choices. Relying on electoral and fiscal outcomes for the past three complete electoral terms and a quasi-experimental diff-in-diff approach, the empirical analysis estimates how fiscal policy differs on average between re-eligible and term-limited incumbents. Results indicate that rather than engaging in opportunistic behavior, lame ducks pursue more conservative fiscal policies. Term limited officeholders choose lower property tax rates and reduced levels of current expenditure relative to re-eligible incumbents. Heterogeneous effects further suggest that ineligible mayors behave more truthfully and do not engage in political business cycles, challenging previous results in the literature.

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1 Introduction

Electoral accountability is often pointed as the foremost reason for holding periodical elections. The rationale is that the regular assessment of an incumbent's performance provides more than a chance to reward or punish an officeholder's conduct, it gives the possibility to constrain opportunistic behavior. Institutional barriers to re-election such as term limits may appear at odds with this theory of electoral accountability. Lame ducks, i.e. elected officials reaching the end of their tenure, are not electorally held accountable leaving voters unable to exert the appropriate control. It often follows that term-limits incumbents have less incentives to be responsive towards the electorate or responsible in performing their duties. A theory corroborated to some extent by empirical evidence inasmuch as Besley and Case (1995, p. 793) wonder '... why term limits exist at all'.

However, and as recent literature suggests, by decreasing the value of holding office term limits may also provide the incentives for more truthful political behavior leading to a better selection effect of elections and a more qualified pool of politicians in the long-run (Smart and Sturm, 2013). The key distinction between the two theories is that one focuses on moral hazard whilst the other is concerned with the problem of adverse selection. By distinguishing politicians solely on the basis of their ability, the first looks at elections as a disciplining device and at term limits as the root of distortionary behavior while the second acknowledges a certain degree of heterogeneity in preferences and motivations and regards term limits as disciplinary.

The conflicting views on the role of elections and politicians' motivations make the quest of identifying the impact of term limits an empirical one. Still, there are only few empirical studies on the impact of gubernatorial term limits on fiscal policy and the existing evidence is shaped mainly by the U.S. experience. This paper attempts to broaden this literature by being one of the first micro-analysis on the impact of gubernatorial term limits on local

policy choices in a context other than the U.S. institutional setting. The identification strategy relies on a natural experiment, a recent constitutional reform introducing mayoral term limits at the Portuguese municipal level, and a difference-in-differences (diff-in-diff) quasi-experimental approach. The methodological approach is subject to a number of tests that validate its reliability in causally identifying the impact of term limits.

Regression results suggest a significant reduction in the property tax rate as well as a decrease in total current expenditures. Heterogeneous effects of the treatment in turn, indicate that results hinge on partisan affiliation suggesting that officeholders' motivations underlie the fiscal response to term limits. Furthermore, incumbents stop engaging in fiscal policy cycles once they are ineligible for re-election in contrast to eligible mayors who continue to pursue expansionary policies before the elections.

This analysis contributes to the literature by challenging previous results on the impact of term limits on fiscal policy. Results show that officeholders do behave systematically different in view of a binding term limit, but not in a manner indicating lack of effort or opportunism. By providing evidence consistent with distortionary elections and emerging policy preferences in face of ineligibility for re-election, this study suggests a new interpretation of previous evidence in light of Smart and Sturm's (2013) model of truth-inducing term limits.

2 Term Limits and Political Incentives

There are two distinct views on the role of competitive elections that produce disparate predictions in the term limits debate. The mainstream view is that elections are a disciplining mechanism that is disrupted by re-election constraints leading to situations of moral hazard (e.g. Barro (1973); Ferejohn (1986)). A different perspective deems elections

as distortionary due to myopic or career concerned officeholders. This leads to adverse selection, warranting the introduction of term limits (Chari et al., 1997).

Political agency models capturing the key features of a representative democracy illustrate both views depending on the underlying assumptions on officeholders' characteristics or motivations.[1] Borrowing the principal-agent terminology, the political agent is compelled to act, or build a reputation of acting, in behalf of the principal, i.e. the electoral district, who since Ferejohn (1986) is generally modeled as a performance-oriented electorate, by the democratic institutional mechanism of periodic elections and thereby increase the probability of re-election.

In the context of disciplining competitive elections, fiscal policy outcomes are assumed to measure an incumbent's ability or competence. Attributing a finite and commonly known time horizon to officeholders, determined by the existence of term limits, creates a last-term effect where there is no control over the officeholder's behavior due to the lack of re-election incentives (Barro, 1973). This results in a departure from the optimal policy choice from the voters' perspective.

However, if politicians behave opportunistically in order to be re-elected, in line with reputation-building models predicting potentially negative effects of career concerned and myopic agents (Morris, 2001; Ely and Välimäki, 2003), elections are distortionary. In this context, a system of judicial power where no officeholder may be re-elected, i.e. a one-term limit, can dominate other forms of government (Maskin and Tirole, 2004).

The two theories boil down to one testable hypothesis: that ineligible and eligible incumbents behave differently. Moreover, being fiscal policy the instrument used by politicians to maximize re-election prospects implies that term limits are liable of having an impact on fiscal performance.

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[1]For a review of political agency models see e.g. Besley (2006).

Empirical evidence, mostly based on the U.S. experience, appears to support the disciplining effect of elections and the existence of a term-limits-induced moral hazard (Besley and Case, 1995, 2003; Crain and Tollison, 1993; Crain and Oakley, 1995).[2] There is, nonetheless, one relevant exception: List and Sturm (2006) provide significant evidence of distorting policy choices for a sample of U.S. governors between 1970 and 2000 showing that environmental policy is substantially different in years where a governor may or may not run for re-election, with policy changes strategically hinging on the composition of the electorate.

More recently, the discussion has focused on determining the optimal length of term limits. Results support, in general, longer average tenures. Alt et al. (2011) show that from the perspective of disciplining elections, increasing average tenure matters as voters become able to weed out good from bad politicians. Smart and Sturm (2013) in turn, propose a model with a pool of public-spirited and biased politicians, where even the former are inclined to implement distorted policies in order to increase their re-election probability. In this context, term limits are welfare improving as they decrease the value of holding office, aligning officeholders' incentives and thereby inducing truthful behavior, again enabling voters to better select re-eligible candidates. They support a mix of electoral accountability and judicial power, i.e. two-terms or longer term limits.

This study fits the model of distortionary elections and public-spirited politicians by Smart and Sturm (2013). In addition, it tries to reconcile the early reputation-building models of moral hazard inducing term limits with the present evidence on ideologically driven politicians. Instead of a lack of effort, previous evidence could be interpreted in light of different preferences by politicians and voters that emerge in face of binding term limits. The problem at hands appears to be one of distortionary elections and adverse

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[2]Recent evidence on the disciplining effect of elections relies on outcomes other than fiscal policy. See e.g. Ferraz and Finan (2011) and Janvry et al. (2012), that find less corruption and a more effective implementation of a decentralized conditional cash transfer program aimed at reducing school dropout, respectively, in Brazilian municipalities governed by re-eligible mayors.

selection. Incumbents distort fiscal policy in pursue of pleasing the electorate and securing re-election, ambitions that become unimportant upon ineligibility. Lame duck terms thus show the incumbents true colors and distortions to the local economy cease. This reasoning is consistent with the electorally-induced political business cycles (PBC) literature.

3   Institutional Details

3.1   Local Politics and Finances

This paper relies on the Portuguese first sub-national level of government, i.e. municipalities, as a laboratory to study the impact of introducing mayoral term limits on local public accounts.[3] In Portugal, there are in total 308 municipalities, the equivalent to a U.S. town/city. The focus is upon the 278 mainland municipalities for increased comparability and due to different institutional details regulating the autonomous regions of Azores and Madeira.

Municipalities are responsible for several aspects of the local public administration from the supply of public goods to territorial organization, promotion of local development and external cooperation.[4] For the purpose, there are two political institutions at this level, the executive municipal council and the legislative municipal assembly.

