NOVA SCHOOL OF BUSINESS AND ECONOMICS

UNIVERSIDADE NOVA DE LISBOA

Dissertation, presented as part of the requirements for the degree of
Doctor of Philosophy in Economics

Reforming Local Governance:
Fiscal Federalism and Political Accountability

Ana Catarina Silva Dias Alvarez, Student Number 14336

A dissertation carried out on the PhD in Economics, under the supervision of
Professor Susana Peralta.

November 8, 2019

Abstract

This thesis explores three reforms implemented in Portugal to obtain causal inference about the manifold impacts of the rules that shape local government behavior. The first chapter focuses on the electoral response to a property tax reform, aiming to promote a higher degree of decentralization and autonomy of municipalities. The second chapter tests the hypothesis of tax capitalization by exploring an unexpected reduction in the upper bound of the property tax rate. The last chapter - using an original dataset on mayors' characteristics - exploits a recent reform introducing mayoral term limits to identify its causal impact on political selection.

Keywords: public finance, fiscal federalism, property taxation, political accountability.

Acknowledgments

I have to confess that writing the acknowledgments has been what kept me going to finish this dissertation. This was not an easy path - I guess it never is! If the result is here now materialized, it was solely due to all the fantastic people who have always surrounded me and to whom I can now express my immense gratitude.

First and foremost, I would like to thank my supervisor, Susana Peralta. Not because I should, as is the convention, but because I truly want to. Without Susana, I would most certainly never be writing these words. My years as a Ph.D. student were unstable, with the only constant being Susana's support. She is a brilliant researcher, but it is not only because of that she is the best role model I could ever have. Since the beginning, this was not a mere supervisor-student relationship, she was always open and available to help me with my most personal problems. This is so rare to have from a supervisor - I know that very well. For that, I am forever grateful, and I will never forget. Completing a doctoral degree is so hard, but if everyone had a Susana by their side, the academic world would be a happier and better place.

I am also extremely grateful to my two amazing "non-official supervisors": Amedeo Piolatto and Mariana Lopes da Fonseca. Amedeo gave me the support I needed in one of the most challenging periods of my academic life and in a place where, for me, this support was so scarce. Amedeo helped me to take one of the hardest decisions I had to make and that I will never forget, thank you!

To Mariana... I thank Mariana every day we talk. She is my main academic inspiration, and I am incredibly lucky to have her as a co-author, and especially as such a good friend. I think a lot about the day we met in Dublin and how it was so crucial for the completion of this journey.

A special thank you is due to all my amazing hosts at several research visits in Germany. Namely, Sebastian Siegloch in Mannheim, Andreas Peichl and Clemens Fuest at CESifo in Munich.

I must thank the faculty members at Nova SBE for having shared their knowledge with me, either by attending their classes or by giving me such helpful comments at the research groups or even through fruitful casual conversations in the walkways. In particular, I want to mention particularly Professor José Tavares. Also to the teams of Calculus I and II, namely to Professor Maria Helena Almeida, Professor Patrícia Xufre, João Farinha and Pedro Chaves. A special thanks to the Research Office staff - specifically Sofia Vala - for all the administrative help.

The best part of this journey was, without a doubt, the number of incredible people I was able to meet throughout the years and that are now spread around the world. Starting with Willem, my Yoda and confidant - he, annoyingly, knows how special he is to me and how much he has helped me. To everybody in Barcelona, Lisbon, Munich, and at all the conferences I have been attending. Just to name a few: Marta, Sharmin, Ilias, Xavi, María, Marta, Risa, Kevin, Carolina, Lisa, and Laura. To the *ninjas*, João and Bruno. And an extraordinary mention to my Catalan family, Mariona and Pablo.

Of course, then there are all the others outside the academic world: my family and friends. The ones that never quite understood what I was doing or why, but never failed to give me their unconditional support. To my parents, to my sister, to Marga, to Kiko: thank you so much! Never forgetting the furrier ones: Spice, Reef, Nacho, Chilli, and Pêra.

Finally, a special thank you to someone who only has been part of this journey over the last month, but what a tough and crucial month. Thank you, Manel!

This work is dedicated to Avô Eurico, the reason why I decided to study Economics.

This work had the support of Fundação para a Ciência e Tecnologia (FCT) under the grant SFRH/BD/131804/2017 and Associação de São Bartolomeu dos Alemães de Lisboa (ASBAL) under the 2018 Nova SBE Scholarship.

Contents

Introduction

The last decades witnessed an unprecedented move towards more decentralized spending and taxing powers. While there is extensive literature, dating to Tiebout (1956) and Oates (1972) seminal contributions about the merits of decentralization, less is known about how specific details about its design have an impact on economic outcomes. The classical argument in the literature is that decentralization allows for a better representation of local preferences and so better accountability of local politicians Lockwood (2006).

On the other hand, there is also a more skeptical strand of the literature. The main criticism is that decentralization is not equivalent to separation, therefore even when decisions on critical public services are delegated to local governments, the financing responsibility often largely remains in the hands of the central government (Ambrosanio and Bordignon, 2006, Boadway, Marchand, and Vigneault, 1998). Consequently, instead of strengthening, decentralization may weaken political accountability, leading to less efficient equilibria (Devarajan, Khemani, and Shah, 2007). The intuition is that if citizens are uncertain about whom to blame for the taxes they have to pay, they will be less able to punish or reward the right politicians for their behavior. Hence, this leads to more tolerance in judging fiscal decisions.

Recently, there have been several contributions that exploit quasi-experimental variation in institutional design at the local level to obtain causal inference about the manifold impacts of the rules that shape local government behavior. This thesis contributes to this strand of the literature by exploring three reforms implemented in Portugal and its impacts.

In Portugal, there are three levels of governance: central, municipal, and civil parishes. There are in total 308 municipalities, from which 278 mainland municipalities. Portuguese municipalities have control over their spending - subject to prevailing laws and regulations - but they do not score very high in terms of local revenue autonomy (OECD, 1999). The reason for the reduced local autonomy is twofold: high reliance on transfers from the central government and reduced freedom to set local tax rates (on centrally set tax bases). Local revenue sources consist mainly of local taxes, transfers from the central and regional governments, and transfers from the EU. There are different local taxes, namely an indirect tax on the transfer of the real estate, the local property tax, a variable tax share of the central government personal income tax, and a municipal surcharge on corporate income tax.

The property tax is the main fiscal tool of Portuguese local governments: it is their primary source of own revenue and over which they have some discretionary power over the property tax rate. Every year, property tax rates are decided at the municipal assembly - within an interval typical to every municipality and defined by the central Government.

From 1926 to 1974, Portugal was governed by an authoritarian regime. In 1974, democracy was restored, and regular elections have been held: presidential elections every five years, and parliamentary and local elections every four years. At the municipal level, voters decide their representatives in the municipal council (executive branch) and the municipal assembly (legislative branch). The head candidate of the most voted list to the municipal council is elected as mayor, the local top chief executive - the most prestigious and influential political position at the local level.

Until 2013, Portuguese local politicians did not face term limits. This means that they could run indefinitely for office, which happened in fact and with a high success. For example, in 2009, two politicians were re-elected for their tenth term, meaning that they have been kept in office since the first local elections held in 1976. However, in 2006, a law introducing mayoral term limits entered into force, but it was only binding in 2013.

The first chapter focuses on the electoral response to an important tax reform occurred in Portugal, aiming to promote a higher degree of decentralization and increased the autonomy of local governments. This reform introduced a new property tax (*IMI*), substituting the property tax in place (*CA*) and introducing a new tax code with a different method for calculating the tax base based on the properties assessed value. This would imply a significant increase in the tax base of the property tax, whose revenues are used and managed by the local governments. The central government was responsible for the execution of the reassessments, but it had not conducted a simultaneous wave of property revaluations after the reform. In fact, after 2003, a transitory regime was in place, where two tax rates were being applied: the old tax rate if the property was not reassessed, and the new one if the property was reassessed or bought/sold. By law, this transitory regime should have lasted at most ten years, so that by 2013, all properties were already reassessed according to the new code. However, when Portugal entered the financial assistance program in 2011, it was revealed that approximately 70% of the housing stock was not yet reassessed and, so, one condition of this program established that the reassessment process must be completed by the end of 2012. This put in place an urgent wave of property reassessment in all municipalities, extremely salient and with significant media coverage, for it would imply a shock in tax liabilities.

Whether voters reacted to the impact of the property reassessments at the parliamentary elections of 2015 and how they voted poses critical empirical questions. On the one hand, the reform represented a significant increase in property tax liabilities. On the other hand, given the political unpopularity and people's adverse reaction to the reform, the Government decided to implement a tax cap policy until 2014 - which smoothed out payment of the increase in tax burden due to the reassessments over the pre-election period - reducing the salience of the shock significantly. Hence, the first empirical prediction is that there should not be a significant overall response at the subsequent parliamentary elections. Inspired by Casaburi and Troiano (2016), I test this prediction estimating a continuous difference-in-differences model based on the intensity of the property reassessments - i.e., the municipal share of urban properties non-reassessed in 2011. It focuses on six electoral outcome variables: vote share of the incumbent government party in 2011 (right-wing coalition); vote share of the main opposition party (socialists); difference in vote share between these two parties; sum in the vote share of left-wing parties; sum in the vote share of parties with no parliamentary representation; and the sum of blank and null votes (percentage). I obtain a not statistically significant impact in any of the outcomes, corroborating the first empirical hypothesis.

Furthermore, the reform is not likely to affect the entire population in an even manner. By exploiting the growth rate of the property tax base, it is possible to isolate the de facto effect of the reassessments on the increase of the tax liabilities. This growth rate presents relevant heterogeneities across municipalities, which might impact how voters react to the shock. Thus, another empirical prediction is that voting patterns might differ between municipalities with different real effects of property reassessments. I implement then a triple difference approach exploiting these heterogeneities, and I obtain that in municipalities with a higher property tax base growth rate punish more the incumbent government party at the time of the reform, the right-wing coalition.

The second chapter tests the hypothesis of property tax capitalization by exploring an unexpected reduction in the upper bound of the Portuguese property tax rate for urban real estate in 2008.[1] From a theoretical point of view, the property tax capitalization is based on the assumption that both house characteristics and factors affecting the cost of living determine the net present value a prospective buyer is willing to pay for a house [Oates (1969) and Yinger (1982)]. Therefore, if a property tax reduction is viewed by the buyers as a decrease in the cost of living, they will be willing to pay higher prices. Taking the supply of land and housing as fixed, lower present, and future tax payments are then expected to inflate the market value of the real estate. Empirically, there are two main identification issues that must be taken into account. First, the level of public goods is positively correlated with the property tax level, and both independently affect house prices. For this reason, it is difficult to isolate the effect of the tax separately. Second, there is a likely simultaneity bias between the property tax rate and house prices when local governments set their own tax rate: areas with a high house price level, all else equal, are able to set a lower tax rate to collect a certain amount of tax revenues.

This chapter tackles these issues by exploiting a quasi-natural experiment: an unexpected reduction in the upper bound of the Portuguese property tax rate for urban real estate announced by the Prime-Minister on July 2, 2008. This reform allows dividing mainland municipalities in treated (those that were forced to decrease their tax rate) and comparison municipalities. Under the assumption of parallel trends in housing transaction developments of the two groups, the difference-in-differences setting estimates the causal effect of this reform on real estate values. In Portugal, the property tax is set by the municipality, on a tax range defined by the central Government. The value of the tax base (i.e., the fiscal value of the property) is also decided centrally. We take advantage of a comprehensive dataset based on a single country where all local

[1] This Chapter is co-authored with João Pereira dos Santos

governments operate under the same institutional framework. This dataset includes socio-economic, fiscal, and political variables on all mainland Portuguese municipalities from 2005 to 2011.

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

Finally, the last chapter investigates the selection effects of the introduction of term limits.[2] That is, whether term limits, by creating more rotation in power, lead to the entry and selection of better politicians. We construct an extensive and unique dataset on Portuguese mayors' personal characteristics, and we take advantage of a

[2]This Chapter is co-authored with Mariana Lopes da Fonseca

recent reform introducing mayoral term limits in Portugal to identify its causal impact on political selection.

There is a rich and extensive literature with arguments both for and against the introduction of term limits, as it eliminates incumbency advantage and removes long-tenured incumbents from office. Advocates for term limits argue that it allows the entry of potentially better politicians and eliminates policy biases in favor of specific interest groups represented by long-serving incumbents. On the other hand, term-limits impose a constraint on voters' choice of representatives, whose long tenure may be due to higher quality; or it may be that voters prefer a representative with more extended political longevity and stronger influence, who is able to more successfully broker resources and legislation to their own benefit. By this token, the introduction of term limits poses a relevant empirical question. For which evidence is still scarce, mainly due to data limitations (Dal Bó, Finan, Folke, Persson, and Rickne, 2017).

We study the selection effects of term limits, and our contribution is twofold: we construct an extensive and unique dataset on Portuguese mayors' personal characteristics, and we take advantage of a recent reform introducing mayoral term limits in Portugal to identify its causal impact on political selection.

Term-limits were introduced in Portuguese local governments in 2006, and the first effects came into play solely at the 2013 local elections. In total, 150 mayors from the 278 mainland municipalities were forced to leave office, creating the first exogenously determined open-seat elections in Portugal. We take advantage of this reform as a quasi-natural experiment to examine its political selection effects. Our identification strategy relies on a difference-in-differences approach estimating how these mayors' personal characteristics differ on average between municipalities with re-eligible and term-limited incumbents.

For this purpose, we constructed an extensive and unique dataset on personal characteristics of Portuguese mayors at the past four local elections: 2001, 2005, 2009 and 2013 - a total of 586 individuals in 278 municipalities. Some of these personal characteristics are publicly available, but self-reported by the elected politicians after taking office, and the overall data is somewhat incomplete and imprecise. Thus, we complemented it manually using the information found online or contacting the municipal council directly. Taking all the available information, we compiled the following variables based on mayors' characteristics: their gender, the age when they took office for the first time, the level of education, the area of education, and the occupation before taking office.

Our baseline results show that municipalities affected by the reform elected politicians, on average, older (around four years) and with a past political career. The results are robust to several falsification tests, in particular concerning any potential anticipation effects at the 2009 local election.

Chapter 1

Electoral Response to a Large Property Reassessments Reform

1.1 Introduction

Property tax is an extremely important fiscal tool for local governments of most developed countries (Norregaard, 2013). This is motivated by its immobile tax base and all of its virtues – low efficiency costs, benign impact on growth and high score on fairness –, making it fit perfectly the criteria for a good local tax by respecting the benefit principle and promoting high political accountability. However, in order for property taxation to reach its potential both in terms of efficiency and equity it requires an adequate management of the property assessment system. That is, the method the tax authority uses to compute and update the taxable value of properties (i.e., the property tax base), which is intended to be an approximation of their market value. Continuous property reassessments are, therefore, crucial to maintain equity by keeping assessed values in line with their true market value.

Due to the high salience and political unpopularity of the property tax (Cabral and Hoxby, 2012), motivations behind property reassessments may not be necessarily related to the maintenance of equity. These hidden motivations make reassessments more complex than a simple administrative practice and are at the base of the theory of the creation of a sort of "fiscal illusion" from this procedure.[1] This theory is based on the widespread voter's perception that politicians set tax rates instead of levies. Since reassessments are usually translated in an augmented property tax base, this gives local politicians an opportunity to raise revenues without facing the adverse political consequences of increasing the tax rate – consequences which are particularly severe in the case of the property tax. In other words, politicians can take advantage of a reassessment to meet their budgetary and/or rent-seeking goals without jeopardizing their reelection chances.

The literature related with the effects of property reassessments is scarce and mostly focused on its impact on the level of revenues. Bloom and Ladd (1982), Ladd (1991) and Ross and Yan (2013) documented that indeed property tax revenues tend to spike up in the year of a community–wide reassessment and attributed this fact to voter fiscal illusion. On other note, Strumpf (1999) analyzes the distortions of infrequent assessments.

Nonetheless, to the best of my knowledge, no study has yet focused on the electoral effects of conducting a mass reappraisal. In this paper, I provide a valuable contribution for the literature by studying the electoral response to an exogenous and urgent wave of property reassessments in Portugal.

In 2003, an important tax reform occurred in Portugal aiming to promote a higher degree of decentralization and increased autonomy of local governments. This reform introduced a new property tax (*IMI*), substituting the property tax in place (*CA*) and introducing a new tax code with a different method for calculating the tax base based

[1] Oates (1988) defines fiscal illusion as: "The notion that systematic misperception of key fiscal parameters may significantly distort fiscal choices by the electorate."

on the properties assessed value. Thus, property reassessments had to be conducted in order to optimally approximate the assessed value to market value. This would imply a significant increase of the tax base of the property tax, whose revenues are used and managed by the local governments. The central government was responsible for the execution of the reassessments but it did not conducted a simultaneous wave of property revaluations after the reform. In fact, after 2003 transitory regime was in place, where two tax rates were being applied: the old tax rate if the property was not reassessed; and the new one if the property was reassessed or bought/sold. By law, this transitory regime should have lasted at most 10 years, so that by 2013 all properties were already reassessed according to the new code. However, when Portugal entered the financial assistance program in 2011, it was revealed that approximately 70% of the housing stock was not yet reassessed and, so, one condition of this program established that the reassessment process must be completed by the end of 2012. This put in place an urgent wave of property reassessment in all municipalities, extremely salient and with significant media cover, for it would imply a shock in tax liabilities.

Whether voters reacted to the impact of the property reassessments at the parliamentary elections of 2015 and how they voted poses important empirical questions. On the one hand, the reform represented a significant increase in property tax liabilities. On the other hand, given the political unpopularity and people's negative reaction to the reform, the Government decides to implement a tax cap policy until 2014 - which smoothed out payment of the increase in tax burden due to the reassessments over the pre-election period - reducing significantly the salience of the shock. Hence, the first empirical prediction is that there should not be a significant overall response at the subsequent parliamentary elections. Inspired by Casaburi and Troiano (2016), I test this prediction estimating a continuous difference-in-differences model based on the intensity of the property reassessments - i.e., the municipal share of urban properties non-reassessed in 2011. It focuses on six electoral outcome variables: vote share of the incumbent government party in 2011 (right-wing coalition); vote share of the main opposition party (socialists); difference in vote share between these two parties; sum in the vote share of left-wing parties; sum in the vote share of parties with no parliamentary representation; and the sum of blank and null votes (percentage). In fact, I obtain a not statistically significant impact in any of the outcomes, corroborating the first empirical hypothesis.

Furthermore, the reform is not likely to affect the entire population in an even manner. By exploring the growth rate of the property tax base it's possible to isolate the de facto effect of the reassessments on the increase of the tax liabilities. This growth rate presents relevant heterogeneities across municipalities, which might impact how voters react to the shock. Thus, another empirical prediction is that voting patterns might differ between municipalities with different real effects of property reassessments. I implement then a triple difference approach exploring these heterogeneities and I obtain that in municipalities with a higher property tax base growth rate punish more the incumbent government party at the time of the reform, the right-wing coalition.

The rest of the chapter is organized as follows. Section 1.2 explains in more detail the Portuguese institutional framework and the tax reform under study. Section 1.3 describes the empirical analysis - the dataset, the treatment and the identification strategy. Section 1.4 presents the results. In section 1.5, the internal validity of the results is advocated. Finally, Section 1.6 concludes.

1.2 Institutional Background

1.2.1 Portuguese Political Overview

From 1926 to 1974, Portugal was governed by an authoritarian regime. In 1974, democracy was restored, and regular elections have been held: presidential elections every 5 years, and parliamentary and local elections every 4 years. Since then, the parliamentary elections have been won either by the center-right party (*Partido Social Democrata*, PSD) or by the center-left party (*Partido Socialista*, PS). However, three other important parties are represented in the parliament: the communist party (*Partido Comunista Português*, PCP)[2], a left-wing party (*Bloco de Esquerda*, BE) and a conservative right-wing party (*Partido Popular*, CDS-PP). CDS-PP often forms coalitions with PSD and has been part of the Portuguese government (1980-83, 2002-05 and 2011-15), however PSD has kept most of the high level executive branch positions.