The mayor is the top chief executive position in a municipality. He is the head of the municipal council, which is composed by an additional four to ten councilmen depending on the municipality's population size.[5] The three-terms limitation introduced by the electoral reform concerns the mayoral position. In Portugal however, there are no explicit direct mayoral elections. Instead, the mayor is the first name on the winning list running for the

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[3]There is a second level of local administrative units in Portugal, the parishes. Currently, there are 3,902 parishes with each municipality consisting of at least one parish.

[4]Law no. 159/99 in Diário da República 215, Series I-A, 14th September 1999.

[5]Two exceptions are the Lisbon and Oporto municipalities with 16 and 12 councilmen, respectively.

municipal council elections.[6] For this reason, these are the relevant election results used in the empirical analysis.

Elections for the municipal council are defined exogenously from the perspective of the local authorities. They take place simultaneously countrywide every four years with a proportional representation system of closed lists in place. Parties and independent lists of organized registered electors may contest the elections with seats being distributed according to the D'Hont method. Local politics are dominated by the local branches of the main parties represented in the national parliament. From left to right in the political spectrum these are the Communist Party (PCP), the center-left Socialist Party (PS), the center-right Social-Democrats (PSD) and the right-of-center Popular Party (CDS-PP).

On the financial side, the laws regulating local governments in Portugal are bound by the principle of decentralization. Municipalities have their own assets, finances and personnel. Still, and as is the case with several local governments across Europe, spending decentralization outweighs revenue decentralization. Conditional and unconditional transfers from higher levels of government still represent the main source of municipal funding. However, despite the reduced fiscal autonomy, in the past years there has been an increase in the relative importance of both local taxes and user charges.

Municipalities publish their current and capital accounts yearly. The empirical analysis investigates the impact of term limits on the former as these are of an operative and year-by-year nature.[7] On the revenue side the focus is on the few items entirely under the control of the municipality: user charges and the local property tax rate.[8] User charges are set on a yearly basis by the municipality. However, the scope of services provided

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[6]Even though votes are cast for the party, the electoral campaign is led by the mayoral candidates who are the faces of the different parties.

[7]Capital accounts often involve long-term commitments and are thus less susceptible to change.

[8]Mayors also set the local corporate income tax in a range of 0-1.5% since a reform in 2007. However, due to the small time span of data available this outcome is not considered in the empirical analysis. For the remaining municipal taxes the rate and/or base is set by higher tiers of government and thus exogenous to local authorities.

by the municipalities is very heterogeneous with no systematic records on the type of service and prices charged. Relying on revenues from fines and fees as a proxy gives the possibility to indirectly infer upon the impact of term limits on the level of user charges in the municipality. The local property tax rate for urban properties in turn, is since a reform in 2003 set within a range of 0.3% to 0.5% at the beginning of each year by the municipality.[9] Additionally, local tax revenues as a whole are also analyzed in order to determine whether any change to the local property tax rate had an effect on the overall tax collection. Finally, on the expenditure side, the focus is on the impact of a binding term limit on current aggregate expenditures which primarily encompass expenditures with personnel and current transfers to parishes.

3.2 Electoral Reform

On July 25th 2005, the draft Law on the implementation of term limits for local office-holders was discussed and approved in Parliament leading up to the Law no. 49/2005 from August 29th 2005, which entered into force on January 1st 2006. The law sets a three consecutive terms limit for mayors after which they are not allowed to rerun for the mayoral position in the same municipality. However, upon entering into force it established that all incumbent mayors could rerun in the following 2009 elections. As a result, term limits were only first binding in the 2013 local elections for incumbent mayors serving their at least third consecutive term. In total, 150 mayors were in this situation in 2013 leaving the same number of municipalities to have exogenously determined open-seat elections for the municipal council.

The timeline of the reform creates an interesting natural experiment. First, the law was voted right before, and came into force right after, the 2005 local elections held in October. Second, upon entering into force in 2006 it allowed all incumbent mayors one

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[9]The local property tax rate for rural properties is fixed at 0.8%.

last chance at re-election in the 2009 local elections, creating a stand-by period where the law was already in existence but not yet effective. Third, it was finally binding in the 2013 local elections in 150 municipalities. As only around half of the mayors were bound by a term limit it is possible to rely on both between- and within-municipality variation to identify the impact of gubernatorial term limits on local policy choices.

4 Data

This analysis relies on a dataset combining fiscal variables and electoral results at the Portuguese municipal level for the past three complete electoral terms, i.e. 2002-2005, 2006-2009 and 2010-2013.[10] The National Electoral Commission's (*Comissão Nacional de Eleições*) and the General Directorate for Internal Affairs's (*Direcção Geral da Administração Interna*) websites provide the data on electoral results. Data are provided at the party level per municipality and consist on the number of votes and seats allocated to each party. In order to ascertain the incumbency status of the mayor the data on electoral results was manually matched to the names of the members of the municipal council – in particular the mayor – provided in the Official Map by the National Electoral Commission published in Diário da República.

Data on local public finances are publicly available and can be retrieved from the General Directorate for Local Authority's (*Direcção Geral das Autarquias Locais*) website for the years from 2003 onward. For the previous years it is provided in the institution's annual publication entitled Municipal Finances (*Finanças Municipais*). The variables measuring

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[10]The decision to analyze this period relates to the nature of the reform that determines a clear pre-treatment period, before 2005, a stand-by period between 2006 and 2009 and a post-treatment period, the electoral term 2010-2013 leading up to the term limited elections. Including only the three electoral terms from 2002 onward, is on the one hand enough to perform the intended analysis and on the other hand more robust to long term issues of co-treatment and past dynamics. Results relying on all electoral periods since 1994 support the evidence presented in the following sections. Before 1994 there is no systematic data on the composition of the municipal council.

tax revenues, revenues from fines and fees and aggregate current expenditure are deflated to the year 2005 by the national consumer price index from the World Economic Outlook Database of the International Monetary Fund. Per capita values are obtained relying on resident population per municipality from the Portuguese National Statistics Institute (INE).

For robustness, the analysis relies on a number of control variables. The vector of control variables includes measures of municipal population size and municipal economic activity, as well as political dummies indicating whether there is a majority in the municipal assembly, whether the same party controls both the council and the assembly and if the mayor if left-leaning.[11] The political variables are constructed based on the local electoral results for both the municipal council and assembly from the sources mentioned above. Municipal population size coincides with the resident population per municipality series from INE and the measure for municipal economic activity is proxied by night light output over the years under study for each municipality. This variable is constructed by combining the following two databases in ArcGis: geodata on Portuguese municipal boundaries and location from The Global Administrative Areas database GADM, i.e. a spatial database collecting the location of the world's administrative boundaries, and data on night light output collected by the Defense and Meteorological Satellite Program satellites and treated and made available by the Earth Observation Group from the National Geophysical Data Center of the National Oceanic and Atmospheric Administration.

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[11]The mayor is considered left-leaning if he is a representative of either the PS or PCP, or smaller parties known to be on the left side of the political spectrum.

5 Methodology

5.1 Empirical Model

The variation in eligibility for re-election introduced by the law limiting the number of consecutive mandates is susceptible of causing an exogenous variation in short-term electoral incentives. Provided that treatment and control municipalities are comparable, it is possible to capture this variation and identify the causal impact of term limits on policy choices simply by assessing the difference in the change in fiscal outcomes for the two groups of municipalities from the pre- to the post-treatment period. Evidence of significant post-treatment group-specific fiscal policy choices points to a causal effect of treatment that resulted in a systematically different behavior between term-limited and re-eligible incumbents.

The nature of the constitutional reform under study establishes pre- and post-treatment periods that allow for a quasi-experimental diff-in-diff approach. As of now, there is only one wave of term limited elections. These took place in municipalities re-electing incumbents into office in the 2009 elections to serve their at least third electoral term. As the focus is on implementation effects, i.e. the impact of a binding term limit, treatment is implicitly assigned by the 2009 local elections results.