1.2.2 Portuguese Property Tax

The current property tax, IMI, was introduced in 2003 as a result of a general reform of the Portuguese tax system. It substituted the previous property tax, *Contribuição Autárquica* (CA), implemented in 1989, and was accompanied by a revaluation of urban property for fiscal purposes, whose implementation spanned several years after the reform. Therefore, with this reform and until 2013, two different property tax rates co-exist in each municipality and every year - namely, one on urban property whose fiscal value was already re-assessed, and another on urban property whose fiscal value was not yet reassessed according to the new rules. Local authorities have some discretionary power over the property tax, for every year tax rates are decided at the municipal assembly. These rates are set within an interval common to every municipality and

[2] It runs in coalition with the green party PEV which is, in practice, a controlled spin–off.

defined by the central government. Since 2003, this interval has been experiencing some changes and Table 1.1 summarizes the evolution of the interval applied to urban reassessed properties over time.

Table 1.1: Reassessed Urban Propery Tax Rates (%)

Year	Min	Max
2003-2007	0.20	0.50
2008-2011	0.20	0.40
2012-2015	0.30	0.50

1.2.3 Property Tax Reform and The Financial Assistance Program

In 2003, an important tax reform occurred in Portugal[3] aiming to promote a higher degree of decentralization and increased autonomy of local governments. This reform introduces a new property tax (*Imposto Municipal sobre Bens Imóveis, IMI*), substituting the property tax in place (*Contribuição Autáquica, CA*) and introducing a new tax code (*CIMI*) with a different method for calculating the tax base based on the properties assessed value. Thus, property reassessments had to be conducted in order to optimally approximate the assessed value to market value - i.e., the assessed value is calculated by implementing a formula set by law and does not depend on the actual market value of the property. This implied a significant increase of the tax base of the property tax. These revenues are used and managed by the local governments, whereas the central government is the one responsible for the execution of the reassessments.

The law established that by 2013 all properties must have been reassessed according to the new property tax code. However, when Portugal entered the financial assistance

[3] *Decreto-Lei n. 287/2003 - 12/11*

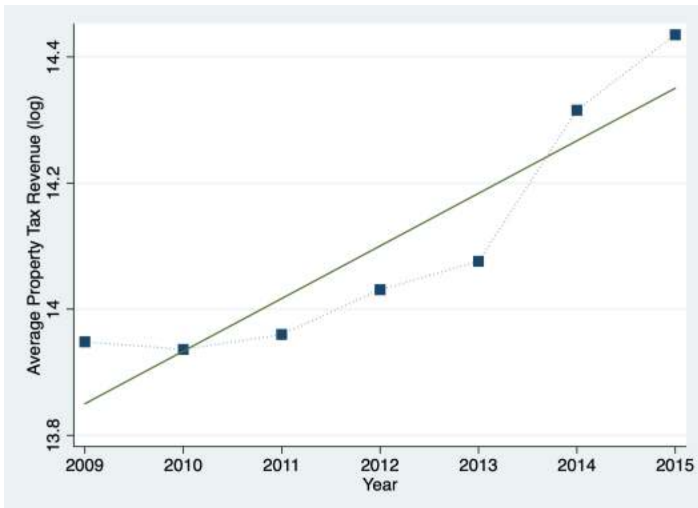
program in May 2011, it was revealed that approximately 70% of the housing stock was not yet reevaluated. The reason was that no effort by the central government was done to conduct these reappraisals, probably due to high logistics and political costs that it would entail. The 30% of the properties reassessed were those transacted after 2003, whose taxable values were automatically updated according to the new code. Therefore, one condition of the financial assistance program was in fact that the reassessment process must be completed by the end of 2012.[4]. This put in place an extensive wave of property reassessment in all municipalities, which was extremely salient at time with significant media cover as it would imply a shock in tax liabilities - see in Appendix A.1 the headlines of the main Portuguese newspapers in the day the reappraisal procedure started, December 1^{st} 2011.

This implied that by the end of 2012 all properties were under the new tax code, thus the property tax bill for this this year would already reflect the augmented tax base and it would have to be paid in April of 2013. However, this was an electoral year, with local elections to be held in September of 2013. So given its political unpopularity and people's negative reaction, the Government decides to implement a tax cap policy - "*Regime de salvaguarda de prédios urbanos*", Article 15-O in Law 60-A/2011 - to moderate the impact of the general revaluation in the property tax liabilities. This policy ensures that this increase in tax liabilities shall not exceed, for the year 2012 and 2013 (i.e. to be paid in 2013 and 2014): €75 or one third of the difference between the tax burden resulting from new tax base and the tax burden in 2011. This way the payment of tax burden increase was not paid immediately in 2013, but smoothed out over two years, reducing the salience of the tax reform. It was only in April of 2015 that taxpayers had to pay the full property tax bill corresponding to the new taxable

[4] "The Government will review the framework for the valuation of the housing stock and land for tax purposes and present measures to ensure that by end 2012 the taxable value of all property is close to the market value" in *The Economic Adjustment Program for Portugal* (2011)

value of their property. Figure 1.1 shows the evolution of the property tax liabilities (in logarithm transformation) over the period 2009 to 2015 and one can observe clearly the smoothing effect of the tax cap policy.

Figure 1.1: **Property Tax Liabilities (log) Evolution: 2009-15**



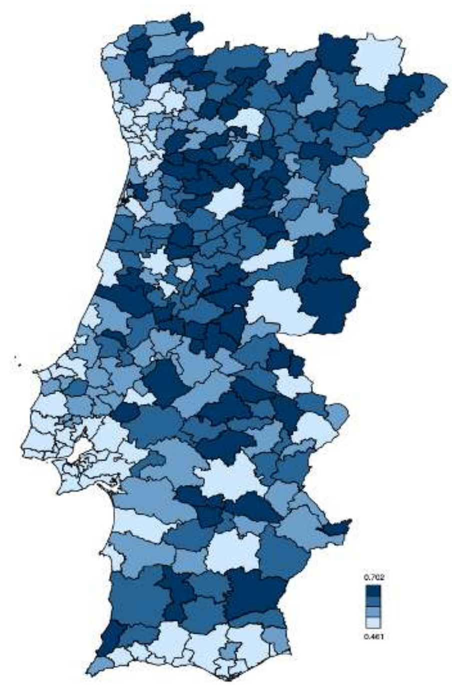
1.3 Empirical Analysis

1.3.1 Treatment Intensity

This process was likely to cause an increase of the property tax liabilities due to this exogenous shock in the tax base. However, the intensity of this shock depends on the share of properties to be reassessed, causing a variation in the change of revenues across municipalities depending on this share. The time-line of this large reassessment reform and tax cap policy creates an interesting quasi-natural experiment, as it all culminated before the parliamentary elections of 2015. By exploring the intensity difference of the reform across municipalities, it is possible to rely on both between- and within-municipality variation to identify the impact this salient future increase of property tax liabilities on the electoral outcomes.

Unfortunately, the share of reassessed/non-reassessed urban properties at municipal level by the time of the implementation of the reform in 2011 is not available. Hence, following the rational that until that period only the taxable value of transacted properties after 2003 was according to the new tax code, I constructed a proxy for this share based on the housing stock and number of transactions per municipality over the period 2003 to 2011. Figure 1.2 shows the spatial distribution of share of non-reassessed properties in 2011 for all 278 mainland Portuguese municipalities - refer to Appendix A.2 for more details about the distribution this variable.

Figure 1.2: **Property Reassessment Intensity in 2011**



This is therefore the main interest variable, providing the property reassessment intensity across Portuguese municipalities due to the reform.

1.3.2 Data

This study relies on an extensive dataset at Portuguese municipal level. The empirical identification is centered on the electoral response to the property reassessment reform at the central elections of 2009, 2011 and 2015. It focuses on six outcome variables: vote share of the socialist party; vote share of the right-wing coalition; difference in vote share between the right-wing coalition and the socialist party; sum in the vote share of left-wing parties; sum in the vote share of parties with no parliamentary representation; and the sum of blank plus null votes (percentage). All variables are constructed using publicly available data from the Portuguese Secretary of Internal Administration's website (*Secretaria Geral da Administração Interna*, *SGMAI*). Panel B of Table 1.2 summarize these variables for the 278 mainland Portuguese municipalities in the pre-treatment election years, 2009 and 2011.

For robustness, the analysis includes also a number of control variables, namely socioeconomic and political characteristics. Table 1.2 summarizes these variables for the pre-treatment period. The socioeconomic variables are described in Panel C and comprise measures of municipal population size, municipal unemployment, a measure for municipal economic activity proxied by a purchasing power index and a dummy indicating whether the municipality is setting the minimum property tax rate. Municipal population size coincides with the resident population per municipality series from Statistics Portugal *(INE)*, and it is also included the share of this population aged below 15 years old together with share of population aged more the than 65 years old. Municipal unemployment is measured by the ratio of resident population aged between 15 and 65 years old who is enrolled as unemployed in Portuguese Institute of Employment and Professional Training (*IEFP*). This latter series is obtained from the Database of Contemporary Portugal (*PORDATA*). The purchasing power index is constructed by Statistics Portugal and it is publicly available at their website. Lastly, the dummy that turns on when the municipality is setting the minimum property tax rate is built based on the property tax rate series obtained from the Portuguese Tax Authority's website (*AT*).

The set of municipality's political characteristics is described by panel D of Table 1.2 and is constructed based on data obtained from *SGMAI*, as the outcome variables. The data is provided at the party level per municipality consisting on the turnout share, the number of votes and seats allocated to each party. The variable of lagged turnout consists on the turnout share in the previous central election, i.e. in 2005 and 2009, respectively. Finally, the other variables are based on incumbent mayor characteristics in each municipality: a dummy indicating whether she is from the same party as the central government, the number of terms she has been in office and a dummy indicating whether she is from a left-wing party.

Table 1.2: Summary Statistics: 2009 and 2011

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**	**N**
Panel A: Property Reassessment Variable					
Share of Non-Reassessed Properties in 2011	0.671	0.033	0.461	0.702	278
Panel B: Outcome Variables - Vote Shares (%)					
Socialist Party	33.437	7.544	9.638	57.830	556
Right-wing Coalition	47.419	14.018	10.672	84.142	556
Right-wing Coalition - Socialists	13.982	19.479	-33.612	74.504	556
Left-wing	48.947	13.766	13.474	86.937	556
No Parliamentary Representation	3.634	1.265	1.174	9.756	556
Blank and Null	3.676	0.956	1.175	6.959	556
Panel C: Socioeconomic Variables					
Population (1,000)	36.134	58.102	1.807	549.998	556
Young Population (%)	13.605	2.453	5.837	19.547	556
Old Population (%)	23.264	6.570	10.123	44.389	556
Unemployment Rate (%)	7.649	2.306	2.415	15.785	556
Purchasing Power Index	77.022	22.785	47.36	232.54	556
Min Property Tax Rate (0/1)	0.135	0.342	0	1	556
Panel D: Political variables					
Lagged Turnout (%)	61.537	5.445	41.058	74.332	556
Mayor Aligned (0/1)	0.397	0.490	0	1	556
Mayor Term (No.)	2.966	1.888	1	10	556
Mayor Left (0/1)	0.509	0.500	0	1	556

Note: Summary statistics refer to the panel of 268 mainland Portuguese municipalities in 2011, where the local incumbent party ran for reelection over the period of analysis.

1.3.3 Identification Strategy

The identification strategy exploits variation across municipalities in the program scope to reassessed the taxable value of urban property. Inspired by Casaburi and Troiano (2016), I implement a continuous difference-in-differences approach based on municipal level property reassessment intensity. The baseline specification is as follows:

$$y_{it} = \beta_0 + \beta_1 Post_t \cdot PRintensity_i + X_{it} \cdot Pre_t \delta + \alpha_i + \lambda_t + \eta_d \cdot \lambda_t + \epsilon_{it} \tag{1.1}$$

where y_{it} is any of the outcome variables under study presented in panel B of Table 1.2 vote share of the socialist party; vote share of the right-wing coalition; difference in vote share between the right-wing coalition and the socialist party; sum in the vote share of left-wing parties; sum in the vote share of parties with no parliamentary representation; and the sum of blank plus null votes (percentage). β_1 captures the effect of interest. $PRintensity_i$ is the intensity of property reassessments in municipality i, captured by the share of non-reassessment properties in 2011 presented in panel A of Table 1.2. The dummy $Post_t$ is equal to 1 for the election year after the reform, 2015. X_{it} is a vector of control variables, including the socioeconomic and political characteristics previously described - in panels C and D of Table 1.2. The dummy Pre_t is equal to 1 for the election years before the reform, 2009 and 2011. α_i are the municipality fixed effects and λ_t the election year fixed effects. Finally, to capture any regional macro shocks occurring between elections, region times election year fixed effects $\eta_d \cdot \lambda_t$ are also included, where d represents the 18 regions in Portugal, *Districts*.

Furthermore, I provide a heterogeneous effects analysis to compare the electoral effects across different types of municipalities in terms of growth rate of the property tax base, in order to investigate whether where voters are more likely to be more harmed react differently this shock in their tax liabilities.The model to estimate is as follows:

$$\begin{aligned} y_{it} = \beta_0 + \beta_1 Post_t \cdot PRintensity_i + \beta_2 Post_t * PRintensity_i \cdot H_i \\ + \beta_3 Post_t \cdot H_i + X_{it} \cdot Pre_t \delta + \alpha_i + \lambda_t + \eta_d \cdot \lambda_t + \epsilon_{it} \end{aligned} \tag{1.2}$$

where H_i is dummy variable based on the growth rate of the property tax base due to the reassessments between 2011 and 2014. The dummy switches on when the tax base growth rate in municipality i is above the median growth rate.

1.4 Results

1.4.1 Electoral Response: Parliamentary Elections

Whether voters reacted to the impact of the property reassessments at the parliamentary elections of 2015 and how they voted poses important empirical questions. On the one hand, the reform represented a significant increase in property tax liabilities. On the other hand, given the political unpopularity and people's negative reaction to the reform, the Government decided to implement a tax cap policy until 2014 - which smoothed out payment of the increase in tax burden due to the reassessments over the pre-election period - reducing the significantly the salience of the shock. Ergo, in this part of the analysis I aim to test the hypothesis that there should not be a significant overall response at parliamentary elections following the reform.

For that, I rely on the 278 mainland Portuguese municipalities data over three central election years - 2009, 2011 and 2015. Table 1.3 collects the difference-in-differences results from the estimation of Equation 1.1 on all outcome variables, presented in each column respectively: (1) vote share of the socialist party; (2) vote share of the right-wing coalition; (3) difference in vote share between the right-wing coalition and the socialist party; (4) sum in the vote share of left-wing parties; (5) sum in the vote share of parties with no parliamentary representation; and (6) the sum of blank plus null votes (percentage).

Table 1.3: **Central Elections Vote Shares: 2009-15**

	Socialist Party (1)	Right-Coalition (2)	Right vs. Socialists (3)	Left-wing Parties (4)	Non-Represented (5)	Blank+Null Votes (6)
PR Intensity x *Post*	-9.898	13.005	22.903	-10.276	-2.728	-1.277
	(7.59)	(7.89)	(14.73)	(7.48)	(1.78)	(1.41)
Obs.	834	834	834	834	834	834

Note: Results are obtained from the estimation of Equation (1.1). All estimates include municipality a vector of control variables in the pre-treatment period, election year fixed effects and district times election year fixed effects. Refer to panels C and D of Table 1.2 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

It is obtained that property reassessments do not have significant effect on any of electoral outcomes. This result suggests that in fact the tax cap policy decreased the salience of the property tax reform and so voters overall did not respond to it.

1.4.2 Heterogeneous Effects: Property Tax Base Growth

The reform is not likely to affect the entire population in an even manner: two municipalities with the same share of non-reassessed properties in 2011, might experience a different impact in their property tax liabilities due to the reappraisal procedure, as it depends on the actual change in the property tax base. Hence, by exploring the growth rate of this tax base it's possible to isolate the real effect of the reassessments on the increase of the tax liabilities.

I calculate the growth rate of the property tax base between 2011 and 2014 as follows:

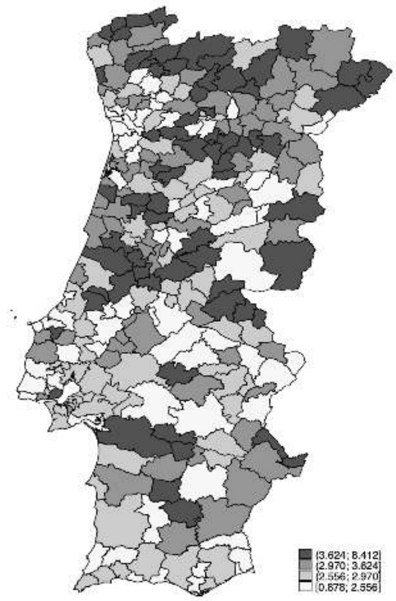
$$\text{Tax Base GR} = \frac{\text{Tax Base}_{2014} - \text{Non-Reassessed Tax Base}_{2011}}{\text{Non-Reassessed Tax Base}_{2011}} \quad (1.3)$$

where $\textit{Tax Base}_{2014}$ is given by property tax revenue of 2014 divided by the property tax rate of 2014. I focus on the tax base of 2014 as this determines the property tax liabilities to be paid in April of 2015, before the parliamentary elections. Which is also the first year after the reform when the tax cap no longer applies and tax payers have to pay the full increase due to the reassessments. On the other hand, $\textit{Non-Reassessed Tax Base}_{2011}$ is computed as the property tax revenue of 2011 times the share of non-reassessed properties and then divided by the property tax rate of 2011. Table 1.4 presents the summary statistics for this variable and Figure 1.3 shows the spatial distribution of property tax base growth rate between for all 278 mainland Portuguese municipalities - refer to Appendix A.3 for more details about the distribution of this variable.

	Mean	**Std. Dev.**	**Min**	**Max**	**N**
Property Tax Base Growth Rate	3.176	1.002	0.878	8.412	278

Table 1.4: Summary Statistics

Figure 1.3: **Property Reassessment Intensity in 2011**



Another empirical prediction is that voting patterns might differ between municipalities with different real effects of property reassessments. Hence, I take the property tax base growth rate variable and I implement then a triple difference approach exploring these heterogeneities across municipalities. Table 1.5 collects results from the estimation of Equation 1.2 on all outcome variables, presented in each column respectively. I obtain that in municipalities with a higher property tax base growth rate punish more the incumbent government party at the time of the reform, the right-wing coalition.

Table 1.5: **Central Elections Vote Shares: 2009-15**

	Socialist Party	Right-Coalition	Right vs. Socialists	Left-wing Parties	Non-Represented	Blank+Null Votes
	(1)	(2)	(3)	(4)	(5)	(6)
PR Intensity x *Post*	-10.579	16.313**	26.892*	-12.700	-3.613**	-1.091
	(7.64)	(8.18)	(14.97)	(7.72)	(1.83)	(1.35)
x Tax Base	3.467	-18.373**	-21.839	13.509*	4.864*	-1.147
	(8.01)	(7.85)	(14.61)	(7.16)	(2.70)	(1.19)
Obs.	834	834	834	834	834	834

Note: Results are obtained from the estimation of Equation (1.1). All estimates include municipality a vector of control variables in the pre-treatment period, election year fixed effects and district times election year fixed effects. Refer to panels C and D of Table 1.2 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

1.5 Internal Validity

The second important identification assumption relies on the "parallel trends" condition (Meyer, 1995). For it to hold, the trend in each of the dependent variables under study must be the same for all municipalities in the absence of treatment. This assumption can be tested through different procedures. The most standard approach consists in regressing the outcome variable on yearly dummies indicating the treatment group (Moser and Voena, 2012). This can be assessed by the following model:

$$y_{it} = \beta_0 + \sum_{j=-q}^{m} \beta_j PRintensity_{i,j} + X_{it} \cdot Pre_t \delta + \alpha_i + \lambda_t + \eta_d \cdot \lambda_t + \epsilon_{it} \tag{1.4}$$

where the sum allows for q pre-treatment effects and m post-treatment effects. The remaining variables are defined as before. Figure 1.4 provides the annual average treatment effects obtained from the estimation of Equation 1.4.

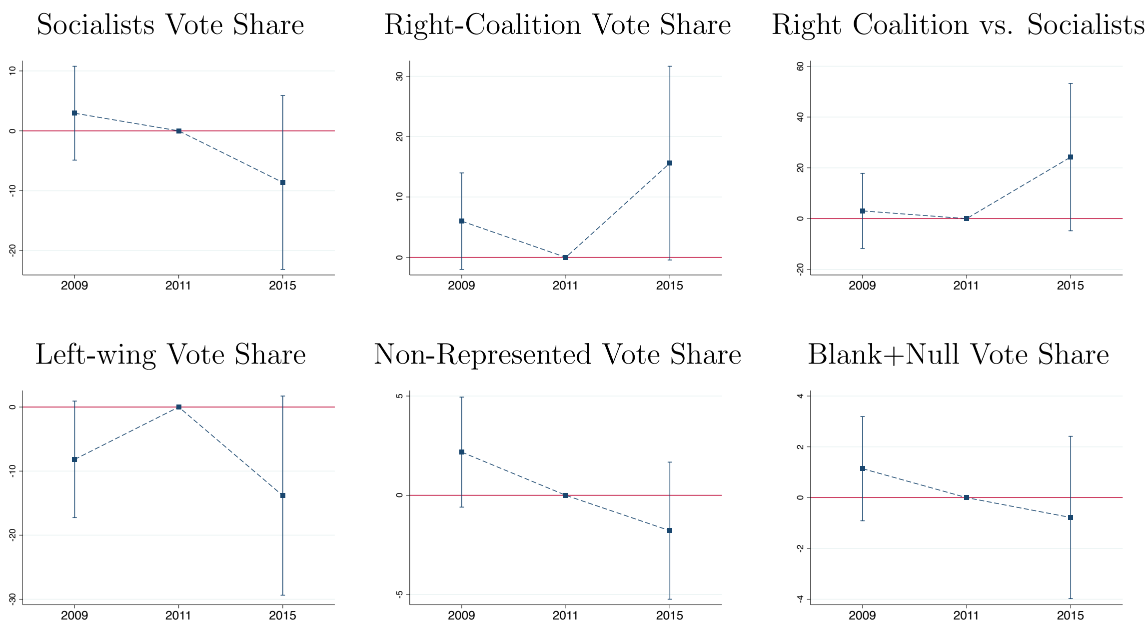


Figure 1.4: **Event Study** Plots depict the Coefficients estimates obtained from regressing Equation 1.4 on each electoral outcome variable. Capped lines indicate 95 % confidence intervals

There are no difference in trends before treatment,supporting the assumption of common trends in the electoral outcomes under study. Thus, DD coefficient estimates can be assumed to capture the causal effect of treatment intensity on the electoral outcomes.

A potential problem with DD estimations that should be taken into account relates to issues of selection bias. A key identification assumption is that municipalities are exogenously assigned to treatment. Solely when this holds, one can causally identify the effect of the reform as the differential change in the outcome variables from pre- to post-treatment period. If it is not the case causality is then undermined.[5]

One can test for selection bias in a number of ways. One possibility is to study

[5]For a discussion on the importance of choosing careful comparison groups to evaluate place-based policies see Neumark and Simpson (2015).

whether some specific municipality characteristics at the time of treatment determined the intensity of property reassessments. The most common approach in the context of a DD framework to test for selection bias is to devise placebo tests. These tests usually consist on reestimating the baseline results relying on a placebo treatment setting in at a fake treatment year. Here, the sample is restricted to the pre-treatment period, i.e. focusing only on the parliamentary elections of 2009 and 20011, and 2011 is considered as the treatment year. As Table 1.6 shows, all estimates are insignificant - with the exception of the vote share for parties with no parliamentary representation - dismissing any remaining concerns of a possible selection bias.

Table 1.6: **Central Elections Placebo: 2009-11**

	Socialist Party	Right-Coalition	Right vs. Socialists	Left-wing Parties	Non-Represented	Blank+Null Votes
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo x *Post2011*	-1.020	-3.076	-2.056	6.000	-2.925**	-0.489
	(4.46)	(4.27)	(8.11)	(4.71)	(1.42)	(1.21)
Obs.	556	556	556	556	556	556

Note: Results are obtained from the estimation of Equation (1.1). All estimates include municipality a vector of control variables in the pre-treatment period, election year fixed effects and district times election year fixed effects. Refer to panels C and D of Table 1.2 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

The same test is conducted for the triple difference estimation and Table 1.7 presents the results. All estimates are once again insignificant, dismissing any remaining concerns of a possible selection bias.

Table 1.7: **Central Elections Placebo: 2009-11**

	Socialist Party	Right-Coalition	Right vs. Socialists	Left-wing Parties	Non-Represented	Blank+Null Votes
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo x *Post2011*	-4.040	-2.892	1.148	5.868	-2.976*	-0.137
	(5.60)	(5.68)	(10.59)	(6.32)	(1.57)	(1.42)
x Tax Base	7.681	0.738	-6.943	-1.916	1.179	-0.117
	(5.18)	(5.78)	(10.22)	(6.19)	(2.09)	(1.42)
Obs.	556	556	556	556	556	556

Note: Results are obtained from the estimation of Equation (1.1). All estimates include municipality a vector of control variables in the pre-treatment period, election year fixed effects and district times election year fixed effects. Refer to panels C and D of Table 1.2 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

1.6 Conclusion

In this article, I study the political consequences of a mass reappraisal reform. For that, I focus on the financial assistance program to Portugal in 2011, which put in place an urgent wave of property reassessments in all municipalities, and the electoral response at the subsequent parliamentary elections in 2015. I use an original database for all 278 mainland Portuguese municipalities three electoral years: 2009, 2011 and 2015.

The baseline results show that the intensity of reassessments had not a statistically significant impact on any vote share outcome. This might be explained by the reduction of salience of the reform due to tax cap policy that smoothed out the increment in tax liabilities. Heterogeneous effects based on the real effect of the reassessment, in turn, indicate that in municipalities where, on average, tax liabilities augmented more, voters punished more the incumbent government party, the right-wing coalition.

Chapter 2

Property Tax Capitalization: Evidence from a Reform in Portugal[1]

2.1 Introduction

The question of whether property taxes are capitalized into real estate market values is an old topic in public economics.[2] The answer has scholar but also practical relevance since property taxes affect both individual and government budgets and may, therefore, have real efficiency and equity consequences.[3] Nevertheless, a clear answer to this question has been proven challenging, mainly due to serious empirical concerns.

From a theoretical point of view, the property tax capitalization is based on the assumption that both house characteristics and factors affecting the cost of living determine the net present value a prospective buyer is willing to pay for a house [Oates

[1]This is a joint work with João Pereira dos Santos

[2]For a survey on the broader implications of house price capitalization see Hilber (2015).

[3]For example, recent micro contributions show that changes in house prices may have implications for aggregate consumption (Mian, Rao, and Sufi, 2013).

(1969) and Yinger (1982)]. Therefore, if a property tax reduction is viewed by the buyers as a decrease in the cost of living, they will be willing to pay higher prices. Taking the supply of land and housing as fixed, lower present and future tax payments are then expected to inflate the market value of real estate.

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

In this paper, we tackle these issues by exploiting a quasi-natural experiment: an unexpected reduction in the upper bound of the Portuguese property tax rate for urban real estate announced by the Prime-Minister on July 2, 2008. This reform allows to divide mainland municipalities in treated (those that were forced to decrease their tax rate) and comparison municipalities. Under the assumption of parallel trends in housing transaction developments of the two groups, the difference-in-differences setting estimates the causal effect of this reform on real estate values. In Portugal, the property tax is set by the municipality, on a tax range defined by the central government. The value of the tax base (i.e., the fiscal value of property) is also decided centrally. We take advantage of a comprehensive dataset based on a single country where all local governments operate under the same institutional framework. This dataset includes socioeconomic, fiscal, and political variables on all mainland Portuguese municipalities from 2005 to 2011.

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

Several recent empirical studies have focused on the impact of taxation on the housing market, namely the effect of transaction taxes [Dachis, Duranton, and Turner (2012), Besley, Meads, and Surico (2014), Kopczuk and Munroe (2015), Hilber and Lyytikäinen (2017), Slemrod, Weber, and Shan (2017),Best and Kleven (2018)] and income taxes (Basten, von Ehrlich, and Lassmann, 2017). Nevertheless, the empirical literature on property taxation is still scarce due to the endogeneity concerns previously mentioned. Most early work on property taxation supports a full or partial capitalization in house prices [for a survey, see Ross and Yinger (1999), Sirmans, Gatzlaff, and Macpherson (2008) or Hilber, Lyytikäinen, and Vermeulen (2011)]. These studies are mainly focused in a local government context with cross-sectional variation in property tax rates. However, despite the fact that both identification issues previously described have been known and discussed since the seminal paper by Oates (1969), few studies have successfully dealt with them. For instance, Cushing (1984) and Palmon and Smith (1998) solve the simultaneity and measurement problems by estimating capitalization in a context where the property tax rate is unrelated to local public services and find support for full capitalization. However, both analysis are based on limited datasets.

More recently, Elinder and Persson (2017) focus in a national property tax reform in Sweden, thus unrelated to local public goods, providing good identification of a large property tax cut on house prices. In this setting, the authors find no evidence that the tax reduction led to increases in house prices with the exception of a small segment of the market containing properties with very high tax values. Moreover, Bradley (2017) exploits temporary idiosyncratic differences accruing to new homebuyers in the Michigan property tax system to study the degree to which these initial tax obligations are capitalized. The idea is to assess whether households recognize intertemporal discontinuities in property tax bases and obligations. The author finds that homebuyers overcompensate sellers of homes with relatively low tax obligations, as if such rules would persist indefinitely.

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

2.2 Institutional Background

2.2.1 Portuguese Local Finance

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

The current Portuguese property tax, IMI, was introduced in 2003 as a result of a general reform of the Portuguese tax system. Local authorities have some discretionary power over the property tax and every year they set tax rate within an interval common to every municipality. Local property taxe rates are set within a range which is defined by the central government, as displayed in Table 2.1. This table displays the lower and upper bound of property tax rates for both urban and rural real estate, before and after the reform.

Table 2.1: **Property Tax Rates: Minimum and Maximum Values**

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

Source: Portuguese tax authority.

In order to better understand the consequences of the reform, it is important to discern the differences between the legal definitions of urban and rural real estate in Portugal. According to the Portuguese property tax code (CIMI[4]) the characterization of the real estate depends on its use. It is defined as urban all residential, commercial or industrial real estate and any land already approved for construction. In contrast, it is considered a rural real estate when it does not meet the criteria of an urban real estate, namely those without any construction in place or approved, waters, plantations or when used for agricultural purposes.

2.2.2 Property Tax Reform of 2008

On July 2, 2008, the Portuguese Prime Minister unexpectedly announced a reform forcing the property tax rate upper bound to decrease from 0.5% to 0.4% for urban real estate, as we can observe above in Table 2.1. The politician promised that this would benefit *hundreds of thousands of real estate owners*.[5] This surprise announcement caused an immediate reaction by the President of the Mayor's Association (*Associação Nacional de Municípios*) who accused the central government of *easing the taxpayers' fiscal burden at the expense of someone else's money*. This representative forecasted the

[4] *Código do Imposto Municipal sobre Imóveis* approved by Decree-Law no. 287/2003, of 12 November.

[5] Headline in Expresso, an important Portuguese newspaper.

impact at 12.5% of total revenues.[6]. All this significantly amplified the media attention devoted to the reform.

The reform allows us to divide the municipalities in mainland Portugal in two groups.[7] One group includes the 94 municipalities setting a tax rate higher than 0.4% before the reform, i.e. those that were forced to decrease it. This is our treatment group and all other municipalities compose the comparison group. In appendix B.1, figure B.1 (a) depicts the spatial distribution of these two groups. For robustness, we contrast all estimation results using the full sample of mainland municipalities with a more restricted version where we focus on municipalities with a more homogeneous choice of property tax rates in 2007, the year before the shock. In order to keep intervals of similar length, we remove municipalities who choose a tax rate lower or equal to 0.3 in 2007. This means that we compare treated municipalities choosing a tax rate on the following interval (0.4; 0.5] with control municipalities in the the interval (0.3; 0.4]. This restricted sample is shown in figure B.1 (b). The 65 removed municipalities are mainly clustered in more remote areas close to the border with Spain.

Furthermore, it is also important to notice above in Table 2.1 that this reform did not alter the tax rate applied to rural real estate with no buildings, that was kept at 0.8%. This allow us to build a counter-factual analysis, supporting the main analysis if no effects of the reform are found this type of properties.

[6]See the news articles in *Público* newspaper and in *TVI* website.

[7]Peralta and Pereira dos Santos (2018) use a similar empirical strategy to study the impact of a tax revenue cut on the mayoral decision of seeking re-election.

2.3 Data and Identification Strategy

2.3.1 Data

This analysis relies on an extensive dataset at Portuguese municipal level over the period 2005–2011. The outcome variable is a logarithmic transformation of the mean value of urban real estate transactions per municipality (in real terms) obtained from Statistics Portugal (*INE*).[8] As a counter-factual exercise, we also look at the the mean value of rural real estate transactions of land with no buildings per municipality (in real terms).

For robustness, the analysis includes also a number of control variables, namely socio–demographic, economic and political characteristics, local public finance variables and electoral results. Table 2.2 presents the descriptive statistics of the two outcome variables and all control variables.

The socio–demographic variables comprise measures of the municipal population, the share of the working population with a tertiary degree, and the share of immigrants employed in the municipality. Population density is taken from *INE* and, to account for different age-structures, we also include the municipal dependency ratio. The average educational level and the share of immigrants are computed from *Quadros de Pessoal*.[9]

As for the economic characteristics, we include two proxies for municipal income level – the consumption of electricity per capita (in logs), taken from *INE*, and the unemployment rate. The latter is measured by the ratio of resident population aged between 15 and 65 years old who is enrolled as unemployed in the Portuguese Institute of Employment and Professional Training (*IEFP*). In addition, we add the share of the

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

[9]The effect of immigration on house prices was analyzed for the US (Saiz, 2007), Spain (Gonzalez and Ortega, 2013), Italy (Accetturo, Manaresi, Mocetti, and Olivieri, 2014), and the UK (Sá, 2015).

Table 2.2: **Descriptive Statistics**

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

touristic area as a proxy for touristic amenities in the municipality. This series is taken from *INE*.

Furthermore, we consider several variables on local public finance and on the provision of public goods.[10] This issue is particularly important since a number of neigh-

[10] See Gibbons and Machin (2008) for a survey on the effects of school quality, better transport, and lower crime rates on house prices.

borhood features tend to be difficult to observe by the econometrician. In this regard, we include the average national exam score (mean of the 9th-year exams of mathematics and Portuguese) in each municipality to control for the quality of public schools.[11] Other control variables related to public good provision included are: the municipal crime rate; a dummy for the existence of a court of first instance in the municipality; a dummy for the existence of a hospital in the municipality; and a dummy indicating whether there is one or more than one official clinic in the municipality. We consider also transportation infrastructure provision by adding a binary variable which takes the value one if there is at least one highway crossing a given municipality.[12] All of these variables are taken from *INE*.

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

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

[12] Audretsch, Dohse, and dos Santos (2017) highlight the importance of highways for regional development in Portugal.

2.3.2 Empirical Model

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

The nature of the property tax reform establishes pre- and post-treatment periods that allow for a quasi–experimental difference–in-differences (DD) approach.[13] Our unit of observation is the municipality. Let D_i be the dummy indicating treatment, equal to one for all municipalities in the treatment group and zero for all municipalities in the control group. That is, this dummy takes the value one if municipality i was setting a tax rate higher than 0.4% in 2007 and zero otherwise. Throughout the analysis we consider two samples that vary solely in the control municipalities included. The *Full Sample* includes all 278 mainland Portuguese municipalities and the control group includes all those not in the treatment one. In contrast, in the *Restricted Sample* the control municipalities are the ones setting a tax rate in 2007 within the the interval (0.3%; 0.4%]. By excluding municipalities setting a lower tax rate allow us a more robust analysis, as we are comparing groups with more similar fiscal preferences.

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

Inference on the average treatment effect of the reform is based on the following

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

general DD regression model:

$$y_{it} = \beta T_{it} + X'_{it}\delta + \gamma_i + \lambda_t + \epsilon_{it} \tag{2.1}$$

where y_{it} is any of the outcome variables under study, $T_{it} = (D_i \cdot d_t)$ and β_1 captures the effect of interest, and X_{it} is a vector of control variables, including the socio–economic and political characteristics previously described. Finally, γ_i are the municipality fixed effects (to control for time-invariant factors) and λ_t the time-period fixed effects.

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

$$y_{it} = \sum_{j=-m}^{q} \beta_j T_{it+j} + X'_{it}\delta + \gamma_i + \lambda_t + \epsilon_{it} \tag{2.2}$$

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

Finally, for the purpose of studying the intensity of treatment effects the regression model in Equation 2.1 is extended to encompass interaction with the imposed decrease of the tax rate. The treatment intensity effects are obtained within the following regression framework:

$$y_{it} = \beta(I_i \cdot T_{it}) + X'_{it}\delta + \gamma_i + \lambda_t + \epsilon_{it} \tag{2.3}$$

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

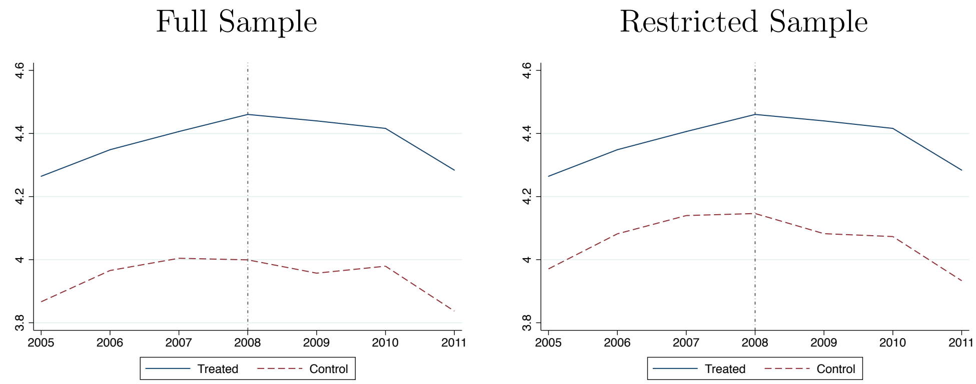
2.4 Empirical Evidence

2.4.1 The Common Trends Assumption

Internal validity of a DD estimation relies on the standard "parallel trends" condition (Meyer, 1995). For it to hold, the trend in each of the dependent variables under study must be the same for all municipalities in the absence of treatment. This assumption can be tested through different procedures. One common approach is to compare the evolution of the different outcome variables in treated and control municipalities during period of analysis (Angrist and Pischke, 2009). Figure 2.1 provides mean plots for mean value of real estate transaction of urban dwellings (in logs and real terms) for both the full and the restricted sample.

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

Figure 2.1: **Evolution of Urban Real Estate Transactions**



a) b)

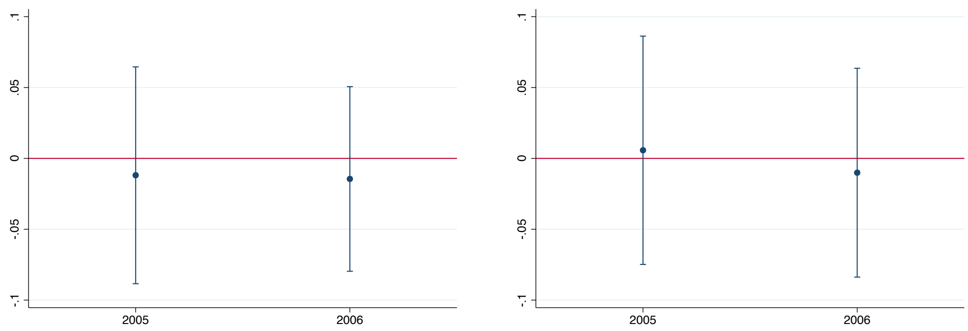
Notes: Evolution of mean values for treatment and control groups over the period 2005–2011.