Let C_i be the dummy indicating treatment, equal to one for all municipalities that elected a lame duck in 2009 and zero otherwise.[12] Treatment assignment occurs at t_0, corresponding to the 2009 local elections. Yet, 2009 is still part of the pre-treatment period as fiscal policy is decided in the yearly budget prepared at the beginning of each year. The post-treatment period starts in 2010, with the new local executive taking office and passing the first budget. Accordingly, let d_t be a time dummy that switches to one the

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[12]In the following treatment is assumed to fall upon the mayor, council or municipality interchangeably.

year after treatment assignment, i.e. $d_t = 1[t > t_0]$. Meaning d_t indicates the four years of the 2010-2013 electoral term.

Inference on the average treatment effect of term limits on local policy choices is based on the following general diff-in-diff regression model:

$$Y_{it} = \gamma_i + \gamma_t + \delta \left(C_i \cdot d_t \right) + Z'_{it}\beta + \epsilon_{it} \tag{1}$$

where Y_{it} is any of the outcome variables under study, $B_{it} = (C_i \cdot d_t)$ indicates a binding term limit and Z'_{it} is the vector of socio-economic and political control variables described in the previous section. The parameter δ measures the average treatment effect of term limits on the different outcome variables for the entire electoral term.

The model is fully identified by including municipality and year fixed effects, γ_i and γ_t, respectively. For robustness, more conservatives versions of the baseline model include district trends, $\gamma_s \cdot t$, and district-year fixed effects, γ_{st}.[13] The former to control for district-specific trends and the latter to allow for unobservable district-specific variables to vary over time. Particular validity and robustness tests also include a linear time trend, t, and a treatment group specific time trend, $C_i \cdot t$.

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

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

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[13]Municipalities are classically grouped into 18 districts created in 1835. Districts are not an official local administrative unit. Still, they group to a certain extent similar municipalities.

where the sum allows for m lags or post-treatment effects. The number of lags is $m = 4$, one for each of the four years of the 2010-2013 electoral term. The remaining variables are defined as before.

Finally, for the purpose of studying heterogeneous treatment effects the regression models in Equations (1) and (2) are extended to encompass both a second dummy variable and its interaction with the binding term limit. Average term effects are obtained within the following regression framework:

$$Y_{it} = \gamma_i + \gamma_t + \delta\, B_{it} + H_{it}(\alpha + \rho B_{it}) + Z'_{it}\beta + \epsilon_{it} \tag{3}$$

where H_{it} is the additional dummy variable. Average yearly effects in turn, are estimated in the context of the regression model in Equation (4), with all variables defined as before.

$$Y_{it} = \gamma_i + \gamma_t + \sum_{\tau=1}^{m} \delta_{-\tau}\, B_{i,t-\tau} + H_{it}(\alpha + \sum_{\tau=1}^{m} \rho_{-\tau}\, B_{i,t-\tau}) + Z'_{it}\beta + \epsilon_{it} \tag{4}$$

This study focuses on one source of heterogeneous effects. In order to identify possible mechanisms behind any significant effect of the term limits treatment on local fiscal policy, partisan affiliation is introduced in the analysis with H_{it} indicating left-leaning mayors. Additionally, the heterogeneous effects regression models above are also estimated as an extension of the baseline results in Section 7.2 to analyze the resigning mayors phenomenon taking place in the electoral term 2010-2013.

5.2 The Common Trends Assumption

Internal validity of a diff-in-diff framework hinges on the common trends assumption. In short, the trend in each of the dependent variables under study must be the same for all municipalities in the absence of treatment. This assumption can be tested through different

procedures. For the purpose, in the following, the sample is restricted to the pre-treatment period, i.e. 2002-2009.

One common approach is to compare the evolution of the different outcome variables in treated and control municipalities during the pre-treatment period (Angrist and Pischke, 2009, p. 231). Figure 1 provides mean plots for the four fiscal variables under study: revenue from fines and fees, the property tax rate, tax revenues and total current expenditure. With the exception of the initial distinct trends in the collection of revenue from fines and fees, the graphs do not provide substantive evidence of differential trends between treatment and control councils capable of undermining the empirical design.

A second approach consists in regressing the different outcome variables on yearly dummies indicating the treatment group (Moser and Voena, 2012). Similar to Equation (2) yet, instead of lags, this test estimates leads of the treatment variable, i.e. $\sum_{\tau=1}^{q} \delta_{+\tau} B_{i,t+\tau}$. The number of leads, $q = 8$, corresponds to each year of the pre-treatment period. The omitted category is 2004, the year before the law limiting mayoral terms came to a vote in Parliament, and the model specification includes municipality and year fixed effects. Coefficient estimates in Figure 2 measure how outcome variables differ between treatment and control municipalities. As suggested by the previous test, the only significant difference relates to the revenues from fines and fees for the year 2002. Evidence on the remaining outcome variables shows that, on average, treatment and control councils did not execute significantly different fiscal decisions in the pre-treatment period.

Finally, Figure 3 provides a third and last standard test of the common trends assumption. Each outcome variable is regressed on a linear time trend and a group-specific trend (De Jong et al., 2011). As before, the model specifies both year and municipality fixed effects. Coefficient estimates assess the existence of a pre-existing differential trend in municipalities subject to treatment. All estimates are insignificant.

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All in all, the three tests support the assumption of common trends in the fiscal outcomes under study for treatment and control municipalities. In light of these tests however, results on the impact of term limits on the revenues from fines and fees should be taken with a grain of salt whenever the model does not control for differential trends. For the remaining fiscal variables, diff-in-diff coefficient estimates are assumed to capture the causal effect of treatment.

6 Empirical Evidence

6.1 Electoral Term and Yearly Effects

Table 1 collects the diff-in-diff results for the average treatment effect of term limits on fiscal policy decisions for the whole 2010-2013 electoral term. Coefficient estimates are obtained from estimating different variations of the diff-in-diff model in Equation (1). Model (1) provides the baseline relying solely on municipality and year fixed effects, while models (2) and (3) test the robustness of the results to district-specific time trends and district-year fixed effects. Specifications (4), (5) and (6) replicate (1), (2) and (3) including the vector of control variables. Both the magnitude and significance of the coefficient estimates is consistent across all specifications indicating their robustness to omitted variables (Altonji et al., 2005).

Results suggest that lame ducks, on average, significantly reduce both the local property tax rate and aggregate current expenditures. Coefficient estimates point to a 0.013 percentage points reduction in the tax rate and a decrease of 3.6% (approx. $ 780.000 for the average municipality) in total current spending.

Accordingly, the average treatment effect on tax revenues is consistently negative. Estimates are however never significant. Tax revenues depend on a number of other municipal taxes and therefore do not mimic the movements of the property tax rate alone. Increases

or decreases in this tax are thus susceptible of having no significant impact on the whole of tax revenue collection.

User charges also appear to decrease in the aftermath of treatment. Revenues from fines and fees, used as a proxy for the change in user charges, are negative though insignificant. It can be that lame ducks changed specific user charges with not enough economic impact on the entire revenue item, similar to the relation between the property tax rate and the resulting collected tax revenues. On the other hand, the overall revenue from fines and fees is already small when compared to other sources of municipal funding, not leaving much leeway for incumbents to significantly decrease it further given the available instruments.

The annual average treatment effects in Table 2 in turn, obtained from estimating Equation (2) for the most conservative specification, i.e. model (6), are in line with the previous assessment and provide additional information as to the timing of the mayoral response to term limits.[14] The yearly treatment effects show that the significant drop in the property tax rate occurs in the last two years of the electoral term, whilst the drop in current expenditure happens throughout the first three years. In fact, in the election year lame ducks no longer spend significantly less than re-eligible mayors. Either all mayors choose to spend less in the year leading up to the election or lame ducks use the cushion created in the preceding years to restore expenditure levels right before the election.

In addition, although the average electoral term treatment effect on tax revenues is insignificant, the study of annual effects provides evidence of a significant decrease in tax revenue collection in the election year. This comes presumably as a result of the significantly lower property tax rate in the later years of the electoral term.