A second approach consists in regressing the outcome variable on yearly dummies indicating the treatment group (Moser and Voena, 2012). This can be assessed by the model in Equation 2.2 but just focusing in the pre–treatment period as follows:

$$y_{it} = \sum_{j=-m}^{0} \beta_j T_{it+j} + X'_{it}\delta + \gamma_i + \lambda_t + \epsilon_{it} \tag{2.4}$$

where the sum allows for m "leads" or pre–treatment effects. The remaining variables are defined as before. The omitted category is 2007, the year before the announcement of the reform. Figure 2.2 presents the results, where the coefficient estimates measure how outcome variables differ between treatment and control municipalities before the reform. As suggested by the previous test, on average, treatment and control real estate markets did not differ significantly in the pre–treatment period in both samples.

Figure 2.2: **Common Trends Study**



a) b)

Notes: Coefficient estimates for the a model given by Equation 2.4 over the period 2005–2007. Caped lines indicate 95% confidence intervals.

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

2.4.2 Post-Reform Effects

We first estimate Equation 2.1 considering the mean value of urban real estate transactions as the dependent variable and vectors of control variables are gradually included: in column (1) no control variables are included in the model; in column (2) we consider population density, dependency ratio, education level and immigrants share; in column (3) we include also the unemployment rate, consumption of electricity per capita (log) and share of touristic area; in column (4) we then consider the average exam score (mean of mathematics and Portuguese), crime rate, dummy for court, dummy for hospital, dummy for more than one official clinic and dummy for highway access; finally, the political control variables are included in column (5), namely the share of left–wing seats, mayor aligned with central government and majority in the local government. All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level. The results are presented in Table 2.3 and are very similar in all specifications and for both the full and the restricted samples.

Table 2.3: **Property Tax Reform Effects on Urban Real Estate Transactions**

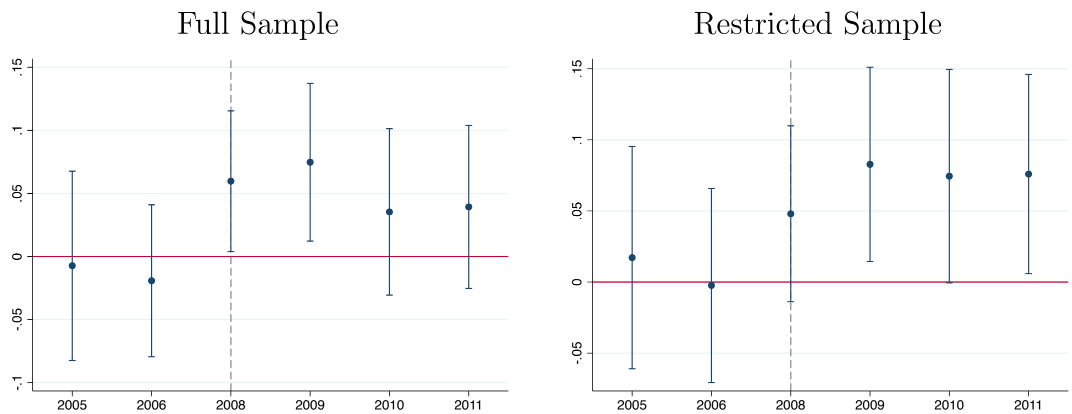
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample					
T_{it}	0.063	0.060	0.064	0.060	0.062
	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Obs.	1946	1946	1946	1946	1946
Panel B: Restricted Sample					
T_{it}	0.066	0.064	0.068	0.062	0.065
	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Obs.	1491	1491	1491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

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

We find a positive and significant effect of the reform on the mean value of total real estate transactions. Point estimates marginally decrease as more controls are included, and in the more conservative model, we obtain an effect of around 6%, which suggests that property tax capitalization is at place. The results are robust to a plethora of checks, which we explore in more detail in Section 2.5.

The annual average treatment effects in Figure 2.3 in turn, obtained from estimating Equation 2.2, are in line with the previous assessment and provide additional information as to the timing of the buyers response to property tax reform. In fact, this parametric event study suggests that the bulk of the effect is concentrated in the first year after the announcement. Additionally, as time goes by, mayors are able to better cope with the consequences of the reform by re-adapting their policies to the new environment.

Figure 2.3: **Yearly Effects of the Reform on Urban Real Estate Transactions**



Note: This figure provides plots depicting the coefficient estimates for yearly dummy variables indicating the treated group over the period, 2005–2011. Coefficients are obtained estimated the a model given by Eq. (2.2), including a vector of control variables and controlling for municipality and year fixed effects. Caped lines indicate 95% confidence intervals.

Finally, we proceed to the estimation of the treatment intensity effects by estimating Equation 2.3. Table 2.4 presents the results for both the overall and the restricted sample. We find that municipalities who experienced a higher decrease in the tax rate also experienced a higher increase in their real estate mean transaction value.

2.5 Robustness

The results are robust to a plethora of checks. Firstly, we replicate the baseline estimation in Table 2.3 but applying different clustering levels for the standard errors - at the NUTS III and regional level. Appendix B.2 presents the results that are basically identical to those presented before. Secondly, we take into account some heterogeneity in the dynamics of urban real estate transactions that might affect the magnitude of the reform effect by including population and regional trends. Moreover, we address a

Table 2.4: **Intensity Effects on Urban Real Estate Transactions**

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

Note: Results are obtained from the estimation of Equation (2.3). The dependent variable is the logarithm of the mean value of total real estate transactions (in real terms). All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

crucial problem with DD estimations related to issues of selection into treatment. For that, we replicate the estimations by excluding some critical groups of municipalities, we run the standard placebo tests and a counter-factual analysis of rural real estate transactions. Finally, we focus on the identification concern related to the local public goods provision.

2.5.1 Population and Regional Trends

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

2.5, are once again extremely similar to our baseline ones.

Table 2.5: **Robustness: Including Population and Regional Trends**

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

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

2.5.2 Selection Bias

A potential problem with DD estimations that should be taken into account relates to issues of selection bias. A key identification assumption is that municipalities are exogenously assigned to treatment and control group. Solely when this holds, one can causally identify the effect of the reform as the differential change in the outcome variables from pre- to post-treatment period between in treated and non-treated municipalities. If it is not the case and there may be intrinsic differences between the two groups, causality is then undermined.[14]

[14]For a discussion on the importance of choosing careful comparison groups to evaluate place-based policies see Neumark and Simpson (2015).

One can test for selection bias in a number of ways. One possibility is to exclude more urban municipalities. This has the advantage of providing a more homogeneous sample, as urban municipalities may differ from the rest of the country in a number of ways. Therefore, by solely focusing on less urban areas, we mitigate the risk of mistakenly reflecting confounders by rulling out possible outliers. We test this possibility restricting the full sample in four ways: i) excluding the metropolitan areas of Lisbon and Oporto, ii) excluding all coastal municipalities; iii) excluding the 18 district capitals[15]; and iv) restricting to municipalities applying even more similar property tax rates in 2007. This latter test is very demanding as it leaves us with a particularly small sample. Table 2.6 shows the results where Equation 2.1 is reestimated excluding these several groups of municipalities. In columns (7) and (8), the *Conservative Sample* only considers municipalities setting property tax rates between [0.4% and 0.5%) in 2007. From the treatment group, we remove municipalities setting the tax rate at the previous maximum whereas from the control group we only consider the municipalities setting a tax rate equal to 0.4%. All other municipalities are excluded from the estimation. Even though statistical significance decreases, point estimates are consistent with previous results.

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

Table 2.6: **Selection Bias Tests I: Restricted Samples**

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

Note: Results are obtained from the estimation of Equation (2.1), without and with the vector of control variables. The dependent variable is the logarithm of the mean value of urban real estate transactions (in real terms). The control variables included are: population density; dependency ratio; education level; immigrant share; unemployment rate; consumption of electricity per capita (log); share of touristic area; average exam score (mean of mathematics and Portuguese); crime rate; dummy for court; dummy for hospital; dummy for more than one official clinic; dummy for highway access; share of left–wing seats; mayor aligned with central government; and majority in the local government. All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

The most common approach in the context of a DD framework however, is to devise placebo tests. These tests usually consist on reestimating the baseline results relying on a placebo treatment setting in at a fake treatment year. Here, the sample is restricted to the pre-treatment period, i.e. 2005-2007, and 2006 is considered as the treatment year. As Columns (1) and (2) Table 2.7 show, estimates are close to zero and always insignificant.

As a further analysis, we focus also on the effect on the rural real estate. The property tax law provides a counterfactual since land with no buildings is subject to a different tax regime that suffered no changes in 2008. Columns (3) and (4) of Table 2.7 present the results. Unsurprisingly, we find that there is no effect on the rural buildings. This provides additional support for the validity of our results as the reform did not change the tax rate applied to this type of buildings.

Table 2.7: **Selection Bias Tests II: Placebo and Rural Transactions**

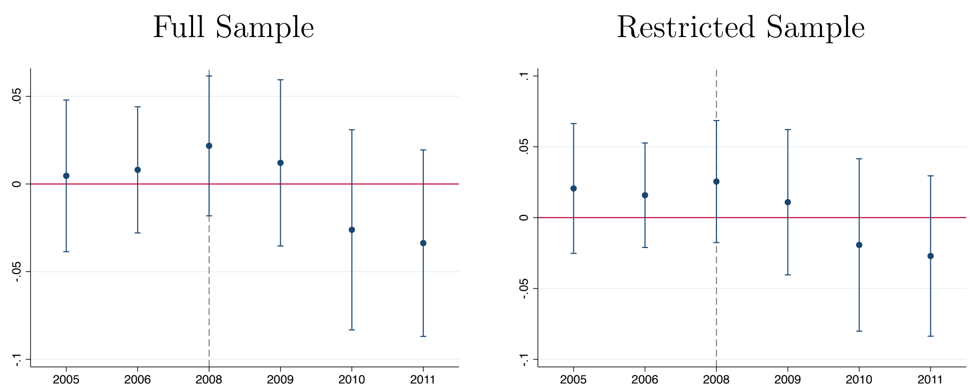
	Placebo		Rural	
	(1)	(2)	(3)	(4)
Panel A: Full Sample				
T_{it}	-0.005	0.004	-0.104	-0.041
	(0.03)	(0.03)	(0.06)*	(0.07)
Obs.	834	834	1946	1946
Panel B: Restricted Sample				
T_{it}	-0.005	0.004	-0.025	0.024
	(0.03)	(0.03)	(0.07)	(0.07)
Obs.	834	834	1491	1491
Controls:		✓		✓

Note: Results are obtained from the estimation of Equation (2.1). In columns (1) and (2) the dependent variable is the logarithm of the mean value of urban real estate transactions (in real terms). In columns (3) and (4) the dependent variable is the logarithm of the mean value of rural real estate transactions (in real terms) The control variables included are: population density; dependency ratio; education level; immigrant share; unemployment rate; consumption of electricity per capita (log); share of touristic area; average exam score (mean of mathematics and Portuguese); crime rate; dummy for court; dummy for hospital; dummy for more than one official clinic; dummy for highway access; share of left–wing seats; mayor aligned with central government; and majority in the local government. All estimates include municipality and year fixed effects. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

2.5.3 Local Public Good Provision

A critical identification concern of property tax capitalization is related with the local public good provision and so it must be also taken into account in our identification strategy. The main intuition is that if the reaction is substantially different in treated and comparison municipalities, this risks the introduction of biases to our estimates. In theory, one should note that if the property tax cut was translated into a lower supply of public goods in the comparison group, the average real estate prices should decrease – and not increase in these regions. Therefore, our estimates would represent a lower bound of the true effect of the reform. Even though we cannot totally dismiss these concerns related to unobservables, we present some suggestive evidence that these factors are not extremely important in this setting. If we run the event study design 2.2 with the log of total municipal expenditures net of interest payments as outcome variable, we do not observe statistically significant results, as shown by Figure 2.4.[16] This means that affected mayors were able to mix their tax choices, at least in the short run, to keep expenditures constant. In addition, controlling for this variable in our baseline specification leaves the point estimates basically unchallenged.[18]

Figure 2.4: **Total Expenditures (log)**



Notes: Coefficient estimates for the a model given by Eq. (2.2) over the period 2005–2011. Caped lines indicate 95% confidence intervals.

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

[16]The same results hold if we substitute this variable by the log of capital expenditures (in real terms). Both data series can be retrieved from the General Directorate for Local Authority's (*Direcção–Geral das Autarquias Locais, DGAL*) website.[17].

[18]We decided not to include this control variable in our analysis due to endogeneity concerns. Results adding this covariate are available from the authors upon request.

2.6 Conclusion

In this paper, we attempt to determine if property tax differentials on real estates are capitalized. The scarce empirical evidence has reported mixed results. The aftermath of a reform introducing a lower maximum tax rate for urban properties is the perfect laboratory to study whether there is an effect on housing prices. We take advantage of this reform to establish causality using a rich dataset on the universe of mainland Portuguese municipalities. We believe that this strategy allows for credible inference upon the causal effects of this reform. Since the present paper studies the impact of a nationwide policy, rather than a number of local jurisdictions, we also believe that it presents a higher degree of external validity.

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

Chapter 3

Term Limits: A new political scene or business as usual? [1]

3.1 Introduction

The discussion of the introduction of term limits dates back to early democratic societies. In one of the earliest definitions of democracy, Aristotle listed as a key characteristic that "no man should hold the same office twice."[2] Accordingly, in Ancient Greece one-year limits were imposed on some of the officials elected by random lottery. In fact, this issue has occupied a prominent place in the political debate of modern democracies since the founding fathers, as expressed by Thomas Jefferson *"to prevent every danger which might arise to American freedom by continuing too long in office the members of the Continental Congress"*.

In the academic context, this debate has contributed to a rich and extensive literature with arguments both for and against the introduction of term limits, as it eliminates incumbency advantage and removes long-tenure incumbents from office. Advocates for

[1] This is a joint work with Mariana Lopes da Fonseca

[2] THE POLITICS OF ARISTOTLE 258 (Ernest Barker trans., Oxford University Press 1958).

term limits argue that it allows the entry of potentially better politicians and eliminates policy biases in favor of specific interest groups represented by long-serving incumbents. On the other hand, term-limits impose a constraint on voters' choice of representatives, whose long tenure may be due to higher quality; or it may be that voters prefer a representative with longer political longevity and stronger influence, who is able to more successfully broker resources and legislation to their own benefit. By this token, the introduction of term limits poses a relevant empirical question. For which evidence is still scarce, mainly due to data limitations (Dal Bó et al., 2017).

In this paper we study the selection effects of term limits and our contribution is twofold: we construct an extensive and unique dataset on Portuguese mayors' personal characteristics and we take advantage of a recent reform introducing mayoral term limits in Portugal to identify its causal impact on political selection.

Term-limits were introduced in Portuguese local governments in 2006, and the first effects came into play solely at the 2013 local elections. The law limited to three consecutive terms in the same municipality. Nevertheless, at the subsequent local elections of 2009 all incumbent mayors were still able to rerun. Hence, as of 2013, many Portuguese mayors were in office for twenty or thirty years, and two of them since the first local elections in 1976. In total, 150 mayors from the 278 mainland municipalities were forced to leave office, creating the first exogenously determined open-seat elections in Portugal. We take advantage of this reform as quasi-natural experiment to examine its political selection effects. Our identification strategy relies on a difference-in-differences approach estimating how these mayors' personal characteristics differ on average between municipalities with re-eligible and term-limited incumbents.

For this purpose, we constructed an extensive and unique dataset on personal characteristics of Portuguese mayors at the past four local elections: 2001, 2005, 2009 and 2013 - a total of 586 individuals in 278 municipalities. Some of these personal characteristics are publicly available, but self-reported by the elected politicians after taking office and the overall data is rather incomplete and imprecise. Thus, we complemented it manually using information found online or contacting directly the municipal council. Taking all the available information, we compiled the following variables based on mayors' characteristics: their gender, the age when they took office for the first time, the level of education, the area of education and the occupation before taking office.

Our baseline results show that municipalities affected by the reform elected politicians, on average, older (around four years) and with a past political career. The results are robust to several falsification tests, in particular concerning any potential anticipation effects at the 2009 local election. This might suggest that before the implementation of term limits, being a mayor in Portugal could be considered as a career path, which one would stat at a young age and with no previous political experience. Nevertheless, in order to corroborate this idea we need explore further the implications of the reform, namely by looking at in detail at the type and number of years of political experience. We are able to perform this analysis, since we have also data on all elected councilmen.

The remainder of this chapter is organized as follows. In section 3.2, we review the related literature. Section 3.3 details the institutional background regarding the Portuguese local politics and the electoral reform under analysis, as well as Portuguese mayors' characteristics. Section 3.4 presents our data, the empirical identification and the main results. In section 3.5, we discuss the internal validity of the results obtained. Finally, section 3.6 concludes.

3.2 Related Literature

In democracies, frequent elections can be viewed as the main tool voters have to hold politicians accountable. In that sense, term limits may seem paradoxical, as they reduce voters' ability to hold politicians liable for their policy choices. This is the base argument of agency literature (e.g., Barro (1973), Ferejohn (1986)) that emphasizes the disciplining role of elections and discusses the opportunistic behavior in which term-limited incumbents engage during their last period, once reelection incentives disappear. More recently, this argument has been contested by another strand of literature which regards elections as distortionary under myopic or career concerned officeholders (Morris (2001), Ely and Välimäki (2003) and Maskin and Tirole (2004)). This leads to adverse selection, justifying the introduction of term limits (Chari, Jones, and Marimon, 1997). Furthermore, it also claimed that under a binding term limit incumbents set policies that "truthfully" reflect their preferences and interests allowing a better screening by the voters (Glazer and Wattenberg (1996), Lopes da Fonseca (2019), Smart and Sturm (2013)).

On another note, a large body of theoretical literature emphasizes the importance of term limits in removing the barriers to entry created by incumbency advantage and hence potentially improving political selection. That is, term limits eliminate incumbency advantage periodically, increasing probability of entering for new politicians - from different parties, coalitions, or political sectors - who would be less likely to enter running against an incumbent and who could be also more productive. This way, rotation in power increases, potentially eliminating biased policies in favor of the coalitions represented by senior incumbents (Tabarrok (1996), Glaeser (1997) and Cain, Hanley, and Kousser (2006)).

Given these theoretical predictions, the empirical assessment of the consequences of term limits shall be studied in two dimensions. On the one hand, analyzing the reaction of incumbents when facing a term limit and whether elections create a disciplinary/distortionary effect. Empirical evidence – mostly based on the U.S. experience and on fiscal outcomes – appears to support the disciplinary effect of elections (Besley and Case, 1995, 2003, Crain and Oakley, 1995, Crain and Tollison, 1993). However, there are a couple of exceptions: List and Sturm (2006) provide significant evidence of distorting policy choices for a sample of U.S. governors between 1970 and 2000.

In addition, it might be also identified whether term limits, by creating more rotation in power, lead to the entry and selection of better politicians. Fowler (1992) and Grofman and Sutherland (1996) argue that term limits may increase the reelection rates of incumbents because high–quality challengers postpone running until the seat becomes open by mandatory rotation. Cain et al. (2006) find that while the introduction of term limits successfully increases the turnover of individual incumbents and the fraction of contested races, it fails to make races more competitive or increase party turnover. Moreover, seats held by incumbents are less likely to be contested and that incumbents tend to face challengers with less previous political experience. Querubin (2016) studies a reform in the Philippines finds no increase in the turnover of incumbent families in congress – distorting behavior of incumbents and challengers.