Overall, the present results appear in contrast to previous evidence in the literature. Rather than having a positive and significant effect on spending (Besley and Case, 1995, 2003) and tax revenues (Alt et al., 2011), term limits lead to a general reduction in current

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[14]Results are robust to the different specifications. These are available upon request.

expenditure and the property tax rate, with lame ducks pursuing a more conservative fiscal policy. Still, evidence indicates a possible term-limits-induced fiscal policy cycle as argued in Besley and Case (1995). Section 7.1 studies whether that is the case in the present institutional setting. Nevertheless, average electoral term effects show that overall, both spending and tax rates are on average significantly lower in treated municipalities.

There are multiple possible explanations for the more conservative behavior exhibited by lame ducks. Even though certain hypothesis coming from the standard literature on term limits such as lack of effort or opportunistic behavior do not appear to fit the present evidence, theories base on the increased experience of long-term officials could motivate the assessed outcomes. It is however not clear what would be the incentives behind such a commitment in the face of a binding term limit except for perhaps party loyalty or reputational concerns. In fact, 97 of the 150 termed-out mayors continued to pursue a political career closely related to public administration. The study of heterogeneous effects in the following section is meant to clarify the mechanisms behind these results.

6.2   Heterogeneous Effects

In order to identify the motivation behind the lame ducks' response to term limits, this section analyzes the presence of heterogeneous effects. For the purpose, inference is based on the estimation of both Equations (3) and (4) with H_{it} identifying different non-overlapping groups, in particular left- from right-leaning lame ducks.

Testing for ideologically motivated treatment effects provides a solid ground to infer upon a number of possible mechanisms beyond political preferences. If left- and right-leaning term-limited officeholders behave differently, encompassing explanations, as experience, party loyalty, or lack of effort, do not justify the treatment effects adequately. Indeed, in case any of these mechanisms is behind the assessed results, these should on average not significantly hinge on partisan affiliation. If there are significant heterogeneous

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effects however, the causal effect of term limits is presumably working through politicians' intrinsic preferences or motivations.

Results collected in Tables 3 and 4 show significant evidence of heterogeneous effects contingent on party affiliation. The fiscal policy choices of right-leaning ineligible mayors appear to be driving the baseline results, with coefficient estimates for left-leaning lame ducks being either insignificant, in the case of the property tax rate, or symmetric to the previously assessed results.

That right-leaning lame ducks are responsible for the fiscally conservative response to term limits identified in the baseline results is consistent with the findings in Besley and Case (1995) attributing the loose fiscal policy caused by binding term limits to the Democrats. Taken together, one possible explanation for both results is that when reputation becomes less important due to ineligibility, rather than less effort as suggest in Besley and Case (1995), officeholders act more truthfully as modeled in Smart and Sturm (2013). Ideology may show once mayors are no longer electorally held accountable.

This possible mechanism is in addition, in line with evidence suggesting that the U.S. electorate is fiscally conservative (Peltzman, 1992). The rationale is as follow, given a conservative electoral district, re-eligible incumbents choose on average more conservative policies to boost their re-election probability. Once faced with binding term limits, Democrats choose unconstrained optimal fiscal policies that are on average looser than Republicans'. By the same token and relying on the evidence above, the Portuguese electorate appears to be fiscally liberal. Thus right-leaning ineligible incumbents feel free to pursue a more conservative fiscal policy. Furthermore, this theory would also be in line with the fact that the median voter in Portugal is likely poorer than in the U.S. and thus more prone to favor redistributive policies.

7 Extensions

7.1 Political Business Cycle

Evidence from the PBC literature usually identifies an electorally induced expansionary policy in the year(s) preceding the election financed by a recessionary beginning of the term. Even though the baseline results in the previous section provide evidence of a similar pattern for aggregate current expenditures, in order to assess whether term limits introduced a fiscal cycle in the current accounts of treated municipalities it is necessary to identify the general fiscal policy pattern in control municipalities. Hence, this section re-analyzes the impact of term limits on fiscal outcomes controlling for the specific year of the electoral term so that it is possible to identify any significant changes to the pattern of fiscal policy in treated municipalities. Inference is based on the following regression model:

$$Y_{itj} = \gamma_i + \gamma_t + \sum_{j=1}^{4} ey_j(\gamma + C_i + \sum_{\tau=1}^{m} \delta_{-\tau} B_{i,t-\tau}) + Z'_{it}\beta + \epsilon_{it} \tag{5}$$

where $j = 1, \ldots, 4$ indexes the four years in an electoral term. The dummy variables ey_j indicate each year of the electoral term and the resulting γ_j parameter estimates can be thought of as electoral year fixed effects. The second interaction controls for group-specific patterns in fiscal policy decisions for each specific year of the electoral term. Finally, the third interaction identifies the change in decision patterns from pre- to post-treatment period in treated municipalities controlling for the electoral year. In other words, the δ parameters estimate how policy decisions of lame ducks compare with the ones of re-eligible incumbents for the same year of the electoral term.

Regression results obtained from estimating Equation (5) indicate whether term limits introduced or simply altered a pre-existing cycle in fiscal policy. Table 5 collects the

coefficient estimates for the most conservative specification.[15] The first year of the electoral terms is the omitted category.

Altogether, results suggest that rather than introducing a fiscal cycle, term limits mainly induce a change in how the increase in current aggregate expenditure in election years is financed. Evidence points to the existence of a PBC in both treated and control municipalities, with mayors increasing expenditures in election years independently of eligibility for office. However, re-eligible officeholders finance this increase in expenditure through higher tax revenues whilst lame ducks appear to rely on the cushion created by lower spending on the first years of the electoral term.

7.2 Mayoral Resignation

In the 2010-2013 electoral term, as in previous terms, a number of mayors resigned from office. Yet, during this particular term 24 mayors stepped down in comparison to five and eleven during the previous 2006-2009 and 2002-2005 electoral terms, respectively. Moreover, all resigning mayors were lame ducks.

It appears as though a number of officeholders, faced with the impossibility of running for re-election, strategically decided to resign and give their successor in the party list a chance to take the lead. In fact, when a mayor resigns, the vice-president, a fellow councilman often from the same party, steps in and assumes office until the next election. In 20 out of the 24 municipalities where a lame duck resigned, the substitute mayor ran for the top chief executive position in the 2013 local elections.

Including this strategic response by officeholders into the analysis as a second treatment shows whether results are somehow driven by this small sample of municipalities or if there are significant heterogeneous effects that can shed more light into the underlying mechanics of term limitations. By letting H_{it} in Equations (3) and (4) indicate every municipality-

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[15]Results are robust to the different specifications. These are available upon request.

year observation where a mayor resigned from office, it is possible to compare fiscal policy decisions by re-eligible incumbents, lame ducks, and eligible substitute officeholders.

Most resignations took place in the year preceding the election meaning that up to that point all term-limited municipalities were under the control of a lame duck, while in the election year substitute mayors were already in office. Thus in the following it is assumed that coefficient estimates for the first three years of the electoral term provide heterogeneous effects for resigning lame ducks, and election year coefficient estimates refer to policy choices by substitute eligible mayors.

Term and annual average treatment effects are collected in Tables 6 and 7, respectively. Eligible substitute mayors behave no different than the average re-eligible mayor. Their incentives should indeed be the same provided eligible officeholders on average run for the elections, as is most often the case. Also resigning and non-resigning term-limited incumbents appear to face similar incentives inasmuch as both significantly decrease aggregate current expenditure. Resigning mayors however, do it to a larger extent. This result is presumably due to the fact that the majority of resigning lame ducks belong to parties on the right side of the political spectrum.

Furthermore, evidence shows that lame ducks incur significantly lower current expenditures also in the election year. The insignificance of the baseline coefficient estimates is thus likely driven by the increased expenditure of substitute mayors. In fact, evidence suggests that term-limited incumbents on average pursue a consistent low spending policy combined with reduced tax rates throughout the whole electoral term whilst substitute mayors engage in a fiscal policy cycle as does the average re-eligible mayor.

8   Robustness Checks

8.1   Selection Bias

The first robustness test tackles a potential problem with diff-and-diff regressions that relates to issues of selection bias. In principle, municipalities are exogenously assigned to treatment and control group. Only then can a differential change in the outcome variables from pre- to post-treatment period be attributed to the change in treatment assignment. If that is not the case there may be intrinsic differences between the two groups that are affecting the results which can no longer be interpreted as causal effects of treatment.

It is possible to test for selection bias in a number of ways. The second test on pre-existing group-specific trends in section 5.2 for example, also serves the purpose of testing for selection. The fact that year dummies indicating treatment municipalities during the pre-treatment period are insignificant points to an exogenous treatment assignment. Moreover, the fact that coefficient estimates remain consistent both in terms of magnitude and significance after the introduction of a vector of variables controlling for a number of observables asserts their robustness not only to selection on observables but also to selection on unobservables as proposed in Altonji et al. (2005).

The most common approach in the context of a diff-in-diff framework however, is to devise placebo tests. Placebo tests for diff-in-diff methodologies usually consist on re-estimating the baseline results relying on a placebo treatment setting in at a fake treatment year (De Jong et al., 2011). Here, the sample is restricted to the pre-treatment period, i.e. 2002-2009, and the regression model in Equation (1) is re-estimated with d_t indicating a placebo treatment setting in after the 2005 election, for the 2006-2009 electoral term. As Table 8 shows, estimates are close to zero and always insignificant dismissing any remaining concerns of a possible selection bias.

8.2 Controlling for Anticipation Effects

As suggested in Malani and Reif (2015), often pre-trends can and should be interpreted as anticipation effects. In the context of the reform under study, controlling for anticipation effects is of particular interest as the law limiting consecutive mayoral mandates established a stand-by period of one electoral term before becoming effective.

It is possible that forward-looking officeholders reacted to the now limited perspectives of long-term mayorships already in the 2006-2009 electoral term. If that was the case, credibly identifying the treatment effect involves controlling for anticipation effects. Following Malani and Reif (2015) and taking into account that the stand-by period during which anticipation effects may occur is known and finite, inference is based on the following quasi-myopic model:

$$Y_{it} = \delta\, B_{it} + \sum_{\tau=1}^{q} \delta_{+\tau}\, B_{i,t+\tau} + Z'_{it}\beta + \gamma_i + \gamma_t + \gamma_s \cdot t + \gamma_{st} + \epsilon_{it} \tag{6}$$

where the sum allows for q leads or anticipation effects with the dummies $B_{i,t+\tau}$ equal to one if the reform takes place in year $t + \tau$. The number of leading indicators that address anticipation is $q = 4$, the four years of the electoral term 2006-2009.

Results in Table 9 are in line with the baseline average treatment effects. There is a significant anticipation effect for the property tax rate series in the year 2008 in line with the actual treatment effect for the year 2012. This effect suggests that strategic decisions already took place in anticipation of treatment. Still, the magnitude of the average treatment effect on the property tax rate even after controlling for anticipation effects remains basically unchanged. So does the magnitude of the average treatment effect on current expenditures.

8.3 Controlling for Pre-existing Trends

Finally, although the pre-trends analysis in section 5.2 does not provide evidence of differential trends – except for the revenues from fines and fees item that is insignificant throughout the regression results – and the previous section shows that results are robust to anticipation effects, this section re-estimates the treatment effects controlling for pre-existing time trends. Group-specific trends are controlled for in two ways based on the second and third tests in section 5.2 and the focus here is on the annual treatment effects.

The first approach combines Equations (2) and (6) in a regression model allowing for leads, or group-specific differential outcomes, and lags, or annual treatment effects (Angrist and Pischke, 2009; Autor, 2003; Reber, 2005). In particular, coefficient estimates are obtained for the following extended regression model:

$$Y_{it} = \sum_{\tau=1}^{m} \delta_{-\tau} \, B_{i,t-\tau} + \sum_{\tau=1}^{q} \delta_{+\tau} \, B_{i,t+\tau} + Z'_{it}\beta + \gamma_i + \gamma_t + \gamma_s \cdot t + \gamma_{st} + \epsilon_{it} \qquad (7)$$

where the sums specify m lags and q leads. Whilst the number of lags is again $m = 4$, one for each year of the 2010-2013 electoral term, the number of leads is $q = 8$, as in section 5.2 instead of $q = 4$ as before. Each lead corresponds to one year of the pre-treatment period, from 2002 to 2009. Again, 2004 is the omitted category.

For simplicity and compactness evidence is presented in the form of a graphical analysis. Figure 4 shows a graph for each outcome variable under study. The first plot shows that even when controlling for pre-existing group-specific trends, revenues from fines and fees are not affected by the introduction of term limits.

Graphs for the remaining outcome variables show insignificant coefficient estimates for the whole duration of the pre-treatment period and depict, for the post-treatment years, a path in line with the results obtained for the baseline annual treatment effects. The average property tax rate is significantly lower in treatment municipalities for the last two

years of the 2010-2013 electoral term, tax revenues are significantly lower in 2013 and the meaningful drop in current expenditure occurs in the early years of the term, even when controlling for pre-existing differential trends.

In the second approach, Equation (2) is re-estimated for a specification including, in addition, both a linear time trend and a treatment group specific pre-trend (Moser and Voena, 2012). Table 10 collects the coefficient estimates. These too, are in line with the previously assessed results lending increased robustness to the baseline estimates. Altogether, the various tests validate the reliance on the diff-in-diff approach as a suitable identification strategy.

9 Conclusion

The introduction of term limits at the Portuguese local elections level provides an opportunity to analyze the economic consequences of an exogenous variation in eligibility for re-election. Using electoral and fiscal data on the 278 municipalities in continental Portugal for the past 12 years encompassing the last three complete electoral terms, inference is based on a quasi-experimental diff-in-diff approach that allows for a causal interpretation of the estimated treatment effects.

In contrast to the U.S. centered literature, results provide robust evidence of a decrease in the local property tax rate and aggregate current expenditure in treated municipalities with lame ducks engaging in a more conservative fiscal policy driven by a lack of re-election incentives. Significant heterogeneous effects indicate that political preferences are behind the change in fiscal policy choices of ineligible mayors. Results fit a model of distortionary elections predicting a more truthful behavior of politicians bound by term limits as in Smart and Sturm (2013). Not only do fiscal cycles pre-date the introduction of term limits, but also only eligible officeholders, rather than lame ducks, appear to engage in distortionary

policies. Furthermore, despite the long tenures of the present lame ducks there is no clear evidence of their relatively higher experience.

This study challenges the standard approach of disciplining elections, opportunistic lame ducks and term limits induced fiscal cycles established by Besley and Case (1995). However, even the results therein could fit a model predicting a more candid behavior by term limited incumbents as Democrats are responsible for the identified loose fiscal policy of lame ducks. This theory is in turn consistent with the evidence of a conservative electorate in the U.S. (Peltzman, 1992) and is consistent with distortionary elections and the existence of electorally driven PBC instead of disciplining elections and undesirable term limitations.