More recently, and in the Portuguese context, Lopes da Fonseca (2019) explores the same reform as we focus here but looks at another dimension of the consequences of term limits - its disciplinary or distortionary effects - by studying its the short-term impact on local policy choices. She finds that lame ducks pursue more conservative fiscal policies and this effect is primarily reflecting the behavior of right-wing politicians.[3]

[3] Also in the Portuguese context, Peralta and Pereira dos Santos (2019) look at political selection effects from the property tax reform studied in Chapter 2.

3.3 Institutional Background

3.3.1 Portuguese Local Politics

In Portugal, there are three levels of governance: central, municipal, and civil parishes. There are in total 308 municipalities, from which 278 mainland municipalities. Portuguese municipalities have control over their spending - subject to prevailing laws and regulations - but they do not score very high in terms of local revenue autonomy (OECD, 1999). The reason for the reduced local autonomy is twofold: high reliance on transfers from the central government and reduced freedom to set local tax rates (on centrally set tax bases). Local revenue sources consist mainly of local taxes, transfers from the central and regional governments, and transfers from the EU. There are different local taxes, namely an indirect tax on the transfer of the real estate, the local property tax, a variable tax share of the central government personal income tax, and a municipal surcharge on corporate income tax.

The property tax is the main fiscal tool of Portuguese local governments: it is their primary source of own revenue and over which they have some discretionary power over the property tax rate. Every year, property tax rates are decided at the municipal assembly - within an interval typical to every municipality and defined by the central Government.

From 1926 to 1974, Portugal was governed by an authoritarian regime. In 1974, democracy was restored, and regular elections have been held: presidential elections every five years, and parliamentary and local elections every four years. At the municipal level, voters decide their representatives in the municipal council (executive branch) and the municipal assembly (legislative branch). The head candidate of the most voted list to the municipal council is elected as mayor, the local top chief executive - the most prestigious and influential political position at the local level.

Until 2013, Portuguese local politicians did not face term limits. This means that they could run indefinitely for office. This meant that they could run indefinitely for office, which happened in fact and with high success rate as illustrated in Table 3.1. This also created room for considerable variation in government longevity, as displayed in Table 3.2. It shows the distribution of mayors by the number of terms in office in the three previous local elections before the introduction of the term limits. For example, in 2009, we can observe that two politicians were reelected for their tenth term, meaning that they been kept in office since the first local elections held in 1976.

Table 3.1: Rerunning & Success Rates.

		1997	2001	2005	2009
Panel A: Officeholder					
Rerun	N	218	227	233	238
	%	79	83	84	86
Win	N	186	188	211	205
	%	85	83	91	86
Panel B: Party					
Win	N	219	211	234	230
	%	80	76	84	83
Obs.		275	278	278	278

Source: Lopes da Fonseca (2017).

3.3.2 Term Limits Reform

The draft Law on the implementation of term limits for Portuguese mayors was discussed on July 25th 2005 and approved in Parliament, but only entered into force on January 1st 2006. [4] The law sets a three consecutive terms limit for local politicians, after which they are not able to rerun for the mayoral position in the same jurisdiction. Nevertheless, it also established that the law was only effective at the 2013 local elections, implying that at the local elections of 2009 all incumbent mayors could still

[4]Law no. 49/2005 from August 29th 2005.

Table 3.2: Mayor's Number of Terms in office

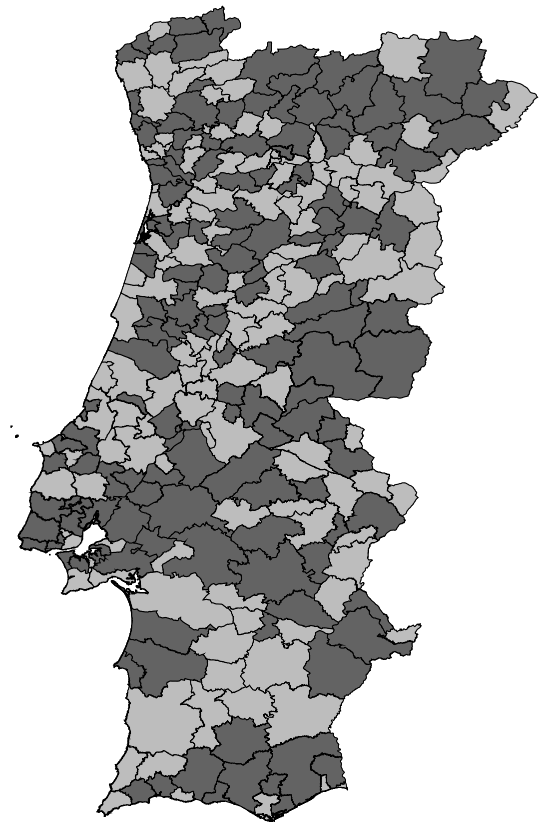
Term	2001	2005	2009
1	88	66	74
2	75	75	54
3	52	56	62
4	26	34	34
5	16	20	24
6	8	13	12
7	8	5	9
8	5	4	3
9		5	4
10			2

Source: Own calculations, based on data provided by the Portuguese national elections commission (CNE).

rerun. In other words, term limits were only first binding for incumbent mayors serving their at least third consecutive term in the 2013 local elections - the first exogenously determined open-seat elections for the municipal council in Portugal. In total, 150 mayors were in this situation out of the 278 mainland municipalities.

The timeline and design of the reform creates an interesting quasi-natural experiment, as it is possible to rely on both between- and within-municipality variation to identify the impact of mayoral term limits on political selection. Figure 3.1 shows the spatial distribution of Portuguese mainland municipalities affected and non-affected by the reform: municipalities that elected a lame duck in 2009 are represented in dark gray.

Figure 3.1: Spatial Distribution of Term Limited Municipalities



Source: Lopes da Fonseca (2019).

3.3.3 Portuguese Mayors Overview

This analysis focus on the 278 Portuguese municipalities and relies on an original and extensive dataset on mayors' personal characteristics at the past four local elections: 2001, 2005, 2009 and 2013 - a total of 260 individuals.[5] 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 both on electoral results – the number of votes and seats at the party level per municipality –

[5]Our dataset comprises data since the first local elections in 1976. However, in 2001 other electoral reform took place, allowing independent lists to run. We focus then only on the subsequent elections in order to abstract from potential selection bias effects.

and some mayor's characteristics – name, party, age, education, occupation and place of birth. These personal characteristics are self-reported by the elected politicians after taking office. However, it is rather incomplete and imprecise. Thus, we complemented it manually using information found online or contacting directly the municipal council.

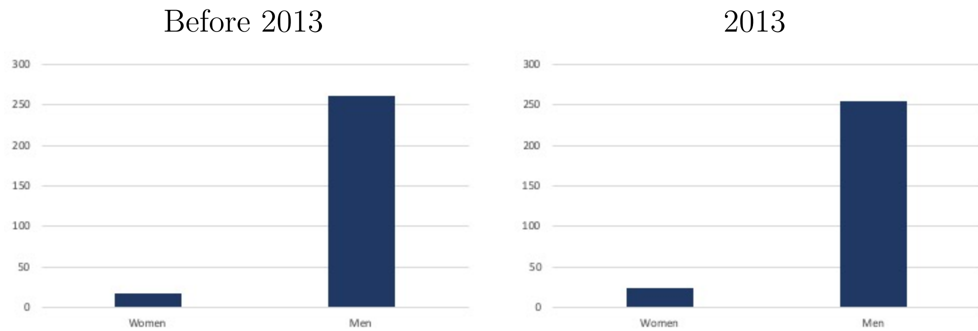
Taking all the available information, we compiled the following variables based on mayors' characteristics: a dummy if women; the age when they took office for the first time; the level of education an index from 1 to 6 (1 being four years of schooling and 6 having a doctoral degree); the area of education, namely medicine, law, economics, engineering, arts, etc.; and the occupation before taking office, namely lawyer, medical doctor, school teacher, architect, etc.

Firstly, with respect to the number of women taking office between 2001 and 2013, Table 3.3 and Figure 3.2 describe the data and its distribution, respectively. We observe that until 2013, on average, solely around 6% of local incumbents were women - around 17 out of the 278 municipalities each year - and this share has not change significantly after the term limit reform.

Table 3.3: Summary Statistics - Women in Office

Variable	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
2001	278	0.054	0.226	0	1
2005	278	0.058	0.233	0	1
2009	278	0.076	0.265	0	1
2013	278	0.083	0.276	0	1

Figure 3.2: Distribution - Women in Office



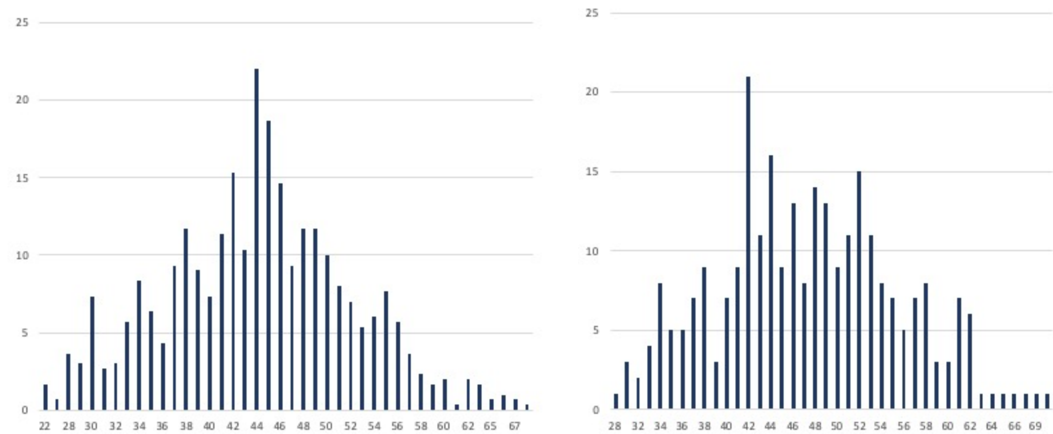
Regarding the age local politicians take office for the first time, we were able to collect this information for almost all elected mayors, with exception of three newcomers in 2013, and Table 3.4 and Figure 3.3 summarize the data. Before the reform, incumbents took office with approximately 44 years old. However, the mean and median age has increased after the reform, as well as its minimum and maximum values, shifting the distribution to the right.

Table 3.4: Summary Statistics - Age at the First Term

Variable	Obs	Mean	Std. Dev.	Min	Median	Max
2001	278	43.583	8.185	22	44	67
2005	278	43.896	8.272	22	44	72
2009	278	44.626	7.855	22	45	66
2013	275	47.294	8.264	28	47	70

Figure 3.3: Distribution - Age at the First Term

Before 2013 | 2013

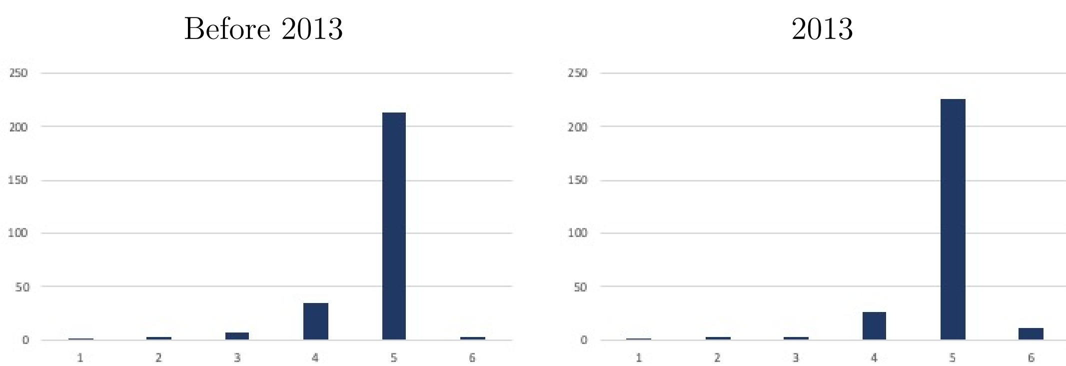


One important characteristic is the education level, the most commonly used in the political selection literature as a measure of incumbent's quality. We compiled this information for the most part of Portuguese mayors and we constructed an index from 1 to 6: level 1 for four years of education (mandatory until 1966); level 2 for six years of education (mandatory until 1986); level 3 for nine years of education (mandatory until 2009); level 4 for twelve years of education; level 5 for tertiary education, including a bachelor degree, a master degree or an MBA; and level 6 for a doctoral degree. Table 3.5 and Figure 3.4 reports this information for the winning politicians at four local electoral years. The majority of the incumbents have completed tertiary education and the overall situation was maintained after the reform.

Table 3.5: Summary Statistics - Education Level

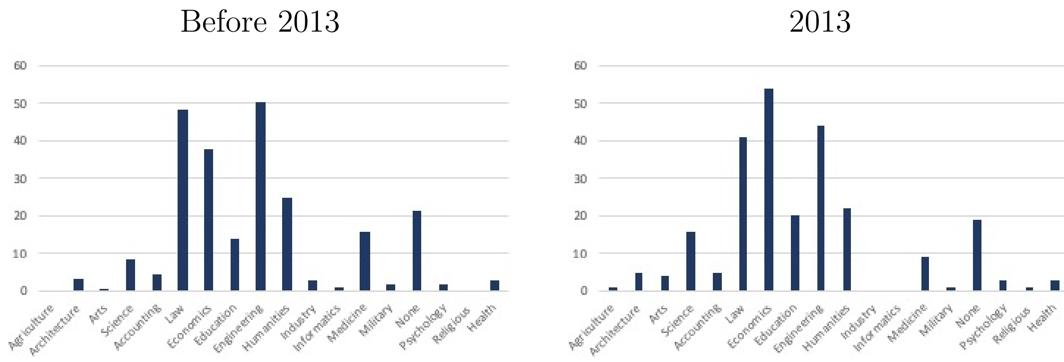
Variable	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Median**	**Max**
2001	241	4.772	0.600	1	5	6
2005	249	4.791	0.613	1	5	6
2009	258	4.841	0.566	1	5	6
2013	264	4.882	0.570	1	5	6

Figure 3.4: Distribution - Education Level



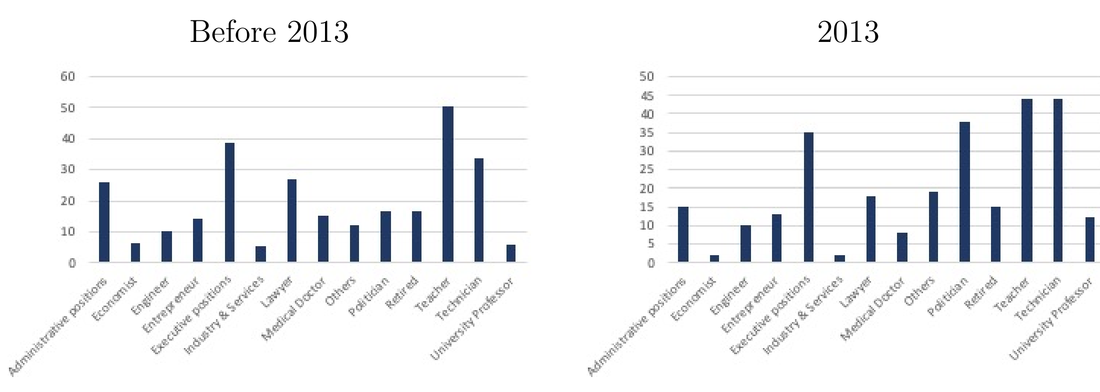
In addiction to the level education, we also collected information regarding the field of education. This gives us further knowledge about the pool of politicians and their "quality", as some of the fields are more complex and given the lower vacancy rates of some university degrees in Portugal, namely Law and Medicine. This data is not publicly available and to the best of our knowledge we were the first to collect it, we were able to find it for the majority of politicians. Figure 3.5 describes the distribution of field of Education of mayors in office during 2001 and 2013. We can observe that main areas of specialization are Law, Engineering and Economics, and the overall composition has not change significantly after the term limits implementation.

Figure 3.5: Distribution - Field of Education



Finally, we also assemble details regarding mayors' professional occupation before taking office. For that, we contrasted the publicly available data - self-reported - with the biographic information found online or provided by the city council. We organized this data into fourteen categories as shown by Figure 3.6. The most prominent former professions are executive positions (chief executives, administrators or directors of a company), school teachers or technicians. The main striking difference after the reform is the fact that significantly more officeholders had followed a career path related to local politics before taking office, either they had already been deputies at the municipal council for several years or were party officials.

Figure 3.6: Distribution - Previous Professional Occupation



3.4 Empirical Analysis

3.4.1 Data

The empirical analysis relies on an extensive dataset at Portuguese municipal level over four local elections: 2001, 2005, 2009 and 2013. We aim to identify the causal impact of biding term limits on political selection. For that purpose, we take our original dataset described in the previous section and we construct nine outcome variables about mayors' personal characteristics: a dummy indicating woman as local incumbent; the age when they took office for the first time; a dummy indicating tertiary education, i.e. if the incumbent has obtained an undergraduate or graduate degree, including a doctoral degree; dummies identifying mayors' fields of education - Law, Medicine or Engineering; and dummies indicating the professional occupation before taking office - executive positions, politician or retired. Panel A of Table 3.6 summarizes these variables, where for each one we only include municipalities with available information over the four electoral years. For robustness, we replicate the analysis including only municipalities for which we have no missing data, i.e. comparing the same local incumbents in all outcome variables.

Table 3.6: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A: Outcome Variables					
Women (0/1)	1112	0.067	0.251	0	1
Age (No.)	1100	44.926	8.240	22	72
Tertiary Education (0/1)	976	0.837	0.369	0	1
Medicine Degree (0/1)	808	0.058	0.234	0	1
Law Degree (0/1)	808	0.200	0.401	0	1
Engineering Degree (0/1)	808	0.207	0.405	0	1
Executive Positions (0/1)	1100	0.137	0.344	0	1
Politician (0/1)	1100	0.079	0.270	0	1
Retired (0/1)	1100	0.059	0.236	0	1
Panel B: Control Variables					
Population (1,000)	1112	35.873	57.498	1.768	563.149
Young Population (%)	1112	14.019	2.606	5.086	22.548
Old Population (%)	1112	22.665	6.649	8.599	44.275
Unemployment Rate (%)	1112	7.037	2.812	1.517	18.295
Electricity Cons. per capita	1112	4120.5	4938.6	1114.3	79965.1
Candidates (No.)	1112	4.268	1.116	2	10
Turnout (%)	1112	63.568	8.480	37.77	82.35
Mayor Before (0/1)	1112	0.038	0.191	0	1

The analysis also includes a number of control variables, namely socioeconomic and political characteristics and Panel B of Table 3.6 summarizes them. The socioeconomic variables comprise measures of municipal population size, municipal unemployment, a measure for municipal economic activity proxied . Municipal population size coincides with the resident population per municipality series from Statistics Portugal *(INE)*, and it is also included the share of this population aged below 15 years old together with share of population aged more the than 65 years old. Municipal unemployment is measured by the ratio of resident population aged between 15 and 65 years old who is enrolled as unemployed in Portuguese Institute of Employment and Professional Training (*IEFP*). This latter series is obtained from the Database of Contemporary Portugal (*PORDATA*). Lastly, the electricity consumption series is also publicly available at Statistics Portugal and weighted then by resident population per municipality.

The set of municipality's political characteristics is constructed based on data obtained from *SGMAI*: number of candidates running for office, the turnout at the current local election and a dummy indicating whether the incumbent politician had been in office before, either in other municipality or if she had stepped down/lost a reelection and came back.

3.4.2 Identification Strategy

The introduction of binding term limits implied exogenous variation in eligibility for office, which in turn might create differences in composition of elected local politicians. We test this hypothesis empirically, using a quasi-experimental difference-in-differences approach. We use data of Portuguese municipalities over four local elections and we focus on the impact in first wave of term-limited elections, in 2013. These were held in municipalities which in 2009 re-elected incumbents to serve at least a third mandate, so the 2009 local election results implicitly assign treatment. If treated and control municipalities are comparable, it is possible to capture the variation in eligibility and use it to identify the causal impact of term limits on political selection.