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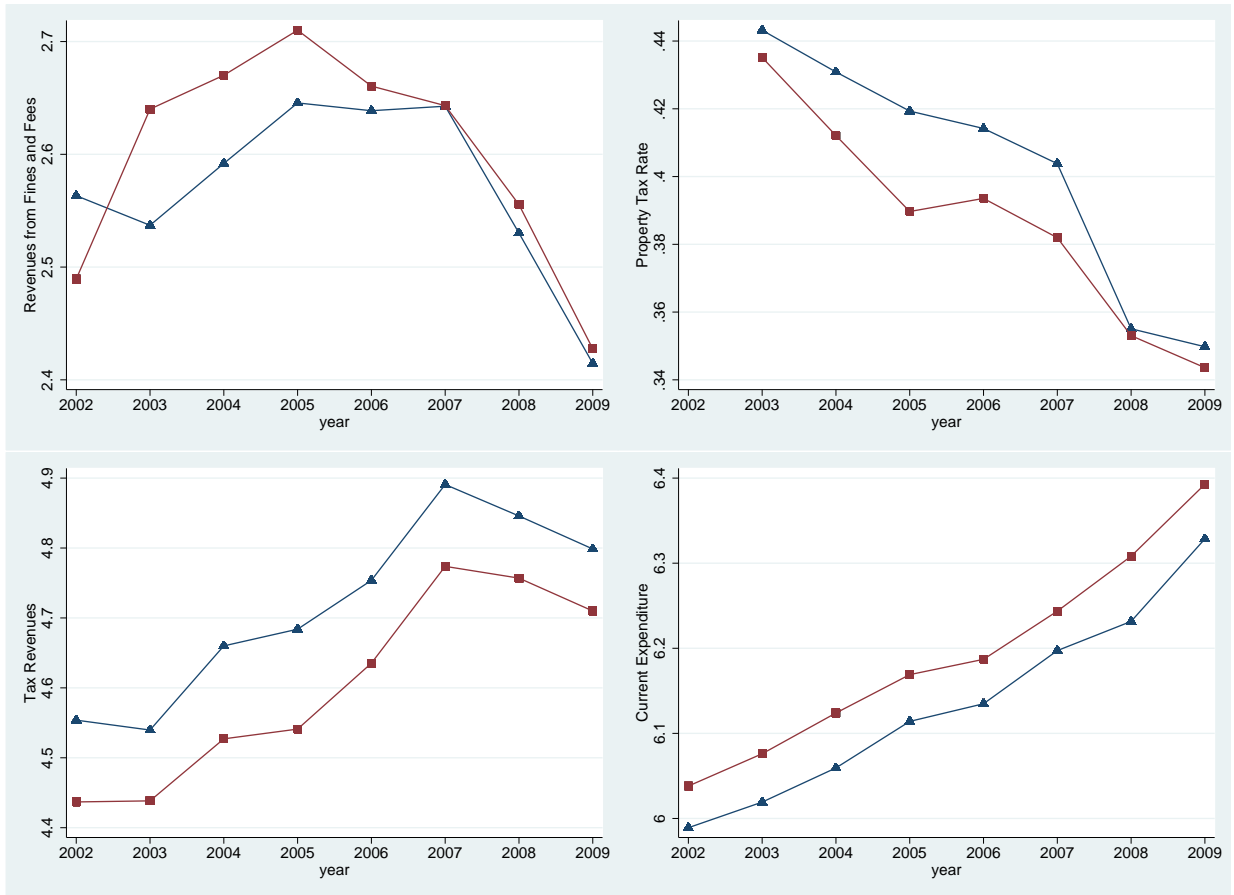
Figure 1: Common Trends Assumption: Mean Plots. This figure provides plots depicting the logarithm of the mean of each of the fiscal outcomes under study in treated (square) and control (triangle) municipalities for the pre-treatment period, 2002-2009. Data on the local property tax rate is only available from 2003 onward due to a fiscal reform.
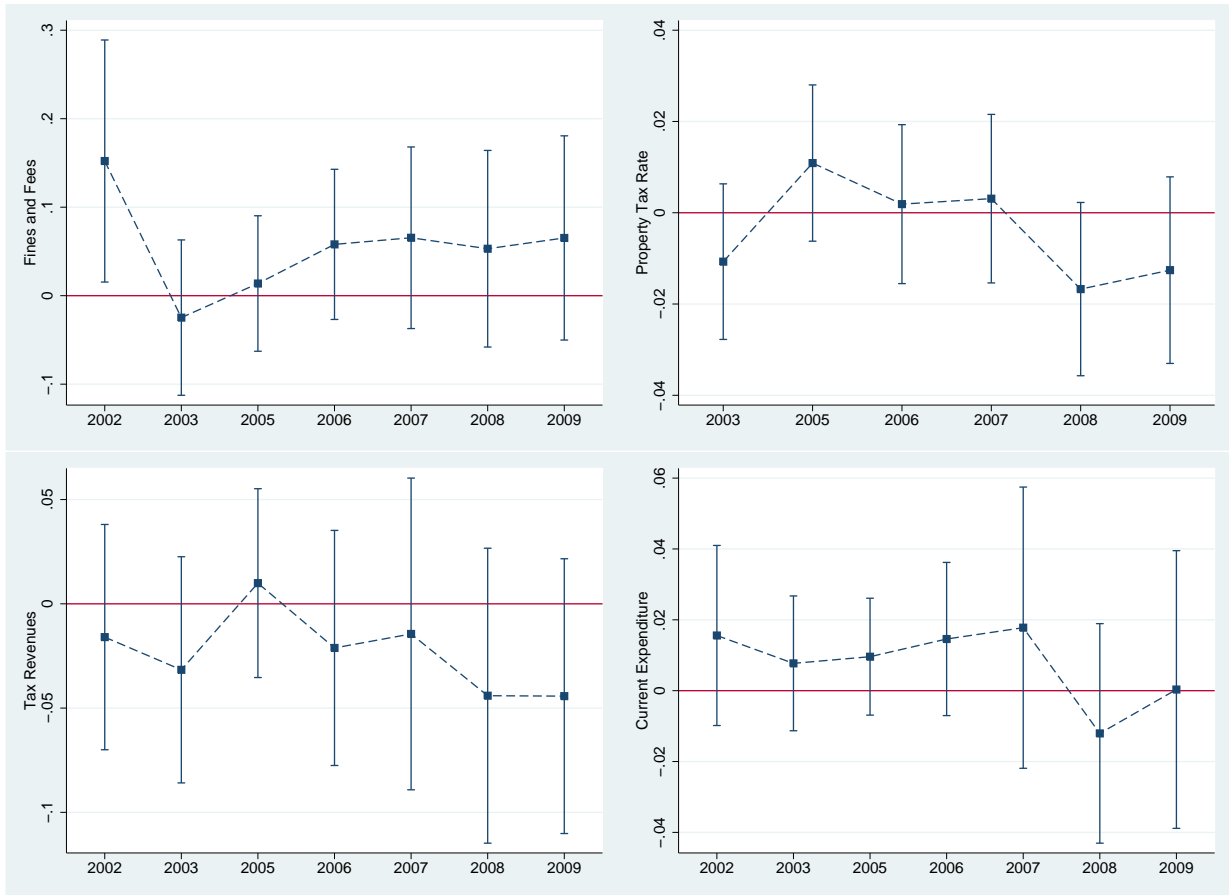
Figure 2: Common Trends Assumption: Yearly Dummies. This figure provides plots depicting the coefficient estimates for yearly dummy variables indicating the treatment group for the pre-treatment period, 2002-2009. Data on the local property tax rate is only available from 2003 onward due to a fiscal reform. Coefficients are obtained in a model controlling for municipality and year fixed effects. Caped lines indicate 95% confidence intervals.
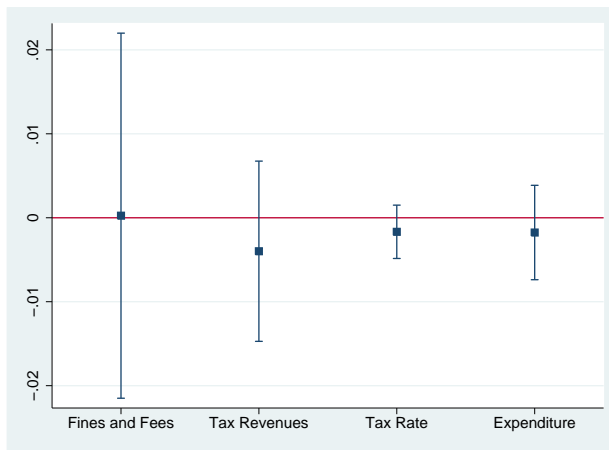
Figure 3: Common Trends Assumption: Group-specific Trends. This figure provides a plot depicting the coefficient estimates for a variable capturing group-specific trends in the pre-treatment period, 2002-2009. Coefficients are obtained in a model controlling for a linear time trend and municipality and year fixed effects. Caped lines indicate 95% confidence intervals.
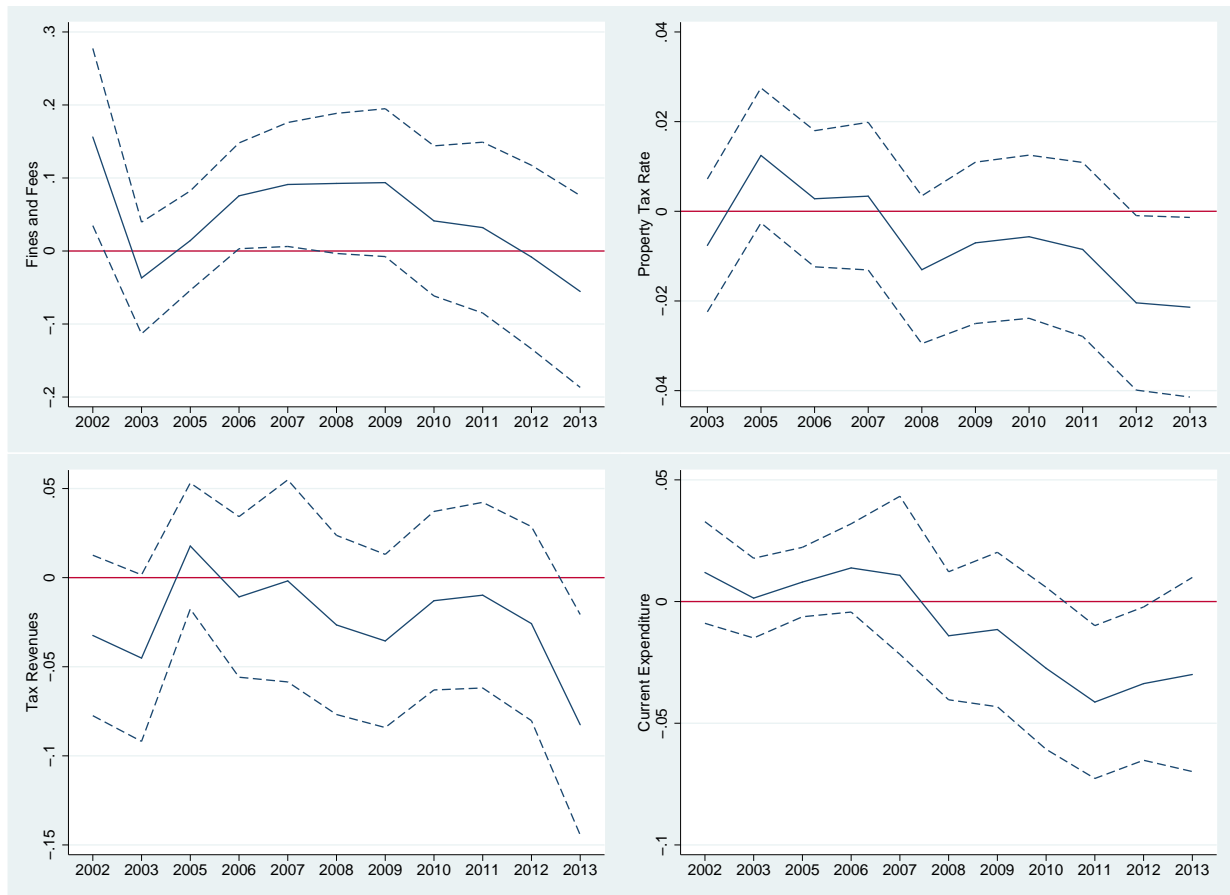
Figure 4: **Local Policy Choices: Yearly Effects.** This figure provides plots depicting the coefficient estimates for yearly dummy variables indicating the treatment group for the whole sample period 2002-2013. Data on the local property tax rate is only available from 2003 onwards due to a fiscal reform. Results are obtained from the estimation of Eq. (7). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Dashed lines indicate 90% confidence intervals.