We estimate then differences in mayors' characteristics both between treatment and control municipalities and from pre- to post-treatment period. Formally, let T_i be the dummy that indicates treatment, equal to one if municipality i elected a lame duck in 2009, and zero otherwise. Treatment assignment occurs at t_0, which corresponds to the 2009 local elections. The post-treatment period corresponds to the 2013 elections. Accordingly, let d_t be a dummy that switches to one in 2013. Inference on the average treatment effect of term limits on mayors' characteristics is based on the following general differences-in-differences regression model:

$$Y_{it} = \lambda_i + \lambda_t + \delta(T_i \cdot d_t) + X'_{it}\beta + \epsilon_{it} \tag{3.1}$$

where Y_{it} is any of the outcome variables, $D_{it} = T_i \cdot d_t$ indicates a binding term limit and δ parameter measures the average treatment effect of term limits on the different outcome variables. X'_{it} it is the vector of socioeconomic and political control variables described in the next section. The model is fully identified by including municipality and electoral year fixed effects, λ_i and λ_t. For robustness, in more conservative versions of the baseline model, we add district trends, $\lambda_d \cdot t$ to control for district-specific trends, and district-year fixed effects, λ_{dt}, to allow for variations in unobservable district-specific variables over time.[6]

3.4.3 Results

Tables 3.7, 3.8 and 3.9 display the difference-in-difference results, where average treatment effects from different regression models are presented in each cells. All estimations include municipality and year fixed effects. The outcome variable and corresponding number of observations are indicated in the first column. Model (1) corresponds to the baseline estimation of of Equation (3.1). Models (2) and (3) include also district-specific

[6] Although districts are not an official local administrative unit, they have existed since 1835 and cover similar municipalities.

electoral year trends and district-electoral year fixed effects, respectively. In models (4), (5) and (6) we replicate each of these three models, respectively, including the vector of control variables described in Table 3.6. In general, for all outcome variables, the magnitude and significance of the coefficient estimates are consistent across all model, suggesting their robustness to omitted variable bias (Altonji, Elder, and Taber, 2005).

Firstly, Table 3.7 presents the estimation results for the more comprehensive mayors' personal characteristics: a dummy indicating woman as local incumbent (*Women*), the age when they took office for the first time (*Age*) and a dummy indicating tertiary education (*Tertiary Education*), taking the value one if the incumbent has obtained an undergraduate or graduate degree, including a doctoral degree. We obtained only statistically significant results coefficients for *Age* and robust for all model specifications. On average, in 2013, term-limited municipalities local elected older politicians with approximately four more years old. In the contrary, the introduction term limits has not a statistically significant impact in the share of elected women or mayors who had completed tertiary education.

Table 3.7: **Treatment Effects**

	(1)	(2)	(3)	(4)	(5)	(6)
Women	-0.044	-0.046	-0.040	-0.052	-0.051	-0.047
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	1112	1112	1112	1112	1112	1112
Age	3.944***	4.096***	4.038***	4.129***	4.085***	4.078***
	(1.06)	(1.04)	(1.10)	(1.07)	(1.05)	(1.10)
Obs.	1100	1100	1100	1100	1100	1100
Tertiary Education	-0.065	-0.060	-0.068	-0.077	-0.066	-0.071
	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)
Obs.	976	976	976	976	976	976
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

In turn, Table 3.8 shows the impact of term limits on the composition of the pool of elected politicians. We observe statistically significant impact solely on the share of mayors with a Law Degree, robust to all model specifications. On average, in 2013, term-limited municipalities local elected less 12% of politicians with this degree than the comparison group. However, no effect was found on the share of elected mayors with a Medicine or Engineering Degrees.

Table 3.8: **Treatment Effects: Education Area**

	(1)	(2)	(3)	(4)	(5)	(6)
Medicine Degree	0.014	0.009	0.008	0.006	-0.002	-0.004
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	808	808	808	808	808	808
Law Degree	-0.101*	-0.116**	-0.123**	-0.111*	-0.120**	-0.126**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Obs.	808	808	808	808	808	808
Engineering Degree	-0.006	0.005	0.008	-0.009	-0.003	-0.000
	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	(0.07)
Obs.	808	808	808	808	808	808
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Finally, Table 3.9 displays the differences-in-differences estimation results related to the previous professional occupation of local incumbents. Interestingly, we find a statistically significant impact only on the share of mayors with a previous political career, which is robust to all model specifications. After the reform, in term-limited municipalities local elected approximately 10% more incumbents connected to local politics. No effect was found on the share of elected mayors retired or that occupied executive positions.

In summary, our baseline results suggest that before the implementation of term limits, being a mayor in Portugal could be considered as a career path, which one would stat at a young age and with no previous political experience.

Table 3.9: **Treatment Effects: Previous Professional Occupation**

	(1)	(2)	(3)	(4)	(5)	(6)
Executive Positions	-0.005	-0.006	-0.012	0.004	-0.006	-0.011
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Obs.	1100	1100	1100	1100	1100	1100
Politician	0.107***	0.106***	0.099**	0.101**	0.102**	0.096**
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	1100	1100	1100	1100	1100	1100
Retired	-0.036	-0.038	-0.027	-0.029	-0.035	-0.022
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Obs.	1100	1100	1100	1100	1100	1100
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

3.5 Internal Validity

3.5.1 Common Trends

The main identification assumption relies on the common trends condition (Meyer, 1995). For it to hold, in the absence of treatment the trend in each outcome variables should be the same for all municipalities. The standard approach to test this assumption consists in restricting the sample to the pre-treatment period and regressing the dependent variables on yearly dummies indicating the treatment group (Moser and Voena, 2012). This can be assessed by estimating the following model:

$$Y_{it} = \lambda_i + \lambda_t + \sum_{j=1}^{q} \delta D_{i,t+j} + \epsilon_{it} \tag{3.2}$$

where the sum allows for q pre-treatment effects. The remaining variables are defined as before. The estimation includes the electoral years: 2001, 2005 and 2009. The baseline year is 2005, as it was electoral year before the law entered into force. For compactness, Figure 3.7 shows the results from the estimation of Equation 3.2 for each of the studied outcome variables in graph format. The y-axis indicates the outcome variables and the x-axis the years. The horizontal line at zero represents the control group.

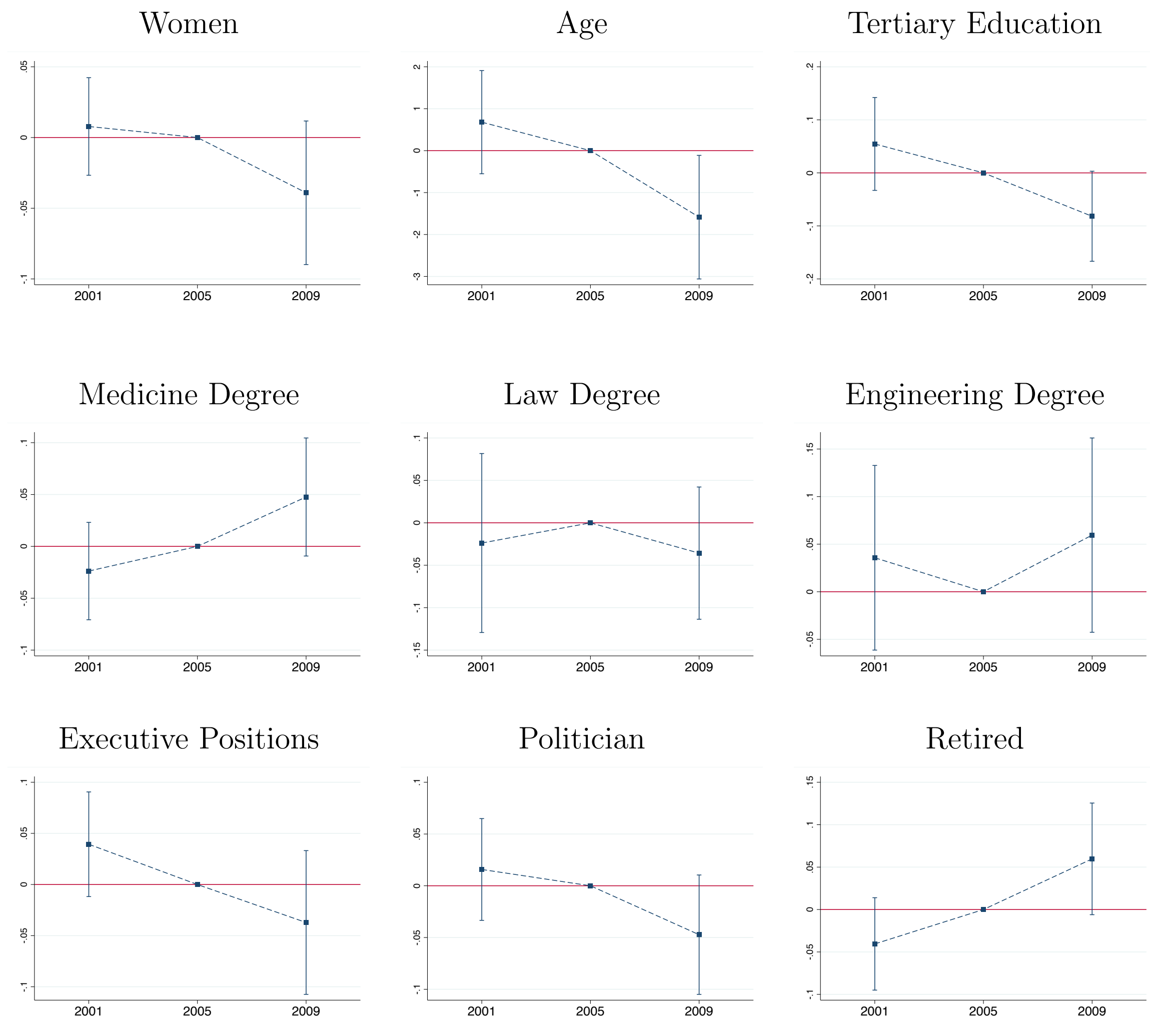


Figure 3.7: **Year Effects** Plots depict the coefficients estimates obtained from regressing Equation 3.2 on each outcome variable during the pre-treatment local elections: 2001, 2005 and 2009. The baseline year is 2005. The y-axis lists the outcome variables. The x-axis indicates the years. Capped lines indicate 95 % confidence intervals

In general, the plots show insignificant coefficient estimates for the pre-treatment period, providing support for the common trends hypothesis. Robustness results with control variables are presented in Appendix C.1.1.

3.5.2 Controlling for Anticipation Effects

Given the time-line of the reform, it is important to take to account possible anticipation effects that might be biasing the results. As previously described, the law that introduced mayoral term limits entered into force in the beginning of 2006 but it was only binding in 2013, allowing all incumbent mayors one last chance at reelection in the 2009 local elections. Therefore, this might have political selection effects already at these elections, before term limits even became effective. It might distort incentives for mayors to rerun in 2009 or might make a certain type of politician leave the local political career.

In this section, we test for anticipation effects hypothesis in several ways. Firstly, we reestimate baseline differences-in-differences model of Equation 3.1, excluding the 2009 local elections. Tables in Appendix C.1.2 present the results. All estimates are in line with the baseline results, both in terms of the magnitude of the coefficients and its significance, dismissing any possible concerns of anticipation effects bias.

Moreover, we extended the yearly effects model of 3.2 including the treatment year as follows:

$$Y_{it} = \lambda_i + \lambda_t + \sum_{j=1}^{q} \delta D_{i,t+j} + \delta_{2013} D_{i,2013} + \epsilon_{it} \tag{3.3}$$

where the sum allows for q pre-treatment effects and δ_{2013} gives the average treatment effects. In first step, we estimate the model including the four electoral years: 2001, 2005, 2009 and 2013. The baseline year is 2005, as before. In a second step, we re-estimate Equation 3.3 but now excluding the electoral year of 2009. Figures C.2 and C.3 in Appendix C display the respective results of each step in a graph format. Once again, all estimates are in line with the baseline results. Once again, all estimates are in line with the baseline results dismissing further concerns of anticipation effects bias.

Finally, we test for possible anticipation effects by restring the sample including solely municipalities where incumbent decided to rerun in 2009. This allows us to control for any possible distortionary impacts in rerunning incentives at these local elections. In the local elections of 2009, 41 incumbents decided not to rerun and so we re-estimate the baseline model excluding these municipalities over the local elections of 2001, 2005, 2009 and 2013. Tables C.4, C.5 and C.6 in Appendix C present the results. All estimates are in line with the baseline results, both in terms of the magnitude of the coefficients and its significance, dismissing any further concerns of anticipation effects bias.

3.5.3 Selection Bias

A potential problem with DD estimations that should be taken into account relates to issues of selection bias. A key identification assumption is that municipalities are exogenously assigned to treatment. Solely when this holds, one can causally identify the effect of the reform as the differential change in the outcome variables from pre- to post-treatment period. If it is not the case causality is then undermined.[7]

One can test for selection bias in a number of ways. In fact, the evidence of existence of common trends found in Figure 3.7 suggests treatment assignment to be exogenous. This is validated even further by the fact the magnitude and significance of the baseline coefficient estimates remain consistent, even after the introduction of a vector of control variables.

Two common approaches to test for selection bias in the context of a DD framework is to devise placebo and balance tests. These tests usually consist on reestimating the baseline results relying on a placebo treatment setting it at a fake treatment year (de Jong, Lindeboom, and van der Klaauw, 2011). Here, the sample is restricted to the pre-treatment period, i.e. focusing only on the local elections of 2001 and 2005, and 2005 is considered as the treatment year. We exclude 2009 since, by then, the treatment assignment had already occurred. As Tables C.7, C.8 and C.9 in Appendix C.1.3

[7]For a discussion on the importance of choosing careful comparison groups to evaluate place-based policies see Neumark and Simpson (2015).

show the placebo test for the baseline results reestimating Equation 3.1. As before, each cell provides the average treatment effect from a different regression, and the first column indicates the corresponding outcome variable. All estimates are close to zero and insignificant dismissing any concerns of a possible selection bias.

Moreover, balance tests offer an alternative mechanism to test whether treatment and control municipalities differ on observable characteristics (Pei, Pischke, and Schwandt, 2017). Here, we regress socioeconomic and political variables on the dummy variable T_i indicating treatment in order to identify fundamental differences between treatment and control municipalities. We restrict the time frame only to 2001 so as to include only the local elections before the law came into force. Table 3.10 displays the results.

Table 3.10: Balance Tests - 2001

Variable	**Coefficient**	**Std. Error**	**P-value**
Population (1,000)	9.485	6.916	0.171
Young Population (%)	0.081	0.305	0.790
Old Population (%)	-0.528	0.815	0.518
Unemployment Rate (%)	0.063	0.289	0.829
Electricity Cons. per capita	109.485	526.349	0.835
Candidates (No.)	0.050	0.127	0.692
Turnout (%)	-3.333	0.876	0.000
Mayor Before (0/1)	-0.011	0.019	0.559

Note: Balance tests obtained from regressing each variable on the left on a dummy indicating treatment in 2001. Standard errors are robust to heteroscedasticity and clustered at the municipality level.

Coefficient estimates show significant differences between treatment and control municipalities for turnout. Treatment municipalities on average a lower turnout rate. All other variables suggest that the treatment and control municipalities are comparable.

Furthermore, an additional concern that might be creating a sort of selection bias is the fact that there some missing information and for each outcome variable some municipalities have more data available than other for different outcome variables. Therefore, in order to obtain a comparable dataset, we only include the municipalities that have no missing data with respect to any dependent variable - 199 out of the 278 mainland municipalities. We re-estimate the baseline model and Tables of Appendix C.1.4 present the results. All estimates in line with baseline results, dismissing any concerns of a possible selection bias from missing information.

3.6 Conclusion

In this paper, we analyze the political selection effects of introducing mayoral term limits in Portugal. For that, we compiled an original dataset on personal characteristics of Portuguese mayors at the past four local elections: 2001, 2005, 2009 and 2013 - a total of 586 individuals from the 278 mainland municipalities. Our identification strategy relies on a difference-in-differences approach estimating how these mayors' personal characteristics differ on average between municipalities with re-eligible and term-limited incumbents.

The baseline results show that municipalities affected by the reform elected politicians, on average, older (around four years) and with a past political career. The results are robust to several falsification tests, in particular concerning any potential anticipation effects at the 2009 local election. This might suggest that before the implementation of term limits, being a mayor in Portugal could be considered as a career path, which one would stat at a young age and with no previous political experience. Nevertheless, in order to corroborate this idea we need explore further the implications of the reform, namely by looking at in detail at the type and number of years of political experience. We are able to perform this analysis, since we have also data on all elected councilmen.

Conclusion

This thesis explores three reforms implemented in Portugal to obtain causal inference about the manifold impacts of the rules that shape local government behavior. In the first chapter, I study the political consequences of a mass reappraisal reform. For that, I focus on the financial assistance program to Portugal in 2011, which put in place an urgent wave of property reassessments in all municipalities, and the electoral response at the subsequent parliamentary elections in 2015. I use an original database for all 278 mainland Portuguese municipalities three electoral years: 2009, 2011 and 2015. The baseline results show that the intensity of reassessments had not a statistically significant impact on any vote share outcome. This might be explained by the reduction of salience of the reform due to tax cap policy that smoothed out the increment in tax liabilities. Heterogeneous effects based on the real effect of the reassessment, in turn, indicate that in municipalities where, on average, tax liabilities augmented more, voters punished more the incumbent government party, the right-wing coalition.

In the second chapter, we want to determine whether property tax differentials on real estates are capitalized. The scarce empirical evidence has reported mixed results. The aftermath of a reform introducing a lower maximum tax rate for urban properties is the perfect laboratory to study whether there is an effect on housing prices. We take advantage of this reform to establish causality using a rich dataset on the universe of mainland Portuguese municipalities. We believe that this strategy allows for credible inference upon the causal effects of this reform. Since the present paper studies the impact of a nationwide policy, rather than a number of local jurisdictions, we also believe that it presents a higher degree of external validity. We perform several robustness checks and falsification tests to have a better view regarding possible mechanisms. Our findings suggest that agents rationally incorporate the decreased cost of living in their buying decisions, which are reflected in a market price increase with the net present value of the tax reduction. Our results thus corroborate standard capitalization theory. In our most conservative estimate, we observe an increase close to 5%.

Finally, in the third chapter, we analyze the political selection effects of introducing mayoral term limits in Portugal. For that, we compiled an original dataset on personal characteristics of Portuguese mayors at the past four local elections: 2001, 2005, 2009 and 2013 - a total of 586 individuals from the 278 mainland municipalities. Our identification strategy relies on a difference-in-differences approach estimating how these mayors' personal characteristics differ on average between municipalities with re-eligible and term-limited incumbents. The baseline results show that municipalities affected by the reform elected politicians, on average, older (around four years) and with a past political career. The results are robust to several falsification tests, in particular concerning any potential anticipation effects at the 2009 local election. This might suggest that before the implementation of term limits, being a mayor in Portugal could be considered as a career path, which one would stat at a young age and with no previous political experience. Nevertheless, in order to corroborate this idea we need explore further the implications of the reform, namely by looking at in detail at the type and number of years of political experience. We are able to perform this analysis, since we have also data on all elected councilmen.

Bibliography

Accetturo, A., F. Manaresi, S. Mocetti, and E. Olivieri (2014). Don't stand so close to me: the urban impact of immigration. *Regional Science and Urban Economics 45*, 45–56.

Altonji, J., T. Elder, and C. Taber (2005). Selection on observed and unobserved: Assessing the effectiveness of Catholic schools. *Journal of Political Economy 113*(1), 151–184.

Ambrosanio, M. F. and M. Bordignon (2006). Normative versus positive theories of revenue assignments in federations. In Ahmad and Brosio (Eds.), *Handbook of Fiscal Federalism*. Edward Elgar.

Angrist, J. and J. Pischke (2009). *Mostly harmless economics: An empiricist's companion*. Princeton: Princeton University Press.

Audretsch, D. B., D. Dohse, and J. P. dos Santos (2017). Do toll-free highways foster firm formation and employment growth? results from a quasi-natural experiment. Technical report, Kiel Working Paper.

Barro, R. (1973). The control of politicians: An economic model. *Public Choice 14*, 19–42.

Baskaran, T. (2014). Identifying local tax mimicking with administrative borders and a policy reform. *Journal of Public Economics 118*, 41–51.

Basten, C., M. von Ehrlich, and A. Lassmann (2017). Income taxes, sorting and the costs of housing: Evidence from municipal boundaries in switzerland. *The Economic Journal 127*(601), 653–687.

Besley, T. and A. Case (1995). Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits. *The Quarterly Journal of Economics 110*(3), 769–798.

Besley, T. and A. Case (2003). Political institutions and policy choices: Evidence from the united states. *Journal of Economic Literature 41*(1), 7–73.

Besley, T., N. Meads, and P. Surico (2014). The incidence of transaction taxes: Evidence from a stamp duty holiday. *Journal of Public Economics 119*, 61 – 70.

Best, M. C. and H. J. Kleven (2018). Housing market responses to transaction taxes: Evidence from notches and stimulus in the u.k. *The Review of Economic Studies 85*(1), 157–193.

Bloom, S. and H. F. Ladd (1982). Property tax revaluation and tax levy growth. *Journal of Urban Economics 11*, 73–84.

Boadway, R., M. Marchand, and M. Vigneault (1998). The consequences of overlapping tax bases for redistribution and public spending in a federation. *Journal of Public Economics 68*, 453–478.

Bradley, S. (2017). Inattention to deferred increases in tax bases: How michigan home buyers are paying for assessment limits. *Review of Economics and Statistics 99*(1), 53–66.

Cabral, M. and C. Hoxby (2012). The Hated Property Tax: Salience, Tax Rates, and Tax Revolts. *NBER Working Papers 18514*.

Cain, B., J. Hanley, and T. Kousser (2006). Term limits: A recipe for more competition? In M. Mc.Donald and J. Samples (Eds.), *The Marketplace for Democracy*. Washington D.C.: Brookings Institution Press.

Casaburi, L. and U. Troiano (2016). Ghost-house busters: The electoral response to a large anti–tax evasion program. *The Quarterly Journal of Economics 131*(1), 273–314.

Chari, V. V., L. E. Jones, and R. Marimon (1997). The economic of split-ticket voting in representative democracies. *American Economic Review 87*, 957–976.

Crain, W. and L. K. Oakley (1995). The politics of infrastructure. *Journal of Law and Economics 38*(1), 1–17.

Crain, W. M. and R. D. Tollison (1993). Time inconsistency and fiscal policy : Empirical analysis of U.S. States, 1969-89. *Journal of Public Economics 51*(2), 153–159.

Cushing, B. (1984). Capitalization of interjurisdictional fiscal differentials: An alternative approach. *Journal of Urban Economics 15*(3), 317–326.

Dachis, B., G. Duranton, and M. A. Turner (2012). The effects of land transfer taxes on real estate markets: evidence from a natural experiment in toronto. *Journal of Economic Geography 12*(2), 327–354.

Dal Bó, E., F. Finan, O. Folke, T. Persson, and J. Rickne (2017). Who Becomes A Politician?*. *The Quarterly Journal of Economics 132*(4), 1877–1914.

de Jong, P., M. Lindeboom, and B. van der Klaauw (2011). Screening disability insurance applications. *Journal of the European Economic Association 9*(1), 106–129.

Devarajan, S., S. Khemani, and S. Shah (2007). *The Politics of Partial Decentralization*. Washington: The World Bank.

Dhar, P. and S. L. Ross (2012). School district quality and property values: Examining differences along school district boundaries. *Journal of Urban Economics 71*(1), 18–25.

Downes, T. A. and J. E. Zabel (2002). The impact of school characteristics on house prices: Chicago 1987–1991. *Journal of Urban Economics 52*(1), 1–25.

Elinder, M. and L. Persson (2017). House price responses to a national property tax reform. *Journal of Economic Behavior and Organization 144*, 18–39.

Ely, J. C. and J. Välimäki (2003). Bad reputation. *The Quarterly Journal of Economics 118*, 785–814.

Fack, G. and J. Grenet (2010). When do better schools raise housing prices? evidence from paris public and private schools. *Journal of public Economics 94*(1-2), 59–77.

Ferejohn, J. (1986). Incumbent performance and electoral control. *Public Choice 50*, 5–25.

Figlio, D. N. and M. E. Lucas (2004). What's in a grade? school report cards and the housing market. *American Economic Review 94*(3), 591–604.

Fowler, L. (1992). A comment on competition and careers. In G. Benjamin and M. Malbin (Eds.), *Limiting Legislative Terms*. Washington D.C.: Congressional Quaterly Press.

Gibbons, S. and S. Machin (2008). Valuing school quality, better transport, and lower crime: evidence from house prices. *Oxford Review of Economic Policy 24*(1), 99–119.

Gibbons, S., S. Machin, and O. Silva (2013). Valuing school quality using boundary discontinuities. *Journal of Urban Economics 75*, 15–28.

Glaeser, E. (1997). Self-Imposed Term Limits. *Public Choice 93*, 389–394.

Glazer, A. and M. Wattenberg (1996). Promoting legislative work: A case for term limits. In B. Grofman (Ed.), *Legislative Term Limits: Public Choice Perspective.* Boston: Kluwer Academic Publishers.

Gonzalez, L. and F. Ortega (2013). Immigration and housing booms: Evidence from spain. *Journal of Regional Science 53*(1), 37–59.

Grofman, B. and N. Sutherland (1996). *The Effect of Term Limits When Competition is Endogenized: A Preliminary Model*, pp. 175–182. Studies in Public Choice. Boston: Kluwer Academic.

Hilber, C. (2015). The economic implications of house price capitalization: a synthesis. *Real Estate Economics 45*(2), 301–339.

Hilber, C. A. and T. Lyytikäinen (2017). Transfer taxes and household mobility: Distortion on the housing or labor market? *Journal of Urban Economics 101*, 57 – 73.

Hilber, C. A., T. Lyytikäinen, and W. Vermeulen (2011). Capitalization of central government grants into local house prices: Panel data evidence from England. *Regional Science and Urban Economics 41*(4), 394–406.

Kopczuk, W. and D. Munroe (2015). Mansion tax: The effect of transfer taxes on the residential real estate market. *American Economic Journal: Economic Policy 7*(2), 214–57.

La, V. (2015). Capitalization of school quality into housing prices: Evidence from boston public school district walk zones. *Economics Letters 134*, 102–106.

Ladd, H. F. (1991). Property tax revaluation and tax levy growth revisited. *Journal of Urban Economics 30*, 83–99.

List, J. A. and D. M. Sturm (2006). How Elections Matter: Theory and Evidence from Environmental Policy*. *The Quarterly Journal of Economics 121*(4), 1249–1281.

Lockwood, B. (2006). Fiscal Decentralization: a Political Economy Perspective. *Warwick Economic R. P. No. 721*.

Lopes da Fonseca, M. (2017). Identifying the source of incumbency advantage through a constitutional reform. *American Journal of Political Science 61*(3), 657–670.

Lopes da Fonseca, M. (2019). Lame Ducks and Local Fiscal Policy: Quasi-Experimental Evidence from Portugal. *The Economic Journal*.

Lyytikäinen, T. (2012). Tax competition among local governments: Evidence from a property tax reform in finland. *Journal of Public Economics 96*(7), 584–595.

Maskin, E. and J. Tirole (2004). The politician and the judge: Accountability in government. *American Economic Review 94*, 1034–1054.

Meyer, B. D. (1995). Natural and quasi-experiments in economics. *Journal of Business and Economic Statistics 13*(2), 151–61.

Mian, A., K. Rao, and A. Sufi (2013). Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics 128*(4), 1687–1726.

Morris, S. (2001). Political correctness. *Journal of Political Economy 109*, 231–265.

Moser, P. and A. Voena (2012). Compulsory licensing: Evidence from the trading with the enemy act. *American Economic Review 102*(1), 396–427.

Neumark, D. and H. Simpson (2015). Place-based policies. In *Handbook of Regional and Urban Economics*, Volume 5, pp. 1197–1287. Elsevier.

Norregaard, J. (2013). Taxing Immovable Property: Revenue Potential and Implementation Challenges. *IMF Working Papers No 13/129*.

Nunes, L. C., A. B. Reis, and C. Seabra (2015). The publication of school rankings: A step toward increased accountability? *Economics of Education Review 49*, 15–23.

Oates, W. E. (1969). The effects of property taxes and local public spending on property values: An empirical study of tax capitalization and the tiebout hypothesis. *Journal of Political Economy 77*(6), 957–971.

Oates, W. E. (1972). *Fiscal Federalism*. Harcourt Brace Jovanovic.

Oates, W. E. (1988). On the nature and measurement of fiscal illusion: A survey. In G. Brennan, S. Grewel, and P. Groenwegen (Eds.), *Taxation and Fiscal Federalism*. Sydney: Australian National University Press.

OECD (1999). Taxing powers of state and local government. *Tax policy studies No 1*, Paris.

Palmon, O. and B. A. Smith (1998). New evidence on property tax capitalization. *Journal of Political Economy 106*(5), 1099–1111.

Pei, Z., J.-S. Pischke, and H. Schwandt (2017, March). Poorly measured confounders are more useful on the left than on the right. Working Paper 23232, National Bureau of Economic Research.

Peralta, S. and J. Pereira dos Santos (2018). Who seeks re-election: Local fiscal restraints and political selection. Technical report, Gabinete de Estratégia e Estudos, Ministério da Economia.

Peralta, S. and J. Pereira dos Santos (2019). Who seeks reelection: local fiscal restraints and political selection. *Public Choice*.

Querubin, P. (2016). Family and politics: Dynastic persistence in the philippines. *Quarterly Journal of Political Science 11*(2), 151–181.

Ries, J. and T. Somerville (2010). School quality and residential property values: evidence from vancouver rezoning. *The Review of Economics and Statistics 92*(4), 928–944.

Ross, J. M. and W. Yan (2013). Fiscal Illusion from Property Reassessment: An Empirical Test of the Residual View. *National Tax Journal 66*(1), 7–32.

Ross, S. and J. Yinger (1999). Sorting and voting: A review of the literature on urban public finance. In P. C. Cheshire and E. S. Mills (Eds.), *Handbook of Regional and Urban Economics*, Volume 3 of *Handbook of Regional and Urban Economics*, Chapter 47, pp. 2001–2060. Elsevier.

Sá, F. (2015). Immigration and house prices in the uk. *The Economic Journal 125*(587), 1393–1424.

Saiz, A. (2007). Immigration and housing rents in american cities. *Journal of urban Economics 61*(2), 345–371.

Sirmans, S., D. Gatzlaff, and D. Macpherson (2008). The history of property tax capitalization in real estate. *Journal of Real Estate Literature 16*(3), 327–344.

Slemrod, J., C. Weber, and H. Shan (2017). The behavioral response to housing transfer taxes: Evidence from a notched change in dc policy. *Journal of Urban Economics 100*, 137–153.

Smart, M. and D. M. Sturm (2013). Term limits and electoral accountability. *Journal of Public Economics 107*, 93–102.

Strumpf, K. (1999). Infrequent Assessments Distort Property Taxes: Theory and Evidence. *Journal of Urban Economics 46*(2), 169–199.

Tabarrok, A. (1996). Term limits and political conflict. In B. Grofman (Ed.), *Legislative Term Limits: Public Choice Perspective*. Boston: Kluwer Academic Publishers.

Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of Political Economy 64*(5), 416–424.

Yinger, J. (1982). Capitalization and the theory of local public finance. *Journal of Political Economy 90*(5), 917–943.

Appendix A

Chapter 1

A.1 Media Coverage


iInformação 02 Dez 2011 Sexta Lisboa 13ºC

Hoje Portugal Mundo Dinheiro Boa Vida Desporto iTV iFotogaleria Opinião iSab

Abriu a caça ao IMI. Fisco já está a avaliar mais de 5 milhões de casas em todo o país. Cuidado

Por António Ribeiro Ferreira, publicado em 2 Dez 2011 - 03:00 | Actualizado há 13 horas 18 minutos

Trabalho tem de estar pronto até final de 2012 para o Estado receber mais dinheiro no imposto cobrado em 2013

Imagem

Imprimir Enviar

0 Share

Tweet


Diário de Notícias

IMI: Fisco começa a avaliar cinco milhões de imóveis

01 DE DEZEMBRO DE 2011 ÀS 11:40


Jornal de Notícias

Início | Multimédia | Blogues | Tendências | Opinião | Dossiês | Cidadão Repórter | Serviços

Política Sociedade Segurança Economia País Mundo Desporto Cultura Gente Tecnologia Últimas

Economia

Fisco começa hoje a avaliar cinco milhões de imóveis para efeitos de IMI

Publicado

Share Tweet Share

O fisco começa esta quinta-feira a avaliar cerca de cinco milhões de imóveis que não foram transacionados desde 2004, quando entraram em vigor as novas regras do IMI, dando cumprimento ao estabelecido no segundo Orçamento Rectificativo deste ano, publicado quarta-feira.

A avaliação vai incidir sobretudo

FERRAMENTAS

Comentar

Imprimir

Distribuir

Enviar

Aumentar

Diminuir

ESTATÍSTICAS

11407 Visualizações

58 Comentários

18 Envios

45 Impressões

Patrocínio


SOL

Pesquisa sapo.pt

Início Opinião Política Sociedade Economia Internacional Cultura Desporto Tecnologia Vida

Fisco começa hoje a avaliar 5 milhões de imóveis para efeitos de IMI

Tweet Share

1 de Dezembro, 2011

O fisco começa hoje a avaliar cerca de cinco milhões de imóveis que não foram transaccionados desde 2004, quando entraram em vigor as novas regras do IMI, dando cumprimento ao estabelecido no segundo Orçamento Rectificativo deste ano, publicado quarta-feira.

A avaliação vai incidir sobretudo sobre prédios urbanos que ainda

Imprimir

Aumentar texto

Comentar

Partilhar

Mais Notícias »

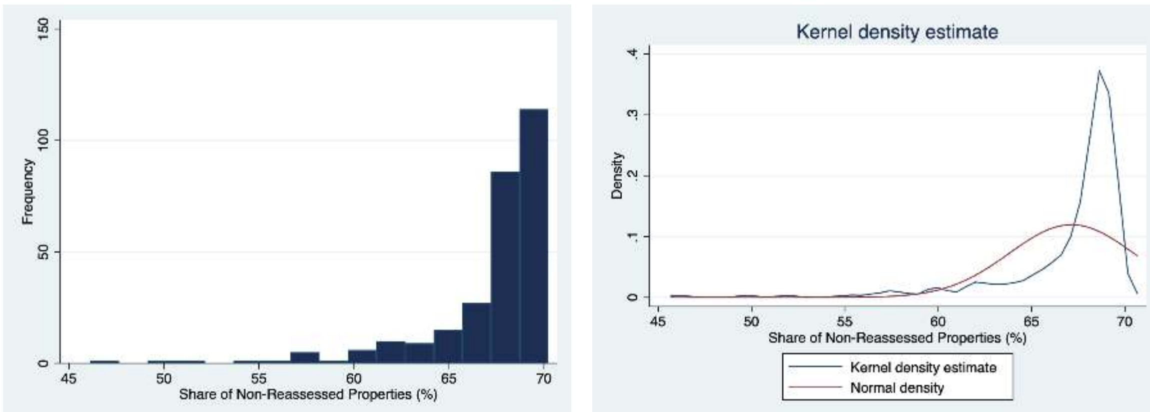
Honda quer recuperar lucros com lançamento do novo Civic