Table 1: Local Policy Choices: Term Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Fines & fees	-0.079	-0.056	-0.054	-0.078	-0.060	-0.059
	(0.058)	(0.054)	(0.055)	(0.058)	(0.008)	(0.055)
Tax rate	-0.014**	-0.012**	-0.012**	-0.014**	-0.012**	-0.013**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Tax revenues	-0.029	-0.013	-0.018	-0.026	-0.013	-0.016
	(0.030)	(0.023)	(0.023)	(0.025)	(0.021)	(0.020)
C. Expenditure	-0.036**	-0.034**	-0.034**	-0.036**	-0.036***	-0.036***
	(0.016)	(0.014)	(0.014)	(0.015)	(0.013)	(0.013)
District trends	No	Yes	Yes	No	Yes	Yes
District x year	No	No	Yes	No	No	Yes
Controls	No	No	No	Yes	Yes	Yes
Observations	3333	3333	3333	3297	3297	3297

This table presents the baseline results on the average treatment effect of a binding term limit on fiscal policy. The left column lists the dependent variables. Results are obtained from the estimation of Eq. (1). All estimates include municipality and year fixed effects. Model (2) adds district-specific time trends and model (3) district-year fixed effects. Models (4), (5) and (6) replicate (1), (2) and (3), respectively, adding the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 2: Local Policy Choices: Annual Treatment Effects

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
2010	-0.020	-0.004	0.004	-0.030*
	(0.051)	(0.006)	(0.022)	(0.016)
2011	-0.029	-0.007	0.007	-0.044***
	(0.061)	(0.007)	(0.023)	(0.016)
2012	-0.069	-0.019**	-0.009	-0.036**
	(0.067)	(0.007)	(0.023)	(0.015)
2013	-0.116	-0.020**	-0.065**	-0.033
	(0.073)	(0.008)	(0.030)	(0.020)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the baseline results on the annual treatment effect of a binding term limit on fiscal policy. The first row lists the dependent variables. Results are obtained from the estimation of Eq. (2). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 3: Heterogeneous Term Treatment Effects

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
2010-13	-0.071	-0.0106	-0.00522	-0.0598***
	(0.061)	(0.00709)	(0.0244)	(0.0149)
H_{it}	-0.006	0.0152**	0.0144	-0.0117
	(0.037)	(0.00731)	(0.0161)	(0.00994)
$H_{i,t>t_0}$	0.032	-0.00169	-0.0190	0.0510***
	(0.075)	(0.00731)	(0.0281)	(0.0162)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the results on average heterogeneous treatment effects of a binding term limit on fiscal policy when a lame duck is left leaning. The left column lists the dependent variables. Results are obtained from the estimation of Eq. (3). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 4: Heterogeneous Annual Treatment Effects

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
2010	-0.006	0.000	0.028	-0.054***
	(0.054)	(0.006)	(0.025)	(0.019)
2011	-0.055	-0.002	0.014	-0.073***
	(0.069)	(0.007)	(0.028)	(0.018)
2012	-0.079	-0.020**	0.004	-0.059***
	(0.073)	(0.009)	(0.027)	(0.018)
2013	-0.145*	-0.021**	-0.067*	-0.054**
	(0.082)	(0.010)	(0.035)	(0.025)
H_{it}	-0.006	0.015**	0.014	-0.012
	(0.037)	(0.007)	(0.016)	(0.010)
$H_{i,2010}$	-0.021	-0.008	-0.048	0.051***
	(0.068)	(0.007)	(0.031)	(0.018)
$H_{i,2011}$	0.063	-0.010	-0.011	0.064***
	(0.084)	(0.007)	(0.031)	(0.021)
$H_{i,2012}$	0.028	0.004	-0.025	0.048**
	(0.092)	(0.010)	(0.031)	(0.020)
$H_{i,2013}$	0.061	0.007	0.009	0.041
	(0.096)	(0.011)	(0.040)	(0.025)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the results on annual heterogeneous treatment effects of a binding term limit on fiscal policy when a lame duck is left leaning. The left column lists the dependent variables. Results are obtained from the estimation of Eq. (4). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 5: Political Business Cycle