Juros batem máximos em Portugal

Reformados bancários continuam a receber 14 pensões

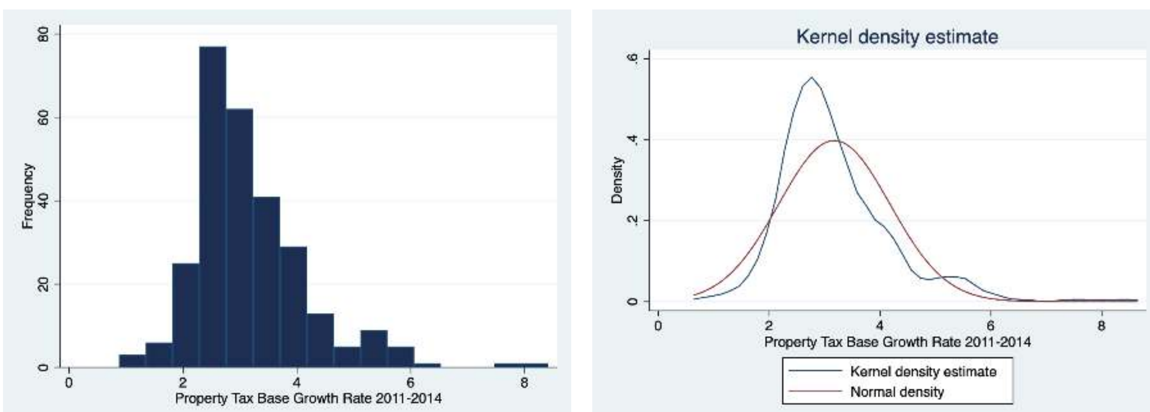
A.2 Treatment Intensity - Distribution

Figure A.1: **Intensity of Treatment - Histogram and Density**



A.3 Tax Base Growth Rate - Distribution

Figure A.2: **Property Tax Base Growth Rate - Histogram and Density**



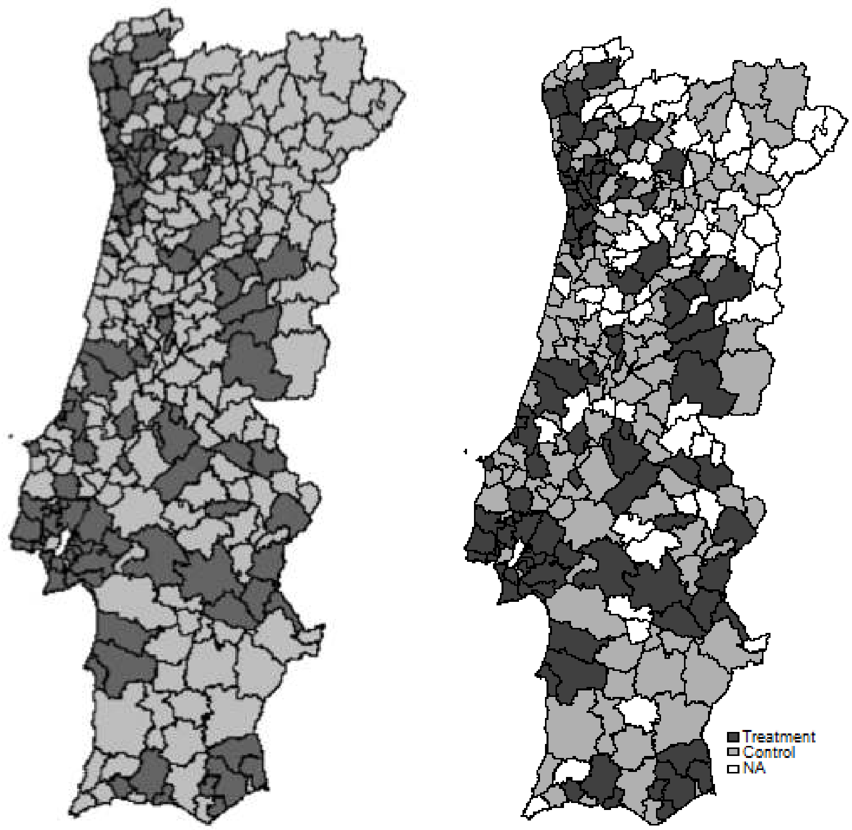
Appendix B

Chapter 2

B.1 Treated vs. Control Municipalities

Figure B.1: **Treated vs. Control Municipalities**

(a) Complete Sample (b) Restricted Sample



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

B.2 Robustness I: Different Clustered Standard Errors

NUTS III Level Clustering

Table B.1: Property Tax Reform Effects on Urban Real Estate Transactions

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

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

Regional Level Clustering

Table B.2: Property Tax Reform Effects on Urban Real Estate Transactions

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

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

Appendix C

Chapter 3

C.1 Internal Validity

C.1.1 Common Trends: Controlling for Observables

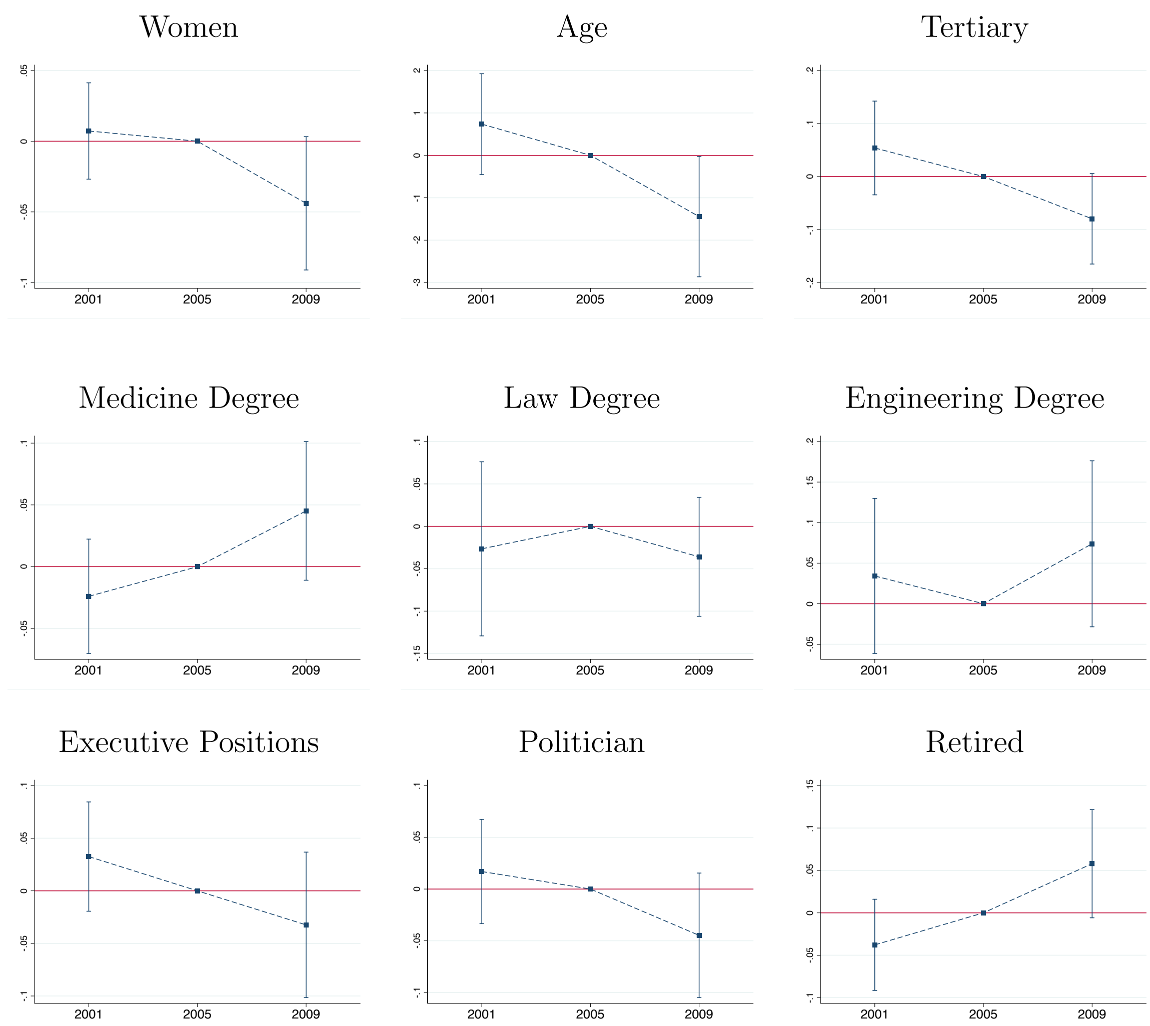


Figure C.1: **Year Effects** Plots depict the coefficients estimates obtained from regressing Equation 3.3 on each mayors' characteristic outcome variable during the four local elections: 2001, 2005, 2009 and 2013. The y-axis lists the outcome variables; The x-axis indicates the years. All estimates include municipality a vector of control variables. Capped lines indicate 95 % confidence intervals

C.1.2 Controlling for Anticipation Effects

Baseline Results: 2001, 2005 and 2013

Table C.1: **Treatment Effects: 2001, 2005 and 2013**

	(1)	(2)	(3)	(4)	(5)	(6)
Women	-0.059	-0.059	-0.058	-0.066*	-0.064*	-0.065*
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	834	834	834	834	834	834
Age	3.302***	3.454***	3.393***	3.523***	3.488***	3.451***
	(1.20)	(1.18)	(1.20)	(1.20)	(1.19)	(1.21)
Obs.	825	825	825	825	825	825
Tertiary Education	-0.102	-0.100	-0.101	-0.113*	-0.107*	-0.107*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Obs.	732	732	732	732	732	732
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.2: **Education Area - 2001, 2005 and 2013**

	(1)	(2)	(3)	(4)	(5)	(6)
Medicine Degree	0.034	0.029	0.027	0.027	0.019	0.018
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	606	606	606	606	606	606
Law Degree	-0.109*	-0.131**	-0.134**	-0.116*	-0.136**	-0.137**
	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)
Obs.	606	606	606	606	606	606
Engineering Degree	0.008	0.021	0.028	0.009	0.019	0.027
	(0.08)	(0.08)	(0.08)	(0.07)	(0.07)	(0.08)
Obs.	606	606	606	606	606	606
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.3: **Previous Professional Occupation - 2001, 2005 and 2013**

	(1)	(2)	(3)	(4)	(5)	(6)
Executive Positions	-0.024	-0.028	-0.030	-0.021	-0.036	-0.038
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Obs.	825	825	825	825	825	825
Politician	0.089**	0.083*	0.083*	0.081*	0.081*	0.081*
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Obs.	825	825	825	825	825	825
Retired	-0.009	-0.005	-0.005	-0.004	-0.006	-0.005
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Obs.	825	825	825	825	825	825
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Year Effects: 2001, 2005, 2009 and 2013

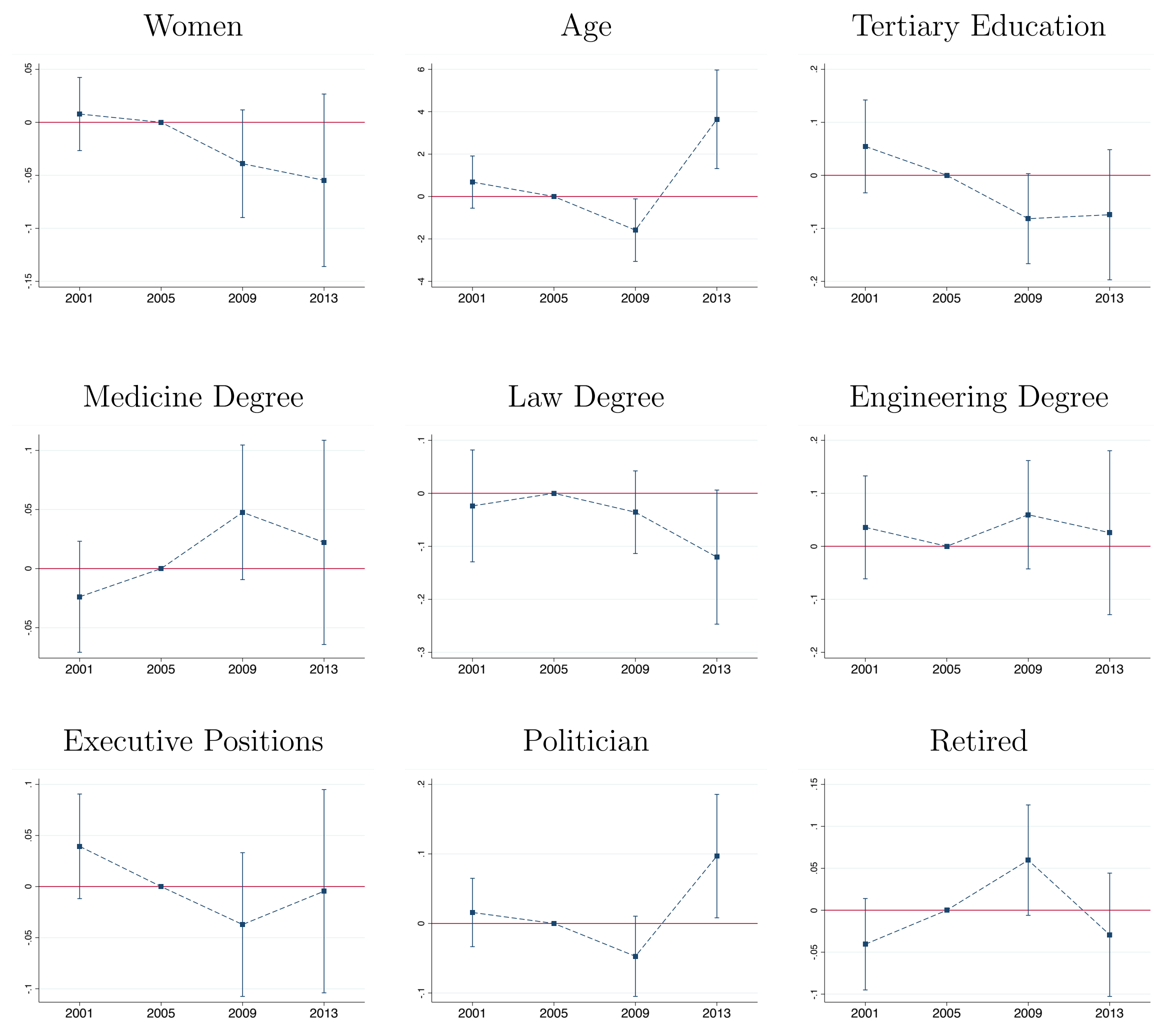


Figure C.2: **Year Effects** Plots depict the coefficients estimates obtained from regressing Equation 3.3 on each outcome variable during four local elections: 2001, 2005, 2009 and 2013. The baseline year is 2005. The y-axis lists the outcome variables. The x-axis indicates the years. Capped lines indicate 95 % confidence intervals

Year Effects: 2001, 2005 and 2013

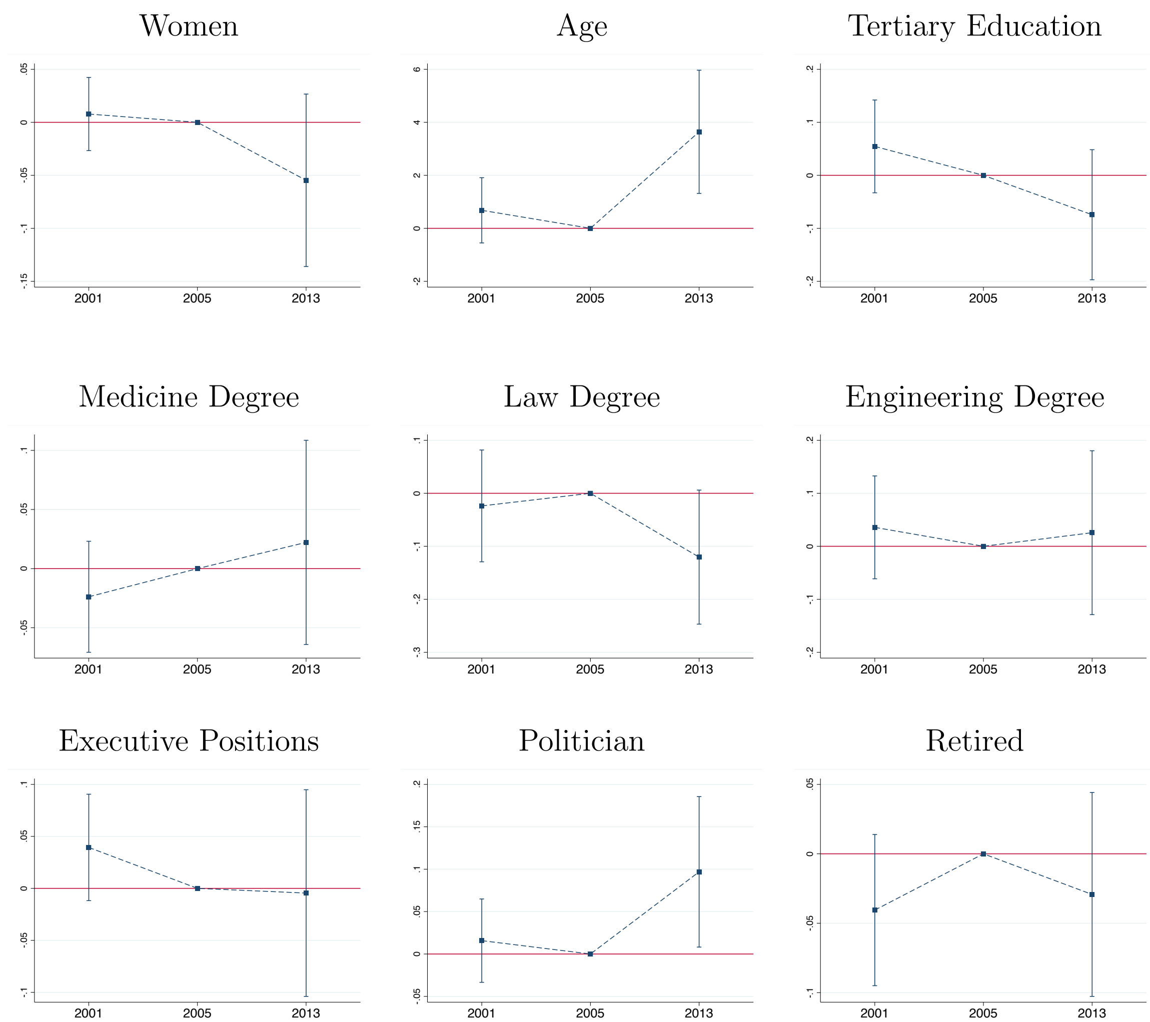


Figure C.3: **Year Effects** Plots depict the coefficients estimates obtained from regressing Equation 3.3 on each outcome variable during four local elections: 2001, 2005 and 2013. The baseline year is 2005. The y-axis lists the outcome variables. The x-axis indicates the years. Capped lines indicate 95 % confidence intervals

Without mayors that didn't rerun in 2009

Table C.4: **Treatment Effects**

	(1)	(2)	(3)	(4)	(5)	(6)
Women	-0.004	-0.003	0.007	-0.014	-0.013	-0.003
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	948	948	948	948	948	948
Age	4.323***	4.236***	4.058***	4.770***	4.301***	4.129***
	(1.12)	(1.11)	(1.19)	(1.11)	(1.08)	(1.15)
Obs.	936	936	936	936	936	936
Tertiary Education	-0.028	-0.019	-0.031	-0.036	-0.024	-0.033
	(0.05)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)
Obs.	840	840	840	840	840	840
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panels B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.5: **Treatment Effects: Education Area**

	(1)	(2)	(3)	(4)	(5)	(6)
Medicine Degree	0.022	0.015	0.012	0.021	0.009	0.002
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	696	696	696	696	696	696
Law Degree	-0.085	-0.117*	-0.135**	-0.099	-0.123**	-0.138**
	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.07)
Obs.	696	696	696	696	696	696
Engineering Degree	-0.064	-0.054	-0.060	-0.060	-0.047	-0.052
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Obs.	696	696	696	696	696	696
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panels B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.6: **Treatment Effects: Occupation**

	(1)	(2)	(3)	(4)	(5)	(6)
Executive Positions	0.015	0.033	0.030	0.022	0.033	0.032
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Obs.	936	936	936	936	936	936
Politician	0.109**	0.104**	0.100**	0.105**	0.099**	0.095**
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
Obs.	936	936	936	936	936	936
Retired	-0.061*	-0.071**	-0.061*	-0.053	-0.061*	-0.052
	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)
Obs.	936	936	936	936	936	936
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panels B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

C.1.3 Selection Bias

Placebo Tests

Table C.7: **Placebo Test: 2001 and 2005**

	(1)	(2)	(3)	(4)	(5)	(6)
Women	-0.008	-0.013	-0.013	-0.012	-0.013	-0.013
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	556	556	556	556	556	556
Tertiary Education	-0.055	-0.053	-0.053	-0.052	-0.052	-0.052
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Obs.	488	488	488	488	488	488
Age	-0.680	-0.455	-0.455	-0.725	-0.604	-0.604
	(0.62)	(0.61)	(0.61)	(0.60)	(0.62)	(0.62)
Obs.	550	550	550	550	550	550
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.8: **Placebo Test: Education Area - 2001 and 2005**

	(1)	(2)	(3)	(4)	(5)	(6)
Medicine Degree	0.024	0.029	0.029	0.027	0.027	0.027
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Obs.	404	404	404	404	404	404
Law Degree	0.024	0.027	0.027	0.019	0.024	0.024
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Obs.	404	404	404	404	404	404
Engineering Degree	-0.036	-0.056	-0.056	-0.035	-0.057	-0.057
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Obs.	404	404	404	404	404	404
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.9: **Placebo Test: Occupation - 2001 and 2005**

	(1)	(2)	(3)	(4)	(5)	(6)
Executive Positions	-0.039	-0.037	-0.037	-0.036	-0.038	-0.038
	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Obs.	550	550	550	550	550	550
Politician	-0.016	-0.020	-0.020	-0.019	-0.023	-0.023
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Obs.	550	550	550	550	550	550
Retired	0.041	0.044	0.044	0.038	0.039	0.039
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Obs.	550	550	550	550	550	550
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panel B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

C.1.4 Controlling for Missing Information

Table C.10: **Treatment Effects**

	(1)	(2)	(3)	(4)	(5)	(6)
Women	-0.065	-0.064	-0.055	-0.073*	-0.067	-0.055
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	796	796	796	796	796	796
Age	4.474***	4.861***	4.815***	4.390***	4.698***	4.703***
	(1.19)	(1.16)	(1.23)	(1.18)	(1.19)	(1.26)
Obs.	796	796	796	796	796	796
Tertiary Education	-0.101*	-0.102*	-0.110*	-0.106*	-0.103*	-0.108*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Obs.	796	796	796	796	796	796
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panels B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.11: **Treatment Effects: Education Area**

	(1)	(2)	(3)	(4)	(5)	(6)
Medicine Degree	0.014	0.008	0.006	0.005	-0.003	-0.006
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Obs.	796	796	796	796	796	796
Law Degree	-0.112*	-0.125**	-0.132**	-0.121**	-0.129**	-0.135**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Obs.	796	796	796	796	796	796
Engineering Degree	0.002	0.012	0.016	-0.001	0.003	0.005
	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	(0.07)
Obs.	796	796	796	796	796	796
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panels B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.

Table C.12: **Treatment Effects: Occupation**

	(1)	(2)	(3)	(4)	(5)	(6)
Executive Positions	-0.023	-0.017	-0.013	-0.018	-0.020	-0.015
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Obs.	796	796	796	796	796	796
Politician	0.107**	0.095*	0.088	0.093*	0.085*	0.078
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Obs.	796	796	796	796	796	796
Retired	-0.043	-0.036	-0.028	-0.043	-0.037	-0.028
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Obs.	796	796	796	796	796	796
District trends		✓			✓	
District x year FE			✓			✓
Controls				✓	✓	✓

Note: Results are obtained from the estimation of Equation (3.1). All estimates include municipality and year fixed effects. Refer to panels B of Table 3.6 for the list of control variables. Robust standard errors clustered at municipal level are presented in parentheses. *, ** and *** denote 10%, 5% and 1% significance, respectively.