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
γ_2	-0.064	0.067*	-0.057	0.009
	(0.125)	(0.036)	(0.036)	(0.023)
γ_3	-0.050	-0.040	-0.070	-0.047
	(0.082)	(0.034)	(0.044)	(0.029)
γ_4	-0.017	0.089*	0.167***	0.082***
	(0.064)	(0.050)	(0.042)	(0.028)
$\gamma_2 \cdot C_i$	-0.064*	-0.001	-0.005	0.007
	(0.037)	(0.005)	(0.020)	(0.010)
$\gamma_3 \cdot C_i$	-0.045	-0.005	0.006	-0.007
	(0.036)	(0.005)	(0.016)	(0.009)
$\gamma_4 \cdot C_i$	-0.037	0.004	0.010	-0.001
	(0.036)	(0.003)	(0.015)	(0.009)
$\gamma_2 \cdot B_{it}$	0.005	-0.006	0.014	-0.047***
	(0.070)	(0.008)	(0.029)	(0.018)
$\gamma_2 \cdot B_{it}$	-0.055	-0.014*	-0.013	-0.027
	(0.068)	(0.008)	(0.025)	(0.016)
$\gamma_2 \cdot B_{it}$	-0.109	-0.024***	-0.073**	-0.029
	(0.072)	(0.008)	(0.029)	(0.019)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the results on the treatment effect of a binding term limit on the pattern of fiscal policy. The first row lists the dependent variables. Results are obtained from the estimation of Eq. (5). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 6: Resigning Mayors – Term Treatment Effects

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
2010-13	-0.058	-0.013**	-0.006	-0.032**
	(0.057)	(0.006)	(0.021)	(0.013)
R_{it}	-0.063	0.001	-0.015	0.014
	(0.073)	(0.012)	(0.038)	(0.019)
$R_{i,t>t_0}$	0.070	0.003	-0.051	-0.041
	(0.135)	(0.015)	(0.049)	(0.029)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the results on average heterogeneous treatment effects of a binding term limit on fiscal policy when a lame duck resigns. The left column lists the dependent variables. Results are obtained from the estimation of Eq. (3). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 7: Resigning Mayors – Annual Treatment Effects

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
2010	-0.014	-0.004	0.009	-0.027
	(0.051)	(0.006)	(0.023)	(0.017)
2011	-0.021	-0.007	0.019	-0.037**
	(0.064)	(0.007)	(0.024)	(0.016)
2012	-0.085	-0.020**	-0.000	-0.029*
	(0.070)	(0.008)	(0.024)	(0.016)
2013	-0.110	-0.022***	-0.052*	-0.035*
	(0.076)	(0.008)	(0.031)	(0.021)
R_{it}	-0.063	0.001	-0.015	0.014
	(0.073)	(0.012)	(0.038)	(0.019)
$R_{i,2010}$	0.037	-0.005	-0.017	-0.035
	(0.170)	(0.015)	(0.046)	(0.031)
$R_{i,2011}$	0.019	0.000	-0.070	-0.064*
	(0.135)	(0.015)	(0.051)	(0.033)
$R_{i,2012}$	0.188	0.002	-0.042	-0.068**
	(0.140)	(0.016)	(0.055)	(0.033)
$R_{i,2013}$	0.035	0.014	-0.076	0.004
	(0.158)	(0.019)	(0.063)	(0.048)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the results on annual heterogeneous treatment effects of a binding term limit on fiscal policy when a lame duck resigns. The left column lists the dependent variables. Results are obtained from the estimation of Eq. (4). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 8: Placebo Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
Fines & fees	0.025	0.041	0.043	0.031	0.049	-0.051
	(0.046)	(0.044)	(0.044)	(0.046)	(0.044)	(0.044)
Tax rate	-0.006	-0.004	-0.004	-0.007	-0.005	-0.005
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Tax revenues	-0.022	-0.006	-0.005	-0.021	-0.005	-0.006
	(0.025)	(0.021)	(0.021)	(0.023)	(0.020)	(0.020)
C. Expenditure	-0.003	-0.003	-0.003	-0.005	-0.006	-0.007
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
District trends	No	Yes	Yes	No	Yes	Yes
District x year	No	No	Yes	No	No	Yes
Controls	No	No	No	Yes	Yes	Yes
Observations	2221	2221	2221	2197	2197	2197

This table presents the results of a placebo treatment on fiscal policy. The left column lists the dependent variables. Results are obtained from the estimation of Eq. (1) with d_t indicating a placebo treatment for the electoral year 2006-2009. All estimates include municipality and year fixed effects. Model (2) adds district-specific time trends and model (3) district-year fixed effects. Models (4), (5) and (6) replicate (1), (2) and (3), respectively, adding the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 9: Quasi-myopic model

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
Term limit	-0.031	-0.016*	-0.018	-0.038**
	(0.066)	(0.009)	(0.026)	(0.016)
Ex ante effect $(t+1)$	0.060	-0.009	-0.021	-0.017
	(0.061)	(0.008)	(0.025)	(0.018)
Ex ante effect $(t+2)$	-0.059	-0.015**	-0.012	-0.019
	(0.058)	(0.008)	(0.027)	(0.014)
Ex ante effect $(t+3)$	0.057	0.002	0.012	0.005
	(0.053)	(0.007)	(0.032)	(0.019)
Ex ante effect $(t-4)$	0.042	-0.001	0.003	0.008
	(0.043)	(0.006)	(0.022)	(0.009)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the quasi-myopic results on the average treatment effect of a binding term limit on fiscal policy. The first row lists the dependent variables. Results are obtained from the estimation of Eq. (6). All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 10: Group-specific Trends

	Fines & Fees	Property Tax Rate	Tax Revenues	Current Expenditure
2010	0.009	-0.010	0.006	-0.041*
	(0.086)	(0.013)	(0.035)	(0.024)
2011	0.000	-0.013	0.009	-0.055**
	(0.092)	(0.014)	(0.036)	(0.021)
2012	-0.040	-0.025*	-0.007	-0.047**
	(0.095)	(0.014)	(0.036)	(0.022)
2013	-0.087	0.026*	-0.064	-0.043
	(0.099)	(0.014)	(0.042)	(0.027)
District trends	Yes	Yes	Yes	Yes
District x year	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3297	3025	3297	3297

This table presents the results on the annual treatment effect of a binding term limit on fiscal policy controlling for group-specific pre-existing trends. The first row lists the dependent variables. Results are obtained from the estimation of an extended version of Eq. (2) including a linear time trend and a treatment group specific time trend. All estimates include municipality and year fixed effects, district-specific time trends, district-year fixed effects and the following control variables: municipal population size, municipal economic activity, majority in the municipal assembly, partisan alignment in council and assembly and left-leaning mayor. Standard errors are clustered at the municipal level and robust to heteroscedasticity. Stars indicate significance levels of 10%(*), 5%(**) and 1%(***).

Table 11: SUMMARY STATISTICS

Variable		Mean	SD	Min.	Max.	N
Fines and Fees	overall	16.572	18.088	.741	589.077	3333
	between		13.183	2.526	113.342	278
	within		12.404	-69.329	492.308	11.989
Property Tax	overall	.380	.085	.2	.5	3058
	between		.064	.218	.464	278
	within		.056	.143	.607	11
Tax revenues	overall	140.068	124.081	6.582	1312.731	3333
	between		117.361	36.677	811.033	278
	within		40.725	-134.751	660.493	11.989
Current Expenditures	overall	554.899	272.574	85.451	2007.682	3333
	between		254.888	211.185	1650.912	278
	within		97.580	110.862	1311.723	11.898
Resignation rate	overall	.050	.219	0	1	3336
	between		.126	0	.667	278
	within		.179	-.616	.717	12
Aligment	overall	.976	.152	0	1	3336
	between		.087	.417	1	278
	within		.125	.0597	1.560	12
Left	overall	.511	.500	0	1	3336
	between		.434	0	1	278
	within		.249	-.405	1.428	12
Local Economy	overall	283.503	214.436	12.073	3792.383	3300
	between		193.111	12.286	2498.046	275
	within		93.890	-677.495	1577.84	12
Inhabitants	overall	36165.72	57587	1634	549766	3336
	between		57607	1768.083	514263	278
	within		2939.726	-8588.284	83074.13	12